MINUTES OF EVIDENCE

TAKEN BY THE

INDIAN PLAGUE COMMISSION

WITH

APPENDICES.

VOL. III.

EVIDENCE TAKEN FROM 11TH FEBRUARY 1899 TO 20TH MAY 1899.

Presented to both Houses of Parliament by Command of Her Majesty.

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THE INDIAN PLAGUE COMMISSION.

VOLUME III.

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MINUTES OF EVIDENCE

TAKEN BEFORE THE

INDIAN PLAGUE COMMISSION.

NOTE.—Each witness, as far as was possible, put in a précis of the evidence he was prepared to give. The précis, when available, was printed, and copies of it were distributed to the Members of the Commission prior to each witness' examination. The précis does not form a part of this record of the Commission's Proceedings, but is referred to in the questions put to witnesses in examination.

At The Secretariat, Bombay.

Saturday, 11th February 1899.

FORTY-FIFTH DAY.

PRESENT:

PROF T. R. FRASER, M.D., L.L.D., F.R.S. (*President*).

Prof. A. E. WRIGHT, M.D.
Mr. A. CUMINE.
Dr. M. A. RUFFER.

Mr. C. J. HALLIFAX (*Secretary*).

Mr. R. B. STEWART, I.C.S., called and examined.

Mr. R. B. Stewart, I.C.S.

11 Feb. 1899

16,878. (*The President.*) You are Secretary to the Government of Bombay?—I am Acting Secretary.

16,879. You have had experience of plague both in the Nasik district and in Bombay?—Yes.

16,880. In what part of the Nasik district was your first experience?—In Igatpuri.

16,881. At what time did the plague occur there?—We found it first on the 3rd of August 1897. I went down there on that day with Dr. Anderson, who was the Deputy Sanitary Commissioner. We had heard that there were cases there, and we went down to enquire into them. I was Acting Collector and District Magistrate at the time. We found that there were cases, and we reckoned that they must have begun about a fortnight before that—about the middle of July.

16,882. How did you get information?—We heard on the railway line; we were out inspecting a famine camp, and we heard from the railway people that there were cases at Igatpuri.

16,883. Was there no arrangement for obtaining early information of plague cases in this place?—There was a railway surgeon there, but he was not very energetic over it. I think he was rather afraid of going into the houses and examining the patients. The Mamlatdar would have reported the cases had he known of them, but there was no medical man, other than the railway surgeon, to tell him that cases were actually occurring.

16,884. Did you ascertain where the plague had come from?—As far as we could make out it came either from Bombay or Poona. There was plague at both those places at the time. One lot of people had come from Bombay and one lot had come from Poona. We could not make out definitely which had established the plague in Igatpuri; there were cases of plague in both families.

16,885. What action did you take?—A lot of the people had run away.

16,886. Why?—They ran away directly cases occurred.

16,887. They were frightened of the plague?—Yes, they were frightened on account of the plague. Of course at that time of the year it was quite impossible to put anybody out into camp, or huts. The rainfall averaged about 1 inch a day. It was just at the edge of the Ghauts, and the total rainfall during the five months of the monsoon is between 100 and 120 inches, so that we could not do anything in the way of putting people outside. We closed the Municipal school, and took the building for a hospital, and then all we could do was to put the people attacked into hospital, to clear up and disinfect houses which had been vacated by their owners, and put the people from infected houses into them.

16,888. By cleaning up do you mean something different from disinfection?—We had them emptied and washed out and brushed out, and flushed down with perchloride.

16,889. What effect did these measures have on the epidemic; was it checked, or not checked?—I should say it was not checked; it ran its course. The town is really only a long strip alongside the Bombay-Agra road, and there are about two rows of native houses.

16,890. The chief measure you adopted was disinfection?—Yes, and the removal of patients to hospital.

16,891. Notwithstanding that, plague extended?—Yes, it went right through.

16,892. How long did it last?—It lasted for just about 13 weeks.

16,893. Do you know the total number of cases?—No, I could not tell you that.

16,894. What is the total population of Igatpuri?—About 6,000.

16,895. What was the total number of patients?—I cannot give the number of patients in hospital. There were 518 cases of plague and 421 deaths.

Mr. R. B. Stewart, I.C.S.

11 Feb. 1899.

16,896. People ran away. Did they carry the plague elsewhere?—Yes. As far as we could tell, they took it first into Ghoti, which is about four miles off, and is a considerable market town in that part; it is the largest market village about there. They took plague there first. We did not hear about it for some time, and then I heard rumours of cases there, so I sent the Mamlatdar to make inquiries. He went there with an Assistant Surgeon and others and they were assaulted, and several of them badly hurt.

16,897. Do you know why you did not get early information?—Simply from the neglect of the village officials.

16,898. You had an organisation in operation?—We had no organisation, except orders to the officials to keep plague-stricken people out, to be very careful who they let into the village, and to report at once any increase in the number of deaths.

16,899. How large is Ghoti?—The population is about 2,000.

16,900. You had nothing in the nature of house-to-house visitation?—Nothing whatever.

16,901. What action did you take in each case?—Immediately after the riot nearly all the people fled away.

16,902. Where to?—To different villages. Of course there was a police inquiry into the riot. We turned all the rest of the people out, the few who remained—about 200 or so—into huts. Ghoti is a little further away from the edge of the Ghauts than Igatpuri, and the riot took place at the end of September, when the rains were beginning to cease, so we turned them out and disinfected their houses. They got rid of the plague very shortly, but of course the people had nearly all left the village.

16,903. In what time did those that remained become free of plague—the segregated people?—They had a few cases for a week or two only, and then it stopped altogether.

16,904. Another village which was affected was Munmar?—Yes, that was a separate infection. That was not infected from Igatpuri. It is a good deal further up the line; it is a junction between the Dhond and Munmar State Railway and the G. I. P. main line.

16,905. Was your action similar in this case to the last one?—Yes, except that there we got most of the people out of their houses.

16,906. Where did they go to?—They went into their fields; we had better examination there. We had men down there, and they examined the people who remained in the village, and also the people in the fields.

16,907. Those that had removed from their original houses?—Yes, from their houses into their fields; it was dry then.

16,908. Were the houses disinfected?—Yes.

16,909. How long did they remain out of the village?—I could not say definitely how long they remained out. They were not all back when I left Nasik. I left Nasik in February 1898.

16,910. In what month did they go out?—In September and October.

16,911. Had plague ceased when you left?—It had not absolutely ceased.

16,912. That measure, also, was not very effective?—There were very few cases. Of course it is very difficult to control them unless you can keep them all in a camp.

16,913. Were not these people in a camp?—They were not in a camp; they were in the fields.

16,914. When out of control, how do you think the plague continued to extend?—That is very difficult to say. Of course they got back to their houses at night.

16,915. Were not the doors sealed?—No.

16,916. Did they go back, to your knowledge, and sleep in those houses?—They did not go back with our permission.

16,917. Did they sleep in their houses?—Some of them did, undoubtedly.

16,918. You had plague in Nasik itself?—Yes.

16,919. That is a larger town?—It is a town of about 25,000 people.

16,920. Do you know when plague first occurred in Nasik?—The first indigenous cases occurred on the 12th of October 1897.

16,921. Do you know how it was introduced into Nasik?—I believe it was from Ghoti.

16,922. On what grounds do you base that belief?—I did not personally investigate the first one or two cases, so I could not be certain.

16,923. Were you told so, or did you fancy it was so?—I was told by the Civil Surgeon, but I would not be certain whether it was from Bombay or by people coming from Ghoti.

16,924. Did you take measures immediately after the knowledge of the outbreak in Nasik?—We started disinfecting the houses at once.

16,925. What houses?—The houses in which cases occurred.

16,926. How did you effect the disinfection if the people were still in their houses?—The people were taken out.

16,927. The first thing you did was to remove them?—Yes.

16,928. Then you disinfected the houses?—Yes.

16,929. Did you remove any people who lived in the neighbourhood of the infected houses?—Not at the very beginning.

16,930. How long did you wait before you did this?—About a fortnight or three weeks after the first cases. Then we began more wholesale removals.

16,931. In this fortnight in which you were not removing people from neighbouring houses, what was the course of the epidemic: was it checked by the measures then adopted or not?—No.

16,932. Then you adopted more stringent measures?—Then we began moving people out of houses on each side.

16,933. How many houses on each side?—At first we took two on each side; then we found that that was not enough, so we took three on each side, and six opposite. Still it went on, so then we evacuated whole quarters. The town was divided up into wards, and we evacuated gradually the wards as they got infected.

16,934. The wards were completely evacuated?—Yes.

16,935. And a large part of the city was therefore finally evacuated?—The greater part, nearly the whole.

16,936. One side of the river?—One side of the river, yes.

16,937. What was the result of the larger measure—of these final steps?—The plague went on of course. It resulted in, I think, a comparatively small number of cases. There were only about 450 cases in the whole town.

16,938. Before you adopted the final measures how many cases were there?—There were 40 cases up to the second week in November. Then evacuation by wards was commenced.

16,939. After you adopted the final steps how many cases did you have?—It was still going on. We did not evacuate the whole town at any one time.

16,940. The greatest step you took, I understand, was to evacuate all on one side of the river?—It was not all done at once.

16,941. Was the final result that the whole town was evacuated?—All on that side of the river.

16,942. When was that result accomplished?—In the first week in January the last of the blocks on the south of the Godaveri River was evacuated.

16,943. How many cases occurred after the final evacuation?—57 in January and 18 in February. I left Nasik on February 15th.

16,944. Were you fairly well satisfied with the effect of the final action you took?—Yes, I think we would have done better had we evacuated the town more rapidly.

16,945. Could you state generally how it affected the epidemic?—Of course I had not seen other places, but, compared with the figures given for other places, I think the number of cases we had was comparatively small.

16,946. When did the disease finally cease in Nasik?—I was not there. There were a few cases after I left

in February. The last cases I have got down here are for the week ending the 25th of March 1898. There have been no cases since then until about three weeks ago.

16,947. You cannot speak of these?—No.

16,948. I think you assumed work in Bombay in the middle of February 1898?—Yes.

16,949. Which ward had you charge of?—Edward Byculla and Mazagon.

16,950. Is that a large ward?—Yes.

16,951. What is the normal population?—Between 180,000 and 185,000.

16,952. What steps did you adopt here?—Chiefly disinfection. We removed patients to the hospital, and contacts; but of course the contacts are a very different matter in Bombay to what they are up country.

16,953. Where did you remove the contacts to?—To camps; we had different camps; I had one large camp in Byculla, and there was a camp attached to the Mahratta Hospital, to which a number of people went.

16,954. This measure was interrupted, was it not?—Yes. It was interrupted on the 9th of March 1898.

16,955. How was that?—It was interrupted by a riot.

16,956. Subsequently to the riot what has been the procedure?—We continued much the same procedure, except that the wholesale evacuation of large chawls was lessened. Of course that could not go on after the rains began, because almost all vacant spaces for camps are on low-lying ground, where the water lies.

16,957. I think we already have the figures showing the extent of the epidemic at this time?—Yes.

16,958. That constitutes your personal experience?—Yes.

16,959. Have you any knowledge of the influence of rats in the propagation of the plague?—No, I have not.

16,960. You have had considerable opportunity of observing the plague in different places. Can you associate its extension anywhere with rats?—No, I have never been able to trace the spread of infection to rats.

16,961. Have you any opinion on that subject based on what you have observed?—No, I have had no experience of rats at all.

16,962. Have you seen plague extend itself where there appeared to be no conveyance by means of rats?—We had no signs of conveyance by means of rats in Nasik at all.

16,963. You had no appearance of dead rats?—We found dead rats occasionally.

16,964. Not in large numbers?—No, and we had no means of having them examined.

16,965. They were not examined bacteriologically?—No; but they were certainly not found anywhere there in large numbers.

16,966. How, then, do you think that plague is extended or carried from one house to another, or from one district to another?—As far as I can see it is brought in by people who are infected. People come from infected places and live in clean houses, and apparently after some while they infect the houses. We had imported cases from time to time in Nasik. The first case I saw was in March 1897. That was an imported case, The house was disinfected and nothing more happened. And then there were similar cases from time to time up till October, when we got indigenous cases.

16,967. In your experience, if you get early information of an imported case, and deal with that case as you have described, plague may not become indigenous in a town or village?—I think so if you get early information.

16,968. Early information seems to be a matter of great importance?—It is.

16,969. As to other methods to adopt in dealing with an epidemic, upon what do you place the greatest trust?—Evacuation and cleaning.

16,970. Cleaning, including disinfection?—Yes, including disinfection, but also including cleaning the surroundings.

16,971. Do you generally find the surroundings very unhealthy?—Yes, particularly in Bombay.

16,972. In Nasik?—In Nasik they are not so bad. Of course you do not get so much water lying about here; it is not so damp.

16,973. What kind of houses are they in Nasik—in the plague district?—In Nasik itself there are all kinds of houses; there are very poor houses. Plague began in the south.

16,974. Are they small houses?—Yes, many of them.

16,975. One floor?—They are chiefly one floor.

16,976. Of how many chambers does a house consist?—Two or three, sometimes; often there is only one room.

16,977. Generally speaking did you find that the worse the houses in a district, the more prevalent plague was?—No, I could not say that.

16,978. Still, all the houses in which it chiefly occurred were of a bad character?—They were bad in some way or another. Some of the houses were exceedingly well built—the big houses, but they are dark, badly lighted, and badly ventilated.

16,979. Is there much overcrowding in Nasik in the poor houses?—No, I do not think there is much overcrowding.

16,980. Supposing you take one of these houses, consisting of one chamber, how many people would reside in such a house?—Probably only the father and mother and two or three children.

16,981. How large is this chamber?—About 10 to 12 feet square.

16,982. This is undoubtedly overcrowded, is it not?—Of course that is overcrowded, but there is nothing like, for instance, the chawls in Bombay.

16,983. What means of ventilation would a one-chamber house have?—Practically nothing but the doorway; there is a window, but it is generally blocked up.

16,984. What was the kind of floor?—Mud.

16,985. In Bombay you think the sanitary condition is worse than in Nasik?—It is worse; because there are more people, and the overcrowding is very great.

16,986. I think you said also that they are dirtier?—Yes, they are dirtier. I am talking particularly of the chawls; the occupants have less interest in them, as they are not their own houses.

16,987. In the last paragraph of your précis of evidence you have expressed your opinion as to the methods of dealing with the disease?—Yes, it is as follows:—"As regards methods of dealing with the disease, I can speak only of evacuation and disinfection, as I have practically no experience of inoculation. In the efficacy of evacuation and disinfection I fully believe, and I would resort to wholesale evacuation wherever possible. In the mufassil such measures are naturally less difficult of accomplishment than in Bombay, and in Nasik I had the great advantage of having been in the district for some years, so that the people knew me, but even in Bombay I believe that evacuation could be largely carried out. It is difficult, of course, to deal with a large shifting population, and especially when the officers in charge and the people are mutually unacquainted, but it is not impossible, and though inoculation may assist by giving confidence, I would rather trust to evacuation with disinfection. Thorough cleaning is one of the most difficult things to carry out, and especially when an epidemic is severe. The coolie does not understand, even after months of instruction, that the whole work of cleansing a room may be nullified by his leaving a corner untouched, and the best of them will not grasp the importance of cleaning every spot inside and out. The consequence is that an immense amount of personal supervision is required, supervision over the materials and their preparation, supervision of the men and examination of their completed work, but if a house or row of houses is once thoroughly cleaned, and can be kept reasonably clean, as far as I have seen, there is little danger of plague breaking out there again. House-to-house visitation and inspection are practicable in the mufassil when the families living in the different houses are known and the absence of any one member is at once noticed, but even then it is difficult to make it effectual without considerable annoyance to the people. In Bombay, with its immense chawls full of lodgers, who have little kit, and can shift at any moment, house-to-house visitation is of comparatively little use, and the only way to discover cases of plague seems to be to employ

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spies, and to keep as careful a watch as possible on the cemeteries. Many cases have been discovered in large searches with troops, but such searches cause great alarm, and I would not use troops until there is good reason to believe that an area is badly infected, and other means of discovering cases have failed."

16,988. Have you anything further to say with regard to this paragraph, or as to any other point that I have asked you about?—Not as regards the value of evacuation and disinfection.

16,989. You consider they are of great value and of paramount importance?—Yes.

16,990. (*Dr. Ruffer.*) Did you find that the people at Nasik began to leave before you were informed of the presence of plague?—Not at the beginning.

16,991. They only began to leave after you arrived?—After we began to take measures.

16,992. You mention in your précis of evidence "the "newly-organised flying columns." What did each of the flying columns consist of?—The one we had at Igatpuri had an Assistant Surgeon in charge, and he had two Hospital Assistants under him. Then they brought up, I think it was, two gangs of trained coolies from Bombay, who knew the disinfection work.

16,993. How many men were there in each gang?—As far as I remember there were ten in each gang. Then, of course, we supplied other coolies who learnt the work from them.

16,994. Who prepared the disinfecting solutions, the perchloride of mercury, and so on?—In Igatpuri the Assistant Surgeon did it. It was done there by powder in the first instance; the powder was poured into the tubs and stirred up.

16,995. Did you add any acid?—Yes.

16,996. How much?—I do not know. After that, in Nasik, it was prepared chiefly by Dr. Maynard.

16,997. In how many plague houses did you find dead rats?—I could not give you the numbers.

16,998. Would the number of houses be considerable?—No. We found no rats practically; I do not think we found a single rat more than you would find in the ordinary way in walking down any street.

16,999. You say in your précis: "The only way to "discover cases of plague seems to be to employ spies, "and to keep as careful a watch as possible on the "cemeteries." What do you mean by "as careful a "watch as possible?" Do you intend to say that the corpses should be inspected at the cemeteries?—No, I do not mean the corpses should be inspected, but you should see that no body is buried or disposed of at the cemetery until the fact of the death is known to the authorities in the town. I mean, keep a watch so that people do not take away corpses and dispose of them without you knowing where they come from.

17,000. I suppose the friends give the address of the corpse at the cemeteries?—Yes.

17,001. They can give you a wrong address?—They very often do, down here.

17,002. In what proportion of the cases do they give a wrong address?—It is difficult to give any accurate figures. I should say about five per cent., but that is a pure guess.

17,003. You said that the living rooms in Nasik were 10 to 12 feet square?—Yes.

17,004. Is that exactly what you mean?—That is the case in small huts. In the better class of houses the rooms are often large and well proportioned.

(Witness withdrew.)

Lieut.-Col. G. Waters, I.M.S.

Lieutenant-Colonel G. Waters, I.M.S., called and examined.

17,005. (*The President.*) You are in the Indian Medical Service?—Yes.

17,006. What are your medical qualifications?—Licentiate of the Royal College of Surgeons, Edinburgh, and Physicians.

17,007. You are the Presidency Surgeon for the third division of Bombay?—Yes.

17,008. What duties are implied in that office?—I have to attend all Government servants in that district and their families. I have to look after a large charity school—the Byculla School; and I have two jails, one called the House of Correction in Byculla, and the other the Common Jail in a neighbouring district called Umarkhadi. I am Joint Inspector of Factories for that division also. That is an attached duty.

17,009. How do your duties bring you into relationship with plague?—Plague has attacked each of the institutions with which I have to deal, that is to say, the School, the House of Correction, and the Common Jail in Umarkhadi; but the Common Jail, beyond one case, was not attacked at all during my presence there. I may say that on the 9th of October last year, I went on sick leave.

17,010. As regards the origin of plague outbreaks, have you any knowledge, or distinct opinion?—In Bombay, the plague began in a district called Mandvi, and the houses attacked, seemed to me to have a particular bearing with regard to the direction from the opposite side of the road. The inhabitants of the higher and the more prominent houses facing the Argyle road, and in the immediate vicinity of the Argyle road, were most attacked with the plague.

17,011. Does that cause you to form any opinion as to the origin of the outbreak?—My opinion as to the origin of the outbreak in Bombay was that inasmuch as the cases occurred first in that neighbourhood, the causative factor existed there. They occurred first in that neighbourhood, and in the immediate vicinity of those houses we have the granaries, the grain stores of Bombay. They are just across the road from the places which I have been speaking of.

17,012. You think, therefore, that the granaries have had something to do with the origin of plague?—My attention was directed to the granaries from the fact that the first cases I heard of occurred in one of them, in the one belonging to Messrs. Ralli Brothers. There were 13 persons attacked. This was, I think, about the first incident of the plague—about 13 employés were attacked in one granary and they all died.

17,013. This was the first occurrence of plague in Bombay?—I think this was one of the first known incidents of the plague.

17,014. Were there any cases prior to the cases in this granary?—I daresay there may have been.

17,015. Then the cases you speak of, may not have been the first?—No, that was the first incident of which I was aware, and the next thing I became aware of, was noticing that the people in the higher houses in the immediate neighbourhood had the most plague victims.

17,016. Do you mean chawls?—We call them chawls but they are large tenements. These large tenements had victims in the upper storeys of the higher houses, and not in the lower storeys. At first, the people attacked were all in that particular neighbourhood, and then in the streets leading from Argyle road inwards towards Mandvi, the prominent houses in these streets leading off Argyle street, the houses with exposure towards Mandvi, were the houses that had the victims.

17,017. Do I understand you to imply that plague first gained a hold in the grain depôts, and then extended from them to the higher floors of the chawls?—No, I gave no theory as to that.

17,018. What do you wish us to understand?—I mean that some emanation, some fungus from the grain, blew in that direction, and affected the people who were most exposed to it.

17,019. What reason have you for supposing that the stores of grain had anything to do with the origin, beyond what you have said?—I had the knowledge that in Garhwal, on the southern slopes of the Himalayas, they had frequently outbreaks of plague, and that was supposed to be connected with the storage of grain.

17,020. What is the connection with the storage of grain, in your opinion?—My idea is that by the protracted and faulty storage of grain, grain to which damp has got access becomes attacked with this fungus, and it becomes the scene of the operations of this fungus.

17,021. What fungus is this?—I mean the bacillus—the plague germ — and generated either there, or quickened into activity by that contact, it then spreads by the wind.

17,022. Have you any knowledge as to how this bacillus is introduced into the grain?—I have no knowledge as to that. That is only a supposition, a theory, that I have formed from my own observations.

17,023. And you think the dissemination to a given spot is air-borne?—I think it is mostly air-borne.

17,024. Do you find there is a greater diffusion in windy weather than there is in still weather?—It is difficult to say in Bombay which days are windy and which not. We always have a sea breeze here.

17,025. Have you any still days?—Absolutely no still days. I have made no observation as to that.

17,026. What are the facts on which you base the idea that the bacillus in its dissemination is chiefly wind-borne?—The prevailing breeze from that point is in the direction in which most people were attacked. The breeze is not always constant from one point, but that is the prevailing breeze. There was another grain store in Kolaba, and Kolaba and Mandvi were the two points where plague started in Bombay. From those two points plague spread inwards and southwards. It It spread mostly in the direction the wind blows—from Mandvi right up by Umarkhadi and Byculla, and the two jails are in the direct line of fire from Mandvi, judging by the prevailing breeze.

17,027. Do you know whether plague extends more rapidly in breezy health camps, or in breezy temporary hospitals, than it does in the still and crowded localities of the towns?—I am not in a position to give an answer to that question.

17,028. You have no knowledge of that?—No.

17,029. You do not know whether it does or does not extend in camps?—No.

17,030. If I tell you that it does not usually extend in camps, whereas it extends very much more readily in crowded localities where there cannot be much movement of wind, how far do you think that will affect your theory?—The germs die when they are relieved from crowded localities; they are killed mostly by the sun. They are more exposed to the sun in that quarter than in the original quarter.

17,031. If they are killed by the sun how is it that infection may be carried by the wind?—I think they are all more or less diminished in vitality by the sun, but those that fall in damp places re-acquire virulent power, and so do mischief.

17,032. You have no other reasons to give for your theory than those which you have stated?—No, beyond that the first cases arose in a grain store, and the greatest prevalence of plague with the highest mortality occurred in the most exposed houses in the immediate neighbourhood, and in the direction of the prevailing breeze; and we had also something to go upon in the almost annual outbreak of plague in Garhwal, on the southern slopes of the Himalayas, where it is supposed to be associated with the grain storage under the houses.

17,033. You have nothing more to say about wind diffusion than these examples which you have given us?—Yes, I have something more to say. The Umarkhadi Jail, the Common Jail, is very close to this quarter. There was a tremendous mortality in the immediate vicinity of the jail. Meanwhile it did not attack the jail.

17,034. How did the wind blow with regard to the jail?—The wind blew right over the jail.

17,035. Towards the jail, from the infected district?—Yes.

17,036. Do I understand you to say that no cases occurred in the jail?—None for the first year.

17,037. How does that tally with your theory?—The jail is surrounded by a wall 20 feet high, and the germs are not derived from sources so high, generally, and the germs blown by the wind would strike upon the walls and not go inside as a rule. Some of them, no doubt, would fall, but those that did fall within the jail enclosure were destroyed by the sun, because in the jail I took very great care to keep the place absolutely dry, so that the plague germs could have nothing to revive them once they fell. I stopped all watering of the surface of the ground, and simply maintained as much dryness as I possibly could, and I used limewash.

17,038. Therefore wind-carried bacilli are killed by dry soil?—Not by the soil, but by the play of the sun on the dry soil.

17,039. That refers only to wind-carried bacilli?—Yes.

17,040. What about the bacilli which are not wind-carried?—I can hardly conceive of any bacilli spreading otherwise than by the wind.

17,041. You know that in crowded localities plague extends from house to house, and within houses, where the atmosphere is quite still, so that it overtakes a large area in a district where there cannot be much influence of wind?—In crowded places and narrow lanes there are many spots where the sun cannot reach. The sun is the great germicide in my opinion.

17,042. Have you any experience with regard to curative serums?—Yes.

17,043. What experience?—Plague broke out in the House of Correction late in January, and in February of 1897. That is four or five months after it had started in Bombay. I called in M. Haffkine to use his curative serum—which he kindly did—but the result, in my opinion, was negative.

17,044. Have you got the facts; will you please state what the facts are?—The facts are that as many died who were treated with the serum, as who were not treated with the serum.

17,045. What was the total number?—Altogether there were 34 cases. Half of them recovered. To put it shortly there was no beneficial result to speak of from the curative serum.

17,046. What was the incidence of the plague among these people, how many were affected?—Thirty-four people were attacked with plague and 17 died.

17,047. The result was that 50 per cent. recovered?—Yes, 50 per cent. recovered.

17,048. That is not unfavourable, is it?—It is very favourable.

17,049. Why do you say "very avourable"?—Such a low mortality to cases is rare. The figures include those who were treated with curative serum, and those who were not.

17,050. Will you distinguish between the one and the other? I should be glad if you will give us the following information; how many cases were treated; how many cases recovered, and how many cases died?—Six cases were treated with the curative serum; three died and three survived.

17,051. The result was unsatisfactory?—The curative treatment was unsatisfactory.

17,052. Will you give us any information you possess from your own observation with regard to inoculation?—From my own observation it was distinctly successful.

17,053. Can you give us the facts?—There was only a very small field to work on, because plague stopped suddenly in the jail.

17,054. Stopped after inoculation, or before?—The plague stopped in the jail altogether amongst the non-inoculated as well as amongst the inoculated. It stopped quite suddenly. There were 164 persons inoculated in the workhouse, including the jail staff, and there were 191 people who were not inoculated.

17,055. How many cases of plague occurred in these two places, in the workhouse, and in the jail?—There were 34 cases altogether.

17,056. How many cases occurred among the inoculated?—Among the inoculated there were only five cases, and two deaths. Mr. Haffkine very rightly cut off three of the five because they showed plague the day after, or two days after, inoculation. They never recovered from the fever of inoculation.

17,057. How many deaths were there among the non inoculated?—Among the non-inoculated were 12 deaths.

17,058. How many cases of plague occurred before inoculation was commenced in these places?—There were 17 cases before that.

17,059. Then out of the 34 there were only 17 which occurred after the inoculation?—Yes, that is so.

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17,060. How many of the 29 cases among the non-inoculated occurred before inoculation had commenced?—I think only three. After the inoculation we had 12 deaths among the non-inoculated—and only two, really, amongst the incculated, if you cut off, as Mr. Haffkine did, the three that had plague the day after they were inoculated.

17,061. Can you tell us the ages of the people?—This will be given by Mr. King, Assistant Surgeon in the House of Correction.

17,062. (*Dr. Ruffer.*) I am not quite clear with regard to your figures. You had 327 prisoners, of whom you inoculated 164?—Yes.

17,063. Are these your own figures, or M. Haffkine's figures?—They are all the same.

17,064. In his evidence M. Haffkine says, 154. I want to know whether it is a misprint, or whether it is not?—I think M. Haffkine's figures are correct. There were other persons inoculated besides those M. Haffkine counted in the jail. There were some people in the House of Correction, and the staff.

17,065. How many in the House of Correction and the staff were inoculated? Perhaps by adding those to M. Haffkine's figures, we shall get yours?—Assistant-Surgeon King was asked by me to give the evidence required.

17,066. All the inoculations were made on January 30?—Yes.

17,067. Could you tell us the dates of the various cases of plague among the inoculated?—There were three attacked, and three died, amongst those who were inoculated on the 30th January.

17,068. I want the dates of these attacks?—It was like this: they were inoculated on the 30th January, and the inoculation fever merged into the plague fever: there was no differentiation between the two.

17,069. I want to know where the buboes were in the three cases in which the inoculation fever merged into the bubonic fever. Also, the ages of these three people, and the ages of all the prisoners?—Mr. King I have asked to give this evidence.

17,070. When was the next case among the inoculated?—There were the three cases on the 30th, which I have given you. Then there was one case on the 31st, among those who were inoculated. That person recovered.

17,071. What was the age of this person; and what was the position of the bubo?—26 years. The bubo was situated in the right axillary region.

17,072. Was there any other case?—The other case among those who were inoculated occurred on the 6th of February. That case also recovered.

17,073. Were there any other attacks among the inoculated?—Not besides the three. M. Haffkine cut the cases I mentioned out of the returns altogether, because they occurred continuously with the fever of inoculation.

17,074. But did the last two cases die?—Yes, besides the three that he excluded.

17,075. I will read to you what M. Haffkine says: "On the 31st January there were two cases of plague "amongst the non-inoculated lot, of whom one proved "fatal, and there was one case amongst the inoculated, "who recovered. On the second day after the "inoculation, on the 1st February, there was one case "amongst the non-inoculated that proved fatal, and "there was no case amongst the inoculated."?—That is quite right.

17,076. There were no more cases among the inoculated till the seventh day, regarding which M. Haffkine says: "On the seventh day, the 6th "February, there were five cases among the non-"inoculated, of whom one proved fatal, and one case "amongst the inoculated that ended in recovery"?—Yes.

17,077. You said it was fatal just now. That is why I wanted to know?—There were two attacks, and no deaths.

17,078. You said two attacks, and two deaths?—I was wrong.

17,079. Could you tell us whether these prisoners were under exactly the same circumstances, the non-inoculated, and the inoculated?—They were under exactly the same circumstances. I took them out myself and they were all together on the platforms.

17,080. Have you a plan* of the jail showing the cells where all the cases of plague occurred?—Yes.

17,081. Did the inoculated and uninoculated cases occur all over the jail?—Yes. (The witness explained the plan to the Commssioners.)

17,082. How many prisoners are there in each cell?—Usually there are three or four, according to the size of the cells, which vary a great deal. There are from 640 to 700 cubit feet for each prisoner.

17,083. Have you any register showing who was in each cell?—I do not think so. It would not be very easy, because there were people coming and going constantly. We had them all out on these platforms.

17,084. Can you tell us when the inoculated prisoners left the jail; have any of them left the jail?—Almost all the prisoners in the jails in Bombay are not very long-time prisoners. They are almost all in for under one year.

17,085. Can you put in a table showing where the various people were in the jail at the time of inoculation, and when they left the jail?—There were a a hundred people transferred to Ratnagari Jail, and we transferred a great many of the plague cases amongst them—the cases that had recovered.

17,086. I want to know, when did the inoculated people leave the jail. How long did you have them under observation?—Mr. King will give this information.

17,087. Did you hear of any deaths amongst the inoculated and non-inoculated after leaving the jail?—I have no knowledge of them after leaving the jail.

17,088. (*Prof. Wright.*) I believe there was another epidemic of plague in your other jail; can you tell us about that?—That was in the following year—1898.

17,089. Could you give us the particulars of that epidemic? Inoculations, also, were performed in connection with that epidemic, were they not?—Yes.

17,090. Where you in charge of the jail at the time?—No; I was on furlough. I, however, have the details here.

17,091. Where did you get the details from?—I got them from the jail records. I have a statement here with regard to the plague epidemic.

17,092. Who was in charge of the jail at that time?—There were two. One was Dr. Barry, appointed to act for Dr. Collie who was appointed to act for me. Dr. Barry was at the jail at the time.

17,093. Where is Dr. Barry now?—He is on furlough.

17,094. Is he out of India?—Yes.

17,095. (*Dr. Ruffer.*) Did you mention anybody else?—Surgeon-Major Collie.

17,096. (*The President.*) Where is he?—He is in Matheran.

17,097. (*Prof. Wright.*) You say Dr. Barry was in charge?—Yes.

17,098. Is he in the Indian Medical Service?—Yes. I see the ages of all the plague cases are recorded.

17,099. Would you give us a brief summary of this epidemic of plague?—In the Common Jail there were 19 attacks in January 1898; one or two of them were attacked on the last day of December 1897, but they all lived into January 1898. Of the 19 attacks, nine died. Three of the persons who died were attacked before the inoculation was performed. Of the remaining 16 there were six deaths. The remainder recovered.

17,100. Is this a long-time jail?—It is a short-time jail—much shorter than the other one.

17,101. When did the first case occur, was the patient a prisoner who had been recently admitted to jail?—The first case had been 23 days in jail when he was attacked, on the 31st of December.

17,102. (*Dr. Ruffer.*) Perhaps the best way will be to read M. Haffkine's evidence to you, and ask you whether you agree with it. He says: "At the end of "December 1897, the plague broke out in the second "Bombay jail. There are two jails here, the Byculla "House of Correction mentioned already, and the "Umarkhadi Common Jail. Three cases of plague, all

* Not reprinted with the Proceedings of the Commission.

"of which proved fatal, occurred before inoculation "was offered to the prisoners. This was done on "Saturday the 1st of January, when the prisoners "were questioned as to their willingness or otherwise "to be inoculated. The whole number of them "volunteered. In view, however, of the novelty of "the treatment, and with the object of demonstrating "its harmlessness, and the extent to which it gives "protection, only half the prisoners were allowed to "undergo inoculation. To avoid making any special "distinction the whole of the 402 prisoners present on "that day were paraded in 10 rows, an officer was put "upon them, and every second prisoner, just as they "happened to have come and were seated in the rows, "was sent to the inoculation table and received the "inoculation. We have been emboldened by our "previous experience of the after effect of inoculation "not to refuse it to anyone who chooses to undergo "it. I mean to say, no condition of health is con-"sidered to disqualify person from being inoculated, "except his having fever or plague on him at the "time he presents himself for inoculation. By "accident, however, it happened that the officer in-"oculating noticed that a man had fever on him "when the actual injection was already performed; "his temperature was taken, and found to be "102°F. While the inoculation proceeded one "came across another man, with an exactly similar "temperature, and who was left uninoculated. "Neither of these two men showed plague afterwards. "We quoted afterwards that almost incredible inci-"dent in order to emphasize the fact that in every "respect the inoculated and uninoculated prisoners "appeared to be under absolutely similar conditions, "but for the fact of inoculation. It was agreed with "the authorities that that day being Saturday and the "next Sunday, when no prisoners were expected to "work, if any of the inoculated required rest on "Monday, the whole number of the inoculated and the "uninoculated should be given leave in order that "there should not be a single circumstance which could "be pointed out as constituting a difference in the "conditions of the inoculated and the uninoculated." Have you any personal knowledge of that?—No, no personal knowledge.

17,103. Then M. Haffkine goes on: "It is because "in this instance every second man has been taken, "and because we did not wait for the volunteers com-"ing out of the crowd, as was the case with the first "inoculation in the Byculla Jail, that I consider this "experiment as having been made in more precise "order and under stricter conditions than the first "experiment. The whole lot of the prisoners were "taken in the order in which they came, without any "distinction of any kind being made"?—The second time he did that. He took every other man. I do not understand that, because there was such a number of the inoculated. 205 were inoculated, and only 70 were left uninoculated. I did not understand how he could do that. M. Haffkine told me that he inoculated every other man, and that he inoculated 205 out of a population of 275. I do not see how that came about. This was the second jail inoculation, but the first in the Umarkhadi Jail.

17,104. (*The President.*) Perhaps it was over two periods?—No, it was only once. I asked the Superintendent about that. He told me that he could not understand that. He wished to have them all done; but he suddenly remembered that he could not get all the bad ones looked after, if they were all done, so they left 70 out of the 275.

17,105. (*Prof. Wright.*) Who is the Superintendent?—Mr. Mackenzie.

17,106. Is he still here?—Yes.

17,107. (*Dr. Ruffer.*) Then M. Haffkine goes on to say: "Not to leave out anything, I must mention the "case of two prisoners who were not willing to under-"go inoculation, and who were left alone. It happened "in two instances, out of 402 persons who were there, "that when their turn came to be inoculated, because "they were second in the row, they said they did not "wish it to be done, and these two were not inoculated. "I consider this experiment in the Umarkhadi Jail as "presenting a complete elimination of the errors "inherent to observations in human communities "generally, I mean to say from the point of view of "the precision of the conditions of the experiment, and "as regards the infallibility of the results." Is that right?—Yes.

17,108. Do you agree that plague continued for a month?—Yes.

17,109. M. Haffkine says: "The plague continued for "about a month, and on 11 different days there occurred "13 cases"?—According to my information there was a case on the 18th of March, one on the 18th of February, and one on the 10th of February amongst the inoculated.

17,110. I will read you the whole statement, and perhaps you will take a copy and work it out. This is what M. Haffkine says: "During that month a certain "number of prisoners were discharged, and others "admitted to the jail. Taking day by day the num-"ber of inoculated and uninoculated who remained in "the jail during that month of plague, from the "original group of 402 prisoners, the average daily "strength of the inoculated appears 147, and of the "uninoculated 127. The number of inoculated who "stayed in the jail all through the epidemic was "therefore larger by about one-seventh than that of "the uninoculated. All the 13 cases of plague occurred "among the prisoners of the original group who were "present in the jail on the day of inoculation, and no "cases occurred in those admitted since then. There-"fore the distribution of the cases of plague amongst "the inoculated and the uninoculated, represents the "distribution of the two original absolutely comparable "groups of people which I have described. 10 of the "13 cases were amongst the smaller number of un-"inoculated, and six of these 10 died." Is that correct?—Yes, that is correct. Six deaths occurred amongst those after M. Haffkine had to deal with the jail, six deaths occurred among the non-inoculated.

17,111. "There were three cases among the larger number of 147 inoculated, and all three recovered." Is that correct?—Yes.

17,112. I believe you did not see the cases, so that you cannot tell us about the symptoms?—No.

17,113. Then M. Haffkine's account is substantially correct?—Yes. I would like to read you a few remarks I made on the inoculations in the two jails. It gives the mortality from all causes, as well as from plague. It is addressed to the Inspector-General of Prisons. The remarks are as follows:—

"The Inspector-General of Prisons is aware that I gave M. Haffkine the first opportunity of publicly testing the effect of his prophylactic serum, on the occasion of the outbreak of plague in Her Majesty's House of Correction. He will also remember that I took this responsibility on myself, as I did not see that he, as a layman, could authorise me in this regard. The Surgeon-General afterwards made it clear to me that his sanction to such dealing with the prisoners would not have been accorded. Fortunately the matter was entirely in the hands of the Inspector-General of Prisons; and I was aware that Mr. Filgate would support any measure that I could show reasonable grounds for adopting.

"The field of observation of the result of inoculation in the House of Correction was contracted by the speedy cessation of plague on the establishment of dryness within the residential portion of the jail. Still, the result achieved encouraged the prosecution of the work of inoculation, and thus eventually it obtained a wide acceptance. It has, indeed, succeeded beyond what my experience of it in the House of Correction led me to expect. And I had no further opportunity of personally observing its value as a prophylactic during my absence on sick leave. However, my doctrine of dryness as the most effective preventive of plague was not in accordance with the belief of those who successively held my appointment, and these gentlemen pursued quite a different plan, with the result that the Common Prison, which had hitherto almost completely escaped attack, despite its situation in the midst of the most severely plague-stricken spot of Bombay, became the scene of an outbreak of the disease. On this occasion recourse was had to prophylactic inoculation. I must here say that three prisoners were attacked before inoculation was adopted, and so they should be left out of account in judging the result of preventive treatment. Besides these three there were sixteen prisoners attacked with plague in the Common Prison in January of last year, and of these six died. Of the inoculated, four were attacked, and none at all died. I attach hereto a detailed statement*

* The statement referred to was not put in by witness. *See* statement put in by Mr. Mackenzie (App, No. LIX. in this Volume).

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of the men attacked, of deaths and recoveries. The three asterisks indicate the cases which preceded the inoculation, and which I have left out of account. I should add that a second inoculation was performed just four days before I returned to duty early in July last year; 205 persons were then inoculated, and only 70 left untouched. The result of this inoculation, curiously enough, was that one case of plague occurred among the inoculated, and the convict in question was transferred to the Arthur Road Plague Hospital, where he recovered; and two other minor cases occurred among the inoculated, and none at all among the other inmates of the jail, who already greatly outnumbered the remnant of the inoculated, the Umarkhadi Jail being a short-term one. By this time I had been three weeks on duty since my return from leave on the 9th July, and I had meanwhile put the jail back under my own plan of plague prevention. That is to say, I discontinued the practice described in the subjoined letter of the Superintendent of the Jail, and resumed my own simple plan of keeping the premises as dry as possible with pans of burning charcoal.

"'With reference to your enquiry as to the measures adopted to prevent the occurrence of plague in this prison during your absence on sick leave from 9th October 1897 to 9th July 1898; I have the honour to say that the practice of burning fires in the sleeping quarters was discontinued as superfluous, and that the precautions you took to maintain a thorough system of dryness and freedom from damp were quite departed from; the entire prison was frequently flooded with lime-wash, and twice daily, morning and evening, the sleeping rooms and the whole of the quadrangle of the prison were sprinkled with disinfectants. The entire prison, it may be said, was, in consequence of this application of moisture, in a condition of perpetual dampness, perhaps more particularly in places where the direct heat of the sun could not penetrate. It would not be out of place, I think, to mention here that in addition to the eighteen deaths which occurred under this régime, there was an abnormal number of prisoners constantly unfit for hard labour from sickness.'

"Lime-washing was also had recourse to at first, but finding that the subordinate jail executive could not be trusted with seeing that the lime-washed cells were properly dried with burning charcoal before they were again tenanted, I discontinued it, and for the last three months my whole concern has been to keep the working and sleeping quarters of the jail absolutely dry, and for this purpose I allow the sun's rays full scope, and where these cannot penetrate with force I have charcoal burnt, morning and evening, for the space of two hours each time. In my report on the outbreak of plague in the House of Correction early in 1897, I stated that that occurrence was due to my inability to restrain the dampness of the residential portion of the jail. And it was seen that when the dampness was obviated by the cutting off of the water outside the residential yard, the epidemic quickly—indeed, I may say abruptly — subsided. In that same report I remarked that no plague appeared, to speak of, in the Common Jail, because no water whatever was then delivered in the residential yard, and so I had no difficulty in keeping the place scrupulously dry. As I have said, however, this plan was not in operation during my absence on sick leave. My system was then replaced by one of which the daily sprinkling of the residential yard and sleeping quarters with a liquid disinfectant was a prominent feature. I here subjoin a tabular statement of the medical history of the Common Jail throughout the year; the first six months being under the régime which was not mine, and the second half of the year under the plan which was altogether my own.

Population - -	413·5	406·2	499·5	559·3	397·5	306·9	300·7	368·1	401·6	382·9	387·2	341·9
Months - - -	Jan.	Feb.	Mar.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.
Deaths - - -	8	2	2	2	2	2	0	1	0	1	0	1

"This shows a mortality of 18 in the first six months of the year (9 being plague cases) and only three in the last half of the same year. I am inclined to think that some of the cases of the remaining nine partook also of a pestilential nature; and this view of the case is strengthened by the fact that a second inoculation was deemed advisable at a short interval before my arrival in July last year. I would here, however, say that I do not for a moment wish to imply that the system pursued in my absence was not undertaken with a high sense of duty. And though it is my business at this moment to discriminate, I wish at the same time to put disparagement far from me. I specially wish this to be well understood, as I am given to understand that the measures in question had the approval of the Surgeon-General. I have no wish to do other than what is just and courteous to all men, and I therefore hope that my remarks on the course pursued by others shall be held altogether free from prejudice to anyone. I only wish to place in relief the fact that my plan of the strictest maintenance of dryness arrested the plague in the House of Correction in the Spring of 1897, and kept the Common Jail free from plague up to my departure on sick leave on the 9th October 1897; and that during my absence moist disinfectants were freely used in both places, with the result that there was a reappearance of plague and grave mortality in the House of Correction, and a fierce outbreak of the pestilence in the Common Jail, where it had not previously appeared. I would here say that plague seems to become weaker and weaker in successive years in any place attacked. And that it only broke out in the Common Jail in the second year of its prevalence, despite the strongly plague-stricken environment of the jail in question, shows that the means adopted for its prevention in the first year of its prevalence were effective, whilst the measures conceived for its prevention in the second year were abortive. This view of the case is further strengthened by the fact that the resumption, in July last year, of my simple plan of dryness, called such a decided halt in the mortality of both jails.

"Returning to the question of inoculation, I find that 138 inmates in the House of Correction were thus dealt with on the 3rd January 1898, there being then 128 other prisoners in that jail who were left untouched, and who thus served test purposes. Almost the whole of the 138 were inoculated after a short interval. As will be seen from the following statement, there were 11 deaths in the House of Correction in the first six months of the year.

Population - -	276	289	333	328	308	286	274	273	270	254	284	333
Months - - -	Jan.	Feb.	Mar.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.
Deaths - - -	Nil.	1	4	4	1	1	Nil.	Nil.	1	Nil.	Nil.	1

Total Number of Deaths - - - 13.

Total Population - - - - 3508.

"Of these 11, there was one case, that of Rama Gunpal, who had been inoculated two years running; his death was due to phthisis. There were altogether three cases of plague, two died and one recovered; all three were not inoculated. The remainder, nine, still left an enormous figure for the six months mortality of a small jail, and I may add, that of these nine cases, four were suspected at the time to be plague cases,

only they lacked pathognomonic plague phenomena, and were therefore not declared to be plague cases. It will still be observed that the mortality mainly decreased in March and April; and it should be noted that on the 7th of January 1898, the water which all the time since January, 1897, had not been allowed access to the residential quarter of the jail, was again let on, and remained on, till I stopped it early in July last, and at the same time had recourse to the burning of charcoal to overcome the dampness and keep the place as dry as possible. I here subjoin Mr. Hall's account of what was done in my absence.

'MEASURES adopted for the PREVENTION, &c. of PLAGUE in the HOUSE OF CORRECTION during the period 16th October, 1897 to 15th July, 1898.

'From October to about the end of December 1897, white-washing and the burning of charcoal fires were continued. In the early part of January, 1898, the medical officer — Major Collie — recommended that quarters for the warders should be provided, that portions of the roofs of cells should be removed to admit sunlight, and that the white-washing should be more frequent.

'About the 7th January, 1898, the medical officer—Major Barry—recommended that, in addition to white-washing the walls, the floors of all the cells should be whitewashed twice a week, that the drains and latrines should be flushed with a deodorising antiseptic, and that the same solution should be sprinkled about the yards of the prison. The clothing and bedding of the prisoners was also to be steeped once a week in the antiseptic solution and suspected admissions were to be washed with carbolic soap. These measures were continued until the 14th June, 1898, when they were discontinued on the recommendation of the medical officer—Major Collie. The water supply, which was let in to the rotunda pipes about the early part of January 1898, was cut off about the end of July 1898, at the request of the medical officer—Lieut.-Colonel Waters.

W. HALL.
Superintendent H.M's. House of Correction.'

"As a result of the change from the wet to the dry system, a glance at the same tabular statement will show that we only had two deaths in the last six months of the year, *i.e.*, two as against 11 in the first six months of the same year. But it may be asked, was this not coincident with the general diminution of plague in the neighbourhood; and to this I answer, by no means. The plague diminished in prevalence through the hot months of April and May, and reached its minimum in the first few days of July. But it then began to rise again and displayed a fresh epidemic which lasted throughout the monsoon, reaching its maximum in October. Taking the two jails together, we get a mortality of 29 in the first six months of the year, and only five in the last six months of the same year; that is, a death rate in the thousand of 41 in the first case, and only five in the second. As to how far the mortality was arrested by inoculation, it is to be observed that there was no inoculation in the House of Correction since early in January 1898, and then only half of its inmates (including jail executive) were inoculated. And in July, most of the inoculated had been released on the completion of sentence, so it cannot be at all averred that the diminished mortality of two in the last six months of the year was in any way due to prophylactic treatment. Then, as regards the common prison, it being a short term jail, few, very few of those inoculated on the 4th July, remained in prison six weeks later. Here indeed, we saw that one inoculated man was attacked with plague, and sent to the Arthur Road Hospital late in July, whilst none of the non-inoculated prisoners were at all attacked, and meanwhile the latter numbered 250, whilst the remnant of the former (the inoculated), did not amount to 100. But, I wish it by no means to be understood from this that I am an opponent of inoculation. As already stated, I was the first to give M. Haffkine an opportunity of demonstrating the efficacy of his prophylactic serum, and I am proud of its subsequent success. I was unwilling at first to give a hasty credential to prophylactic inoculation; but now that I have had occasion to review instances of its extended and enhanced effect, I gladly accord my testimony in its favour. I would, however, at the same time, point out that still more success is bound to accrue on the wide application of the principle I have enunciated, as to the obviation of dampness in, and in the close vicinity of, inhabited places. The best, the very best plague prophylactic, is dryness. This is the main thing I have learnt in regard to plague, and though it may still seem to some people a very small thing, it yet enables me to do much; and am thus in a position to hold the pestilence at bay. And be it remembered that the two Bombay jails have severally the most severely plague stricken environment of plague stricken Bombay, each jail being in the centre of localities giving the maximum of mortality."

17,114. (*Prof. Wright.*) You said the men were inoculated a second time in jail?—Yes.

17,115. Why were they inoculated a second time?—That I cannot tell you. There were 18 deaths in the first six months of the year. I have told you of nine cases of plague. Eight of those occurred in January, and one in February, and there was one other case in February. March, April, May, and June, each had two deaths; they were not put down as plague deaths, but I suspect from the second inoculation taking place so shortly before my arrival, that there must have been a suspicion that plague was at work among them.

17,116. There were about 400 people present on the 1st January?—Yes.

17,117. And the population of the jail had sunk to 275 on the 1st July?—Yes.

17,118. Then second vaccination was resorted to?—Yes.

17,119. And 205 were inoculated?—Yes.

17,120. Do you know how many of those were second inoculations, and how many were first inoculations?—There was only one inoculation.

17,121. How many of the 200 who were inoculated in January were still in jail?—None.

17,122. Had they all gone out?—Yes.

17,123. And this is a fresh series of inoculations on perfectly fresh subjects?—Yes. I have told you that the Umarkhadi Jail is a very short term jail, three months being almost the limit of residence.

17,124. These are another set of primary inoculations in jail?—Yes.

17,125. What is the result of that?—We had no result at all. One case of plague occurred amongst the inoculated about four weeks later. About 70 persons come, and 70 persons go every week to Umarkhadi Jail.

17,126. (*Dr. Ruffer.*) Could you add to your evidence the dates on which the inoculated people left the jail?—Yes, I can furnish it from the jail records, I believe. Mr. Mackenzie and Mr. King will put it in.

17,127. Will you please add to your evidence temperature charts of all prisoners who were inoculated in the Umarkhadi Jail in January 1898?—I cannot find such record on the Umarkhadi Jail; we had too much to do to have time for such record taking.

(Witness withdrew.)

Major B. B. GRAYFOOT, I.M.S., called and examined.

17,128. (*The President.*) You are in the Indian Medical Service?—Yes.

17,129. What are your medical qualifications?—M.R.C.S., L.R.C.P., and the double qualification of Edinburgh.

17,130. (*Mr. Cumine.*) You have been for about nine years Secretary to the Surgeon-General of the Government of Bombay?—Yes.

17,131. I think you have brought a map* of the Presidency with you?—Yes, I have.

17,132. You have marked on these maps at each place where plague appeared, the date of the first imported case which was reported, and also the date of the first indigenous case which was reported.

* Not published with the Proceedings of the Commission.

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have you not?—Yes, the date of the first imported case is marked in red ink, and the date of the first reported indigenous case is marked in black ink.

17,133. Do you think the map shows that plague spread very slowly or quickly in the Presidency?—Very slowly.

17,134. Does it seem to show that a good many places escaped indigenous infection, although they had imported cases?—Yes, it does show that.

17,135. Does it show at all the length of time that it takes for indigenous plague to appear in the town after the first imported case?—The first imported case is at one end of the scale and the first local case at the other, but between the first imported case and the first local case there were a varying number of imported cases, so that, to say the period between the first imported case and the first local case is the time it takes to make a place infected is not strictly accurate.

17,136. You do not mean to say that the indigenous infection necessarily came from the first imported case?—No, it came from the whole series of infection.

17,137. Is there any relation between the number of imported cases a place has had and indigenous plague appearing in it, or does it appear from these figures that indigenous plague is not the result of a regular bombardment of imported cases, but the result of arrival of a particular kind of plague?—I cannot really answer that, because we tried to find out the number of cases of simple bubonic plague, and cases of pneumonic plague. Of course we are dealing mostly with native subordinates in all these places, and their evidence is not always trustworthy. There has been another attempt made to get the number of imported cases which took place before the first local case. At present I cannot give you statistics on that point.

17,138. You mention in your précis of evidence that an outbreak of plague has generally been preceded by high mortality?—Yes; we have noticed that in several places in the Presidency.

17,139. May not that mortality have been plague unrecognised?—I think very probably, and also more efficient registration at the time of an epidemic caused more deaths to be discovered and noted.

17,140. Will you tell us if there are any respects in which the Civil Surgeon of the Bombay Presidency differs from the Civil Surgeon in other parts of India: is he the Civil Surgeon for the district, for instance, or is he rather the Civil Surgeon of the Civil Hospital at the Sadr station?—In Bengal, the North-West Provinces and Oudh, in fact, in all the northern part of India, and I believe in Madras also, the man is gazetted as Civil Surgeon of the district, and is supposed to be and is the head of the district from a sanitary point of view. He administers the dispensaries, he is supposed to travel every three months round his district, and I believe he gets death returns and sanitary returns, etc., He has an Assistant Surgeon as his assistant, a graduate of the Punjab or Calcutta University, whom he leaves behind when he is on duty inspecting any village or town, this man being gazetted to the civil dispensary at the headquarter station. The Civil Surgeon of a district in Bengal is, therefore, more or less a travelling officer, and has the whole district under him, and an establishment to do the clerical work. In Bombay that is not so. In Bombay the officer is gazetted not as Civil Surgeon of Dharwar or Belgaum district but as Civil Surgeon of Dharwar or Belgaum. In Bengal the district is the man's first charge; in Bombay it is the station. The consequence is, in Bombay the Civil Surgeon is supposed to inspect the dispensaries in his district once a year, not every three months. He also inspects the factories, but he has none of the administration of the district under him. All that is done by the Surgeon-General in the Surgeon-General's office, so that the system is quite different in every way.

17,141. Does he receives returns of births and deaths in the villages of the district?—As far as I know, he does not. He gets no returns from dispensaries at all. They go to the Surgeon General's office. That is done for the simple reason that the Bengal system cost a great deal more than ours does.

17,142. He is the sanitary adviser of the Collector for the district, I suppose, if the Collector consults him?—Yes.

17,143. Has he any executive sanitary authority?—No, except that when he is on a tour of inspection he is supposed to send in a sanitary report if the town is of a certain population. If he sees any glaring faults of water supply or conservancy they are reported on a sanitary form which he fills up at the same time that he fills up his report of the dispensary, and sends to the Sanitary Commissioner.

17,144. If plague appeared in a village in a distant part of the collectorate it would be the duty of the Assistant or Deputy Collector to go to the place at once. Would it be equally the duty of the Civil Surgeon to leave the Civil Hospital and go off to the village?—Not under present circumstances. In fact, we find very often that Civil Surgeons find it very difficult to inspect the dispensaries once a year, because when the time comes to go to the dispensary the idea is so imbued in every one that the Civil Surgeon belongs to the station, that if some lady is ill or something is the matter with somebody's baby, he is immediately asked not to leave the station; the people do not hesitate to give such reasons for asking the Civil Surgeon to remain at headquarters. I only wish to bring out the point that in this Presidency the man's chief sphere of duty belongs to the station and not to the district as it does in Bengal.

17,145. So that although plague did break out in a distant village of the district no report about it would go to the Civil Surgeon?—Probably he would hear of it first of all from the Collector's office.

17,146. If he wished to go to that village he has no Assistant to put in charge during his absence, has he?—He can leave his Hospital Assistant, but he has no medical graduate as the man in Bengal would have.

17,147. Side by side with the Surgeon-General and the Civil Surgeon there is a Sanitary Commissioner with Deputy Commissioners and vaccinators?—Yes.

17,148. And the one hitherto has not been subordinate to the other?—That is so.

17,149. (*Prof. Wright.*) The Civil Surgeon is not subordinate to the Sanitary Commissioner?—That is so, and the Sanitary Commissioner is not subordinated to the Surgeon-General. Those two are independent.

17,150. Does not the Civil Surgeon report to the Sanitary Commissioner on sanitary matters?—He sends him occasionally a sanitary report, but he is in no way subordinate to the Sanitary Commissioner; he deals entirely with the Surgeon-General. If he inspects a town with a certain population he fills in a sanitary report which goes to the Sanitary Commissioner, and he might answer any questions that he was asked about it, but he is no way subordinate to him.

17,151. But the Civil Surgeon is subordinate to the Inspector-General of Prisons, is he not?—In his sphere as Superintendent of a jail.

17,152. Why is he not subordinate to the Sanitary Commissioner?—Because he has no work to do with him.

17,153. (*The President.*) What reports does he send to him?—He sends reports always to the Surgeon-General of the sick in his hospital and of the surgical operations and an annual report of the administration of the hospital. They all go to the Surgeon-General and the Sanitary Commissioner only gets the sanitary report spoke of from Civil Surgeons.

17,154. Has he any work of a public health character apart from vaccination?—The Civil Surgeon has nothing to do with vaccination.

17,155. Has he any sanitary work?—Only in an indirect way. He is generally on the Municipality of his station, and he is frequently made the chairman of the Sanitary Committee, and in that indirect way he does a lot of sanitary work. He is always at the disposal of the Collector. He is supposed to be the Collector's official adviser from a medical and sanitary point of view but he has no direct connection with the Sanitary Commissioner.

17,156. (*Mr. Cumine.*) When a famine comes on who takes under his charge the hospitals and the famine works, the Surgeon-General or the Sanitary Commissioner?—According to the Famine Code the Sanitary Commissioner takes all charge of famine. The Sanitary Commissioner requisitions the Surgeon-General for medical officers and subordinates, which are put at his disposal, and the Surgeon-General has nothing more to do with them until they are sent back.

17,157. That disposes of the civil medical administration. Can you tell us about the medical

administration dealing with plague?—Perhaps I ought to preface my remarks by showing the great difficulties we had when plague came. The medical services in India are recruited on this principle; Government count the sanctioned appointments and give a man for each appointment; then they add 20 per cent. for leave, and 5 per cent. for casualties. That, in total, makes up the strength of the Indian Medical Service. At the end of 1896 and in 1897, we had famine, plague, and then afterwards a frontier war with us, and it is easy to see that the strength of the Indian Medical Service was hopelessly inadequate to meet such contingencies. What happened was this. The Government of Bombay asked the Government of India for more men, and they immediately requisitioned on the Military Department for officers; all leave was immediately stopped amongst civil and military officers, and everyone at home not on sick leave was brought out at once. In that way the full strength of the service was practically in India with the exception of a few on sick leave. The Bombay Government got from the Military Department 10, 15, or 20 officers, as many as we could get, and a proportionate number of subordinates. These were posted where they were most wanted, but we did not have enough. Then the frontier war came on just as we had arranged everything, and we had to give up the men lent by the Military Department. In order to meet the difficulty of the insufficient medical aid, the Secretary of State sent out medical officers from England. To meet the difficulty of subordinates, viz., Assistant Surgeons and Hospital Assistants, we re-engaged all the old pensioned Assistant Surgeons and Hospital Assistants. We got those who would come back and put them into the easiest billets we could, to allow the younger men to go on plague and famine duty. Further we engaged everybody who had a medical qualification of any sort, and in that way we got over as best we could the frightful difficulty we had. Then if we had plague in one Collectorate and another Collectorate free, we closed as many dispensaries as we could in the one Collectorate and fed the infected area in that way. Honestly speaking, the whole medical administration of plague was more or less of a make-shift arrangement, simply because there were not enough subordinates and not enough medical officers. The difficulty of want of medical officers had been materially overcome by bringing men out from England. The medical subordinate of India, the Hospital Assistant and Assistant Surgeon, is an Indian product. There are a certain number of them and you cannot make more of them. It is now six months since I left the Surgeon-General's office, and I believe the difficulty is just the same. For three years not one of the subordinates has had leave, although as a great favour they have been allowed ten days casual leave occasionally. That is the way the principle of getting medical aid has been met. But as I say, it has been more or less of a make-shift arrangement because we have had to shut up dispensaries to send men to infected areas and never had enough subordinates to meet all demands for them.

17,158. Will you tell us about the flying columns which you organized?—In many places we did not have any medical subordinate of any kind. The original constitution of the flying column was supposed to be a Commissioned Medical Officer or an English doctor, two native assistants, if possible a female Hospital Assistant (this was very seldom possible), a couple of muccadums of disinfection, and 20 or 30 trained coolies, who had been trained in disinfection. We generally got them from men who had worked in Bombay city.

17,159. When a Collector telegraphed for them, you sent them up with their buckets and disinfectants and pumps, and so forth?—We had a regular equipment. We always tried to keep one column in advance. If we had a telegram from a Collector asking us to send up a flying column we sent it up with the coolies, and we immediately started equipping another one, teaching the subordinates, coolies, and all the rest. Of course the constitution of the flying column varied. If it was a little place we would not send a Commissioned Officer, but perhaps a medical subordinate; but the idea of the flying column was that they were to go to the place and immediately form the nucleus of a trained agency to meet with the plague, and to have all the implements and everything ready at hand.

17,160. The Surgeon-General and the Civil Surgeon inspect hospitals aided by Government, throughout the Mofassil generally. Has the Surgeon-General any right to inspect the hospitals in Bombay city?—All the hospitals are Government hospitals except one.

17,161. I mean the Municipal hospitals in Bombay city?—He has no jurisdiction over them. Of course, as a matter of courtesy, he could go and see them, but he only gets returns from them once a year for his annual report, and then he has to ask the Government to get them for him. As a matter of courtesy, if he goes to a Municipal dispensary there is never any objection made to his inspection, but as far as I know, he has no right to go and demand it.

17,162. (*Prof. Wright.*) Is there a Sanitary Board in Bombay?—Yes. The Sanitary Board consists of the Sanitary Commissioner of the Government and the Engineer.

17,163. What does the Sanitary Board do?—As far as I know, it has now assumed the advisory functions to the Government, which the Sanitary Commissioner used to have before. As far as I understand, it advises the Government on all matters, such as water supply of big towns and drainage schemes, and it collects and publishes a large sanitary report every year.

17,164. Are there only two members on the Sanitary Board?—Yes.

17,165. What happens if they disagree?—I do not know. At present, I believe, the Sanitary Commissioner is to be put under the Surgeon General, and the Surgeon General is to be made the President of the Sanitary Board, but there is nothing in black and white; it is not yet settled, I believe.

17,166. Who is the Sanitary Commissioner?—Lieutenant-Colonel Clarkson, now on furlough.

17,167. Who takes his place in the interval?—One of the Deputy Sanitary Commissioners acts for him.

17,168. On what principle are the Sanitary Commissioners selected? Are they selected for eminence in bacteriology?—Lieutenant-Colonel Clarkson has been 14 or 15 years in the Sanitary Department, he is not, as far as I know, an expert in bacteriology.

17,169. What is the Sanitary Department?—The Sanitary Department of Bombay consists of a Sanitary Commissioner and five or six Deputy Sanitary Commissioners. The whole Presidency is divided up into sanitary circles, and each Deputy Sanitary Inspector has his circle. Their chief function and most important work is vaccination. They have all the vaccination of the Presidency under them, and that, I think, forms the major part of their work. Of course, I am not in the Sanitary Department.

17,170. Who selects the men for the Sanitary Department?—As a rule the officers of the Sanitary Department are chosen by the Sanitary Commissioner himself in conjunction, generally, with the Surgeon General, who recommends to Government, because the Governor appoints every one.

17,171. Are these appointments financially desirable appointments?—No, they are not particularly well paid.

17,172. Are they sought after?—No; they used to be paid better than they are now; because they used to have a fixed travelling allowance in the old days, now that is cut. The Sanitary Commissioner to the Government of Bombay starts with Rs. 1,200 a month, and the Engineer gets Rs. 1,400, I think.

17,173. Does a member of the Indian Medical Service do himself financial damage by going into the Sanitary Department?—I would not say that.

17,174. But at any rate, he does not advantage himself?—The man in the Sanitary Department is travelling in the jungle for seven or eight months in the year, during which time he has very few expenses; he meets very few people; there are no social calls on his money, and he can, practically, live very cheaply.

17,175. But if these appointments are not sought after, I presume they are not worth having; is that the state of things?—No, I do not say that. Many men like them. I know one man who told me he would refuse any good Civil Surgeonship, because he preferred to be in the Sanitary Department.

17,176. Has a medical officer to get the Diploma of Public Health, or study bacteriology before he is admitted in the Sanitary Department?—I believe it will be so now, but the majority of men, Colonel Clarkson for instance, were in the Sanitary Department before bacteriology was an important subject or taught in the medical schools.

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17,177. Then at present no qualification in bacteriology is required?—Knowledge of bacteriology is not made a *sine quâ non* now, but, as a matter of fact, many of the men coming in here had sanitary diplomas.

17,178. When the Surgeon-General wants advice on sanitary matters, such as disinfection, does he go to the Sanitary Commissioner?—No.

17,179. Where does he get his information from?—I think the Surgeon-General knows as much about disinfection as the Sanitary Commissioner.

17,180. I understand the Sanitary Commissioner is supposed to be a specialist, and the Surgeon General is supposed to be a purely administrative officer; does he not go to any specialist to get information on disinfection?—The Surgeon-General is the adviser to Government on all medical subjects. The last time we had to do a great deal with disinfection, we consulted M. Haffkine and Mr. Hankin, and every conceivable source we had.

17,181. But you have no official adviser?—No official bacteriologist.

17,182. Is there no bacteriology connected with the Sanitary Department?—No Government bacteriology. The only real laboratory we have is the one in the Jamsetjee Hospital; which is for the use of the Professor of Physiology and the Professor of Pathology.

17,183. And the Sanitary Department has not any laboratory at which bacteriological work can be done?—It has no experimental laboratory.

17,184. Supposing you want to know whether a particular disease spreads by air, wind, or water, or anything of that sort, whom do you ask?—We do not ask anybody. At present we can ask M. Haffkine to report on any subject, but before he came to Bombay we had nobody, and we have no institute and no Government bacteriologist for Bombay Presidency, whom we could ask to carry out a series of experiments on the life history of anything.

17,185. Has it been represented to Government that you want information and cannot get it? I presume you represent the Surgeon General's office? Has it ever been represented to Government that advice is wanted on matters of this kind?—I do not know; I am not aware that the Surgeon-General has made any special representation that he requires an adviser in bacteriology. The question has been, I believe, considered by Government, who know what is required.

17,186. Were the flying columns, of which you spoke, under the Surgeon-General?—Yes.

17,187. Not under the Sanitary Commissioner?—No. The rule was that the Surgeon General equipped them and they were sent to various places, and the medical officer was supposed to work in conjunction with the local revenue officer, that is to say, the Assistant Collector, or whoever was the head of the Civil administration.

17,188. Was the Medical Officer in charge of the flying column trained in bacteriology?—No; he was very often a Hospital Assistant or Assistant Surgeon, or anybody we could get. It was very seldom we could get a Commissioned officer.

17,189. Had you any guarantee that he knew anything about disinfection when he set out?—He knew as much about disinfection as most medical men do.

17,190. How much is that?—That varies according to the man.

17,191. No steps were taken to see that he had more knowledge of the matter than the average medical man?—No, we could not do it, because we were working at high pressure and had to take any man we could get. We had these sudden calls upon us, and our subordinates had not been trained in bacteriology. The medical officer had had practical experience of disinfection of houses with an acid solution of perchloride, and his coolies under him had been trained in the use of the wooden Chinese pumps, and all the technical details of disinfecting a house. We do not pretend they were scientifically perfect columns, but what we maintain was that they were the best means at our disposal of supplying a trained agency in those jungle places where they went.

17,192. Then these men simply knew how to use a pump and how to pour on a certain fluid?—They could not take the sweepings off the floor and tell you if there were bacilli there, but you could not expect that. As I say, they were not scientifically perfect, but they were the best we could get with the material at our disposal to meet the difficulty of plague breaking out in small places where the people know nothing. We sent Captain Leumann to Lanauli, and of course the columns were fairly good when we had medical officers going with them, but in some jungle places we only had a Hospital Assistant and perhaps 20 coolies to start the nucleus with the Mamlatdar, the village constable and the Patel.

17,193. Were these so-called flying columns reported upon by the Surgeon-General as being trained columns or did the Surgeon-General say they were not trained?—He called them flying columns; we never pretended at the time they were perfect.

17,194. Were their imperfections fully reported?—No, there was no need to do that because Government knew it as well as we did. All Collectors know the capabilities of a Hospital Assistant and an Assistant Surgeon, because they deal with them every week. If we had gone on reporting on our own imperfections in the way of all the things we wanted, we should have been reporting all day long, because when you have famine, plague, and a frontier war all on at once, you must be disorganised. In India in normal times the policy of the Government is only to have a sufficient number to do the actual work of the country, leaving a small margin for deaths and leave.

17,195. (*The President.*) These columns which you sent out were not expected to do original investigation?—No; they were just simply to meet the local requirements of the place.

17,196. They were not expected to do original investigations under the circumstances, even though they were able to do it?—No.

17,197. They required sufficient training merely to carry out the best method of disinfection?—Yes, and they were supplied with all our instructions. We issued instructions in disinfection which we had drawn up after we had had elaborate experiments done by Mr. Hankin on the question of the best disinfectant.

17,198. Supposing you had a large number of trained bacteriologists in India, it is not at all probable that you would have told off one of these men to each of the flying columns?—It is a thing I should never have thought of doing for one moment.

17,199. You spoke of the functions of the Sanitary Commissioner; he has a Deputy, I understand?—Yes, five Deputies.

17,200. Each of whom has a district under his immediate control?—Yes.

17,201. How large is this district?—The district generally consists of two or three collectorates. As a matter of fact the districts are rather large; they cannot get round their whole district during one touring season. The touring season generally commences at the beginning of October and goes on to the end of May. They have no time to go round the whole district in a year; it generally takes them two to three years.

17,202. You stated that the functions were largely to look after the water supply and large schemes of conservancy?—Any sanitary function of that kind—the ordinary advisory powers of a Health Officer at home, and on the same things that he would advise his Corporation about. He would point out obvious mistakes in the water supply. For instance, if they saw that the local authorities had placed the village latrine above the inlet where they take the water, they would point that out. They would advise on any local sanitary work, but, as I said before, I think their best and greatest work is vaccination.

17,203. Who else does sanitary work officially?—The Sanitary Commissioner and his five Deputies constitute the official Sanitary Department of this Presidency.

17,204. There is no one else officially connected with it here?—No. Every Civil Surgeon is supposed to be the medical and sanitary adviser of his Collector; that is to say, if his Collector had in his district, or in his special town, anything that he saw was wrong, he would ask his Civil Surgeon what he thought about it, and if it was sent up officially to Government it would then immediately fall into the Sanitary Department's hands. The Civil Surgeons do a lot of sanitary work because they are generally in the Municipality of their station, frequently chairman of the Sanitary Committee, and in that way they do a great deal in the way of helping the Municipalities with their advice.

17,205. Who looks after such questions as overcrowding, ventilation, and so on, in houses?—If it came under the Civil Surgeon's notice, if he saw any epidemic disease he would report it to the Collector, but there his functions would cease. He could do nothing.

17,206. Supposing there was no epidemic disease, and nothing to draw his attention specially to the condition?—He would report it once a year when he made his annual sanitary report, that in such and such a quarter overcrowding was very great, just as the Health Officer to the Bombay Municipality has for years reported that there is overcrowding and insanitary chawls in Bombay; and they still exist.

17,207. Whom would the report go to?—The report would go to the Surgeon-General's office, and when he saw it he would send it on to the Collector of the district, and request him to do anything he could to remedy it, and the probability is that nothing would be done for the lack of money.

17,208. The Collector would have the ultimate decision?—The Collector, as the head executive and revenue officer of the district, must take the initiative. The Sanitary Department have no power. They can only advise. They have no power to insist on anything being done.

17,209. Suppose the Collector does nothing and the sanitary official is still impressed with the insanitary condition, what more can he do?—He can report to the Sanitary Commissioner that he has recommended certain measures, and that nothing has been done, and the Sanitary Commissioner can refer the question to the Government.

17,210. That means to whom?—To the Governor and Council. He would write to the Secretary of the Government in the General Department, reporting the whole case, and the ultimate decision must rest with the Government whether any action is to be taken or not.

17,211. In a good many instances such reports, I believe, have been sent to the Collector?—The reports are going on all the year round on this point.

17,212. Do you know of any cases in which any action further than that has been taken?—In the majority of cases nothing further has happened; the Collector might want to do it, but he has not the money to do it.

17,213. In a good number of cases nothing has been done?—If you look up the records of the Sanitary Department of this country you will see there has been a most valuable lot of advice given with very little action, because, although the Government and everybody are most desirous to do these things, every sanitary reform resolves itself into a question of money.

17,214. Therefore, your answer is that in a large number of cases these reports have been sent in and no action has taken place?—Yes.

17,215. With regard to these cases in which no action has been taken, do you know if the person who originally sent in a report, that is the sanitary official, has done anything further?—I cannot tell you that. I do not know it within my own knowledge.

17,216. (*Dr Ruffer.*) Did you say the Sanitary Board consist of the Sanitary Commissioner and the Engineer?—Yes

17,217. How often does the Board meet?—I cannot tell you that.

17,218. Do you know of any great work or any improvements which have been the result of the action of the Sanitary Board?—I am not in the Sanitary Department and I really do not know. The only way I knew of the actions of the Sanitary Board was, that when a dispensary was to be built or anything of that kind done, it fell within the Surgeon-General's jurisdiction, and very often a reference was made upon it to the Sanitary Board and to the Surgeon-General. In that way I got to know what was going on in the Sanitary Board, but I really had nothing to do with it. And my knowledge in this regard is not complete.

17,219. Can you tell me what the total population of the province of Bombay is, roughly speaking?—About 19 or 20 millions.

17,220. There are five Deputy Sanitary Commissioners?—Yes.

17,221. How many Civil Surgeons are there?—Twenty-four.

17,222. How many dispensaries are there?—Taking the Municipal dispensaries as well as the State-aided dispensaries, I think there must be very nearly 300. We have 283 Hospital Assistants who have appointments, and then we have a reserve of 56, and in that reserve there are some who are employed in dispensaries. The reserve is supposed to be kept for leave, but now it is all employed on plague duty. We have, to quote exact figures, 296 dispensaries and there are 87 dispensaries in Native States in the Bombay Presidency.

17,223. What towns do you choose for dispensaries?—We place dispensaries in the headquarters of a district. In the Dharwar Collectorate, Dharwar is the head station of the place, and we had the Civil Hospital there; then in all the large and important towns like Hubli and Gagad and in all the Taluka towns, that is, in every sub-division of a Collectorate, we try to have a dispensary.

17,224. What is the smallest town in which you have a dispensary?—They go down to 3,000 and 4,000 people with a daily attendance of only 30 or 40 people.

17,225. Do the people get their medicines for nothing?—The medicines are supplied from the Government Medical Stores, and are paid for by the Dispensary Fund, which is made up by a grant from Government, a grant from the Local Board and a grant from the local Municipality. The poor get free advice and medicines.

17,226. Does the Civil Surgeon, the Sanitary Commissioner or the Hospital Assistant, or anyone connected with the sanitary service at all, ever visit the smaller villages?—The Sanitary Commissioner visits places of importance, his Deputies go everywhere checking vaccination returns, &c. The Civil Surgeon only visits dispensaries in his district.

17,227. What does the deputy Sanitary Commissioner examine in each village?—He has to check the local vaccinator's returns, but of course, members of the Sanitary Department can give you more minute details than I can.

17,228. What system of notification of infectious diseases have you?—We have no system.

17,229. What system of notification of deaths have you in the province of Bombay?—I think it is chiefly a matter of the police.

17,230. No sanitary officer concerns himself with deaths unless they are brought to his notice?—No, and has nothing to do with them, as far as I know.

17,231. Unless the Head man of the village chooses to report it, nothing is known?—I should imagine so.

17,232. So that plague, for instance, or any infectious disease might be going on in villages for six months without anybody being any the wiser?—That is a point I brought out. We had high mortality in many villages, and from the Surgeon-General's office we told them that there must be plague amongst them. Frequently they denied it, and then it was forced upon them afterwards.

17,233. (*The President.*) You would only know it in the event of the mortality rising above the average mortality to a distinct degree?—Yes.

17,234. And, of course, plague sometimes occurs without such a rise?—Yes.

17,235. (*Mr. Cumine.*) The village Head-men are bound to report the outbreak of cholera or small-pox, I believe?—Yes; they are bound to do it, and, as a matter of fact, cholera is almost always reported.

17,236. Supposing the Sanitary Department told the Collector that in a particular village or Municipal town there was a great deal of over-crowding, has the Collector any legal power to remedy that?—That I do not know; it is a question of the Municipal Act: I should think he ought to have, but I do not know whether he has.

17,237. Do you think he has, as a matter of fact?—I do not know what the Collector's powers in that regard are.

17,238. Do you think the Collector stands principally in need of information that a place is dirty, or in need of money and agency to make it clean?—I think that in India at the present time what is wanted is not so much trained agencies, because we have them now, but what we want is money for works. The trained agency at the disposal of the Government is quite good

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enough to advise for all the sanitary requirements of this country so far as I can see, for many years to come. What is wanted is money.

17,239. (*Dr Ruffer.*) Surely it would be better if you could have a system of notification and registration?—Minor details of that kind, if it can be done, might be instituted, but any large sanitary schemes, such as having a trained bacteriologist at every headquarter town, any gigantic scheme of that kind would be of no use. In every place any doctor can see what is required, from a sanitary point of view.

17,240. I should not call the notification of disease a minor detail?—Those details we might have, but any tremendous Sanitary Department such as you have in Great Britain we do not want.

17,241. (*The President.*) How long has the present Sanitary Department been in existence?—Certainly 20 years in this Presidency. I cannot tell you for the whole of India.

17,242. The great barrier for doing work has been absence of funds?—Yes, and also there are other things which are a great drawback in India. We have no Medical Act, and lots of disadvantages like that. Anybody in India can practise medicine or surgery.

17,243. Is there any immediate or early prospect of the requirements being met if there is want of funds?—I do not think there is.

17,244. Has that barrier of want of funds been found sufficient to prevent very large sums from being expended in Bombay and elsewhere during these epidemics on sanitary improvements?—I think very large sums have been spent in the epidemics of plague, and in that way the Government have done all that they could do.

(Witness withdrew.)

Mr. F. M. Gibson.

Mr. F. M. Gibson recalled and further examined.

17,245. (*Dr. Ruffer.*) The last time you were before us you gave us some evidence about the bacteriology of plague in human beings and rats, and also an account of an attempt to trace the bacillus in nature: have you anything to add to your bacteriological evidence?—I have no further evidence to add in that direction, but the investigations have been, and are still being, proceeded with.

17,246. Have you any more facts which you would like to put on record?—I have noticed recently in rats infected in Bombay an affection of the lungs which, I think, is important as an indication of how rats get the disease.

17,247. Will you describe that?—The lungs in an ordinary rat which has died of plague are usually found collapsed and pale. In the rats which I saw at the beginning of last month the lungs completely filled the thoracic cavity, and showed hæmorrhagic patches, and on section they were found to be tremendously engorged with blood. I failed to get the plague bacillus from the sputum, but it was very obviously present in the blood and the liver. I think it must have been in the sputum, although I failed to get it.

17,248. What is your conclusion?—I think it likely that these rats contracted the disease by inhalation of infected material. So far, I have found the bacillus in the blood of the rat, and also in the urine and the fæces.

17,249. How did you obtain plague bacilli from fæces?—Simply by smearing on a series of agar tubes, and isolating colonies in that way. The intestine was scorched through with a hot iron, so as to avoid contaminating with the blood in which the plague bacillus is, of course, present. Then the fæces were removed from the intestine with a needle.

17,250. How did you ascertain that the bacillus got from the fæces was really the plague bacillus?—That was isolated by smearing a sufficient number of tubes, and getting isolated colonies. Pure cultures were obtained, which were injected into rats, and the rats died of plague, and pure bacilli were recovered again.

17,251. What further conclusions do you draw from these observations?—That the rats are probably instrumental in infecting localities. I do not know how they do it.

17,252. Do you think the rats acquire the disease by inhalation?—I certainly think so in these rats which I got at the beginning of last month.

17,253. Not always?—No; these rats showed a special implication of the respiratory organs.

17,254. Have you any experiments to show that rats can be easily infected through the nose?—No.

17,255. Have you any more experiments with regard to rats becoming infected by feeding with plague cultures?—I have experiments going on, but they are not complete.

17,256. When you were here before, you said you would see whether rats contracted the disease by feeding on other rats dead of plague; have you made that experiment?—I only once got a rat which died after eating part of another rat dead of plague. It died within 10 hours of the original plague rat, and we were unable to recover the germ. Probably, I should say, it died of toxine poisoning.

17,257. They are all the experiments you have made on that point?—Yes.

17,258. Have you any other bacteriological points you would like to put forward?—I have some remarks regarding the occurrence of the bacillus in man. I found it always in the blood of dead patients and always in the sputum of patients who have died of the pneumonic form. Once I recovered it from the urethra of a patient who had died of the bubonic form.

17,259. In how many cases did you examine the urine?—Not more than five.

17,260. In bubonic or septicæmic cases?—Bubonic.

17,261. In how many cases did you find the bacillus?—In one.

17,262. Did you find the bacillus in the fæces?—No, I have never succeeded in doing that.

17,263. Did you examine the case in which you found the bacillus during life or after death?—After death.

17,264. Have you any other facts you would like to give us?—That is all as regards human beings.

17,265. (*The President.*) Have you found any bacilli in the sputum of bubonic cases?—No, only in pneumonic cases.

17,266. You have examined the sputum in many bubonic cases?—Yes.

17,267. And the results have been altogether negative?—Yes.

17,268. With regard to pneumonic cases have you found the bacilli in any excreta except the sputum?—No.

17,269. You have searched for it in the urine?—Yes.

17,270. And in the fæces?—Yes.

17,271. And you have failed to find it?—Yes.

17,272. Although in the same cases you have found it in the sputum?—Yes.

17,273. (*Dr. Ruffer.*) Have you made any other attempt to trace the bacillus in nature?—I have not completed any.

17,274. You have nothing you wish to put forward at the present moment?—M. Haffkine asked me to give some evidence with regard to the testing of the prophylactic.

17,275. Will you kindly give us your evidence as to that. In the first place, since when have you begun testing?—From December. The plague prophylactic is a broth containing chiefly albumose in solution inoculated with plague bacillus, and it is then grown for a period of from two to six weeks. Such flasks as show sufficient growth are then sent to be tested, and that was the work on which I was employed during December.

17,276, Are the flasks tested before sterilization?—Yes. The testing consists of drawing a small portion from the flask with a capillary pipette and smearing that on the surface of a specially prepared agar. It is of great importance to have the agar of a rather dense consistency and well dried. If it is not dry the growth is not characteristic, and it is difficult to see if there are foreign organisms present. Then the agar tubes are incubated in ordinary room temperature in Bombay, namely, 80° F., for three or four days. The growth of plague

thus obtained is quite characteristic. During the month of December I examined some 650 flasks, and of these, 50 were rejected as showing contamination with other organisms. That is a large number to be rejected, but it was chiefly owing to our having only a faulty form of flask with a rather wide neck. Latterly we have got into use a large flask with a very narrow neck. As showing the advantage of this I may say that the last 140 flasks which I examined showed no contamination at all. As to the nature of the contamination it was most frequently a yellow sarcina.

17,277. That is the only test which you have applied before sterilization?—Yes. After sterilization I also examined the flasks and only once found a living organism; but the contents of this flask were rejected as a result of the first test (before sterilization at 65° C.), the same contamination being present. It turned out to be a spore-bearing bacillus which had accidentally got admission. With this exception I never found a living organism in the material immediately after sterilization.

17,278. How was that examined?—It was examined in the same way on agar tubes.

17,279. Did you examine the contents of the flask which had been sterilised or the contents of the small bottles ready to be sent out?—The routine is for the decanters to inoculate an agar tube during the process of decanting a flask; every such tube is incubated for three days or more, and then examined by one of the staff of the laboratory, and the brews corresponding to those which are found contaminated are subjected to further examination with small bottles.

17,280. That has been going on since December?—Yes.

17,281. How much do you inoculate in each agar tube?—If you have sufficiently large tubes, and well dried-out agar, you can inoculate from ½ to ½ c.c.

17,282. Is that after the carbolic acid has been added?—Yes.

17,283. You have found a living organism only once?—Yes. The spore-bearing bacillus above referred to.

17,284. You do not examine each bottle separately after the prophylactic fluid has been decanted into it?—The large number of small bottles renders this impracticable, with each batch of small bottles placed in the sterilizer there was always a large flask of unsterilized broth. If this control broth remained sterile it was judged that the small bottles would also be sterile. The testing of small bottles I speak of refers to experiments carried out with a view to estimating the adequacy of the above test.

17,285. You do not re-sterilize the fluid after decantation?—No.

17,286. Have you anything else you would like to add?—That is all with regard to bacteriology.

17,287. Practically, your opinion comes to this, that Haffkine's fluid at the time it is being decanted is sterile?—Yes.

17,288. You are ready to give us some evidence on other points not connected with bactoriology, as, for instance, the detection and information of plague. We have had a great deal of evidence on that: have you anything special to put forward?—I have nothing special. I have noted, in my précis, instances of plague in relation to particular houses. I had hoped to give a complete series of figures with regard to the houses in my district as to the nature of the floors.

17,289. Can you give us the general results?—The figures are not complete, but I thought I might cite the case of two large houses which are in contiguity; one has stone or cement floors, and the other house has earthen floors, and is also densely populated, but not so much so as the first house. The first chawl contains about 300 rooms, and a population which varies from 2,000 to 2,500. Up to the 31st December 1898, two years and two months from the appearance of plague in the district, there were 18 cases of plague in that one chawl. In the neighbouring chawl, which was much smaller, there was a population of 300; that has been vacant since November 1897. It was first vacated in June 1897, and then re-occupied, and finally vacated in November 1897. There were 14 cases there in one year.

17,290. To what do you attribute the difference between the two?—There is very little difference except the nature of the floors. The races inhabiting the two houses are different—there is something to be said for that. The larger chawl is mostly Pathans and Musalmans, whereas the smaller house is chiefly occupied by low-caste Hindus, among whom plague is more prevalent.

17,291. (*The President.*) Which house had the larger per-centage of cases?—The one with the earthen floors.

17,292. (*Dr. Ruffer.*) Have you anything to tell us with regard to the floors?—With reference to disinfection in the district, I always used a half per cent. acid solution of perchloride of mercury. I think it would be better to use an even stronger acid solution. I think, considering the nature of the floors which are constantly giving up ammonia, acid is one of the most important ingredients of the disinfecting fluid. I do not think it is possible to say yet what effect this disinfection has had. In my district there were 1,344 houses. Of these, up to the 31st December last, 803 had had plague cases.

17,293. How many had plague cases after disinfection?—That would be difficult to say. All the houses in which plague cases had occurred previously would be partially disinfected, *i.e.*, the room and the neighbouring ones to it in which a case had occurred. One hundred and ninety-six had plague cases in two epidemics, and 17 in all three epidemics. The third epidemic has not run its course yet.

17,294. Are you speaking of disinfected houses?—Many of them are partially disinfected. There are 590 houses in which cases occurred in one epidemic.

17,295. Have you any evidence with regard to health camps?—The first health camp in Bombay was opened in my district about the middle of December 1897, and the average number of inhabitants in the camp was 500 during the time I was in the district. It was in the middle of an infected area. Plague cases occurred in the camp on 13 different occasions, all the cases occurring in people who had been less than 10 days in camp, or about whom I was able to obtain a history of their having slept outside the camp. It was not a segregation camp, but it was a camp in which the people had free ingress and egress. I never found that one case in the camp could be traced to another in the camp. I found that taking off the roof of the hut was an ample means of disinfection. Since I left the district in May of last year two cases have occurred in the camp in all the residents.

17,296. (*The President.*) How did these cases orignate in the camp?—The people are allowed free ingress and egress, they might get infection outside.

17,297. (*Prof. Wright.*) Before December had you ever examined the prophylatic as to its sterility?—No.

17,298. Have you ever seen a bottle sent back as suspicious?—No, they are not generally returned. I have not seen any.

17,299. You test the contents of the flask after sterilization by introducing half a cubic centimetre into an agar tube?—It is generally run directly into the tube from the flask.

17,300. Is it the first or last portion of the contents employed for the test?—It is usually in the middle.

17,301. With regard to the addition of carbolic which is made to the prophylactic, we have it from other witnesses that certain of the bottles have been found to be putrid; do you think there is any possibility of the carbolic having ever been omitted from these bottles?—I do not think so: the carbolic is added to the flask.

17,302. In testing the sterility of the prophylactic, do you dilute so as to get rid of the effect of the carbolic, or do you run the undiluted carbolized vaccine on to your agar tube?—It contains half per cent. of carbolic.

17,303. Do you think that the carbolic which is present may prevent the bacteria from growing on your agar tube?—Yes, it delays development.

17,304. But ultimately the carbolic would, you think, evaporate, and then the bacteria, if present, would grow?—Yes.

17,305. How long are the tubes kept incubating?—Four days.

17,306. Have you ever tried to dilute the carbolized vaccine to see whether you could get any greater growth upon agar than you do by your present system of testing?—No.

17,307. Is there any precaution taken to see that the bottles or corks are sterilized? Is there any bacteriological test as to their sterility?—It has been done recently, I think, but I have no information of that.

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17,308. Is not it possible that although you might be decanting a sterilised fluid into the bottles, the bottles and corks might contain contaminations and therefore the fluid you send out might have bacteria in it?—Yes.

17,309. No precaution was taken against that as far as you know?—The bottles are sterilized.

17,310. But they are not tested?—No; at least they have not been till recently. I believe some testing has been carried out recently.

17,311. You say that you think that more than half per cent. of acid ought to be added to the disinfectant which is used for disinfecting the floors of houses?—Yes. Especially if lime has been mixed with the cow dung.

17,312. Have you tested the reaction of your floor after disinfection?—No.

17,313. Why do you think ammonia is given off from a cow dung floor?—The smell of ammonia is quite perceptible in a closed room.

17,314. (*Dr. Ruffer.*) I understand that the fluid was sterilized twice, once by carbolic and once by heat?—Yes, the carbolic is added after the heating.

(Witness withdrew.)

Dr. C. Balfour Stewart.

DR. C. BALFOUR STEWART called and examined.

17,315. (*The President.*) You have been employed in the Plague Research Laboratory?—Yes, since May.

17,316. (*Prof. Wright.*) You have been employed on testing the prophylactic?—Yes, I am responsible for it. I have done all the testing since the middle of May, except for the month of December, when I was away on leave.

17,317. Between May and December what tests were you applying?—The same as we do now. A small quantity was taken out of the flask with a sterilized pipette and inoculated on dry agar.

17,318. That is before the sterilization is undertaken?—Yes.

17,319. But is the testing of the vaccine after the sterilization process a new thing, or has it been done ever since last May?—It is comparatively new to test the bottles after sterilization and after decanting.

17,320. When was that system introduced?—I cannot say. I found it so when I came back.

17,321. When did you come back?—In the beginning of January.

17,322. When did you leave the laboratory?—The last day of November, I think.

17,323. This has been introduced between the end of November and the beginning of January?—I fancy so. I have not anything to do with that. I saw what was being done.

17,324. Your duties consisted chiefly in testing the purity of the broth culture before it was sterilized and converted into vaccine?—Yes.

17,325. Have you done anything else but that?—I have made several experiments with regard to the alkilinity of the broth, and I tried to find some other albuminous substance for the growth of the plague bacillus other than peptone.

17,326. I thought you had some evidence on the purity of the prophylactic, and the condition in which it was sent out?—I have done one or two small experiments to test the tendency of the prophylactic to become putrid from air organisms falling in. For instance, I have left a bottle of prophylactic open for a month, and taken cultivations at the intervals of two or three days with certain results. After it had been left open for 15 days or so I got a few colonies in the tube, and one or two days after that I got no cultivations at all. Again, I took another cultivation three days ago. After shaking the bottle, I took some from the top of the bottle which was fairly clear, and that remained quite sterile; but a cultivation taken from a lower part —from the sediment—gave about 30 colonies which had run together. The rest of the tube was covered with debris, but no growth.

17,327. Do you think living bacteria may be present in the sediment?—I think they may, but, at any rate, this prophylactic, which contains half per cent. of carbolic acid, is not a good medium for the growth of micro-organisms. A small flask of sterile broth put at the side of that bottle of prophylactic began to smell in three days, and it was thrown away. The prophylactic is perfectly sweet now, and there is no smell although it has been open for a month. After doing some inoculations at Tarrapur on the 18th of last month, I brought back the empty bottles. When I was at Tarrapur, immediately after doing some inoculations I poured the dregs of two or three bottles into one bottle and corked it up. I took inoculations from the dregs of that bottle which contained the dregs of two other bottles. That remained quite sterile, and I have inoculated the agar tubes at intervals from that bottle, which I call bottle A. On one occasion I got about 10 colonies, and on another occasion I got three or four colonies of a red bacillus. After that I got two or three tubes with no colonies at all. Probably this red bacillus was no longer present. There are no colonies on the last tube to-day.

17,328. These are isolated experiments; have you a methodical series of experiments or have you merely certain isolated experiments?—I have two or three isolated experiments with this prophylactic.

17,329. You have not entered into any systematic investigation of a number of samples?—No.

17,330. These experiments tend to show that the addition of half per cent. of carbolic renders the prophylactic a bad medium for the growth of bacteria? — Even after keeping some bottles I decanted the dregs of several bottles into one. The bottles had been lying about, and there was a quantity of dust on their necks, but I decanted all the dregs into another bottle and adopted the same process as before. I got a few large colonies and a large number of small colonies containing tetra-coccus. They did not increase in number: the last tubes I used did not show any more bacilli than the first tubes.

17,331. What experiments have you made with regard to the possibility of finding another culture medium?—I wish to find a substitute for peptone for nutrient broth, but nothing very much really came of it. It was necessary to find something which could be easily manipulated.

17,332. You have not succeeded in finding an effectual substitute?—I found the plague bacillus would grow very well in the alkaline albumen from white of egg, but eggs, I found afterwards, were inadmissible. I tried fish and cheese and glutine and pea flour, but all those things could not be manipulated very well. The solution would not filter clear in the slightly alkaline medium, and it only filtered clear at a too high degree of alkalinity for the growth of micro-organism.

17,333. M. Haffkine says in the letter before me, "I "beg to request the favour of your kindly obtaining "that the first two officers, Drs. Stewart and Gibson, "who have been called to give evidence before the "Indian Plague Commission, be questioned in detail "as to the above operations and their results," that refers to the operation of testing the vaccine?—Yes.

17,334. Testing the vaccine before sending it out?—Yes.

17,335. Have you any experiments which bear on the purity of the vaccine?—All my experiments have been in testing the purity of the vaccine after it has left the cultivation room.

17,336. Have you any experiments on the condition of the vaccine after it has undergone the sterilizing process?—I have not done any after sterilization.

17,337. I understood M. Haffkine referred to that?—No.

17,338. (*Mr. Cumine.*) You mention at the bottom of your précis an experiment of cultivating microbes from mud floors after disinfection with perchloride of mercury. Will you tell us what the result of that was? Did you make only one experiment?—Several small experiments were made. I scraped up some mud from the floor of a house which had been recently disinfected. In one case half an hour after disinfection the mud was damp still. In another case I scraped up some mud from the bottom of a pool of disinfectant after sweeping away the water, so that the disinfectant in that case had remained on the surface of the floor

ever since it was squirted. From all those I got a considerable growth of organisms and a good many cocci. Some of the bacilli were spore-forming, and it is quite possible they might not have been killed by the perchloride of mercury lotion, but there were numbers of micro-cocci present, which being without spores certainly ought to have been killed. I looked for plague bacilli, but the means at my disposal were not sufficient.

17,339. (*Prof. Wright.*) Did you disinfect those floors yourself?—I did not do it myself, but it was done under my observation.

17,340. Did you make up the disinfecting fluid?—No. It was made up with perchloride of mercury and a salt solution.

17,341. Do you know how it was made?—It was made according to the Government formula.

17,342. You did not yourself superintend the making up of the disinfecting fluid?—No.

17,343. Then you were testing the disinfecting powers of a disinfectant of unknown strength?—Yes.

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17,344. And the deduction which you draw is that the disinfectant is not effectual?—Yes, with perchloride of mercury, the pistons of the brass pumps which are used become perfectly mirrored with mercury, and the grease which is used for the pump is turned into a mercurial ointment practically.

17,345. You have not made experiments to see if you could disinfect satisfactorily with perchloride of mercury?—No.

17,346. (*Dr. Ruffer.*) How did you neutralize the perchloride of mercury when you made your cultivations?—I scraped up the mud from the bottom, and then inoculated some sterile water with them, and took the cultivations from them.

17,347. But that did contain a certain amount of perchloride of mercury?—Theoretically it would.

17,348. Did you take any steps to neutralize the perchloride of mercury?—No.

(Witness withdrew.)

(Adjourned till Monday next.)

At The Secretariat, Bombay.

FORTY-SIXTH DAY.

Monday, 13th February 1899.

PRESENT:

PROF. T. R. FRASER, M.D., LL.D., F.R.S. (*President*).

Mr. J. P. HEWETT.
Prof. A. E. WRIGHT, M.D.
Mr. A. CUMINE.
Dr. M. A. RUFFER.

Mr. C. J. HALLIFAX (*Secretary.*)

Assistant-Surgeon P. V. SHIKARE called and examined.

Assist-Surg P. V. Shikare.

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17,349. (*The President.*) You are a Licentiate of Medicine and Surgery of the Bombay University?—Yes.

17,350. With what office have your duties in connexion with plague been associated?—I have been Medical Officer in charge of one of the segregation camps since October 1897.

17,351. Have you had the medical care of the patients in that camp?—Yes.

17,352. And have there been many patients under your charge?—There were no actual patients to be treated, because, as soon as a case developed symptoms of plague we sent it to hospital. There was general management, and such cases were to be detected as soon as they appeared in the camp. That was the chief object of a Medical Officer being there.

17,353. You have formed an opinion as to the predisposing conditions leading to plague?—Yes.

17,354. Would you express your opinion?—The predisposing conditions are those conditions that were pre-existing, and favoured the growth and spread of the infection. Thus, the subsoil of the town, including the floors of inhabited houses, was saturated with moisture and decomposing organic matter. Then there was general vitiation of the air on account of overcrowding. The climatic conditions of Bombay were also favourable for the growth of low organic life. Those, in my opinion, were the predisposing conditions.

17,355. What climatic conditions do you refer to?—The temperature and humidity of it.

17,356. Do you mean high or low temperature?—A temperature varying between 80° and 90°. That is suited for the growth of low organic life.

17,357. From where do you think the plague was introduced?—I think the plague was introduced from some of the epidemic areas outside India. I refer especially to the southern ports of China, such as Hong Kong.

17,358. You think it came from China; what is your reason for thinking so?—Because the place where plague was first observed was Mandvi. Now, Mandvi, in Bombay, is a place nearest to the docks, and it shows that the poison was imported by sea somehow or other; the only places outside India where the epidemic has been known in recent times is the southern ports of China; and also the town of Singapore, which was visited by plague in the spring of 1895.

17,359. You exclude Kumaun and Garhwal?—Yes.

17,360. Why?—I think that those places are only endemic areas for plague; no recent epidemic has been recorded in those places, because the latest dates of plague occurring in Kumaun and Garhwal are 1884 and 1886; 1884 for Kumaun and 1886 for Garhwal, and in my opinion, there is a far less chance of the disease being spread by places where it is endemic.

17,361. You exclude them because plague has not recently been present there. Have you any other reasons?—My other reasons are, because there is no direct communication between those places; they are not very important commercial towns; and if pilgrims were to travel from those places, they would take a long time to reach Bombay, as they generally travel on foot, and visit several other places on their way; so, if Bombay was infected, there was also a likelihood of several other places being infected before Bombay was infected, or along with it. But such was not the case, and, therefore, I do not

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think myself that plague was imported from endemic areas such as Kumaun and Garhwal.

17,362. Plague also visited Nasik?—Yes; but not during the first year while Bombay was affected.

17,363. How does that bear on your theory?—This way; the year 1895-6 was a sacred year, and thousands of pilgrims visited the sacred town of Nasik. If they come from all parts of India, there is a likelihood of their coming from these places also in Northern India, and of bringing the plague with them; but Nasik entirely escaped during the year, and so, I think Kumaun and Garhwal were not responsible for the plague of Bombay.

17,364. How do you think the poison has spread?—I think the poison is mainly in the soil, including the floors of houses which get mainly infected. The poison is existing there, and it probably grows in the soil, and from it it is taken into the system.

17,365. What are your reasons for supposing that the virus is in the soil?—I will read them:—Thus we know, from elements of bacteriology, that the lowest form of organic life (pathogenic or otherwise) thrive best in a medium tainted with putrefying organic matter having a certain amount of warmth and moisture in it, and removed from direct exposure to the sun's rays. Are not all these conditions fulfilled in the floors of inhabited dwellings generally, and those in overcrowded towns and cities in particular? The floor of an average native house will be found to have been made without any plinth, and therefore saturated with moisture derived both from the high subsoil water-level as well as from surface oozings. The moisture, again, is not aqua pura, but has in solution and suspension, besides inorganic substances, the products of decomposing materials derived from defective drains, where they do exist, or from a wholesale percolation of sewage water where the drains are absent.

17,366. What is your opinion as to the conveyance of plague by rats? Have you any facts bearing on that?—I have not got any facts to show. The only thing is that, as rats exist in soil and pass 20 hours out of the 24 in the soil, they are likely to be first infected; and this is observed to be the case everywhere.

17,367. What observed facts have you in regard to the incubation period; and what are your conclusions?—Of the cases of plague occurring in my camp, I have observed that the largest number was on the first day—that is, the day of admission—and nearly two-thirds of the entire number of cases developed within the first 48 hours of their stay in the camp, and this number of plague cases fell immensely in proportion to the number of days passed in camp; and there was no case observed amongst contacts beyond the 7th day. The total number of plague cases amongst contacts up to the 31st of December 1898 was 167. Of those, 52 developed symptoms on the day of admission, 35 developed symptoms on the first day, 20 on the second day, 21 on the third day, 12 on the fourth day, 14 on the fifth, seven on the sixth, one on the seventh, no case on the eighth, one case on the ninth, one on the tenth, and two after the tenth day. These three later cases—that is, those on the seventh, ninth, and tenth—were amongst re-contacts—that is, those persons who were in contact with plague cases developed in camp.

17,368. What is your conclusion with regard to the period of incubation?—I think, so far as my camp was concerned, it fell within seven days; and over two-thirds, most of the entire number of cases developed within 48 hours of admission.

17,369. How do you think the virus is introduced into the patient?—In bubonic cases, I think, the virus gets in by a process of inoculation. That is, it enters the system through some wound or abrasion or crack in the superficial structures in lymphatic connection with the particular set of glands affected. It enters the system through such wounds.

17,370. Why do you say that?—Because there is such a large per-centage of bubonic cases with buboes in the groin. I have already said that the poison is mainly in the soil; so of course, as a sequence to that, we expect a large number of cases with bubo in the groin, because the natives of India walk bare-footed, and they sit on uncovered floors.

17,371. You have had no distinct observation in which the virus has been introduced by lesions of the skin?—No.

17,372. Have you had much experience of pneumonic cases?—I have seen some, but I have not much experience with them.

17,373. Have you observed any pneumonic cases that seemed to have proceeded from bubonic cases?—No.

17,374. Have you observed any bubonic cases which seemed to have proceeded from pneumonic cases?—No.

17,375. Have you observed any pneumonic cases which seemed to have proceeded from other pneumonic cases?—No.

17,376. You have expressed the opinion that clothing may hold the virus, and thus communicate it to someone else; is that so?—Yes.

17,377. Have you any facts of your own which show that clothing, or anything but the person of a patient may convey plague?—No, no facts at all.

(Witness withdrew.)

Dr. A. Mayr.

Dr. A. MAYR called and examined.

17,378. (*The President.*) I think you hold medical qualifications from Germany?—From Austria.

17,379. Have you for some time been occupied in Bombay in assisting Professor Haffkine in plague matters?—Yes, since February last.

17,380. (*Prof. Wright.*) I believe you have something to tell us with regard to what you call the fermentation process that goes on during the manufacture of Mr. Haffkine's vaccine?—Yes. My duty has been to inoculate the fluid and prepare it. Dr. Stewart, in his evidence on Saturday, also touched upon this point.

17,381. Have you any new points? I do not know what you understand by the fermentation process; do you mean that you have examined chemically the changes which occur in the culture medium in which the plague-bacillus is grown?—No; it was a term which was suggested by Professor Haffkine. He meant certain trials about different culture media, about reaction of culture media, about influence of temperature.

17,382. Have you made some experiments in connection with the growth of plague in different culture media?—Yes.

17,383. Perhaps you would begin with the question of the reaction of the fluid. Do you find that an alkaline reaction favours the growth of the plague?—The best reaction may be very slightly alkaline, it may even be neutral, especially if there is the growth for a long time in the liquid; whereas in agar and solid medium, a little more alkalinity may be of advantage; as it seems, that during the growth, alkali, and probably ammonia, is produced, which increases the alkalinity, and interferes with the further growth of the microbes.

17,384. Have you made experiments to determine whether the alkalinity of the culture medium increases *pari passu* with the growth of plague?—Yes. It happened by chance that several times our culture liquid got directly acid.

17,385. You mean that the culture medium was acid when it was inoculated with plague?—Before it was inoculated. As it is prepared with acid not sufficient alkali was added, especially as the ammonia sometimes gives decided reaction, before the final sterilization—after heating and sterilizing, the ammonia being evaporated, slightly acid reaction remained. That happened several times. I tried reaction with litmus paper to ascertain the reason why the microbes did not grow in the beginning. I found it was slightly acid reaction. In certain cases I added a small quantity of alkali and then the growth began. In other cases I left the liquid without adding alkali, and, perhaps, after two or three weeks, a growth began to show itself; and if the reaction was tried after that, it was found to be alkaline—not very strong, but still decidedly alkaline.

17,386. Did you find that that alkaline reaction was due to the presence of ammonia?—Partly, no doubt; because, in trying the reaction with red litmus paper, it turned blue, but after being dry the blue colour disappeared almost entirely, showing thus that the

alkaline reaction was chiefly due to volatile alkali (ammonia). Further chemical investigations were not possible, as there there were no means for them.

17,387. Have you made any observations upon different culture media?—Yes. I have tried sugar, gelatine, and glycerine to the common culture media, but no advantage was to be found, especially with sugar. Sugar seems rather to interfere with the growing. No doubt the microbe grows best in soluble albuminous matter. It grows in albumose better than in real peptone; at least it shows a quicker growth—as our present culture liquid contains almost all albumose, and very little peptone.

17,388. Have you made any observation upon the accidental contaminations of the prophylactic fluid?—Yes; but Dr. Stewart referred to that matter.

17,389. Dr. Stewart told us that he had made no examination of the fluid after it had been finally sterilized?—We inoculated last year 1,200 flasks.

17,390. These experiments bear upon the fact of the occurrence of contamination before the prophylactic is sterilized?—Between inoculation with plague-germs and final sterilization.

17,391. These experiments do not bear, do they, upon the possible contaminated condition of the prophylactic as it is sent out?—No; after the inoculation with plague-germs.

17,392. That is one of the intermediate stages of manufacture?—Yes.

17,393. Have you any observations on the fluid as it is actually sent out—observations, I mean, which bear on its sterility or otherwise?—I made certain investigations lately, and there is no doubt that in certain cases a scanty growth, especially in very old bottles which are badly corked, may be found. I am making experiments on the subject at the present time.

17,394. You have no experiments you propose to put in?—No.

17,395. You have only made a few examinations?—I made a few examinations for myself, because I thought the original sterilization of the bottles might not be always sufficient. I made certain experiments, but I have no idea of giving evidence with regard to them, because they are not conclusive enough.

17,396. Is it your opinion that sterilization at 65° C. for an hour is always effectual?—Yes, as regards plague microbes.

17,397. But not with regard to others?—That refers to the original sterilization of the empty bottles in which the liquid is afterwards decanted. When the flask with the culture is heated to 66° C., there are no other microbes in it than plague microbes. All other flasks are rejected.

17,398. Have you made any experiments with Lustig's serum?—Yes; I had the opportunity, when Dr. Galeotti came here, of going to the hospital where he injected; and I observed a few cases which seemed to me of very great interest, and, perhaps, of more value than statistics.

17,399. Was the serum that you saw injected, serum that had been manufactured in Italy?—Yes, it was serum from Italy.

17,400. How many cases did you see injected with this serum?—I saw many cases. I used to go to the hospital. There were only two cases in the very beginning in which bacilli were found in the blood by microscopic examination and by the culture method. After the injection of this serum—two injections in every case—the microbes disappeared, and the patient recovered in a very short time after three injections.

17,401. Do the microbes ever disappear from the blood in cases where serum has not been given?—Such cases are supposed always to end fatally, at least, all the observers say so.

17,402. Have you yourself made any observations upon this point?—I know that Dr. Bitter states that when the blood is infected with microbes no recovery has ever been noticed. The Austrian and German Commissions stated the same. It may be that somebody else may have observed cases where plague microbes have disappeared by themselves, but I could not say anything about it. I only know that there were three cases, two of which I saw myself. I was present at the examination where the plague microbe disappeared and the patient recovered.

17,403. You speak, in your précis, of nervous symptoms improving after the injection of serum?—Yes. This is really a very common symptom, and goes sometimes together with the lowering of the temperature, sometimes it is entirely unconnected with it.

17,404. (*Dr. Ruffer.*) I wish to ask you some details with regard to the few cases in which you said that the microbes disappeared after the injection of Lustig's serum; how long had these cases been ill when you first examined the blood?—The first, on the 11th of April, was the case of a sepoy from a regiment here. He was a very strong man, a Hindu, 20 years old. He was sick one day, and brought to hospital. There was continuous vomiting and diarrhœa from the beginning. He was very delirious and agitated when brought to the hospital. He had, as is usual, to be tied down, and have a special attendant. His temperature was 104½ and pulsations 128. He had a small tumour of the spleen, and a small bubo in the right groin. The symptoms were rather alarming.

17,405. Did you make an examination of the blood?—Yes. After sterilization of the finger, a drop was taken, and coloured in the usual way. Several agar tubes were made before inoculation, and at the microscopic examination microbes, looking like plague microbes, were detected. After two days, the agar tube showed an abundant growth of pure culture of microbes, which were afterwards inoculated into the peritoneal cavity of white mice, and produced death, in one case in 26 hours, and in the other case in 24 hours. The man was injected in the morning with 20 c.c. of serum. I may state that it was the most active serum which has been brought out. The temperature went down to 102, but as the delirium did not change, he was again injected with 15 c.c. in the afternoon. On the morning of the next day he was much better. He only vomited twice during the day, the delirium had nearly passed away, and he looked quite free. In the afternoon he was inoculated a third time. Before the third inoculation another examination of the blood was made in the same way, with coloured preparations and with cultures, but no bacilli could be found. The man recovered very soon. On the 13th April—on the third day of his sickness—he was so well that he did not require a further injection. He was perfectly conscious and had no vomiting or diarrhœa. The small bubo, which was very painful in the beginning, disappeared after five or six days, and the man himself was discharged as cured after 12 days. The second case is that of a ward-boy at the Arthur Road Hospital, a very strong man about 20 years of age. He got sick during the night of the 15th of April (four days after the other man was brought there) with shivering; and then the temperature began to rise. As it was thought that he had got plague he was at once taken to the hospital, and in the morning he was shown to us. His temperature was about 100° and he had a small bubo in the right armpit. He was not thoroughly unconscious, by that I mean to say he was not delirious. He was in a kind of stupor. He did not answer any questions. An examination of the blood was made on the same day. The whole preparation was full of microbes, which looked like plague microbes. Four agar tubes were taken, and the characteristic colonies appeared. White mice were inoculated, with a similar result to that of the first case. This patient was injected for the first time at 10 o'clock in the morning with 20 c.c. In the afternoon he was somewhat better, but as no special amelioration could be noted he was again injected with 15 c.c. The following morning he was much better. His temperature went down to 90°—it was never very high. His pulse, which was rather high—130—went down to 92. His respiration, too, at first seemed to point to some complication in the lungs. A second examination of the blood was made, and no microbes could be found. The same remark applies to the agar tube. The man had a third injection of 10 c.c., and he, also, recovered very quickly without any interference. He was discharged 11 days afterwards. There was another case, but I had not the opportunity of seeing it myself. That other case ended in recovery, notwithstanding the fact that plague microbes were found in the blood. In all three cases the injections were made at the very first moment and the patients recovered very soon.

17,406. In how many cases have you examined the blood for plague microbes—not including these two cases?—Only two. I was not really employed in the hospital. I was present, but I did not make the preparations myself, as I had no microscope at the Arthur Road Hospital.

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17,407. Have you examined urine for plague microbes?—I have not done so myself, but I know the case of a private patient of Dr. Galeotti where plague microbes were found in the culture. That case eventually recovered after a long period.

17,408. Have you ever examined the fæces?—Yes. A short time ago I saw a case where there was extensive bleeding from the bowels, but no microbes were found. I got a cultivation, but I could not find any plague microbes.

17,409. Have you ever examined the fæces of rats?—In a few cases, but I did not make any real methodical experiments.

17,410. Have you ever found plague microbes in the fæces of rats?—No. I only made experiments in a very few cases. But I found plague microbes once in the urine of a rat, but in that case I had not the opportunity of making more experiments.

17,411. What animals do you think are more susceptible to the inoculation of plague, mice or rats?—I think mice.

17,412. In what way—what kind of inoculation?—In the peritoneum. We used to do that when we wanted to be sure—to kill the animal when we were not sure about the virulence.

17,413. Is that better than sub-cutaneous injection?—No doubt it is. It is quicker and more sure.

17,414. Have you any facts to show that rats can take inoculation through the mucous membrane of the nose?—I have made no experiments with regard to that.

17,415. Have you any experiments to show that sub-dural inoculation will produce plague?—No: but I have no doubt about it.

17,416. Nor have I; but I want to know if you have any facts about it?—No, I have not.

17,417. (*Prof. Wright.*) Do you, in connection with the preparation of the prophylactic, inoculate the flasks with plague?—Yes.

17,418. How do you get your plague cultures: do you employ a series of different plague cultures?—When there were more plague cases in Bombay, we used to get every second or third time fresh microbes from patients in the Arthur Road Hospital, from the buboes or the sputum, and carry them on agar, or peptone solution, and inoculate that. Later on, in April and May, it was difficult to get them because we had no patients. We had not, at that time, the opportunity of carrying microbes through rats, and so on, because there were very few hands there, and we had not the means of doing it. We were obliged to carry the microbe from one peptone solution to another. Once we got fresh microbes from Hubli, and at another time from Bangalore. About every month we tried to get the fresh virus. Now, we have more time, and we are not so short-handed. We are beginning to get our cultures by inoculating rats every time, and we take the microbes from the rats.

14,419. Was it your practice to test the virulence of the microbe you used?—No, the virulence was not usually tested. At that time we had no time or possibility of testing the virulence by making the injection in animals.

17,420. Do you think that sometimes you may have used very virulent plague, and at other times a non-virulent plague?—Yes, no doubt; because I remember that, when the Russian doctor was working at the laboratory, I had the opportunity of getting from him virulent microbes, as he was making extensive experiments in connexion with inoculation. He usually had a stock of virulent microbes, so I got them from him. At other times, we had this microbe and another microbe carried through one month from one flask to the other, never passing an animal, so, no doubt, in such cases the virulence was diminished.

17,421. Do you know what brews were made with virulent microbes, and what brews were made with non-virulent microbes; have you records with regard to that?—Yes. That means to say, I know such source was used for such a number of flasks. Others are, I believe, more virulent. The first set, before it was brought in a second peptone solution, ought, no doubt, to be more virulent than the later ones, which were taken, perhaps, in the tenth generation from this one.

17,422. Have you any facts to show that vaccine was more efficacious when you used virulent microbes than when you used non-virulent microbes?—I had not anything to do with the inoculation of the prophylactic itself. It seems that from outside it was stated that in some cases less fever was produced. I do not know that the fever has any very real bearing upon the immunity. That is a question of itself. I have no experience about this.

17,423. Latterly, I believe, you have been making passages through rats to increase the virulence of the microbes?—Yes.

17,424. Have you found, so far, the virulence increases by making passages through rats?—No conclusive experiments have been made till now to ascertain the increase of virulence or otherwise by passages through rats, but such experiments are being commenced. One rat which I inoculated has not died up to date. This is the third day. I made the injection on Friday, but the rat did not die, probably because our microbe was not virulent enough. I wanted the microbe which was used for our inoculation to pass it through the rat, and to get the increase of virulence; but it was not virulent at all. I took one-fourth of the culture in the test-tube, and it did not kill it. I have tried to get another microbe from the hospital. (Note added by witness on correcting proof of evidence:—The rat died the day after I gave evidence. From a culture made on agar from its heart's blood another rat was inoculated with a loop-full, and died after three days. The virulence must, therefore, have been increased in this instance.)

17,425. At the present you are making vaccine with plague microbe which is not virulent enough to kill rats in three days when you inoculate one-fourth of a tubeful?—That is so. It was, however, only one experiment. It may be that the second experiment may prove successful.

17,426. Is this the first time you have begun to increase, or to try to increase, the virulence by passages through rats; have you any previous experiments to show whether the virulence can be increased by such a series of passages?—I was helping and working with Dr. Galeotti and Dr. Vigoura, who had continually to pass their microbes through rats or monkeys to make them more virulent. I remember that Dr. Vigoura got an original microbe from us, which was not virulent sufficient to kill monkeys in three days, and it was passed twice through white mice, and then rats, and two monkeys, until the required virulence was obtained, so that the monkey died within 42 hours; whereas in the commencement there was no possibility of killing a monkey with the same microbe in such short time.

17,427. Then you do not agree with Dr. Yersin, who says that the virulence decreases in passing it through rats?—No; at least, from what I saw, we never had that experience. We always found quite a marked increase by passing it through rats and mice.

17,428. Have you seen enough to make you feel sure that the virulence of a plague culture increases when you make a series of passages through rats?—I never saw it passing through a series of rats.

17,429. How many generation of rats do you think the culture was passed through?—The one I have in my mind just now was passed through two white mice, then a rat, and then two monkeys. Then it was of the required virulence to make the culture for the serum preparation.

17,430. You have not seen a culture pass through three or four generations of rats?—No; we did not do that, because we did not want any more than that. It may be that after several generations of rats it may decrease. I will make experiments with regard to that later on.

17,431. (*Dr. Ruffer.*) Have you tried the effects of Haffkine's prophylactic fluid on animals?—Not myself; but it was made in our laboratory, and I know about it. Experiments were made on monkeys, especially with the deposit of the liquid, as it was supposed that immunity might be conferred by the deposit.

17,432. Has it an immunising effect upon animals?—I do not say it saves from a very virulent microbe; but there is no doubt the resistance is very much increased. The control animal always died, whereas the immunised sometimes survives. Whenever the immunised monkeys died, they always died much later than the other one.

17,433. Have you made any experiments yourself?—No.

17,434. Who made the experiments?—The Russian doctor who was working in Professor Haffkine's laboratory. When he was working there I saw him do it.

17,435. You are not responsible for these experiments?—No.

17,436. (*The President.* Have you any method by which you could exactly define the toxic activity of any virus you have grown?—Yes; it is to be done by inoculating a filter culture. Experiments have been made which I know of (though not performed by myself) which show that strong toxic effects are produced by filtered cultures.

17,437. I mean with the virus containing the living bacilli?—I should not say otherwise than by trying the amount which is required to kill a rat or a white mouse, as is usually done. I did not notice any difference in the growth in different media. It may be that very virulent microbes grow a little slower than the less virulent ones.

17,438. Did you adopt any process for determining exactly the virulence of any virus; have you any method you are in the habit of using?—Yes. The usual way is to have a certain culture. You take a certain amount, a loop-full, and dilute it with a certain amount of water or broth, and then inoculate a given amount. You compare as much as you want from it. If you have a microbe that kills with a quarter of a test-tube, it is supposed to be more virulent than a microbe which kills with an injection of half a test-tube.

17,439. In the case of any virus which you have examined, can you tell us what is the smallest amount that will produce death in any given animal?—I should say with what we call a platinum loop-full—even with less virulent microbes—a rat dies.

17,440. Have you made experiments with the filtrate?—Not myself; but I saw the experiments made. *Dr. A. Mayr.* 13 Feb. 1899.

17,441. You know the result of the experiment with the filtrate; you have observed the experiments?—Yes.

17,442. On what animals were the experiments made?—White mice.

17,443. On mice only?—Yes.

17,444. What is the smallest quantity of this filtrate to a given weight of animal which is capable of producing death?—I could not tell from memory. I think it is a very small quantity; about 1 c.c.—and less if the filtrate is derived from a virulent culture—killed a white mouse in 48 hours.

17,445. I understand you determine the lethality only with the liquid filtrate?—Yes.

17,446. Have you ever passed the virus through mice?—Yes.

17,447. Have you seen any cases in which the virus became attenuated, or weaker, after being passed through animals?—No, I did not notice such a thing.

17,448. Do you know if such experiments have been made in Bombay with regard to the plague bacillus or virus?—Not to my knowledge. Dr. Gibson has made many experiments with regard to rats; but I do not know anything about them.

17,449. Do you know if any experiments have been made by introducing the virus into the alimentary canal?—No, I do not—only by way of feeding. I believe it has been done here by Dr. Gibson, but he has had no positive results that I know of. I believe it has been done by several other observers.

(Witness withdrew.)

Captain C. J. R. Milne, I.M.S., called and examined. *Capt. C. J. R. Milne, I.M.S.*

17,450. (*The President.*) I understand you are conducting investigations in the laboratory here in connection with plague?—Yes, in M. Haffkine's laboratory.

17,451. (*Dr. Ruffer.*) Would you read to us your rules for sterilising the culture flasks?—They are as follows:—

"1. All cultivation flasks ready to be sterilised are placed by the testing officer in the inoculation room on a special table designed for that purpose, and having a label pasted upon it, "Flasks ready for sterilisation."

"2. The flasks are to be transferred into the sterilisation room by the sterilising officer himself, or by his assistant under his immediate supervision.

"3. Only the precise number of flasks ready to be put immediately into the sterilisation tub should be removed from the inoculation room. Any flask which cannot be put into the sterilization tub immediately should be replaced into the incubation room. On no account should unsterilised cultivation flasks be allowed to remain in any part of the laboratory except the incubation room.

"4. A label to be made out for each flask, stating its number, the quantity of prophylactic it contains, and adding the word 'heated.' The date of heating, however, must not be entered on the label till the flask is removed from the hot-water bath and from the cooling tub.

"5. The sterilising officer further puts his initials in red ink on the original label affixed to the flask.

"6. Place the new labels on the plug of the flask and fix the paper cover on the mouth.

"7. Put the cultivation flasks, together with the 'control' flask, into the water bath, and mark in the special note book kept for the purpose the time when the thermometer in the 'control' flask shows 65° C. Leave the flask in the tub for one hour counting from that moment.

"8. On the expiration of the hour remove from the bath and cool.

"9. The original label of the flask to be examined, and to ascertain that it got either detached in the hot-water tub, or that the red ink of the initials shows the effect of the hot water. The new labels to be now removed from where they have been resting on the plugs, the date of heating marked on them, and the labels themselves pasted on the sterilised flasks.

"10. Note the quantity of prophylactic in c.c. in each flask, and add carbolic acid in the proportion of one-half per cent."

17,452. How do you measure the quantity in each flask?—Originally, before the flasks are inoculated, the amount of c.c. is placed on each flask, generally 1,200 or 1,500 c.c. That remains on the flask till it comes to the sterilising officer, who makes a note on his new label of the amount. The rule goes on: "The total quantity "of carbolic acid added to each flask to be marked on "the label."

"11. Mark the fact of carbolising, and the date on which this was done, on the label.

"12. In the book kept for the purpose, enter the date of heating and carbolising against the numbers of the flasks heated and carbolised in the respective columns, and state the actual amount of pure carbolic acid added.

"13. Flasks are now ready for decanting, and should be placed in the special cupboard for the decanting officers."

17,453. How do you add the carbolic acid?—A bottle of carbolic acid is sterilised and sterile pipettes are taken on which the quantity is marked in c.c.

17,454. What strength of carbolic acid do you use?—Pure carbolic acid: the ordinary carbolic acid that we get from the medical stores here, and we sterilise that and use it.

17,455. How long have those rules been applied?—The rules were actually in existence when I came to the laboratory, but the modification in the amount of carbolic acid added took place about four months ago; we used formerly to use one quarter per cent.

17,456. What are your rules for decanting and testing the fluid after sterilisation?—They are as follows:—

"1. The decanting officers to take the flasks from the cupboard where the flasks that have been heated and carbolised are kept.

"2. Examine closely the label, and do not proceed to decant till you learn from it (*a*) the fact of its having been heated and carbolised, (*b*) and the date on which these processes were done.

"3. Having satisfied yourself of this, put your initials on the label.

"4. Provide yourself with the necessary apparatus for decanting, a stand, a pair of forceps, a clip, sterilised bottles, a sterilised syphon, and sterile agar tubes."

17,457. How do you sterilise the bottles?—The bottles are sterilised in a large steriliser; they are first carefully cleaned, and corked, and we have pieces of

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paper tied over the corks, and then they are sterilised in bulk. We generally sterilise about 1,000 at a time.

17,458. Are they sterilised with the cork in the bottle?—Yes.

17,459. By steam?—Yes.

17,460. At what temperature?—I am not positive as to the temperature; I have not charge of that department.

17,461. Please continue the rules?—"5. Heat carefully the neck of the flask to be decanted as far as "it can be done without cracking the glass, and passing "the flame of the burner at the same time over the "surface of the plug.

"6. Fix the flask on the stand.

"7. Sterilise the glass of the syphon to be introduced into the flask by passing it several times through the flame of a Bunsen burner."

17,462. How long is that glass that has been introduced into the flask?—There are two parts. There is a long part which is put into the flask, which is about 12 to 14 inches long, and then there is a piece at the end about three or four inches long. There is only a piece of rubber tubing between the part inserted into the flask and the glass tubing at the end of the syphon, and on this rubber the clip is placed.

17,463. How do you sterilise the whole of it?—The syphons are generally sterilised at the same time as the bottle.

17,464. I suppose you plug the syphons?—Yes: "Next having sterilised in the same way the ends of "the forceps, seize the plug at the mouth of the flask "with the latter, and remove it partially, and introduce "the syphon, finally pushing back the plug in place."

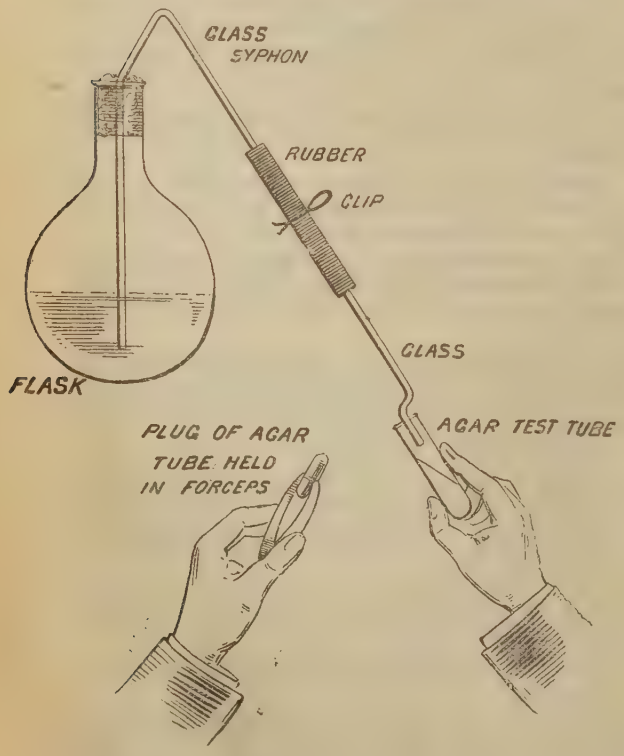
17,465. How do you fix the syphon into the flask—with an india-rubber cork?—No; we remove the cotton wool block, and insert the syphon into the flask by the edge of the cotton wool block.

"8. Fasten the clip in position on the syphon.

"9. Immediately before sucking shake the fluid with its contents well round.

"10. A suction tube (short rubber tube provided with a glass mouthpiece) is next fitted on to the end of the syphon hanging out of the flask, and the prophylactic is then sucked out from the latter down to the level of the prophylactic in the flask."

17,466. How is that done; how is it sucked out? (The witness produced the following rough sketch and explained the same to the Commissioners.) Will you



please proceed with the rules?—Yes; "a current having "been thus established, the suction tube is removed "and the end of the syphon sterilised with the "burner.

"11. Before decanting the fluid into bottles, take a sterile agar tube and fix it between the thumb and forefinger of the left hand, remove the plug with the forceps and place it between the fore middle fingers of the left hand, heating gently the mouth of the tube. Sterilise the end of the syphon and allow some fluid to run through it, then carefully insert the end of the syphon into the agar tube and allow one drop of the fluid to fall on the top of the surface of the agar slant. Then remove the syphon end, and sterilising the mouth of the tube and the cotton plug, re-insert the latter in the former with the sterile forceps. Then write on the label of the tube the number of the flask, the date and your initials.

"12. The liquid is then decanted into the bottles, care being taken—

1st. To shake the flask between every bottle filled.
2nd. To see that the bottles used are all clean and free from cracks.
3rd. That the corks are well fitting.
4th. That the end of the syphon, the mouths of the bottles, and the corks are kept sterile, this being done by repeatedly using the burner.

"13. After decanting, the number, capacity of, and the number of bottles filled from each flask are entered in the book kept for the purpose, and the decanting officer's signature affixed against it. A specimen label for the bottles filled, showing the number of the flask and the date of decanting, to be also made and handed over, with the bottles, to the sepoy who seals them.

"14. After the day's work is over all emptied cultivation flasks to be shown to Dr. Milne, who will affix his initials on the labels.

"15. The flasks are then to be placed in the go-downs in charge of the go-down clerk."

17,467. How long have the rules been in existence?—The rules have always been in existence, but the modification of testing the decanted fluid on agar has been in force since the 12th December.

17,468. Were you in charge of the Department before that?—Not actually in charge of it; only assisting Professor Haffkine.

17,469. You do not know what was done before that?—Yes, I have been nine months in the laboratory.

17,470. What was done in the first instance?—We never tested the fluid before—after its sterilisation and carbolisation—but the other processes are exactly the same.

17,471. Since when have you taken to testing the fluid?—Since the 12th December.

17,472. You do not test the fluid after syphoning into bottles?—Not unless the syphoner's tubes show impurities; in that case I test the fluid in the bottles.

17,473. Could you show me how you put the cork in, and how you seal it in a bottle like *that*? Do you turn it after the cork is in and dip it in the sealing wax like *that* (illustrating with one of the bottles produced by Witness)?—Yes.

17,474. How do you shave off the top of the cork?—We do not shave off the tops of the corks at all. The cork is pushed right into the neck of the bottle.

17,475. Have you made any experiments as to the proportion of tubes that were contaminated?—I have first to explain, that in the beginning of November there was a very great demand on our prophylactic, and we had to increase our staff greatly, and we could not get Hospital Assistants and the class of people we wanted as decanters, and we had to engage clerks, and they had to be taught how to syphon properly. In the early days of the testing of the material with these tubes by the new syphoning officers, more than 50 per cent. were found to be contaminated, although the material itself was actually pure. Out of a series of 79, 36 showed sterile tubes, while 43 showed impurities which, however, at re-examination proved to be due to the defective testing by the syphoning officers, the material itself being sterile. Latterly, the proportion has become less than 1 in 20. An experiment was made on December 28th, 1898, as follows. While syphoning seven flasks, the syphoning officers were ordered to inoculate three agar tubes, thus:—

I. The first at commencement.
II. The second after syphoning into 10 bottles.
III. The third before syphoning into the last bottle.

The result is as follows: All three tubes of the first flask were contaminated; in the second flask the first

and second tubes were sterile, and the third contaminated; in the third the first and second were contaminated, and the third sterile. In the fourth and the fifth, the first was contaminated and the second and third were sterile. The sixth was like the third; the third tube was sterile, and the first and second were contaminated. In the seventh the first tube was sterile, and the second and third contaminated. Eleven of the contaminated tubes showed less than three colonies, two showed extensive growth, while eight were sterile; that is one at least from each flask. The fact that from the same flask sterile and contaminated tubes were forthcoming, pointed conclusively to faulty manipulation on the part of the syphoning officers. When the decanter's tubes showed contamination of any sort, either single or multiple colonies, bottles of the series were examined, and in no instance was a bottle found contaminated. We have examined altogether 985 series since the 12th December, and all of these have proved sterile; and there are 48 series of which the bottles are being tested, the agar tubes not being yet ready for examination.

17,476. How do you test the bottles?—The method of examination was as follows: The sealing wax having been carefully melted, the cork was prised out with a sterile forceps. A sterile pipette was introduced into the fluid, the bottle having been shaken, and a small quantity of the fluid was sucked up and successively transferred to two agar slants and to a sterile flask of broth. The bottles were then re-corked and re-sealed —.

17,477. How much was removed?—As far as I could judge I used to take about a c.c. I made a mark on the pipette.

17,478. You say you put it into sterile broth; how much broth did you use?—I think 50 c.c. of broth. I am not absolutely positive. The bottles were then re-corked and re-sealed, and the bottle with its flask of broth and its two agar tubes, and in many instances a slide of the deposit were arranged in a dark room at the ordinary temperature, and were examined after four days to 21 days.

17,479. That would account for aërobic microbes, but could you detect an anaërobic microbe?—No. I wish to submit samples of recent syphoners' tubes showing series marked "fit" since February 2nd, 1899, and also showing the contaminated tubes in the same period. These (handing a number of syphoners' tubes) are samples of syphoners' tubes which have been marked "sterile," and *here* are some impure ones. The material in the bottles, on being tested in these instances, was found to be pure.

17,480. Have you examined any old bottles of prophylactic?—Yes.

17,481. Can you give us your experience as to that?—Bottles from 125 series have been examined by myself with the assistance of Drs. Gibson and Ransome. Those series have been stored in the laboratory for a variety of reasons, such as the weakness of the brew, &c. One dates from May 1897, while two date from March 1898. The others form a continuous series of those prepared in the laboratory from August to the middle of December 1898. The bottles of each series have been kept stored in boxes or in cupboards in the dark. Of these 125, in 104 series the sealing of the bottles was satisfactory, while in 21 it was broken or had holes in it allowing the cork to be exposed to air and dust. I have stated the method of examination. The bottles are all in the laboratory now with their flasks and tubes, and they are on view. The whole of them were examined finally by Professor Haffkine and myself. In the examination of the results, where in two instances only one of the two agar tubes showed a single colony, while the broth cultivation remained sterile—this was considered accidental, and as having occurred during manipulation, but if the broth was also contaminated, although the two agar tubes showed only one colony, the bottle was marked "contaminated" and the series re-tested. In about half-a-dozen instances the broth alone has shown growth while the agar tubes remained sterile. Professor Haffkine thinks the manipulation of the sterile pipette from the two agar slants to the broth is quite liable to allow of its being contaminated during the interval. He thought afterwards we ought to have used a fresh pipette; that I ought to have inoculated the broth with a fresh pipette separately from the agar tube. In the first instance, of the 104 bottles with satisfactory sealing, 98 were found pure, and six contaminated. Of the 21 series, in which the bottles had broken sealing, one was found to be pure and the remainder contaminated. Of the six contaminated bottles where the sealing was good, in all six broth and agars were affected. Three of these were contaminated with moulds only. One had several single colonies, while in one the surface of the tubes was covered with colonies. Of these six series it has only been possible to re-examine up to date two duplicate samples, and both these have been found sterile. Of the 20 contaminated and badly sealed series, in 18 both broth and agars were effected; in two series the broth remained pure. In 14 of them the agars were covered with mixed growth, while six showed a few isolated colonies. 10 well-sealed duplicate samples of these 20 contaminated series were re-examined, with the result that eight were found to be pure and two contaminated, while in three series satisfactory samples could not be found, and all three again with unsatisfactory sealing were found to be contaminated. Of one series still a third bottle was examined with a similar result. Of the two contaminated ones from the well-sealed series, one has been re-tested for a third time and found pure. The conclusion seems to point emphatically to the contamination of bottles taking place after the material has been bottled and sealed up and being due to defective sealing. This has been taken into account for some time, and experiments are being made to substitute a more effective method of corking and sealing the bottles.

17,482. Would you show us why you think the method of sealing is unsatisfactory?—(The witness explained same to the Commissioner.) Apart from the defects in the sealing, there is also the fact that the cork gets sodden in contact with the carbolised fluid. There are thousands of them stored in the laboratory, and the corks get sodden in time, and the appearance of the sealing wax entirely changes; it gets this open appearance, not as it is at first. *Those* are newly-sealed bottles. I have a bottle here which was perfectly sealed, but it leaked between the sealing wax and the neck of the bottle, and this would account for the contamination of the number of bottles where the sealing wax was apparently intact.

17,483. Have you any other facts to tell us?—No, I have not, except that we hope to institute rubber corks for the bottles. At present we are making experiments—Dr. Stewart and I are doing them—as to soaking the corks in boiling paraffin, and then fixing them in and sealing them over.

17,484. Have you any evidence about anything else that you wish to bring forward?—No.

17,485. With regard to inoculations, I believe you have done a good many?—Yes; I have done over 800 altogether.

17,486. Have you ever seen any abscesses?—No, never.

17,487. Have you seen any severe results following the inoculation?—Never.

17,488. How many of these patients have you seen twice?—I took a series of 200 people in the jail, who were inoculated by Professor Haffkine and myself, and I examined them 7 hours after inoculation, 24 hours, and 48 hours. I took their temperatures and examined them thoroughly.

17,489. Were they all inoculated with the same brew of fluid?—They were inoculated with two different brews made at the same time.

17,490. Did you find that the same brew of fluid produced the same rise of temperature in all the patients?—No, it varied with the patient.

17,491. How much did it vary?—The lowest temperature I recorded was 99, and the highest temperature was 104·6.

17,492. What were the temperatures between these two limits; was there an average temperature?—Yes. The average of the maximum temperatures, for the three times that I took the temperatures, was 101. The average temperature for the whole lot of observations was just over 100.

17,493. We had it in evidence that a temperature of 102 is the ideal temperature?—Yes.

17,494. In these cases you did not get that?—I probably did not get every man at the time when his maximum temperature occurred.

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17,495. We have been told that one of the principles of inoculation was that you should produce a temperature of 102, but if you get a difference of between 99 and 104, how can the rise of temperature be used as a standard?—In most instances we have not been able to follow out the case completely. I inoculated my servant and had him completely under observation after he was inoculated; and I took his temperature every half hour. His temperature reached 102·3; that was the maximum about eight hours after inoculation.

17,496. But supposing you take 100 cases, as you have done in the jail, and the temperatures vary from 99 to 104, how can the temperature be the standard of efficient immunisation?—My experience of the work is insufficient to allow of my judging this question accurately.

17,497. And you have no special observations bearing on that point?—No.

17,498. Have you any facts as to animals getting the disease from one another?—I generally look after the animals kept in the laboratory for experimental purposes. Within the last few weeks a number of rats, under the orders of the Municipal Commissioners, have been brought into it. Living rats and dying rats, and all sorts of rats, and I have noticed that several of the original rats in our laboratory, which were kept in cages alongside of the others, have died of plague. They must have been infected from the other rats which had been brought in. Latterly, all rats which have looked at all ill, in fact every rat brought from Bombay, has been kept in a special cage and room. Most of them have succumbed to plague within a day or so.

17,499. Do you find plague microbes in their blood after death?—If they are examined fresh, I do—heaps of them.

17,500. Does the microbe disappear very quickly?—Very quickly.

17,501. In how many hours?—One has great difficulty in getting it after 10 or 12 hours.

17,502. Does it depend on the temperature or any other condition?—I am not aware of any special conditions.

17,503. Have you examined the urine and fæces of patients for plague bacilli?—No, I have not. I have examined the urine of rats.

17,504. Have you found it there?—No; I have only examined in five cases.

17,505. How did you examine it?—In nearly all the rats which have died of plague, which I have examined, I have found the bladder full of urine. I introduced a sterile pipette and sucked up the urine and made cultures from it, but I have never found any plague bacilli, nor has Dr. Gibson, I think. Dr. Gibson and I have been working together a good deal at this.

17,506. How long have the rats been dead?—They were only just dead.

17,507. There is no doubt about their having died of plague?—Absolutely none.

17,508. Can you tell me whether you have made any experiments as to the prophylactic effect of Haffkine's fluid on animals?—No. I have started an experiment with regard to rats, but I have not yet completed it.

17,509. Have you ever injected the prophylactic fluid into animals?—Only into rats in the experiment on which I am working at present.

17,510. How much did you inject?—I injected into one rat the dose marked on the bottle.

17,511. How much is that?—It happened to be 2½ c.c.

17,512. What effects did it produce upon that rat?—The rat is still living.

17,513. Have you used the same prophylactic fluid on man?—No, not yet. I am working at that just now.

17,514. You do not know whether the rat has been protected or not?—No, that is a point I want to work out.

17,515. (*Prof. Wright.*) I understand it is the first portion which is drawn off from each flask of carbolised vaccine which you test?—Yes.

17,516. In view of the fact which Dr. Stewart told us yesterday, that contaminating bacteria cannot multiply when they are introduced into this carbolised vaccine, do you think that you have a satisfactory test of the sterility of your vaccine when you test it by introducing your undiluted carbolised vaccine on the surface of an agar tube?—I do not think it is myself.

17,517. Have you met with any bottles of M. Haffkine's prophylactic which have become putrid?—Two of the original series which had not been carbolised, were badly smelling, but on testing them on agar and in broth in the series mentioned, they were found sterile.

17,518. When was the method of adding carbolic to the vaccine introduced: I thought an addition of carbolic had always been made to the vaccine?—I do not think it was in the case of the very early ones. I have a bottle dating from May 1897. I am sure the early ones were not carbolised.

17,519. Do you know when the method of adding carbolic was first adopted?—No, that was before I came to the laboratory. I came to the laboratory in June.

17,520. Since then, all the bottles have been carbolised?—Yes.

17,521. Do you think that it is possible the putrid bottles we have heard of may all have been sent out before that period?—I know that bottles alleged to be putrid have been sent out of the laboratory since I joined the laboratory.

17,522. How do you account for this? Do you think the carbolic can have been omitted, or that it can have evaporated?—I think the officer who tests this is an exceedingly careful worker—Dr. Paymaster—and, I do not think, in any instance, he has failed to carbolise the flask. But, of course, an accident may have occurred. As to its having evaporated afterwards, I really could not say. I should think it was possible.

17,523. You say that in certain cases, where the decanting staff found that the vaccine was contaminated, you found, when you came to test the bottles, there was no contamination?—Yes, that is so.

17,524. How comes it that the decanters were so careful in the matter of not introducing bacteria into the bottles, when they were so little careful in the matter of not introducing contaminations into the agar tube? You say they introduced contaminations into 50 per cent. of the agar tubes, and that none the less you found that they did not introduce any contaminations into the bottles; how do you think that came about?—In the one case the organisms fall on a fertile surface, in the other in an antisepticised fluid. The men whom we were obliged to take to do the decanting were men who would be laboratory boys at home, absolutely ignorant of what bacteria are or anything about them. It was hopeless at first to get them to do the work properly.

17,525. How is the vaccine standardised?—Professor Haffkine does that.

17,526. Do you know how it is done? Is there any trade secret as to how it is done?—No.

17,527. Do you know how it is done?—Yes, I know M. Haffkine judges by the amount of sediment in the fluid; he compares the amount of the sediment. *This* bottle (shaking it up and holding it up to the light) he would probably mark 10 c.c., and *this* (shaking it up and holding it up to the light) would be about 5 c.c.

17,528. Do you think it is an accurate way of standardising?—Personally, I do not.

17,529. Do you think you could get a fluid of more constant strength if you decanted off the supernatant fluid?—Yes, I think it would be possible to get a more constant fluid.

17,530. Do you know whether the supernatant fluid is or is not toxic?—I do not know. I mean to try that.

17,531. You have not compared the toxicity of the supernatant fluid with that of the sediment?—No.

17,532. When you say the microbes disappear from the blood soon after death, do you mean that in a case where you have been able to see them under a microscope the bacteria disappear from view in a few hours, or do you mean that after a lapse of hours you cannot get cultivations?—We cannot get cultivations.

17,533. But can you still see the bacteria?—You can see forms which you take to be plague, but, as a rule, you cannot get satisfactory cultivations. Of course, I have examined plague rats 16 hours after death, and got plague bacillus from them both in microscopical tests and in cultures; but, as a rule, the plague bacilli

are crowded out by putrefactive bacilli—absolutely crowded out.

17,534. Have you examined the blood in a dead rat on two successive occasions, and have you, on the first occasion, found the microbes, and on your later examination, failed to find the microbes?—No, I have not.

17,535. Do you know whether the strength of the prophylactic decreases when it is exposed to a boiling temperature?—No, I do not know that.

17,536. Have you made any experiments to determine that point?—No; and I have no experiments also with regard to doing anything to the fluid after it has been decanted.

17,537. (*The President.*) What is the reason for the selection of bottles of the shape used?—Because you can fill up the syringes more easily from them. The neck and the body of the bottle are in the same plane.

17,538. It depends upon the length of your injecting needle?—The needles which we have are all long. We can put them in about *here* (pointing to bottle) quite easily, and in pouring out the fluid you can get the whole of the fluid out.

17,539. In practice, is it found necessary to pour the whole contents into one glass?—In the rules we specially say that that is not to be done.

17,540. You can hardly empty the bottle otherwise?—I have never any difficulty in emptying the bottle.

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17,541. This prophylactic fluid, apparently, consists of two substances, one liquid and one solid?—Yes.

17,542. Which is the prophylactic ingredient?—The solid.

17,543. What is the function of the liquid?—Professor Haffkine, I believe, considers that the fluid part has an effect on the case mortality—that if a person who has been inoculated gets plague the fluid portion of the prophylactic helps him through with it.

17,544. On what grounds do you say that the solid is the prophylactic?—M. Haffkine has told me so. It is not my own observation, because I have done no experiments on the point. I have simply been working under him.

17,545. Is there any special test to determine the prophylactic efficiency of any brew before distribution?—We do not test it in any way.

17,546. You judge simply by the amount of solid matter?—Yes. Of course Professor Haffkine does all that himself. I have nothing to do with that.

(Witness withdrew.)

Lieutenant J. K. Condon, I.S.C., called and examined.

Lieut J. K. Condon, I.S.C.

17,547. (*The President.*) You are Under-Secretary to the Government of Bombay?—Yes.

17,548. (*Mr. Hewett.*) You were employed, I believe, on plague duty at Bantwa, a town in a Native State in Kathiawar?—Yes.

17,549. What is the population of Bantwa?—It is calculated at 8,500. The 1891 census was 8,200 odd.

17,550. At what stage of the epidemic did you arrive at Bantwa?—I arrived about the 10th September 1898.

17,551. The epidemic was a very severe one, was it not?—Yes.

17,552. What was the total number of attacks?—944.

17,553. That represents about 11 per cent. of the total population of the town?—A little more.

17,554. Can you give us an estimate of the number of people who had left the town?—I think about 500 or 600. It is calculated there were 7,500 people there when I was there.

17,555. What number of deaths occurred in the epidemic?—602.

17,556. When you got to Bantwa in the month of September what measures were in operation?—It was raining very hard, and practically there were no measures taken beyond the erection of hospitals and some small camps.

17,557. On what date were measures to clear the people out of Bantwa undertaken?—Between the 25th and 30th September.

17,558. What proportion of the total attacks and deaths had taken place up to the date when you got the people out?—A certain number of people had been turned out by my predecessors in August, but unfortunately a storm took place and they all went back again to the town. That settled the question. 772 attacks and 467 deaths took place before the people went out the second time.

17,559. When did the storm take place?—On the 12th September. There were about 2,000 people segregated from the time of the end of August.

17,560. Did those segregated by the end of August include all the people among whom the plague broke out, or was there also plague among the other people in the town?—No.

17,561. Were the whole of the residents of the infected portions of the town in camp or not?—No, because the infection was all over the town.

17,562. Was the worst part of the epidemic over before September 16th, the date when you were able to begin effective measures?—Yes, apparently; or at least it had reached its climax.

17,563. What was the number of attacks respectively before you took effective measures for getting the people out of the town, and after you had done so?—467 deaths took place before evacuation, but I should like to explain that evacuation, at least as I understand it, never took place in Bantwa at all. There is absolutely no reason, so far as I can see, which anyone can give for the subsidence of the plague there.

17,564. Except that the disease itself began to die out?—Yes. There were never more than 2,000 people—one quarter of the town—evacuated. The decline in the disease might have been due to the weather, because we had a spell of very fine weather immediately after the storm.

17,565. Did anything come under your observation as to the communication of the plague by rats in the town of Bantwa?—In a great many of the houses which were badly infected dead rats were found. In one house in which 16 cases took place we found a lot of rats.

17,566. Has your experience led you to the conclusion that the rats were moving from house to house and carrying the infection in that way?—Yes, for this reason: in the town when plague began the houses where rats were caught and killed were, of course, disinfected, but rats afterwards came to where we were living, and, in fact, appeared in our own stables. On finding them we turned the servants out and disinfected the rooms. None of the servants were attacked.

17,567. Did you observe distinctly the migration of the rats?—I do not think I could make that statement, but that was the general belief. I think they were driven from one place to another, although, of course, I did not see them. It was only a distance of 1½ miles or 2 miles.

17,568. Did any other place in the neighbourhood of Bantwa become infected?—All the surrounding villages—four or five of them within a radius of 3 or 4 miles.

17,569. Were you able to trace the cause of the infection of any of those villages?—Not with certainty.

17,570. Did you turn the people out?—Yes; in the villages every person was turned out.

17,571. Did the epidemic last long in any one of those villages?—I think Nakra lasted within a month as long as Bantwa itself. It was going on after Bantwa.

17,572. Did it begin at the same time?—No, five or six weeks later. I do not think there is any doubt that Nakra was infected from Bantwa.

17,573. When you got the people out of these villages did you prevent any communication between them and the infected site?—As far as it was possible, but I could not rely upon the Police there. They were all State Police and unreliable. In fact, in some cases people did get back again, and the Police were punished in consequence.

17,574. So that you have no security that the segregation of the people was at all effective?—In one village it certainly was effective, and that is Kambhla.

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17,575. How long did it take to suppress the disease in that village?—The epidemic in Kambhla began at the commencement of October and it stopped in the first week in November. There were 23 cases with 21 deaths.

17,576. Why should it have taken so long to suppress the outbreak there if segregation was effective?—Because the people kept going back from the camp to Bantwa.

17,577. Bantwa was also infected at the time?—Yes.

17,578. Then the segregation was not effective in preventing the people who were segregated from going to an infected site?—No, it was not. What I meant was they did not go back to Kambhla.

17,579. But they went to other infected places?—Yes; there is no doubt they went to Bantwa and they might have been to Nakra.

17,580. (*Dr. Ruffer.*) Can you tell me what was the shortest duration of a case of plague that you observed personally?—The shortest case I personally observed was that of a man at work in the fields about 6 o'clock in the evening; he was cutting the crops. I happened to be riding back from Bantwa at night, and they said that this man was taken ill. I saw him between 6.30 and 7 p.m. When I went back the next morning I found he had died about 9 o'clock. That would be about 14 hours. This was the shortest case that came under my own observation. It is possible that many of the cases found dead were more rapidly fatal.

17,581. Did you notice dumbness as one of the sequelæ of plague?—Yes, dumbness came on when the people had had plague about a day or two in hospital. You could not get them to answer, they used to shake their heads.

17,582. Did you find that people who recovered became more or less idiotic?—We had four or five cases of lunacy after plague.

17,583. How long did it last?—I do not know.

17,584. Did they recover?—I cannot say.

17,585. You mention in your précis that loss of sight followed careless handling of the bubonic discharge in some four or five cases; do you mean the patients got ulceration of the cornea?—They lost their sight altogether; the eyesight went.

17,586. Did they get disease of the eye?—The eyes became quite colourless; the blue became a whitish blue. In some cases the eye was lost altogether.

17,587. Did the eye suppurate?—It became very much inflamed and painful. To the best of my recollection there was suppuration and a discharge from it.

17,588. Why do you think it followed careless handling of the bubonic discharge?—That is the only reason we could assign to it. We would open a man's bubo and then he would have to squeeze it himself and wipe it. Very often he would squeeze it carelessly with his hand and then rub his eye or his face or something of that sort, and it was generally after that that it came on. I think there were some six cases of loss of sight altogether.

17,589. I understand you saw one house where there were 16 cases, and some occurred after it had been disinfected?—Yes, that was a most extraordinary case.

17,590. Can you give us the details of that?—I regret I cannot, because cases occurred before I went there—about eight or ten of them. The house was pointed out to me as the worst plague spot in the city. It was in the very centre of the city.

17,591. At that time the plague was all over the city?—Yes.

17,592. So that these people may have infected themselves somewhere else?—That is possible, but all the cases occurred in the house.

17,593. (*Mr. Cumine.*) Has there been any case in Bantwa this year?—Not this year.

17,594. No case since it ceased last year?—Since it ceased on the 5th November there has been no case reported.

17,595. (*The President.*) What kind of house was it in which these 16 cases occurred?—It was a collection of houses; a house with rooms added to it all over the place. It was not strictly a group, but it was built up of several houses. It was in a compound by itself surrounded by a wall from 5½ to 6 feet high.

17,596. What do you mean by dumbness, that the patients were speechless?—Most of them were not absolutely speechless, but some of them were. The majority could make sounds, but they were not intelligible; they could not speak distinctly.

17,597. Did the voice go?—The articulation seemed to go.

17,598. (*Dr. Ruffer.*) Is there any medical man in Bantwa?—On my arrival I found the plague work entirely in the hands of State Hospital Assistants.

17,599. During the whole of the epidemic?—Khan Saheb Fazl Ahmed arrived shortly (about ten days) after I did, and did good work.

17,600. Was there any European Medical Officer?—No. I believe Major Burke, Civil Surgeon at Rajkot, visited the Plague Hospitals in August, but beyond this none of the patients were even seen by a European Medical Officer.

(Witness withdrew.)

Mr. S. R. Arthur, I.C.S., called and examined.

Mr. S. R. Arthur, I.C.S.

17,601. (*The President.*) You are Collector of Kolaba?—Yes.

17,602. (*Mr. Cumine.*) During the fair weather of 1896-7, you were in the Satara Collectorate?—Yes.

17,603. I think some parts of Satara are very much over-populated, and a great many of the people come to Bombay to work as porters, and so forth?—Yes.

17,604. When the exodus from Bombay occurred in 1896-7, did a number of those people go back to their own country?—Yes; and the result was that some two or three villages became infected. I found a number of deaths ascribed to ordinary fever, and afterwards I thought that probably a very large number of those were due to plague, because I noticed they occurred among people who had returned from Bombay. At the time nobody knew much about bubonic plague, and I did not go into it till rather later; but I noticed that several cases occurred together. One man came from Bombay, and died of fever; and just after that, within a fortnight or so, there were several cases of fever in the village, all of which proved fatal very quickly.

17,605. Was there one village which seemed to escape infection in a wonderful manner in spite of a lot of people pouring into it?—Yes, Wai.

17,606. Where did the refugees come from?—A tremendous number of people came from Poona every day.

17,607. And yet that village never got infected?—That is so.

17,608. In March, 1898, you went as Collector to Kolaba?—Yes.

17,609. In the Kolaba District, I believe, there were three centres of plague?—Yes; one, Revdanda and the neighbouring villages; secondly, Karanja and neighbouring villages; and, thirdly, the town of Alibag itself. Panvel was also infected, but the plague stopped just when I arrived in the district.

17,610. With regard to Revdanda and its villages, you have no personal knowledge?—Not very much. I went down there, but there was an officer on special plague duty there.

17,611. With regard to Karanja and its neighbouring hamlets, what was the important point which struck you most?—The course of the plague at Karanja and the neighbouring hamlets in the Uran petha of the Panvel taluka demonstrates no less clearly than Alibag the advantages of evacuation. I went there first about the 23rd March 1898. Four or five cases were then occurring daily. The late Mr. Brooke, my Assistant, had already ordered evacuation just before my arrival, and the order was complied with, but the people were concealing cases, and we had some difficulty in impressing on them the importance of giving immediate information of cases and deaths. Eventually the people promised to comply, and did so loyally. Panches

were appointed, the hamlets were divided into sections, and one Head man held himself responsible for each section, and gave information of cases and deaths immediately on their occurrence, on which the huts in which cases had occurred were burnt down and the occupants segregated. The result was immediately apparent. The number of cases dropped from 23 and 24 in the weeks ending 19th and 26th March respectively, to eight in the week ending 2nd April, three in the week ending 9th, and none afterwards, except a sporadic case here and there. What chiefly struck me was how very quickly it stopped when we got the people out and got the cases immediately reported.

17,612. But the way in which the people found out cases themselves and reported and isolated them was also striking, was it not?—Yes; we had no difficulty with them at all directly they understood what we wanted, and when they knew that we were not going to do anything with them except to put them in different huts.

17,613. With regard to the town of Alibag itself, I think you arrived there just at the beginning of the second epidemic?—Yes.

17,614. The point which you wish to emphasise is, I understand, the number of attacks in the season of 1897-98 as compared with the number of attacks in 1896-97, and the cause of the difference?—Yes. The history of the plague in Alibag is most instructive as showing the advantage of early evacuation. In the first half of the year 1897 there were 266 cases and 244 deaths. This was the first attack. The plague came on the people as a disease of which they had no experience; they did not at that time appreciate the advantage of evacuation, and were slow to resort to it. From July 1897 to March 1898, Alibag remained free, with the exception of one case in October and three in January. When I took over charge of the district on 19th March 1898, the second epidemic had just commenced; seven cases had occurred, and my predecessor, Mr. Quin, had ordered the evacuation of the infected quarter. I continued his policy, and on cases occurring, immediately caused the evacuation of the affected parts. Nor did I find any difficulty in inducing the people to obey the order. Taught by the bitter experience of the previous year they were only too ready to leave on the first appearance of the plague. The results are apparent in the returns. The first three weeks showed 17 cases, the following four weeks showed nine, and the attack then ceased, May, June, and July each producing one sporadic case only.

17,615. From July 1897 to March 1898 Alibag remained free?—Yes, with the exception of one case in October and three in January.

17,616. Do you know whether the outbreak which had just begun in March 1898 was due to an importation of infection about that time? Could you trace importation of infection in February or the beginning of March?—I am afraid I cannot say, because it began some days before I joined there. I did not look into the matter myself. I can speak with regard to the latter epidemic into which I did inquire myself, but I was not in the district when these first cases occurred, and I am afraid I cannot say whether they were imported or not.

17,617. When did the second epidemic in Alibag, which was going on when you arrived there, end?—In epidemic form on 29th April. There were subsequently only three sporadic cases in May, June, and July, one in each month.

17,618. Has plague appeared in Alibag again this fair season?—Yes, it is going on now.

17,619. Can you trace its re-appearance in Alibag to any fresh infection imported?—No, I think not. It started with a great mortality among rats long before cases were reported to me. For about a fortnight or three weeks before there was a great mortality among the rats.

17,620. Have there been any imported cases?—I think not. I think it is a pure recrudescence in the place itself.

17,621. Did it reappear in the part of the town where the last epidemic died out, or among the people among whom the last epidemic remained the longest?—No. This year it is more among the Brahmans, whereas last year it was chiefly among the Kolis, who live quite in a separate quarter.

17,622. What disinfectant had you used during the epidemic which was going on when you arrived in Alibag?—We have used perchloride of mercury all through; we have also, of course, unroofed the houses.

17,623. Did you disinfect every house or only the houses where you thought cases had occurred?—We disinfected the houses in which cases had occurred and also the surrounding houses. I think a good part of the town was probably disinfected.

17,624. But some of the houses were left undisinfected?—Of course Koli Wada received the most attention, where cases were occurring last year. Nearly all the Koli Wada was unroofed and disinfected, and as yet we have had no cases occurring there, although rats have died.

17,625. There were a certain number of houses which were left undisinfected?—I cannot say that the whole of the town was disinfected, but the greater part of it was.

17,626. Who was in charge of the disinfecting operations in Alibag town?—The Deputy Collector, and Assistant Surgeon Pereira.

17,627. As regards the mortality since April to this present recrudescence you have been watching it carefully, I suppose?—Yes.

17,628. Have you noticed whether the mortality in the interval has been heavy among children under five?—I could not say.

17,629. Have you noticed whether the deaths have been occurring in clusters, or more than one in the same house?—Do you mean during the time there has not been any plague?

17,630. Yes?—No, I do not think so. I have noticed carefully the mortality from fever, but nothing else particularly. If several cases of fever had occurred in one house, or in a cluster of houses, we should have noticed that at once, and probably ascribed it to plague.

17,631. You have been looking not merely to the number of deaths but also to the places where they occurred?—That is so. There are general orders in the district. If in one house or in one neighbourhood a large number of fever cases occur we immediately get reports of them.

17,632. Are you aware whether there has been any outbreak of mumps in Alibag during the rains?—Not that I know of.

17,633. I think that in Mora Bandar in the hot weather of 1897 about 393 persons were inoculated?—Yes.

17,634. Were you there at the time?—No, I was not.

17,635. Have you any personal knowledge of what has happened to those people, how many of them are dead and how many are alive?—I have no personal knowledge, but I think I could say generally that Mora has not been affected with plague since. Of course that is astonishing, because Mora is a bandar exactly opposite Bombay, which is very much used, and it is only four miles by road from Karanja, which was very heavily attacked. I did not go to the district till 1898, but from the hot weather of 1897 there has been no plague at Mora, although it is a place which has considerable traffic with Bombay.

17,636. I think that Alibag and Panvel, and also two villages named Thal and Naogaon, are infected at present?—Yes, and Cheul has also been infected since I wrote my précis of evidence.

17,637. How many villages are now infected?—Three. Of course, I am not counting sporadic cases. We are perpetually getting single cases imported from Bombay and other places; they are removed at once, the house is disinfected, and, generally, no further cases occur if the measures are taken promptly.

17,638. You have three villages now with indigenous plague in them?—Yes.

17,639. Does any one of them adjoin a village which was infected last year?—Panvel has been infected each year. I have no villages infected now except Cheul.

17,640. Does that village adjoin a village which was infected last year?—Cheul was infected last year, and so was Revdanda.

17,641. How is plague carried from village to village?—Is there any case known of rats being actually seen

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migrating from one village to another?—No, I do not know of that.

17,642. Do you think it would appear to be carried by human beings?—Yes, but I think it is also very likely carried by rats.

17,643. But you have never seen any rats actually going from one place to another?—We know it is carried from one part of a town to another by rats.

17,644. How do you know that?—I say this because mortality amongst rats has been observed first in one part and then in another; then, within a fortnight or three weeks, one sees plague among human beings. That is the history of plague in Alibag this year.

17,645. Could you be confident that there has not been first a concealed case of plague among human beings in that part where rats have been dying?—I do not think that there has.

17,646. Do you believe in disinfection by perchloride of mercury?—I believe the sun is by far the best disinfectant, because I think it is very difficult to disinfect thoroughly with perchloride of mercury.

17,647. Is it not difficult for the sun's rays to operate in dark houses?—We unroofed almost entirely. I do not believe in taking off a narrow strip, because, of course, the sun reaches very little.

17,648. It would not be feasible to unroof during the monsoon?—No.

17,649. Then you cannot rely on disinfection by the sun during that time?—No.

17,650. Do not you think that the monsoon is a favourable season for plague outbreaks in this part of the country?—As far as my experience goes it has stopped almost entirely before the monsoon. I do not think that in Alibag we had any cases last year, or in Panvel. Each of the infected towns in Kolaba was infected in the cold weather, and the cases dropped off as the hot weather came on. In the rains there were no cases at all.

17,651. There was a great deal of plague in the Satara district in the rainy season, I think?—Yes, in Karad.

17,652. Does not that modify your statement that it does not occur during the rains?—I was speaking with regard to my own experience, but I was also in Satara. The only town in which it occurred last year during the rains was Karad.

17,653. During the rains would you not have to resort to some other method of disinfection?—Yes. I have not, myself, taken plague measures during the rains anywhere; but, of course, I should try to evacuate houses as far as I could, and introduce inoculation.

17,654. But you would have to let the people back into their houses at some time?—Yes.

17,655. Do you think that it would be impossible to effectually disinfect the houses with perchloride of mercury?—I should think that disinfection with perchloride of mercury should be done in the rains; I only said that I thought sun disinfection was the best which one could get. We have a large staff of disinfectors, but I think it is very difficult to get work done very thoroughly so that every atom of space is covered.

17,656. The work involves a lot of supervision?—Yes, and we have not sufficient means of supervision.

17,657. (*Dr. Ruffer.*) Why do you think the sun is the best disinfectant?—From experience it seems to me that directly one unroofs a house that is quite enough to stop everything. We always unroof the houses.

17,658. But you disinfect the houses as well, do not you?—Yes, we generally do, but we unroof a great deal more than we disinfect.

17,659. I want to know on what evidence you base the statement that the sun is the best disinfectant?—I am speaking, of course, from what I have learned from the doctors.

17,660. You have no facts of your own?—No, I have not.

17,661. (*The President.*) Have you had cases in which, having unroofed the houses and allowed the people to return, plague has occurred again in the same house, independently of a fresh importation?—My experience of plague is confined to the hot and the cold weather. When we once evacuate a house, the people themselves do not wish to return till the rains, so that a house remains empty for three or four months generally.

17,662. But they do return at the end of two or three months?—Yes.

17,663. Assuming they return, has plague ever occurred in houses which have been opened up, apart altogether from fresh importations?—Not that I know of.

17,664. It has, therefore, proved practically effective in your experience?—Yes, I think so.

17,665. Do these cases which you speak of include cases in which the houses have not been otherwise disinfected?—No, it does not, because every house in which the plague cases have occurred is otherwise disinfected.

17,666. Your opinion seems to be that evacuation is a very effective measure?—I think it is the most effective of all except inoculation.

17,667. In many cases you have been able to stop plague which has been imported by at once segregating the infected persons?—Yes.

17,668. In order that that measure should be effective you must obtain early information?—That is the most important thing of all.

17,669. What is your organisation for obtaining this early information?—We do it through the district staff. There are general orders in the district about plague. When cases of fever occur in a house or in a cluster of houses in a village one after another, they have to be immediately reported.

17,670. Who reports them?—The village officials report to the Mamlatdar, who reports to me direct. Probably I should know at least within two days.

17,671. How does the village officer obtain the information?—In the ordinary course he has to enter births and deaths in the register immediately on their occurrence and has to keep himself informed for the purpose.

17,672. Before the plague patient dies he may spread the infection widely in a village or town: have you any means by which you can determine that there is a case of plague before death has taken place?—No, I am afraid we could not tell that: I do not think we could ever know until one or two cases had occurred.

(Witness withdrew.)

Dr T. Blaney.

Dr. T. BLANEY called and examined.

17,673. (*The President.*) You are a practitioner in Bombay?—Yes.

17,674. You have been here for a long time?—I have been practising here for half a century, a little more perhaps.

17,675. You have taken much interest in the affairs of the city?—Yes. I have occupied every position which a non-official in a city such as this can occupy.

17,676. You have paid much attention to the epidemics of plague?—I have followed them; I have put aside everything else to pay attention to them.

17,677. In the first place, what is your opinion with regard to the influence of rats in the diffusion of plague or to the introduction of plague into any part of the town?—I submitted some remarks on that point in a paper which I sent to the Plague Commissioner, Bombay, according to his own request. I said in this:—

"One more item of evidence may be offered in connexion with the spread of plague by rats. This evidence is derived from experience in our wet-docks. The docks were filled with ships during both our plague epidemics. During these epidemics the usual interchange of rats between the docks and the shore continued as formerly, and that the interchange should be considerable and not rare or occasional may be inferred from the fact that the great rat infested granaries of the city are situate immediately in front of the docks and at no great distance from them. When the question is asked: 'How were the ships in 'the docks affected by the rats on board?' the

reply is, 'The rats carried no infection to the crews of these ships.' As far as I know, if infection reached the ships at all, that infection was brought there by persons infected in the town, and was not carried on board by rats. It is indeed a remarkable fact that while plague raged all over the Mandvi district, the crews of the ships in the docks remained uninfected during both epidemics, while the distance between the shore residents and the ships was well within striking distance of the infection. It may be mentioned that the crews of the ships in the docks were largely composed of Muhammadans and Portuguese, classes very susceptible to plague infection, and not wholly of Europeans or immune persons. As rats do not desert houses during a plague epidemic on account of the disease, they do not carry the plague from house to house or from district to district. The only influence they exercise on plague may come from their power to increase or intensify the infection existing in the houses by their excretions during the period of infection, or by their bodies after death. The proofs of this power of infection by rats, or the power of rats to carry and spread infection, are yet wanting. It is known, however, that plague may continue in active evidence as well without rats as with them. Rats are not an essential factor in a plague epidemic. Hence when we hear that somebody has killed a thousand or ten thousand rats, the reduction is no proof that plague has been diminished to that extent."

17,678. Will you favour the Commission with your opinion as to what influence rats have in disseminating plague?—Very little, I think. Where rats leave their excretions in the houses, on the bedding, clothes, and so on, while they are infected, they may possibly disseminate the plague in houses, and also when they die on the premises and remain there they are a great source of infection. That I have seen over and over again.

17,679. But they do not diffuse it widely?—No. The crucial point about that is the evidence with regard to the docks, which I have already given.

17,680. What opinions have you formed as to the conditions which influence the rate of plague mortality?—There are different degrees of susceptibility in the classes which are constantly maintained.

17,681. Race susceptibilities?—Yes, and diet susceptibilities, or the susceptibility of habits. I have worked out these figures again and again, but I went through one complete epidemic from the beginning of July and ending the following July. I have worked it out in 20 different ways in 20 different communities in different parts of Bombay, and the figures are almost identical. The table which I have before me contains the official record of plague deaths during the whole period of the duration of the second epidemic, namely, from the week ending the 6th July, 1897, to the 28th June, 1898. With regard to the Jains, a particular caste here who are jewellers, grain dealers, and generally wealthy, the great trading class, the rate of mortality from plague is 38·05. I believe some doubt has been thrown upon the correctness of this rate by the late Plague Committee, Sir James Campbell's Committee, but I have been in correspondence with people in America and different parts of Europe, savants, and also people who are interested in the trade with this city, and I have been able to dissipate the errors which appear to me to be in the report of that Committee.* The rate of mortality among the Jains, surprisingly large as it is, is real, and it is due to the fact that animal life is sacred among the Jains. They will not sweep their staircases or sweep their sleeping rooms out, or their cooking room very often, lest they should destroy some animal life, so scrupulous are they. Only half of the vegetables which are used by the natives in Bombay are eaten by these people, because animal life has been found in them. There are sundry other social peculiarities about them which render their sleeping places and cooking places particularly susceptible to the attraction and preservation of the infection.

17,682. Are these places very dirty?—Not in that sense. They are wealthy people, and their places look comparatively clean. But if there is such a thing as infection which is not apparent to the eye which might be got rid of by brooms and disinfectants and throwing open windows and allowing the light and air to enter and so on, that infection would remain so far as the Jains are concerned. I have seen numbers attacked in the same house again and again where, ordinarily speaking, from their social position, wealth, and so on, they ought not to be attacked. That is the explanation with regard to the Jains. There are those religious peculiarities in them which keep the death-rate at that great height, and it will always remain so.

17,683. That is the plague death-rate?—Yes. I have gone over the statistics for about 25 years with the same community so as to verify or shake the opinion which I have formed about their mortality, and their mortality is in excess of all the other Hindu populations, apart from plague in ordinary years.

17,684. Are their habitations otherwise rather better than the average?—They are. The next on the list are the Bhatias, which is also a very wealthy caste. They hold the whole of the grain trade of Bombay, which means millions, and also the ghee trade, to a great extent, the cloth trade, and many other trades. Their death-rate is 26·48. The difference between the Bhatia death-rate and the Jain death-rate is this, that the population upon which it is estimated is much smaller, the Jains being four times as large as the Bhatias, 25,000 to 6,000. Therefore the figures are, of course, more reliable for the Jains. The Bhatias are a small class, but their mortality must be very large, because, with all their wealth, they ran away from Bombay until there was not one-third of the population of Bhatias left in Bombay, which ordinarily exists here, and still their death-rate for the second epidemic was 26·48, and it was the same the first year. The death-rate amongst them must have been very heavy. I will take next two together, because they are Hindus, one low caste and the other high caste. The difference between the two is only a matter of degree of poverty. There is not much difference between them. Formerly the low-caste Hindus used to eat all kinds of offal and bad stuff, but they do not do it now. They are more well-to-do, being in good employment, and there is no necessity for them to it. The death-rate in the higher caste is 21 per 1,000 as against 24 in the low-caste Hindus. I think those ought to be taken together, giving an average of 22½. The next are the Brahmans, which is an important caste. They are supposed to be of cleanly habits, and they belong to the intellectual classes. They are not in great poverty, and where they are in poverty they prefer it, being professional beggars and living well by their trade. Their death-rate is 20 per 1,000. The next is the native Christian, which is a poorer population. They are what we may call Hindus Christianised. Practically in lives, habits, and hopes they are Hindus. Their death-rate is 15. That shows that, being Christianised, they have improved somewhat, because their death-rate is lower than that of all the classes in the Hindus. When they are Christianised they eat everything. They go in for pork and turkey, roast beef, and so on.

17,685. They do not live in better houses?—No, they are poor. The feeling of the other Hindus is against them. It is hard work for a Hindu to be a Christian in his native country. They are not very well treated, because they are Christians, but their diet is certainly superior, and I think that accounts, to a considerable extent, for their lower mortality with regard to plague. I believe that the native Christian returns are fairly correct. Next come the Parsees, 9·42 per 1,000. They are, of course, a clean-feeding and well-fed people and intelligent, and they have rather a small mortality. The Muhammadans come next, 9·44, which is practically the same. The Muhammadans and Parsees stand upon the same level; their diet is very similar, the only difference being that the Parsees never eat beef, and the Muhammadans always do when they can get it. Then come the Eurasians with a death-rate of 7·30 per 1,000. That is rather high. I do not know why it should be so, because their houses are fairly good and their diet is exactly like ours; there is no difference. Still, they do get attacked in very much larger proportion than the pure Europeans, and when they are attacked they die very heavily. Then come the Jews, which is 5·7. They are a very clean-living population, but their numbers are very small. There are only 4,000 in the whole of Bombay. Still, I think this is a fairly correct representation. Lastly, the Europeans have a mortality of 1·06 per 1,000; they are almost immune.

17,686. Do you think it is entirely a question of race in the case of Europeans?—Habit, diet, housing, and

* Report of the Bombay Plague Committee on the plague in Bombay from the 1st July 1897 to the 30th April 1898, by Sir James Campbell.

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even clothing, I think, are all represented. I think, if this table were constructed 50 times over in 50 epidemics the figures will turn out the same. I have tried it in the last three epidemics, and it is wonderful how exactly they do work out.

17,687. To what extent do you think that poverty influences the mortality?—Exactly to the extent that it affects the mortality in ordinary years, not more. That would be, perhaps, in a ratio of 5 to 10.

17,688. To what extent do you think the housing of the people affects the mortality?—There the Commission has hit the great point. I think Lord Sandhurst's scheme is almost an answer to this; overcrowding is *the* great question in Bombay with regard to plague, with reference to the extent, the severity and preservation of infection in houses. I think this is the great blot on the Bombay system. If the Commission will spare me five minutes just to show them how 400,000 out of our 800,000 live, perhaps it will explain it in a nut-shell. The system is to get house accommodation on the cheapest terms. A single man has 10 rupees a month as an income. If he is a single man he goes to a man who takes in borders and lodgers, and says to him, "Here are 2 rupees 8 annas a month for you, "and you must feed me and give me a place to lie "down in." That is all he asks for; there is no furniture of any kind. The man says, "No, but I will "do it for 3 rupees." That leaves him 7 rupees of his salary untouched. The 3 rupees would include all his food and his house rent. It would not include his clothing or his betel-nut. This is the extent to which he pays for his actual subsistence, and more than that he will not pay. Our Municipality, for years past, knowing deliberately that the great mortality in excess of what it ought to be was caused by this overcrowding, has absolutely made laws—and never attempted to carry them out. I have roared myself hoarse at the Municipal Board meetings again and again for the last 25 years on this subject, and at last Lord Sandhurst's scheme, I suppose, is going to show you the difficulty can be got over. I have given up public life and public work, but I was pressed to go upon this new Trust; they placed me there as a member, and I hope that this overcrowding question will be properly looked into, even if we are going to spend a million, 2 millions, or 5 millions. The dispute at present between Lord Sandhurst and myself is this: he says he will do it for 5 millions, and I say he will not do it under 10 millions. The money would only be rightly spent when the overcrowding is abated. In a simple epidemic of any kind, be it our usual seasonal fevers, measles, or what not, you can go along the verandas and see the people lying there—all attacked in the same way—because they live too closely to one another, and breathe into one another's faces. I only wish to emphasize as much as I possibly can that the overcrowding has had a great deal to do with the intensity of the epidemic and the greatness of it.

17,689. What evidence have you that overcrowding has had an influence upon the incidence or mortality of plague?—I can only say in a general way that I believe it has kept it up and intensified it.

17,690. You have no figures?—No, it is very difficult to get them.

17,691. Can you tell us if plague has occurred chiefly in insanitary houses or insanitary districts?—Yes. I think it may be generally answered so, although, if you ask me if I have seen a great number of very good houses in which plague has been very heavy, I should say yes. I saw that in Mandvi. But as a rule, I think it has occurred in insanitary houses, because when you come to the outlying districts which contain the poorest population and the worst houses there we see the plague has the greatest intensity.

17,692. By insanitary dwellings or insanitary conditions, what do you include besides overcrowding?—Darkness, dampness, and want of ventilation. They are rookeries in lots of cases. In many of the outlying districts a man can hardly stand upright in his house. They are not fit for human habitation at all.

17,693. Do you believe the plague has been influenced by the mental state of the people?—At first it was so.

17,694. On what do you form that belief?—For three years I have had no income in December, January, and February. It was said, and there is some truth in it, that I have the largest medical practice of any man in Bombay, probably due to my very long acquaintance with the natives, and in December, January, and February, they will not come to me, nor will they send to me for fear of the plague officials carrying them off.

17,695. Are you a plague official?—No, but the plague officials follow me. The day before yesterday a wealthy gentleman sent for me. He said: "I was "going away to my country and only wanted you to "see that I was in good health before going away, "but immediately after you had gone two or three "plague officials came here and said: 'Dr. Blaney "'was here; what is the matter; show me your wife; "'show me your servant.'"

17,696. Do you think this state of anxiety has actually affected the mortality?—It did during the first year, but it is not doing so now. The people are brave now.

17,697. Have you been able to measure the effect? Can you state it in any definite form?—Not in any definite form. I was the Registrar of the cholera deaths in Bombay 50 years ago for the Government, and I used to collect often 100 death certificates in a day from cholera. The people took it as being quite a matter of course—it might be catarrh, or diarrhœa, or anything—nobody bothered about it. They took it very philosophically, if that word can be used in such a connection. That is the feeling now. There is nobody who has any fear now. I have, in the first year, seen insanity, following the fear of the measures, again and again.

17,698. How has the mental condition influenced the mortality of plague to your knowledge?—That would require a long time to answer. The statement made in my précis refers to the first year when the people were disturbed with the operations. It was that fear which influenced the mortality. I have seen over and over again people just lying down and giving up, where, under other circumstances, if there had been no fear, they would have, perhaps, striven against it and taken remedies, and done things which they would not do under that fear.

17,699. Have you any knowledge as to how long plague if uninterfered with would last in a city or district into which it had been introduced?—I think it has its cycle of years, but that, of course, is only supposition.

17,700. You have had some experience of the use of curative serums?—A little. In my practice I have estimated that I have done about 24 inoculations, with Yersin's serum and Lustig's serum. I have used none of that which they are now making at Parel at the present time. I selected all the cases which I inoculated, and, therefore, I did not think I could put those cases forward.

17,701. On what grounds did you select them?—As being immediately attacked and as being in perfectly good health before they were attacked.

17,702. You got them in the early stage?—Yes, and I got a good result. The result was that in several of the cases there was almost immediately subsidence of the fever and of the bubo also.

17,703. How many cases had you?—About two dozen. I remember one case, where I stopped with the girl about 70 minutes after I injected her with Yersin's serum, and in that time the temperature went down from 105 to normal. She sat up in bed—she was a fine, young, strapping girl—and said, "Come here, Father, I want some breakfast." She sat down and had a very good breakfast. She was taken away to the hospital and treated there, but she had no further bad symptoms.

17,704. How many of the 24 cases recovered?—About 48 per cent.; nearly half. I did not keep an exact account because I did not think the cases were fair to put before the profession as evidence of the value of the curative serum.

17,705. They appeared to be fair as showing what the serum is capable of doing when the cases were got in the early stage?—Yes, I think so.

17,706. Besides reducing the case mortality, serum treatment had an effect on the course of the symptoms?—Yes, in a few cases.

17,707. What dose did you use?—20 c.c. I think 10 c.c. was given at a time and repeated some times three or four times. I remember one case in which, in consultation with Yersin himself, I gave six injections within 24 hours.

17,708. Did the case recover?—It was a case of pneumonic plague; we did not expect it to recover.

17,709. What do you think is the most efficient measure to adopt to check plague when epidemic?—The best thing is to run away from it.

17,710. Run away from what?—From the germs, from the locality. We saw segregation going on here for a whole year, and saw the people carried out of their houses but with no reduction of the disease.

17,711. In so far as general measures are concerned, which are, in your opinion, the most effective for controlling or preventing an epidemic, apart from removing a patient?—Cleanliness and keeping in the open air as much as possible. I have always advocated that the labouring population of Bombay should sleep outside the houses, whether it rains or not. If they go into their rooms their feet and heads are together, and the whole temperature is vitiated, and even supposing there was no epidemic, they incur every possible risk because of the overcrowding in sleeping rooms. We find that those people who sleep outside the houses are very rarely attacked—those who work out in the docks during the day and keep out in the air at night also, and only spend enough time in their rooms to take their food.

17,712. What do you think are the most important measures for protecting a town or district from plague?—I think you should get the people in the open places as quickly as possible, and encourage them by every possible means to go there. The houses themselves should be well ventilated, and as much sunlight let in as possible, and there should be daily scrubbing and scouring. The main thing is cleanliness, which is not observed in an oriental city at all under the best of circumstances.

17,713. (*Mr. Hewett.*) Has the proportion of deaths to the number of persons attacked been different in the different epidemics?—I am trying to get that question worked out now at our hospitals. I shall not be able to give a complete answer till June or the end of May. There seems to be an opinion abroad that in this third epidemic the cases die in the hospital very quickly, within 24 hours, very much more than they used to do in the first or second epidemics. From my casual observations in the visits I make to the hospitals occasionally it appears so. I see during the last week or two patients are living longer. In the first and second epidemics there would be many patients who lived to the fifth, sixth, seventh or eighth day, whereas at the beginning of the third epidemic in December last they were all dying in a day. Dr. Choksy told me that in they the month of December, in his hospital, it was the same thing, but it is not so now; it is undergoing a change.

17,714. Do you think that the proportion of persons attacked who died in the second epidemic was greater than the proportion of those attacked who died in the first epidemic?—The statistics are not exact. There has been no trouble taken at the burial and burning grounds with regard to verifying the cause of death, but the figures, such as they are, are equal in both years and in all classes and in the different districts, so that no answer can be given to that question. I believe in the two first epidemics it was as nearly as possible equal, but the Commission must remember that at least 400,000 people left the city in the first epidemic, and there was no such exodus in the second epidemic. This year the mortality is decidedly less up to the present.

17,715. Do you think that there has been any great difference in the number of cases which have escaped detection in each of the three epidemics?—No.

17,716. If the mental attitude of the people was so much disturbed in the first epidemic, why did not a larger number of the attacked die than in the other two epidemics?—I believe a much larger proportion of them died, as you will find if you reduce the population from 800,000 to 400,000 and work it out in that way.

17,717. You think there were more attacks by reason of the mental attitude?—No, that is not the statement. The statement is that they were badly affected by their mental attitude.

17,718. I understood you to say that a larger proportion of those attacked by the plague in the first epidemic did not die than in the second and third epidemics?—No, I have not said that.

17,719. Did not you say that you thought the proportion of deaths to attacks was much the same in the three epidemics?—Yes.

17,720. Is not that the same question as I asked you?—No; that is where the difference in population comes in. If correction is allowed for that, then the statement is correct enough.

17,721. The question of population does not seem to come in there at all. I understood you to say that the mental attitude of the people caused the disease to be more fatal among those attacked?—No; that requires a very much longer explanation than two lines, because it requires knowledge of the actualities of the case in the matter of segregation. I was at the Arthur Road Hospital one day when a cart came up with 11 bodies in it, nine of which were dead. That explained the mental attitude a good deal. But I think it is too large a question to be epitomised into two lines.

17,722. What is your experience as to the conveyance of infection from one person to another?—It is very little.

17,723. Why do you say it is very little?—Because we do not find it.

17,724. How do you consider that the disease proceeds from one person to another?—I think I ought to have been attacked 50 times by now, but I have had the sputum in the eyes and down my throat and in my ears and everywhere about me.

17,725. Can you explain how the plague extends if neither the rats nor the people carry it?—The household goods do it; everything which is removed from an infected house will carry it.

17,726. I understand it to be your opinion that the best means of stopping plague is to separate the people from the plague poison?—Certainly.

17,727. The plague poison being located in the houses?—Yes, mostly.

17,728. Then it is a good thing to take the people out of their houses?—Yes.

17,729. Having taken them out of their houses you would, I suppose, disinfect the house as thoroughly as you possibly could?—Yes, that is reasonable, of course, though it does not do very much good.

17,730. Do you think that disinfection does not do good?—I will not say that, but is it possible to disinfect, say, a six-storeyed house? I could show you a six-storey house with 2,000 people living in it, and I would be ready to bet the Commission 50 guineas that they cannot disinfect it.

17,731. Having removed the people out of their houses and got them away from the plague poison what further measure would you take with them?—Now, you come to the question which I put before the Medical and Physical Society, that we must divide the places—a big city like Bombay would be first, then a city of 50,000 inhabitants and so on, going down, because the more you go down the more manageable the whole thing becomes.

17,732. Do you consider that the main object should be to get the people away from the infected place and to destroy the infection in the place?—I think the far better plan is not to have the plague at all. I see I have wasted a great deal of my time in the last three years in seeing how we can cure it. Now, I have left off therapeutics and want to confine myself to the question of how to stop it.

17,733. When plague has come, do you think that the best way to stop it is to clear the people out of the infected neighbourhood and to disinfect the houses?—Certainly; doing the best you can to clean up.

17,734. (*The President.*) You think the best thing to do is to protect a town from plague; what would you do in such a place as this to prepare it against plague?—Rebuild it, I suppose, just as we are doing. I believe we are going to show the world an advance in sanitation which neither Glasgow nor Manchester nor Birmingham nor London had. They have only done it in part, and I believe we are going to do it as a whole in this city.

17,735. (*Mr. Cumine.*) You think that running away is the best thing to do?—If you are not immune.

17,736. But people do not begin to run away until a certain number of deaths have occurred and plague has taken hold upon the town; is not that so?—That is so to a certain extent, but feeling acts upon feeling, people

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in a house act upon people in a house, and people in the street upon people in the street.

17,737. Did not the exodus begin after the plague had taken a pretty firm hold upon Bombay and a lot of people had got infected?—Yes.

17,738. So that probably a large number of people who ran away carried the plague with them?—Yes, probably.

17,739. Is not that a very dangerous thing for India at large?—I think we have made too much of that; India at large cannot be attacked. They say there is no history of plague along the valley of the Ganges.

17,740. So much plague as there is in the Bombay Presidency at this moment has all been carried by people running away from Bombay, has it not?—It is probable it is so. 12 years ago I saw the approach of this sanitary calamity—I could not give it the name of plague. I was in the habit of keeping a mortality record for 30 years, and I saw that the mortality of Bombay was going up and up. When I went to the trade districts I found the trade increasing, the godowns increasing, the rats increasing, every abomination increasing, and over-crowding increasing, because the ground was valuable and the place was necessary for the people to get their living. I went to the Corporation and proposed a loan of 60 lakhs of rupees to improve Mandvi. There it is, on the records of the Corporation now. I only lost that by three votes. They said I was the most extravagant man on the Corporation, but if I could have used the word "plague," if I could have said, "Gentlemen, you will be attacked with plague if you do not give that money" I should have got it, and Mandvi would have been a different district, and perhaps none of us would have been sitting here to day. I saw a sanitary calamity coming into view, but what the name of it should be I could not say.

17,741. I think the word "Jain" is used in a loose sense in Bombay, is it not?—No, not in a loose sense, but in a compound or conjoint sense. A Jain is a man whose religion consists in not destroying animal life, and in that coterie there are the Marwaris and Banniahs —altogether 15 or 16 classes.

17,742. Does not the word "Jain," as used in Bombay, include people who are not of the Jain religion but of the Lingayat religion?—I am not quite sure of that, but the Lingayas are only a population of 3,000.

17,743. These Jains, which you describe as people who will not destroy animal life, are quite a small proportion of the total number of Jains, are they not?—No, I do not think so; I think the registration of 25,000 is correct.

17,744. To explain the figures which you have given you attribute a great deal to diet. But have you made proper allowance for the relative exposures to infection which the European has, for instance, and a Bhatia who lives in Mandvi?—That cannot be shown in a table.

17,745. But in your own mind have you allowed for the difference? There is a great difference in the *chances* of infection, between a European living on Malabar Hill and a Bhatia living in Mandvi, is there not?—Most certainly, and if you take two chawls, one containing 200 people and another containing 50, there is a great difference between them also.

17,746. (*Prof. Wright.*) What steps ought, in your opinion, to be taken to detect plague cases in this city?—I do not think any.

17,747. Do you think doctors ought to be called upon to report plague cases in their practice?—Yes, that is reasonable. We have had that law in force here, and I myself have observed it most rigorously, though I believe I was the only man in Bombay who did so 30 years ago. I have always reported them, even cases of puerperal fever. I think it is quite reasonable.

17,748. Do you think that corpse inspection is reasonable?—I think the Health Officer should have some voice in matters of that kind, because he is held responsible if a serious epidemic arises.

17,749. Do you think that corpse inspection ought to be resorted to in order to detect cases of plague?—Where? That is a very important question. Do you mean at the cemeteries or at the houses?

17,750. At the cemeteries say?—Yes; at the cemeteries you may examine as much as you like. If the Municipality had properly managed the thing, and there had been proper medical men at the cemeteries, the people would never have objected at all; they would have allowed inspection and examination to be made and proper reports of the causes of death. I do not make that statement without the very greatest authority. I was Coroner of this City for 17 years, and, in doing my duty, I have been at these burial and cremation grounds continually, and I have never found the slightest difficulty in finding out everything connected with the case of death there. And that was not because I was Coroner, for my own subordinates could also do it.

17,751. Do you think the patients would give you the right addresses at the cemeteries?—Oh, yes. The police have proved that in their statistics for the last 25 years. We have a double registration here. Were it not for the police and the Municipal registration our figures would be absolutely worthless. At present we look upon the total mortality as correct, and it is correct, and it is correct because the police have to furnish the Commissioner of Police with separate returns every day of every case.

17,752. I understand there is a difficulty experienced in finding out the houses where plague deaths have occurred?—There may be a little in a big city like this.

17,753. Can you see any way of avoiding that difficulty?—I think the Commissioner of Police should tell his sepoys that any increased pay or promotion which they may be looking forward to will be stopped unless the information is obtained. I believe that would stop it immediately. There has been no considerable difficulty with the police in getting all the information which they require, but they were non-professional people and they could not get the causes of death. Here we have to be with a sort of quasi-professional people whom they call Hospital Assistants, and who possibly might be able to diagnose diseases, but they have done nothing; they were hand and glove with the people as far as the statistics seem to show.

17,754. (*Mr. Cumine.*) Can an ordinary coolie tell you the number of the house and the street in Bombay in which he lives?—No.

(Witness withdrew.)

Adjourned till to-morrow.

At The Secretariat, Bombay.

FORTY-SEVENTH DAY.

14th February 1899.

PRESENT:

PROF. T. R. FRASER, M.D., LL.D., F.R.S. (*President*).

Mr. J. P. HEWETT. | Mr. A. CUMINE.

Mr. C. J. HALLIFAX (*Secretary*).

Mr. ISMAIL JAN MAHOMAD called and examined.

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17,755. (*The President.*) You are a Licentiate of Medicine and Surgery?—Yes.

17,756. And you are also a member of the Municipal Corporation?—Yes.

17,757. How long have you been in practice?—This is the 13th year of my practice.

17,758. You have been here all the time of the plague?—Yes.

17,759. What is your opinion as to when the first cases of plague occurred in Bombay?—I think it was about the beginning of August 1896.

17,760. Did you see any cases about that time yourself?—I saw two cases, which I did not diagnose as plague, but which I now think were pneumonic cases of plague. After that I saw two cases with glandular enlargements, and when I saw these cases I communicated with the Health Officer, and told him that I had noticed two cases with high fever, delirium, and enlarged glands in the neck. That was about the 15th or 16th of August 1896.

17,761. You saw two cases before that, you say, which you now think were cases of pneumonic plague. Can you tell us what symptoms those cases had, and why you think now that they were pneumonic cases?—The first of these two cases, of the pneumonic type, was a Multani, who had returned from Nasik. On his return he had fever, and when I saw him his temperature was about 104 degrees, and he was delirious. I examined his lungs, and found no dulness, but there were fine crepitations, and I thought that the symptoms were too severe for a case of broncho-pneumonia, the delirium having come on so soon as that, and being followed by a fatal result within 36 to 40 hours after I had seen him.

17,762. What is the other case?—The other case was also in the same firm—in the same house. He also had been to Nasik, but he did not come back sick. He developed fever after his return here; he was a young man. They sent for me at once, and when I saw him first he had a temperature of 105. In the evening, when I saw him again, he developed the same symptoms of pneumonia, and died within 36 hours.

17,763. Had these cases any relationship with each other?—They were in the same house.

17,764. How long had they been in Bombay?—They had gone from Bombay to Nasik for a pilgrimage. They stayed there for five days, and then returned to Bombay.

17,765. How soon after their return did they die?—One of them returned with fever after his stay at Nasik for five days; and the other developed symptoms of fever after the death of the first. In fact, both the persons died within a week's time.

17,766. What is your view as to the origin of plague here?—When I first saw plague here, I thought that the condition of Mandvi at the time was so filthy, and the houses were so very much overcrowded, that that was the immediate cause of the outbreak. The condition was so filthy that nearly 1,200 cartloads of sewage and filth material were removed from the Mandvi drains at that time. The houses also were very much overcrowded, each room containing five to eight people—a small room of about 10 feet by 16 feet. Although these rooms are well ventilated, still there was very much overcrowding in them.

17,767. Were these rooms in chawls or not?—They were in big houses.

17,768. Not chawls actually?—No, the first cases were in large well-ventilated houses.

17,769. The first cases were the imported cases from Nasik, I understand?—I do not think those were imported cases myself, as I changed my mind afterwards. My first impression was that those two cases were imported cases from Nasik, but subsequently I found out, when I went to this place to treat those Multanis, that there was already a scare among the men in the firm, and many of them were running away from Bombay.

17,770. You said that one of the cases already had fever when he arrived from Nasik?—Yes, but he had left Bombay only five days previously.

17,771. Was there any plague in Nasik at this time?—No, there was no plague at Nasik at all at that time, and plague did not develop in Nasik until last year.

17,772. Will you proceed with your opinion as to the origin?—When I saw those two cases my first impression was that perhaps those cases were imported, but I found out that there was already a scare among those people, and not only that Multanis in that firm, but Multanis in other firms in Bombay, had run away; and I heard that, a fortnight before, there was plague occurring among these people, just about the beginning of August. About six weeks ago I was talking with a resident of Mandvi, and inquiring as to whether anybody had noticed where the dead rats first were found, and I was told that they were found in godowns containing Chinese goods. At that time my attention had not been directed to the fact that there were godowns containing Chinese goods at Mandvi, and I found out that in Argyle Road there were three godowns containing Chinese crackers. These Chinese crackers come from Hong Kong.

17,773. What are the crackers that you refer to—the crackers that you pull or those you eat?—Yes, they are the crackers that you pull. There were also other godowns containing tea and sugar and china silk at Mandvi; and I was told that in one of the sugar godowns dead rats were found.

17,774. That was before these cases you have told us of?—Yes. Curiously enough, on further inquiry I found that these two patients lived in a house which was adjoining one of these godowns; and I found that there was another godown just at the back of this China goods godown; there was a granary, and a number of dead rats were found in that granary, and several plague cases were found in the house on the top of that granary.

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17,775. To what conclusion do these facts lead you?—They lead me to conclude that the poison must have been brought with the goods either in the form of fomites, or, perhaps, a dead rat, which may have infected the rats in that locality; and thus the rats of the locality being infected, they communicated the disease to men. The dwellings in that locality were particularly such as to make these rodents take up their habitations in them; the houses were dirty, and rats made them their hiding places. I believe if a sick rat died in a place where there was a lot of rubbish concealed, the presence of that sick rat dying of plague in that place would communicate the disease to human beings, either through the poison being in the air and thus communicating the pneumonic form of plague, or, perhaps, by the residents walking bare-footed, and getting themselves infected through cuts. I have noticed several cases in which such cuts were the cause of infection.

17,776. You have not told us your opinion as to the manner in which the rats first acquired their infection?—I think it came from either a dead rat or fomites brought in the goods from Hong Kong.

17,777. You think China was the origin?—Yes, I think China was the origin, because particularly at that time, in the month of August, crackers are imported for the Diwali purposes, and also for a Muhammadan festival where crackers are used; and I think Hong Kong has thus a direct communication with Bombay.

17,778. Have you considered the other possible positions from which plague might have been imported?—Yes, I have considered those conditions. It has been said that perhaps it has been imported by Sadhus bringing down the plague from Upper India, and from Nasik. I think there are strong arguments against such a theory. First, as regards Nasik, there was no plague at Nasik at that time at all. We did not hear of plague at Nasik until the next year—the second epidemic—and then, as I said before, there was already a scare among the residents at the time when those two cases occurred, and those residents had never been to Nasik at that time.

17,779. You think the Sadhus have nothing to do with the carrying of plague?—I do not think so, and another thing also is, that if the Sadhus had anything to do with plague, the Sadhus would have communicated it on their way at many other places before they communicated it to Bombay. There was another curious thing about plague at Mandvi, and that is, that for about a month or so plague was confined to a particular locality of Mandvi, just below the Masjid Bridge, showing that the poison was there among the rats in that locality. Now that locality is separated from the other locality by a big railway line, and for one month not a single case occurred, till people were allowed to remove from that place and go and live in the centre of the crowded locality of Mandvi. I believe that in that way it was spread both through rats, as well as through human intercourse, to other places. I noticed in two cases that people suffering from plague, who were living at Mandvi, were taken to Khetwadi. There was not a single case of plague up to that time there. When these two persons were taken there, they died. After their death, for some time no case of plague occurred, but, 15 to 20 days afterwards, dead rats were found in the locality; and after the appearance of dead rats, plague spread; so that I believe that in that case plague was first conveyed from Mandvi to Khetwadi by these two people being carried there. They infected the soil there, and rats were infected from that. Similarly at a village of Versova, where I used to live before, a case was taken there and concealed. He died there, but nothing of plague was heard there for about 20 days, when dead rats were found, and after that plague broke out. I think, therefore, that plague is conveyed from one place to another by human intercourse: first, a plague case is carried there, that infects the soil, and then rats are infected. I believe rats have a good deal to do with the spread of plague.

17,780. In regard to the infection, how do you think that enters the human body?—I think one of the modes is through cuts—direct inoculation—and I have noticed several plague cases where there was a small cut on the foot. The pneumonic form of plague is generally acquired through the air. I have noticed in some cases attendants getting it. In the case of Dr. Manser, and also of some of my private patients I have noticed that.

17,781. Where it is produced by breathing an infected atmosphere?—Yes.

17,782. In those cases where you have observed a lesion of the feet, or of a foot, on which side, and where, did the bubo occur?—I have not particularly marked it, but my impression is that it was generally on the right foot.

17,783. If the lesion was on the right foot, where would the bubo be generally?—On the right side.

17,784. The bubo was generally on the same side as the lesion?—Yes.

17,785. Have you any instance in which a pneumonic case appeared to have been produced from a bubonic case?—I have not particularly noticed such a case, but I believe it to be quite possible.

17,786. You have no facts?—No.

17,787. Have you any instance in which a bubonic case has been produced from a pneumonic case?—I cannot produce any facts.

17,788. You have facts which show you that pneumonic cases may be produced from pneumonic cases?—That I do say.

17,789. Have you facts which show you that bubonic cases may originate from bubonic cases?—I do not think that the mere remaining with a bubonic patient would give rise to a bubonic case. I do not think that plague with a bubo is communicated through the atmosphere.

17,790. There is no immediate communication?—No.

17,791. It must be communicated through the soil to the person who is infected, you think?—Yes.

17,792. Have you had much experience of Haffkine's prophylactic fluid?—Yes, I have.

17,793. Please to state generally the results of your experience?—In my dispensary, about 996 people were inoculated by my partner with Haffkine's fluid. I have done several. I have the records of these here, and I know the after history of most of them. Out of these 996 cases, there were 39 cases where the inoculation was done before the age of five years, there were 10 cases in which inoculation was done after the age of 60 years, and there were 947 cases in which inoculation was done between 15 and 60. There were three cases in which plague developed after inoculation. In one case plague occurred two days after inoculation, in another it occurred 29 days after inoculation, and in the third case it occurred 18 days after inoculation. The one who got plague two days after inoculation recovered, the other two died; but inoculations were performed in such a class of patients that I believe if it had not been for inoculation, many of these 996 cases would have succumbed of plague.

17,794. Were these cases selected in any way?—No; those who used to come to our dispensary were inoculated.

17,795. They came voluntarily?—Yes.

17,796. Do they represent the very lowest, or the middle, or the higher classes?—From the lowest to the highest; all classes.

17,797. Many of them lived, therefore, in the worst sanitary conditions?—Yes.

17,798. And others in good sanitary conditions?—Yes. In addition to the inoculations in our own dispensary, of my own community about one-half were inoculated at H. H. Aga Khan's bungalow, Khushroo Lodge. Still, I used to take a great deal of interest in knowing the after results of these, and I have collected all the figures of the inoculated, the number of cases in those inoculated once, and the number of cases in those inoculated twice. I have got them here, and I will present them to the Commission.* Professor Haffkine has submitted his report† about the first period of inoculation from 20th December 1897 up to the 20th April 1898. I have tried to verify all the results given in that report, and I find that the names of the people inoculated, and the results, are all perfectly correct. After that date, that is from the 20th of April 1898 to December the 21st, 1898, about which M. Haffkine has not submitted a report, I have

* *See* App. No. LV. in this Volume. A report by Dr. Khaja Abdulla regarding cases of persons treated in the Khoja Hospital, Bombay, with a list of inoculated cases, is published as App. No. LV. (1) in this Volume.

† *See* App. No. IV. in Vol. I. of the Commission's Proceedings.

tried to collect the figures, and I am glad to say that the results are more encouraging than for the previous period.

17,799. How many cases were inoculated in this last period which you speak of?—About 5,000 are inoculated, and the other 5,000 are not inoculated.

17,800. What other 5,000?—The Khoja community consists of about 10,000 people.

17,801. There was no selection made for inoculation, was there?—There was no selection, and I think that the Khoja community presents an aspect about inoculation which ought to be instructive for others. In fact, among the Khojas, 90 per cent. of them live at Mandvi, the place where plague first began. Many of this 90 per cent. are middle-class Khojas living in overcrowded chawls and houses, most of them living under the same circumstances, and often in a family some of them are inoculated, and some of them are not inoculated. There are instances of inoculated and uninoculated persons living under the same roof, and under the same circumstances, in which those who were inoculated escaped being attacked with plague, and those who were not inoculated got plague. In the case of my brother's son, who was not inoculated, he got plague, while the others, who were inoculated, all escaped.

17,802. How many others?—Nine.

17,803. Living in the same house?—Yes, I can point out many other instances of that in the Khoja community, in which people living under the same roof, and under the same circumstances and conditions of life, escaped among the inoculated, while those not inoculated got plague. There are two instances which I specially want to bring to your attention. The first is that of a boy of about eight years of age. He got plague a week after he was inoculated a second time. He had a big bubo in the groin, and when I saw him his temperature was 105. I inquired of the parents whether there were any cases of plague in the house, and the answer was that this second inoculation was done because dead rats were found in the house.

17,804. How long after the first inoculation was the second inoculation performed?—It was about 10 or 12 months after the first inoculation. The second inoculation was done because dead rats were found in that house. On further inquiry I found that there was another man who was suffering from plague in the same house. This boy, who was inoculated, recovered; the man who had plague, and was not inoculated, died. Now, this first boy, who was inoculated, and who got plague, recovered. It was a genuine case of plague—I do think his recovery was due to inoculation, and I believe, if he had not been inoculated in time after the dead rats appeared, that, perhaps, he also would have died. There was another similar case in which symptoms of plague developed after the first inoculation, and in which case also the inoculation was done because dead rats were found. This case also recovered, but I believe, if he had not been inoculated in time, that, perhaps, he would have been the worse for it. As to the second period of inoculation among the 5,000 Khojas, I have calculated that out of the 5,000 that were inoculated between the period of April the 21st, 1898, to December 21st, 1898, there were 22 cases of persons who were inoculated, who got plague, and died of it; and there were 26 cases of persons who were inoculated, and who died of other diseases. There were 218 of the non-inoculated who died of other diseases, and there were 44 of the non-inoculated who died of plague.

17,805. In addition to the 218?—Yes. Now, the average mortality among the Khoja community, taking the average of five normal years, is 342, about one death a day; and the average normal mortality for the eight months that I have selected for my calculation would be about 230. Now, I have said about half the community were inoculated, and half not inoculated. 5,000 were inoculated, and 5,000 were not inoculated, so if there is a normal mortality, then we would have 115 deaths in the inoculated persons from ordinary causes in ordinary times. Instead of that, we have got only 48 deaths, so that we get a mortality even lower than the normal mortality.

17,806. That is less than half?—Yes. Now let us take the uninoculated. We have got among the uninoculated a mortality of 218 and 44, a total of 262. Instead of 115 we get 262 there. I have tried to verify these figures from our jamaat books. Our community has a record kept, in which the name of every person dying is given, because the community gets a certain amount of fees for every death, so that the figure for total mortality which I have given here of all the four, 310, is a figure which is quite correct. I have taken it from the books of the jamaat or community. Now, as regards the figures I have given you of people suffering from plague after inoculation, 22, that is approximate; but I think that there are actually less deaths from plague among the inoculated than the figure that I have given you. When I say that, I say it for this reason, that up to this time in the jamaat book there is a record of only four cases of the inoculated dying from plague. I have put it down as 22, because, from my investigation, I found that there were some cases which were put down as brain fever by the practitioners, which were actually cases of plague, so I have added to the original figure 4 18 more of suspicious deaths after inoculation, and so have brought the figure up to 22, but I believe, if careful notification was made, the number of plague cases after inoculation would be even less than 22. In the figure of 218, which I have given of persons dying from other diseases among the non-inoculated, I can find among those many cases of people who have died of plague, but who have not been returned as dying of plague, but from other diseases—as many as 90 out of those 218. If we take all cases of brain fever, and some cases of phthisis, then I think that as many as 90 may be deducted from those as having died from plague. I am now carrying on my investigations in verfying these figures, and I want to bring them out just when Professor Haffkine brings his own figures out.

17,807. So that you think the number of plague cases was erroneously increased by wrong entries?—Yes.

17,808. How do you account for this remarkable difference in the mortality from causes other than plague in the non-inoculated, as contrasted with the inoculated?—I think the non-inoculated death rate has increased in comparison to the inoculated, because several weak people do not get inoculated, and inoculation is not done in very young children, in whom there is a heavy mortality.

17,809. There is a high mortality in the very young?—Yes, at present there is a great mortality in the young children, and that may account for the large majority; but I think there is some protective property in inoculation against other diseases also. I have not noticed up to this time in my community any complaints about the after effects of inoculation. I have found in some cases, where reaction was strong, that people suffered from loss of appetite and weakness, but after a time it passed off. I lately had a case where I re-inoculated a maid servant in a family. She would not undergo inoculation last year. She was suffering from a severe headache for many years. Curiously enough three or four days after inoculation the headache entirely disappeared.

17,810. Has she remained free?—Yes, I inquired yesterday, and it is quite gone.

17,811. What other affections do you think are benefited by inoculation?—I have no facts with regard to that; but my impression from the figures is that it has some effect.

17,812. You think the mere exclusion of the very old and the very young will not chiefly account for it?—I do not think it would entirely account for it.

17,813. Can you give us the ages of the 5,000 who were inoculated in this community, and of those who were not inoculated?—They are not absolutely correct figures. There may have been 200 more or less. Our census is also not very accurate, but I believe that 10,000 is a fairly correct figure.

17,814. In the case of the 5,000 who came directly under your notice, can you give us the age of each of those who were inoculated?—I do not think I can give the age.

17,815. What ages did you exclude in inoculation as a general rule?—When I was practising inoculation, I used to avoid persons who were very weak in constitution, whatever age they might be. I would inoculate an old man of 60 or 65 if he had a good constitution.

17,816. I thought you placed importance on the fact of the young and the very old being excluded, in accounting for the higher mortality in the non-inoculated?—I excluded a great many, and others also, perhaps, remained away themselves.

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17,817. You inoculated all who presented themselves if you found them sufficiently strong?—Yes.

17,818. Age was not a barrier?—No.

17,819. That was your experience of a part of the 5,000; what was your experience of the remainder of the 5,000?—The remainder of the 5,000 were not inoculated.

17,820. I mean those that you did not yourself inoculate?—I have not inoculated these 5,000. Those were all inoculated at H. H. Aga Khan's bungalow at Khushroo Lodge. I have been among these people from the beginning, and I have been getting out of the jamaat books how many persons were inoculated, how many were not inoculated, and how many died after inoculation, and so on.

17,821. Will not these books show you also their ages, and when they were inoculated?—No.

17,822. Was there no distinction between an infant and a grown up person?—No.

17,823. Have you had any experience with regard to the curative treatment of plague?—Dr. Yersin was requested by me to see a case at the Connaught Road Khoja Hospital of a patient who was picked up in the road. Dr. Yersin saw the patient, and he pronounced the opinion that the case was very bad, and he would not inoculate the curative serum. I showed him another patient who was admitted that very evening, and he inoculated that patient, and the inoculation was continued for three days successively. On the fourth day the fever of this patient who was inoculated with Dr. Yersin's curative serum was less, and the patient was on his way to recovery. At the same time the other patient, whom Dr. Yersin refused to inject, was also getting on well.

17,824. That was the patient whose symptoms were apparently more severe?—Yes.

17,825. Have you any other experience?—Yes, I have two other cases that I placed under Dr. Yersin, in the Khoja Hospital, but unfortunately they both got bad and died.

17,826. Was much serum injected?—Yes, each of them had serum injected every day for three days, morning and evening.

17,827. In what day of the illness was the treatment commenced?—One of these two cases was on the first day; the other case was on the second or third day of of the illness. I have noticed four other cases under Dr. Yersin. They were not my cases, they were in a neighbouring hospital. There were five cases altogether—four died and one recovered.

17,828. Was your experience satisfactory or unsatisfactory?—My experience was unsatisfactory.

17,829. You made some reference to insanitary conditions not being accountable for the production of plague. How far do they affect or increase the virulence of plague, assuming it has been introduced into a locality?—I think insanitary conditions conduce to make the plague more virulent, and cases would end fatally. There would be no chance of recovery for people suffering from plague, and living in insanitary houses.

17,830. As to the influence of insanitary conditions on the amount of plague, what do you think of that?—I think that with insanitary conditions plague will certainly increase.

17,831. Have you any facts which will bear out that opinion?—I know of several houses in which one case of plague has occurred, and because the house was not in a sanitary condition the whole number of the people living there—they did not vacate the house—became victims of plague, and died.

17,832. Is it your opinion that people living in insanitary houses are more easily affected?—Yes.

17,833. You said that the plague appears to be very virulent in such houses, or, at least, that the chance of the recovery is less?—Yes.

17,834. As to the localities in this city in which plague has been most prevalent, have you been able to trace any relationship between that prevalence and the sanitary conditions?—As I said before, both Mandvi and Kamatipura are known as the haunts of plague. In Mandvi there are big and well-ventilated houses, but the sanitary condition as regards the drains is very bad, and there is overcrowding in the houses. As regards Kamatipura, the houses are all low, the ventilation is not good, and there is overcrowding there, too.

17,835. These, you say, have been the chief haunts of plague?—Yes.

17,836. Are these the most insanitary quarters of Bombay, or not?—Kamatipura is the most insanitary quarter of Bombay. Mandvi is not so insanitary; but I think Mandvi remains the haunt of plague for some other reason.

17,837. What other reason?—One reason is because the granaries are there, where there are a number of rats. I believe if there was a single house which contained infection, as soon as there is a colony of new rats, they get themselves infected, and spread the plague. After the plague has been over the place, and all the rats are dead, for the time being the plague subsides. Then there comes a fresh colony of rats.

17,838. A fresh importation?—Yes, and they get themselves infected, and plague spreads.

17,839. Have you anything to say with regard to curative serums?—I treated one case with Dr. Lustig's serum. In that case the effect was very good. The patient, a Khoja lad, suffered from fever and a bubo in the groin. I saw the patient. I found he had eczema on the legs, and I thought the enlargement of the gland was due to that. I treated him for one or two days, and he got well. After a month he again got fever, his temperature going up to 105 degrees. He had a small gland just on the side of the chest. On this occasion there was no eczema, or anything to account for the gland. As this patient had been visiting another plague patient, I had grounds for believing that the lad had plague, and I immediately sent for Dr. Galeotti. Dr. Galeotti's Assistant, Dr. Polerini, came and injected the serum. He gave no other treatment, except injecting the serum. For three days the lad had fever.

17,840. Was the serum injected twice a day?—Yes, twice a day. The fever subsided on the third day. Then, again, after three or four days the fever increased, and the bubo suppurated. Then it was opened, and a big characteristic slough of plague gland came away. I think this was a typical case of plague, and that the cure was due to Professor Lustig's serum.

17,841. Was that the only case in which you used Dr. Galeotti's serum?—Yes.

17,842. I understand that in the other cases Yersin's serum was used?—Yes.

17,843. (*Mr. Hewett.*) Among the uninoculated who died, I see as many as 47 were under one year of age?—Yes.

17,844. Whereas among the inoculated who died there were only two?—I have not calculated the figures.

17,845. I shall be glad if you will give me the following information:—1st, the number of inoculated that died under one year of age; 2ndly, the number of uninoculated that died under one year of age; 3rdly, the number of inoculated who died between the ages of one and five; 4thly, the number of uninoculated who died between the ages of one and five; 5thly, the inoculated who died being over 60 and under 70; 6thly, the uninoculated who died being over 60 and under 70; 7thly, the inoculated over 70 who died; and, 8thly, the uninoculated over 70 who died?—The following figures show the number of deaths in the Khoja community at different ages amongst the inoculated and uninoculated. The number of deaths of children inoculated under the age of one year is one only: the number of deaths among children below one year not inoculated is 57. The number of deaths amongst children inoculated between 1 and 5 years is 7; and amongst the non-inoculated between 1 and 5 years is 33. The number of deaths amongst the inoculated between 60 and 70 is 2 only, and amongst the uninoculated 28. The number of deaths amongst the inoculated over 70 years is 2, and amongst the uninoculated 8 only. The above figures show what advantage the inoculated have over the uninoculated at different ages. It would be interesting to know how many were inoculated at the different ages, but unfortunately the jamaat has not got such a record, but I suppose Prof. Haffkine must have got such a register.

17,846. (*Mr. Cumine.*) You gave us two sets of figures; one set you got from the Jamaat book: where did you get the other set from?—I got the other figures after verifying some of the cases myself. I went and

made inquiries in the families, and I found that several of those who were reported to have died of other diseases, actually died of plague.

17,847. Were you ever supplied with a register of the names of all the people who had been inoculated?—No.

17,848. In going through the jamaat books, how did you know, as regards each death entered, whether the person was inoculated or not?—Since inoculation has been introduced great care has been taken in inquiring whether a particular man who has died was inoculated or not, and how many times he was inoculated.

17,849. But you did not test to any extent the correctness of the entries in the jamaat book—you had no register of inoculated persons to refer to?—I had no register; but by visiting the families, and making inquiries, I was able to find out whether a particular person mentioned in the book had been inoculated or not.

17,850. But supposing an inoculated person had died, and his death had never been put down in the jamaat book at all, how would you have known of it?—It is impossible that a death could not be put down. The jamaat book is most accurate.

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17,851. Did you keep any note of how many persons there were in each of those houses so visited: how many persons were inoculated, and how many uninoculated; and how many attacks and deaths there were amongst the inoculated and uninoculated respectively in each house?—I have not got a register of that.

17,852. (*Mr. Hewett.*) Did you attend a man named Narayan Mulji for plague?—I attended Narayan Mulji for axillary abscess, not plague.

17,853. Did he have more than one attack of plague?—He did not suffer from plague at all.

17,854. Please give date and details?—The axillary abscess was opened in 1896, this had left a sinus which suppurated after a couple of months and was opened again.

17,855. What was the position of the buboes in each attack?—The abscess reformed in the same axilla.

(Witness withdrew.)

Mr. George Lund called and examined.

Mr. G. Lund.

17,856. (*The President.*) I believe you are a merchant?—I am in business in Bombay.

17,857. Have you had much to do with plague measures?—Yes, a great deal, for more than twelve months, day by day.

17,858. (*Mr. Hewett.*) Would you specify the districts in which you have been working as a volunteer?—I have been working in the Mazagaon and Tarwadi districts, more particularly just recently in the latter.

17,859. What is the character of the population in those districts?—Principally Ghatis. The population at the last census was close on 55,000: at the present moment it undoubtedly is considerably in excess of that number. In the southern portion, the people are principally Ghatis by caste, labourers and cartmen, living from hand to mouth, and employed in the docks and Mandvi; there are also Portuguese and some fisher folk. In the northern part there are cartmen and mill hands, with several large colonies of Parsees and Muhammadans located here and there all over the districts.

17,860. Is the district a very congested one?—Some parts of it are very congested, particularly at the present time.

17,861. What was the state of the public sanitation when the plague broke out?—The public sanitation was, and is at the present time, not at all good. It is most unsatisfactory, owing to a considerable portion of the district being at so low a level that lifting must be had recourse to to deliver the sewage at the outfall at Mahalakhshmi. The necessary works are now being vigorously pressed on, but it will be some time yet before they can be completed. At the present time the sullage water passes into the side drains, many of which are in a very worn and offensive condition. This, I consider, to be one of the great objections and difficulties in the neighbourhood. The night soil is collected into carts and discharged into a drain for outflow into the harbour, causing a great public nuisance. The domestic sanitation of the district is on the latrine and basket system. Many of the latrines are ill-ventilated and in a very unsanitary condition. The Health Department of the Municipality do all they can to remedy the evil, but owing to the apathy of the landlords, and the unwillingness of Magistrates to inflict adequate fines, the nuisance in a great measure remains unabated. There was one case in which a landlord was had up before a Magistrate for the third time on account of an overflow of a most objectionable character from latrines. He was only fined two rupees. He can go on paying two rupees as long as ever he likes. It is cheaper to pay fines than to make alterations.

17,862. Did you experience any difficulty at any period of the operations in ascertaining the number of plague cases?—We have always found more or less difficulty, particularly under the Plague Committee; they were secreted very considerably under the Plague Committee.

17,863. To what do you attribute this difficulty?—To the harshness of the measures carried out. In a city like Bombay, with a large population, the more rigorously you try to enforce your restrictive measures, the less, I think, you will succeed.

17,864. Can you give us any definite instance in which the effect of these measures was to cause people to disappear?—Yes. On the Port Trust Reclamation on the Reay Road people were hidden in stacks of wood. One Sunday morning we found four people hidden in that way. Stacks had been built up, and big balks of timber and trunks of trees had been moved, and the people covered in so that we should not find them. I may cite one instance on the Telang property. There was a large block of chawls there. I asked whether I might be allowed to vacate it in my own way. The Plague Committee said no, a regular authority had been provided, and it must be done by them. I said, "Very well." The result was that the soldiers and sailors arrived, the building was surrounded, and when we went inside, the whole of the inhabitants, numbering about 300, had entirely disappeared, except two men who were suffering from plague right up at the top of the house. One died before he could be removed, and the other died shortly afterwards.

17,865. Is the objection of the people to the removal of the sick, or to the segregation of the healthy?—It is going to the hospital. They do not like it. It was in consequence of that, that in conjunction with some other gentlemen who kindly assisted me, we started a hospital for this Telang property in which we treated patients. Profiting by the experience gained in the previous year in the attempt at segregation in the Telang property, which consists of six large blocks, a local hospital was established for the use of the inmates. Thirty-seven patients in all were treated in the six weeks it was necessary to keep it open. I consider at the present time, with the building ventilated so much, that there is no indigenous plague in the chawl. We have had some few cases from time to time, but they have been people who have contracted the disease outside. They have probably contracted the disease while at work. They have come home and have been so sick that they have not been able to go to their work any more, and have died in the place. The reason why I do not think there is any plague in the chawl is because the women do not get it, it is almost exclusively confined to the men who are employed in work. As I have already said, 37 patients in all were treated in the six weeks it was necessary to keep this local hospital open. Of 22 cases of relapsing fever and other diseases, one sufferer died, the others all recovered. Those who, from poverty and the consequent necessity of continuing their daily labour till the last possible moment, were not discovered till it was too late for treatment to be of any avail, died. Large numbers of houses and buildings in the district have been reported to the Municipal Executive as insanitary, and many of these have been condemned and destroyed.

17,866. Do you think that you now get the majority of cases reported to you?—We get more than we did, but at the same time there are a good many cases everywhere being secreted.

17,867. Is it your experience that the majority of cases are reported to you when the people are still sick, or

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after they are dead?—We get a great many sick cases reported to us, I think I may say that the majority of cases are reported to us before they are dead.

17,868. That is an improvement?—Yes, with regard to my own district, I certainly think there is a decided improvement.

17,869. Have you, in your district, resorted to the practice of treating sick persons in their own houses?—Not many in my own immediate district. It was done, however, by Mr. Firth in the "E." ward on the other side of the Parel Road. We have treated two or three cases in that way, but not to the extent that Mr. Firth has done.

17,870. Have you had an opportunity of observing its effect in Mr. Firth's district?—In not one of the cases has the disease spread, nor has it spread in my own district. At the present time I have one such case in the Telang property, and the people immediately in contact with the case have not been infected at all.

17,871. Have you disinfected the house in which you have allowed this patient to reside?—We disinfected each floor before the recrudescence began. We have opened out holes through the floors from top to bottom.

17,872. Did you take any other measures?—We inoculated all the people in the same room with this man, and we cautioned the other people not to go into the other room. We told them that if they did go there we should be obliged to send them to camp, and as far as we can judge I do not think the people have gone there. We have, from time to time, gone there unexpectedly, but we have never found more than the two attendants with him.

17,873. Have you disinfected the house since the case occurred?—No; we shall do that afterwards. We use disinfectants, however, in the room at the present time.

17,874. In a case like that, when you allow a sick person to remain in the house, would it be possible to disinfect the remainder of the house?—Certainly.

17,875. But you have not done so?—No, because I do not think it is necessary. The plague is not in the house. The man did not contract the plague there; he got it when he was at his work at Mandvi.

17,876. The only room in this particular house which you regard as infected is the room in which the patient is residing?—Yes. We shall disinfect that room, and the other room next to it. If we have another case we shall disinfect the whole floor. When, however, there is only one single case we merely disinfect the room itself, and the rooms immediately in contact.

17,877. I believe you have had some experience with regard to inoculation?—I have.

17,878. Would you give us the facts that have come under your notice?—Inoculation has been encouraged in every way. Contacts are given the option of being inoculated or going to camp, and opportunity is taken of a clear or doubtful case of plague occurring in a building, to persuade as many inmates as possible to submit themselves to the prophylactic. Of the total number of persons inoculated by the medical officers in the district, and at their dispensaries, up to date—close on 7,000, whose names and addresses are known and noted in the office books—one death only from plague has occurred, and one of complication—in the Telang property—high fever, vomiting and purging, which was successfully treated in the local hospital. In one chawl in Victoria Road 103 persons were inoculated. Since I sent in my report 48 more have been inoculated. In Love Lane, 25 were inoculated, and since my report 43 more. In Chudda Chawl, 167 were inoculated, and on Sunday morning we inoculated 75 more. A case occurred, and the people came forward voluntarily to be inoculated. In Hyderali's Chawl, 86 were inoculated, and 14 have been inoculated since. In Contractor's Chawl, 50 were inoculated. In Jarawalla Chawl, 100 were inoculated. A case occurred there, and this morning 24 people came forward to be inoculated. The room in which the case occurred is now being disinfected, the patient has been sent to hospital. In the Telang property, at the time of sending in my report, 300 were inoculated, but more have been done since. The figures I have just given for the chawls make a grand total of 1035. With regard to this I may mention a very sad fact. Dr. Pithari has since died of plague. He was a gentleman who for many months quite gratuitously devoted his time to the discovery of plague cases. He performed a very large number of inoculations, but unfortunately he did not inoculate himself. He got the pneumonic form of plague. He was taken ill on Sunday, and on Wednesday he was dead, poor fellow. It is very sad.

17,879. You have had, I suppose, a favourable opportunity of observing what has happened to these people who have been inoculated?—I have.

17,880. Have you noticed any case of plague amongst them?—Only one. That was the case of a man who died on the 26th of last month. He was inoculated in September last. That is the only person who has come under my notice as having died.

17,881. Have any cases come to your notice in which uninoculated people residing in the same house with inoculated persons had plague, while the inoculated persons have escaped?—Yes, there was a case this morning.

17,882. Will you state it?—It happened in Jarawalla Chawl. A woman got plague, but the other people who were inoculated did not get it.

17,883. Could you give us the number?—It was only a question of three or four in that one room.

17,884. They have escaped so far?—They have.

17,885. Were you able to ascertain how the woman got plague?—No.

17,886. During the course of the operations you have engaged in, have you resorted to corpse inspection?—No, and I should not approve of it. It would be so objectionable to all the people, particularly Muhammadans, that I should not think it would be advisable to press it. I think the present mode of finding out deaths is all that is necessary.

17,887. From your experience, do you think that corpse inspection would assist materially in finding out plague cases?—I do not think it would. If I might express an opinion, I think that dealing with plague, in a large city like Bombay, is like ploughing the sand. One can, after all, only do one's very best, and trust to Providence for the result. That is my experience. It is possible that more actual plague cases would be identified, but I think the effect upon the people would be very objectionable. It would have a very disturbing effect. The quieter you keep the people the better.

17,888. Have you formed any opinion as to the relative malignity of the three outbreaks in Bombay?—The present outbreak is in many cases of a most virulent type. I have known two or three cases in which the people have come home from their work in the evening and have been dead by the morning. There is a great deal of pneumonic type; but I ascribe a great deal of it to the excessively cold weather we have had, and its long continuance, and the different conditions under which many people live. In my own immediate district, where I will not allow obstructions to be made to the free ventilation of air through the rooms in which the people sleep, I insist upon the place being kept open. I think that has something to do with the virulence of the attack, as far as the pneumonic form is concerned. I am not a medical man, and I speak with diffidence upon the subject; but I do think that the excessively cold weather we have had, and the different conditions under which the people live, have led them to get this pneumonic type of plague.

17,889. Have you noticed any instance in which people in the same house have suffered, one from bubonic and another from pneumonic plague?—Yes. In the Telang property we had cases of both. In Dr. Pithari's case he had both; he had the pneumonic form, and buboes as well.

17,890. Have you, in the Telang property, observed any instance in which a person has apparently caught pneumonic plague plague from a case of bubonic plague, or *vice versâ*?—That I could not say.

17,891. Can you give us any facts which will show the connection of rats with the diffusion of plague?—No, I do not know that I can, except that in Hyderali's Chawl a number of rats died, and we took the precaution of getting the people to take all their belongings out into the compound, or on to the verandahs and other places while we disinfected every room. I consider in that way we prevented a large recrudescence of the plague. We have had one or two cases, but very few; otherwise one might have expected a great many, for there is no doubt that rats do convey the contagion, because where there is contagion dead rats are frequently found. Where we have had a case of death from plague, and

we turned out the people's belongings, their old rags, and pots and kettles, of which they accumulate a very large number, we have frequently found sick and dead rats.

17,892. (*Mr. Cumine.*) Do you know of any case where a certain number of people have been inoculated, and a certain number uninoculated, and an inoculated person has been attacked and the uninoculated persons have escaped?—In the Khudda Chawl a man died last month who had been inoculated, but the other people who had not been inoculated also got the plague.

17,893. Then you do not know of a case?—No.

17,894. When you disinfect a room, do you disinfect it only once, or do you keep on doing it for a week?—In a fatal or a very bad case of plague, we disinfect the room for seven consecutive days.

17,895. That is what Mr. Firth does?—Yes. Those are the orders, which we carry out.

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17,896. (*The President.*) What is the present method of the notification of the causes of death?—We get notices from the police; they give notice to our office.

17,897. How do they obtain the information?—It is information received.

17,898. It does not come through any medical source?—It does sometimes, but not always. If a medical man has a case of plague he notifies it to us or the district in which it occurs.

17,899. In the majority of cases the information comes direct from the police?—We discover a great many cases ourselves; on the other had, we get information of a good many cases from the police as well; sometimes more from one source, and sometimes more from another.

(Witness withdrew.)

Mr. N. N. KATRAK called and examined.

Mr. N. N. Katrak.

17,900. (*The President.*) You are Licentiate of Medicine and Surgery of the Bombay University and a member of the Municipal Corporation?—Yes.

17,901. You have watched the progress of plague in Bombay for some time?—Yes.

17,902. Since the first epidemic?—Yes.

17,903. And you have also watched the effects of treatment and prevention?—Yes.

17,904. You have observed the influence of the prophylactic fluid on the spread of plague, have you not?—Yes.

17,905. Have you any statistics to give us upon that subject?—I have statistics so far as the cases I have inoculated are concerned, but my conclusions have been formed more from the statistics which are laid before the public and it is that which has led me to come to the conclusion that inoculation is a good prophylactic and preventive against plague.

17,906. The conjoint evidence?—Yes.

17,907. How far does your own evidence lead you to that conclusion?—I have inoculated about 500 people, and all these cases have escaped plague except one, who developed plague symptoms within one hour after the inoculation.

17,908. Have you seen any bad effects following inoculation?—None in the 500 which I have done, and I have carefully inquired into the whole matter. So far as my inquiries go I do not think there is much truth in the complaints made from time to time. I should like to say that when we in Bombay read about the evidence of Dr. Lawrie and Dr. Johnston that the plague prophylactic was a putrescent fluid, as I had taken the great interest in the matter, and as I had found no unpleasant effects in any of my cases, I thought there must be some misunderstanding on the point. With that view I opened a certain number of bottles and re-sealed them with a view of seeing if exposure to the atmosphere had an influence in making the liquid putrescent.

17,909. How long did you leave them unclosed?—About 20 minutes. I could give the results of the examinations made by Prof. Haffkine, Dr. Milne, and Dr. Rau.

17,910. Those bottles which you refer to were examined by bacteriologists?—Yes. The results were that two whole bottles which were never opened were found to be sterile. In one bottle which I have mentioned the seal was not intact, and I had some doubt about its purity, and therefore I did not use it for a long time; it was kept in a corner. When Dr. Lawrie's evidence came before us I forwarded that bottle to Dr. Rau, and even that suspicious bottle was found to be sterile. Of the bottles which were opened by me and re-sealed, only two were found to be contaminated, but in one of these the sealing or the re-sealing was unsatisfactory to me, because the cork re-bounded and the seal was again broken. I made a note in that connection even before the bottles were sent to Dr. Rau, that the sealing was not satisfactory. With regard to the other contaminated bottle described by Dr. Milne, I did not take notes, because I had no idea of bringing the matter before the Commission. I have a dozen bottles which have been opened and re-sealed which the Commission can see if they wish.

17,911. Have you examined their contents?—No. They have simply been opened and re-sealed with the dates, numbers and so on marked on them. I have brought them for the use of the Commission.

17,912. Have you seen any good effects outside of the influence of the prophylactic upon plague?—Yes, I have particulars of some cases. The first case was that of a Parsee boy aged 15. He was inoculated during the second epidemic, and a few days afterwards he got plague. He was inoculated with a third of the dose because he wanted to go through three inoculations. He recovered of the plague and since then he felt a sense of chilliness for nearly 6 months or perhaps more. Then he got one inoculation. After that inoculation the sense of chilliness disappeared. After his first inoculation he got plague; he then passed that epidemic and the intervening portion, and during the last recrudescence he got himself inoculated again. Since then that sense of chilliness has disappeared.

17,913. What do you infer from that?—This is one instance in which it has done some good.

17,914. Are you quite sure that the first inoculation did not produce the sense of chilliness which you refer to?—After the first inoculation he went through the whole course of the plague, and a very severe one too.

17,915. That first inoculation might have been the cause of the chilliness?—It is for you to draw conclusions: these are the facts.

17,916. Have you any other facts?—Yes. There was a lady aged about 46 who had a sort of chronic fever lasting for many years This chilliness she got rid of after the first inoculation which was done on the 8th October 1898. Then I have another case of a young man aged 30 who had severe lumbago and a severe pain in the sacral joint. He was treated with blisters and applications, and was relieved temporarily from time to time. He was inoculated during the second epidemic and since that time he is free from the pain. This year he came to me to be inoculated, not as a preventive against plague, but as a preventive against the disease, which he believed would start in this cold season. He got inoculated on the 28th October, and he is still free from pain.

17,917. You have further cases of a similar nature?—Yes.

17,918. You have paid a good deal of attention, as a member of the Corporation, to the measures to be adopted during epidemics of plague?—Yes.

17,919. To what extent do you think insanitary conditions are accountable for the virulence or extension of plague?—My idea about the insanitary condition is that when a person gets the infection he succumbs to it more easily if he is exposed to insanitary conditions. If the sanitary conditions round about him are of a healthy nature, then the percentage of succumbing would be less and perhaps the virulence would be less also. I base my opinion first of all on the observations that I have made and which anybody can make, and also on some experiments made by Alessi which I have quoted in my précis.

17,920. Will you give us the quotation which you refer to?—He took a certain number of animals and put them in a cage and had the cage connected with the drain pipe. Other control animals were placed in

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ordinary conditions. After exposing them a certain time to the influence of the sewer gas the animals were inoculated with pathogenic germs as well as the control animals, with the result that a greater number of those who were exposed to the influence of the sewer gas succumbed. In fact, they first of all got the disease and succumbed in a large number, whereas those other control animals who were not under the influence of sewer gas succumbed in a smaller number, and also had a lower mortality.

17,921. You would include the removal of insanitary conditions among the measures which are the most important?—Yes, of the greatest importance.

17,922. Have you any experience with regard to disinfection in connection with plague?—I have, in my capacity as an official visitor.

17,923. What is the result of your experience?—My idea is that disinfection is possible, and also very efficacious.

17,924. You think it should be included among the measures?—Certainly.

17,925. With regard to the propagation of the disease, what do you find is the influence of rats?—Men as well as rats propagate the disease, but man is a much less important factor in that respect than rats. My observations are that a man cannot communicate the disease to the healthy if he comes in contact with them unless his discharges touch the mucous membrane or a cracked skin. But he can infect locality. How he does it, of course, I am not in a position to say, but my idea is that a plague-infected person, if he goes to a healthy place, infects that locality, and in some way or another, which I am not in a position to state, the disease is communicated to the rats. It may be that the discharge of this infected person soils the ground, or soils some eatables which the rats consume and they thereby get infected. However, this is a point upon which I can give no information. Having done this, the germs seem to get some increase in virulence, and the rodents, by their discharges, scatter the germs of plague broadcast. Practically man as a propagator to another man is a less important factor than rats. Another reason which I give in support of this evidence is that in hospitals where we find that a large number of patients suffering from this disease are congregated, where their discharges are removed from one place to another properly, the inmates, the surgeons, and even the people kept under observation are not affected, and if they are affected I have not been able to see whether the infection was got from the hospital or from their own houses. It is possible the hospital building might become infected just as much as any other house, and I believe a similar thing must have occurred in one of the hospitals of Bombay; I think it was the Mahratta Hospital—where a number of people got the plague. But, however, this is a surmise, and the full details of these incidents are not before me. But I say that a Hospital is as liable to infection as any other house; but the large number of disinfectants used, the intelligent care with which the discharges are handled and the great care taken to see that nothing eatable is thrown into the wards and places where the people live, are good safeguards against the infection of the dwelling itself.

17,926. Have you any instances in which it is clear that rats have actually infected a human being, and where you can exclude human conveyance distinctly?—Yes, if we can rely upon the history given by patients. I have two cases in which the history was something to this effect. A rat was found in one of the rooms. The patient, before she got the infection, took the rat and threw it out, and then with plain water she scrubbed the ground with her hands. In about 6 or 8 hours she felt a sense of chilliness, then fever began, and she went through the whole course and died.

17,927. How do you exclude contact with a plague patient?—There was no plague case at the time.

17,928. Was that in this city?—Yes, in Bombay.

17,929. Was there any plague in Bombay at that time?—Yes.

17,930. But there was no plague in the same house?—That is so.

17,931. What about the houses in the neighbourhood?—The neighbourhood was infected.

17,932. Have you any other case?—A Hamal is reported to have removed two or three dead rats from the house. After that he got a sense of chilliness and went through the whole course. The information was that he had a sense of chilliness within five minutes but, as I say, this case was only reported to me; it is second-hand evidence, and I could not give it with the same confidence as I gave the last.

17,933. What is your view with regard to segregating the sick?—I find that the segregation of the sick is not of so great importance as the segregation of the healthy, because the sick person himself is not likely to communicate the disease to the healthy. He may remain in the house if the house is well ventilated or in a sanitary condition, that is such a house where not only plague patients but any patients may stand a chance of recovery. He has to be removed from the house not because he himself is a dangerous person, but because he remains in the house and therefore other healthy people ought to remain in the house. But since the discovery of Prof. Haffkine's prophylactic I am strongly of opinion that in a large majority of cases plague patients may safely be allowed to remain in the houses and be treated by inoculated persons. Thereby we could reduce the mortality of the plague, because in spite of three years' bitter experience the people still have a dread of hospital, though it is unfounded, and the effect of the nerves or mental condition upon the course of the disease may easily be judged by those of us who are medical men. So if it is decided to treat the patient in sanitary houses with the condition that those who treat the patient must be inoculated, I think the percentage of plague mortality would be reduced, I will not say considerably, but to some extent.

17,934. Have you treated a large number of cases yourself?—No. During the first epidemic I treated about 17 cases, but since then all the plague cases are hospital cases and the medical practitioners do not get opportunities of treating them. We treat them in the beginning until they are declared to be plague, and then they are sent into the hospital. In the first epidemic I treated 17 cases of whom 12 recovered and five died.

17,935. In the first epidemic did you encounter many cases of the pneumonic form of disease?—No. Before the death of Dr. Manser I do not think the pneumonic form was recognised.

17,936. It might have occurred?—Yes. I have a recollection that one case which recovered under me had pneumonia as well as a bubo, but at that time pneumonic cases were not thought of.

(Witness withdrew.)

Mr. J. McDonald.

Mr. James McDonald called and examined.

17,937. (*The President.*) You are Headmaster of the Scottish High Schools, Byculla?—Yes, the Bombay Scottish Education Society's Schools. I should like to say I am in no way a professional medical man, but I shall be very glad to answer any questions.

17,938. (*Mr. Cumine.*) You are a non-official?—Yes.

17,939. You are not put forward by Government as a witness?—I am not. The Surgeon-General wrote to me, that is all.

17,940. How many years have you lived in Byculla?—Upwards of 32 years.

17,941. When the plague broke out did you take charge of the district?—I took charge of a section. The section is so named, but it runs from the Byculla Bridge to Kala Chowki.

17,942. How long have you been in charge of that section?—Since the beginning of plague till the present time.

17,943. Have you found in your section any overlapping of different departments?—I found great difficulty from the overlapping of the Health Department and the Engineering. Later on, Plague Officials increased the

difficulty, for the three departments were not unfrequently found to be engaged in the same inquiries. The Police sometimes made a fourth difficulty, when getting statistics on their own account.

17,944. What are the principal classes of people in your section?—Mostly a low class, either Mochis (shoemakers), Mahars, or workers in the mills.

17,945. What was it which specially enabled you to get a hold on the good-will of the majority of the people?—Firstly, they knew that I lived in the neighbourhood, and had some little position there. Then I was obliged to use a little money to help the poor—not the plague-stricken, but the poor—because there was much destitution in the place. Then I got some funds from Dr. Weir, and also 100 rupees from Sir James Campbell's Plague Committee.

17,946. When you got this money did you experience any difficulty in getting timely information of plague cases?—No; I should say that when I was informed about plague cases I went quietly and sympathetically to the people, and gave them advice quietly. Then, of course, I assisted them in any way I could, sometimes writing five, six, and seven letters in the morning to employers of labour to get employment for those who wanted it.

17,947. Do you think from this way of treating people that the number of cases in your section was probably less or greater than in other parts of the town?—I think considerably less, because they came with timely notice, and, when necessary, I helped them to remove the patients quietly, and they had no fear that there would be any rough treatment.

17,948. In spite of your conciliatory treatment there was a certain number of cases of concealment, I think?—There was, and is; there has been all along.

17,949. Would you attribute the greater part of these cases of concealment to intention or ignorance?—To ignorance, and the hope that it might not be plague, and so removal would be saved.

17,950. When a person had been removed to hospital, did you find it had a good effect to go to the hospital yourself afterwards and see him?—An exceedingly good effect. I know that those who recovered consider that their recovery was due to my having spoken for them to the doctors and nurses, rather than to the great care both doctors and nurses took of them. They believe that, although I had very little to do with it, and told them so.

17,951. Did you find that the nature of the carriage in which the people were removed to hospital had anything to do with their unwillingness to go?—Yes, often.

17,952. What would they have preferred, do you think?—I think they would have preferred a hand-carriage, that is to say, a species of dooli. They do not like to be wheeled through the town in a lying position; they would prefer a light hand-dooli with some covering over it.

17,953. Did you find that the system of having house-to-house visitation by native gentlemen was a permanent success up to the very end?—No, I did not.

17,954. What was your experience?—My experience was that at the beginning the attendance of native gentlemen was pretty fair, but I found as we got a little further on that cases cropped up in which these gentlemen allowed their feelings to get the better of their reason. They did not wish to hurt the feelings of their caste men especially, and they complained that a patient was sent to hospital under the name of some other person. The great object was to get the name of the sender put down as having sent the man to the hospital.

17,955. Did it come under your observation that the plague took any particular line in going across the town?—Yes; it was brought to my knowledge at a very early period. I have for years been observant of sickness in the town, firstly, from a conversation I had many years ago with Mr. Tomlinson, the water engineer here, who assured me positively that if care was not taken, and if the town was not drained properly of the great quantity of water which was being let in and deposited in the soil, and if waste of that water was not prevented, some serious evil would follow. Talking this matter over with Colonel Weir, he called my attention to the line from Mandvi to the Market, and from the Market on through the wards, going gradually northwards. Noticing it was actually taking the line which Dr. Weir had pointed out to me, I said to the people of my section—they are on one side of the railway—"It is coming along that line, and without doubt, "if you are not careful to prevent people from the "other side coming round here, they will come round "here, because they know we are quiet here, and you "will have the plague at such and such a time." And they did have it.

17,956. What is the connection between that line and the subsoil?—The flow of the subsoil water is north-west, in the direction of Worli, and then it turns round to the east and north-east, and in this direction plague goes gradually round. It has done it for these three years. It is pretty heavy to the west of the section, and now is coming right into this section.

17,957. I suppose you got some credit from the people for that?—Yes, especially because I did not tell them whence my information was. Had I told them, they would not have believed it. I am supposed to be a great doctor, although I do not profess to know anything about it. They speak of me as the Bara-Doctor-Sahib.

17,958. From what you have seen of disinfection you think, I understand, that one very important point is the goodness of the material supplied?—I think so, if it is to be of any use. I am not a great believer in disinfection.

17,959. You have something to say with regard to quicklime, I think?—Yes. In some cases they used quicklime, and the lime was certainly anything but quick in many cases, because I tested it when it was brought to disinfect the houses, and complained about it once or twice.

17,960. Do you also attach great importance to keeping the same plague officers in a ward throughout as far as possible?—Yes, very much so. I know that it could not be helped, but the change of plague officers certainly tended to disarrange many things. I had the good fortune to be always in the same section. As I was practically independent, they let me alone, and did not interfere with me in any way.

17,961. Was one of your chief reasons for thinking that European officers should be kept in one ward as long as possible, that then the superior officers know their subordinates, and can prevent them from doing any acts of oppression or extortion?—Yes. I thought in the case of Mr. Stewart, for example, that he knew the men intimately, and was able, when a complaint was made, to use his discretion to more advantage.

17,962. Have you any reason for supposing that the barbers are rather dangerous people in plague?—Yes. I am afraid that they still use razors to cut the buboes, especially about the neck. I have seen them in the neck, I have seen them in the chest, and I have seen them in the groin. I have mentioned in my précis one case which came under my notice at a very early date, but, strange to say, I have had several accounts of cases since which have taken place. I make it a point now, whenever I meet a barber, to caution him not to interfere with any buboes. Many of these barbers are of the species of the old leech, and profess to cure in this way, but I think it is dangerous.

17,963. Have you anything to say with regard to inoculation?—I was inoculated myself, together with my wife, son, daughter, and 14 servants. We were, I think, among the first 50 cases which Prof. Haffkine inoculated. I was a believer in inoculation, I am a believer in it. Two or three things have come under my own observation which perhaps are worth knowing. I have a son who is a medical man in the Army. He was up in the front and was invalided for three months; he took three months sick leave owing to his having very severe fever, and came down here, but could in no way get rid of it. At the end of three months he was obliged to return to duty, and was sent up to Baroda to the 8th Regiment, where plague had broken out. My wife advised him to be inoculated, and he had a desire to be inoculated, but thought that it might increase his fever. However, when he went up there, before he could advise others to be inoculated, he preferred to be inoculated himself, and from that date to this he has had no return whatever of fever. The same happened to his servant. I have also a young man in my house to whom the same thing happened, and there is a case which I know of, of a native who was of a very poor habit indeed, and had asthma for several years. He thought he was dying, but he was

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inoculated, and from that time to this he has recovered so much, that he is now thoroughly stout and has had no return of the asthma till within the last two or three days. The gentleman, whose clerk he was, told him that he wished the other clerks to be inoculated, and asked him about the facts. The clerk told him, "You "see now I am a strong man, but within the last three "days I have had something like a return of asthma, "and I am going to be inoculated again." I think it is not an uncommon idea amongst the people that inoculation either affects them very badly, or that they are likely to recover from fevers if they are inoculated. I think there is something with regard to the effect on fever from what has come under my own experience.

17,964. (*The President.*) That is malarial fever, I suppose?—I presume so.

17,965. (*Mr. Cumine.*) How long ago was the clerk inoculated?—Twelve months ago.

17,966. All this is within your own knowledge?—Yes, he is a clerk of Mr. Vilhaldas Damodhar.

17,967. (*Mr. Hewett.*) Have you any system of corpse inspection?—Oh, yes, they allowed me to do it very freely. I do not insist upon it in any way, but they ask me to do it. I ask them if there are any buboes, and they invariably say, "Come and look," and if I am doubtful about it I do go. I have no difficulty about it, but I think they will object in most cases, and especially would they object to young men examining.

17,968. Do you think that a Hindu would like to have his body examined by a Muhammadan or *vice versâ*?—No, I do not think so. My experience is that they are quite willing that a European should do it, but they do not like dark persons to examine them if they are of another faith.

17,969. Do you think that Muhammadans would resent the examination of female bodies?—I think they might, but they have not.

17,970. With you it is a different thing?—I have no difficulty, but I think they might resent it. The fact is they will resent it in the case of young doctors—they resent the examination of women by them when they are well. I have known of such cases, but I have not found any objection personally. I make myself very familiar with the people. I do not know whether you have seen the printed Marhatti letter of mine to the people. That sort of thing I usually read to them, or get somebody to read to them. They gladly listen to a thing like that. They also prefer that one should hear their story, even if they do not get what they want. I think it is most important that they should be heard, even if one does not agree to give them what they want.

(Witness withdrew.)

Mr. A. B. Davur.

Mr. A. B. Davur called and examined.

17,971. (*Mr. Cumine.*) Are you stone deaf?—Yes.

17,972. Have you any medical qualifications?—I have practised medical electricity for three or four years.

17,973. Have you any degree?—No.

17,974. You have volunteered as a witness, I believe?—Yes.

17,975. Have you treated any plague cases?—No, on account of the strict plague measures.

17,976. You mention in your paper a certain treatment for plague patients; has that treatment ever been applied in practice?—No.

17,977. Have you any personal experience of the results of inoculation?—I have acquired personal experience by way of observation, asking patients, and so on.

17,978. How many cases have you observed?—I was in Arthur Road Hospital one day and examined some patients there.

17,979. You mention in your paper, in speaking of the infective power of rats, that there is a worm of a certain kind in their interior which helps to spread the germ; do you know as a fact that this worm exists?—Only from natural history.

17,980. As you are deaf, perhaps you would like your paper to go in in its entirety as your evidence?—Yes.

"The following causes may be attributed to the origin of the different outbreaks of the plague:—

"*Firstly.*—In a hot country like India, when the summer is very severe and intolerable for two or three years, the soils bring forth very noxious and unwholesome exhalations of a fermentation (*i.e.*, bacterial) nature, just after the abrupt and continuous occurrence of heavy showers of rain for a considerable time. This is a natural cause or a phenomenon.

"*Secondly.*—Again, in a hot country like India, the wholesale merchants, brokers, and dealers in corn and grain, during a year of plenty when the crop is good, make large purchases of different sorts of grains with a view to make a good bargain in future when there is a famine or scarcity of grains. Now, if such large quantities of grain be not sold away in due time on account of the want of good demand they get damaged, and hence become unwholesome and unfit as an article of food. Grains, when hoarded up and kept for a considerable time without being used, produce a very noxious exhalation, a very bad stench which really affects the whole atmosphere of those localities where they are kept; and hence a great mortality of rats during those seasons.

"*Thirdly.*—A very imprudent and erroneous step was taken by the Bombay Municipality in respect with the drainage system, in the year 1893 or 1894, two or three years before the plague first broke out in Bombay, which has ever since produced a very bad effect upon the health of this city; and it is (*a*) the annexation of private or proprietary privies or closets with public gutters; and the other (*b*) is the erection of too many public urinals, which is no better than a mere nuisance. These can be regarded as creative causes.

"I should, therefore, in order to prevent the contagion of the epidemic from any further propagation, like to suggest a word or two about large and small quantities of corn and grain which are kept in stock, that a thorough inspection of all the godowns, store-rooms, and other places where they are kept should be made for at least once or twice a month, and that all damaged and noxious grains should be caused to be destroyed by fire: and the sacks, bags, and other containers ought to be either burned or thoroughly disinfected by boiling them in hot water.

"*About Disinfection.*—The present method of cleansing the houses where dead rats are found, as well as cases of the plague have occurred, with water, phenyle, and other disinfectants, is not good at all in this present situation. They ought to be either destroyed by fire, if possible, or cleansed with very hot sand or ashes, or, after removing some movables (which should be thoroughly disinfected and exposed in the sun for a few days) petroleum oil should be poured upon the ground and walls, and, afterwards, spreading hay upon the ground, they should be burned, provided that proper precautions are taken against fire already. But care should be taken that no water should be used at that time. Of course, the houses should be whitewashed afterwards.

"*About the Manner in which the Disease is communicated.*—The contagion of the plague is communicated through the skin. The plague microbes first make their abode in the corners of the eyes and nostrils, as well as in the middle ears, and from thence they get into the elementary canal through the eustachian tube and the jugular nerves. And afterwards they get into the regions of the chest and the stomach. Sometimes they easily enter the skin through scratches made into the nostrils with points of scissors while clipping the hair therein, or upon the membrana tympani and meatus auditorius internus while pricking the ears. Sometimes they (*i.e.*, the microbes) enter the eyes of other persons (*i.e.*, sound) or of those who attend upon the patients, before whom they (*i.e.*, the patients) either sneeze, cough, or upon whose faces they sometimes spit in a fit of temper. Or they enter through the cuts on heels of those poorer classes of people who go barefooted, who may happen to pass across some place upon some footpaths where some patients might have vomited or made water beforehand. The disease may be communicated by coming in close contact with the bodies of the patients or by putting on their clothes

through mistake. It may also be communicated by way of cohabitation, provided that neither of the parties is professionally a bad character. It can also be communicated by means of hair and shaving brushes and the aprons of barbers, or through some commercial articles, public conveyances, or through the dirty dresses of hawkers in household requisites.

"*About the Effects of Preventive Inoculation.*—As to the effects of preventive inoculation, it can be highly recommended, but not without caution. For there are some objections for it which ought to be very carefully remembered.

"The following can be regarded as some of them:—

1*st.*—The strength, manner (of injecting it into the proper parts of the body), and the number of doses of the serum.

2*nd.*—The general health and the physical condition of the persons to be inoculated.

3*rd.*—The climate and the general physical condition of the cities and villages in which they live.

4*th.*—The rules and sentiments of the people as well as their manner and mode of living.

"The following are some of the general observations upon effects of the serum. It causes fever and pain; the former leaves the persons of itself after a few hours and the latter may be tolerated to a great extent by a purgative if taken a day previous to inoculation. Buboes sometimes appear, generally in the left arm-pit, and very seldom in a groin (right).

"Its effects upon some persons (*i.e.*, those who live in villages as well as in large and comfortable bungalows or houses, with sufficient air and light, and on good food and pure water) are somewhat lasting for about two or a little more than two years; whereas in some persons (*i.e.*, those who live with a number of families in houses, situate in a very thickly-populated city and upon very poor food and bad water) are more temporary.

"The effects of inoculation upon such persons as the following are very good and salutary:—Persons suffering from, or labouring under, some mental disorder, nervous debility, nervous prostration, rheumatism, neuralgia, paralysis, gout, tumours and other morbid growths, hysteria, disease and disorder of the spina cord, scurvy, leprosy, scrofula and other chronic diseases of an infectious nature.

"But the effects of inoculation upon some persons as the following are generally bad, and sometimes they prove even to be fatal in case when such persons are attacked by their old diseases:—Persons suffering from, or labouring under, any disease or disorder of the kidneys, liver, bladder, stomach, lungs, the heart and other viscera and such other diseases which are easily subject to bronchial and catarrhal conditions.

"The following is a list of such persons who are generally free from the epidemic. Nevertheless they are subject to catching the contagion of the disease if in case they neglect to take due notice of any cuts, scratches, bleeding from the nose or gums, or from the fingers when the nails are badly pailed:

"Persons, whose temperature is at 99°, keeping thin yet always enjoying good health, having hot, much thick and red blood with smooth, bright and dry skin rather somewhat wrinkled; teetotalers not easily subject to catching cold, fever and so on; chaste, religious, and of refined habits not given to pleasures; not practising polygamy, of studious, sedentary and laborious occupations; dealers in oils, colours, lims; grogshop-keepers and their employees; mill hands and employees in gasworks, druggists and vendors of medicines, perfumers, tobacconists, mechanics, blacksmiths, and those who work before fire.

"Besides the above-mentioned, there are others who are entirely free from the epidemic:—these are the prostitutes, their paramours and visitors, professional bad characters, heavy drinkers, opium smokers and eaters, and the bhâng and gânjá smokers.

"There is one thing which is very remarkable and I am sure that it will indeed draw the attention of "Experts," which is this, that amongst all the population of Bombay, nay, even throughout all India, which represents a lot of peoples of various nationalities, castes, and creeds, there is not a single nation or a tribe like those of the Chinese and Japanese which is exempt from the terrible dread of the plague. For there is not a single case of the plague known as registered to have been occurred in either of these communities. Nevertheless, after a close observation and minute inquiry, it is not a difficult task at all to assign the cause to any particular thing or things. The causes, as far as I know, are these:—(*a.*) The peculiar formation and texture of their blood; (*b*) their snug and cleanly habits (though whatever may be said of their lives in their native places, but here they live a quite different life); (*c*) their habits of taking hot and weak tea now and then (for they never take cold water in its simple form in their life except fresh rain water); (*d*) they never take English, Native, or any other foreign medicines except those of their own countries; (*e*) and lastly, the thinness of their population in this country.

"*Difference between a False and a Genuine Case of the Plague.*—Seeing that much confusion and many blunders are made in finding out the genuineness of the plague cases on railway stations, detention and segregation camps, I should deem it to be proper to throw some light upon this piece of inquiry.

"The first and the most obvious symptoms of the disease are fever and buboes. The presence of fever and buboes is found in a false case of the plague as well as in a genuine one. But the fever is generally either of a remittent or of a simple nature; the latter is very common and can be easily caused by fatigue, hunger, hard work, want of good food and water or recreation or on account of some slight change in the weather or by fear or anxiety; and therefore it leaves the sufferers very soon and that without any remedy.

"As to the buboes in males, they appear on account of taking a long journey on foot, or fast or excessively running, or by climbing hills or precipitous places, or while wrestling in their athletic pursuit, or it may be a pure venereal case, or through some change in the weather, or on account of gonorrhœa or any other disease of the urinary organs which they may be suffering from.

"In females they (*i.e.*, the buboes) are sometimes of the venereal kind; or on account of their being very much taken up with their household works, or owing to their slender build or weak constitution.

"They are sometimes found on women labouring under pregnancy either a little before or after delivery. This is owing to their miscarriage or extreme weakness or debility or through the damp or moist atmosphere of their houses.

"As to the buboes which are found on children, they easily appear on account of their childish mischief, while playing at the hide-and-seek game or climbing some trees, jumping or by playing at marbles when if some nerves be straitened; or if they get some sudden shocks or sprains in their legs, or through some change in the weather.

"But these kinds of buboes can easily be distinguished from those of the genuine case according to their shape, size, and colour.

"As to the shape of the bubo (in a false case) it is somewhat of a round or circular form. Its size is a little more than a marble or sometimes nearly as large as that of a full grown almond. As to its colour it is somewhat of a white reddish one.

"As to the fever and buboes in a genuine case they are of the following nature:—The fever is of the typhus kind (please see my paper on the Plague). As to the bubo (in a genuine case) it is nothing but a mass of fatty substance somewhat of a yellowish colour like that of wax or cheese (when cut) rather hard in the beginning, but getting softer either when going on to putrefaction or under the influence of poultices, very sticky and of a bad stench. Its shape is somewhat of an elliptical or an oval one. As to its size it is from an ordinary to a full-grown mango stone, much thicker in the middle and very intolerably painful when putrid. The bubo appearing in the throat always proves to be the most obstinate and even fatal than those which are found in the armpits, stomach, and the groins; and hence there is little or no chance for the recovery of such patients. Its colour. It is slightly of an olive colour in the beginning, but of a darkish black when putrid.

"It will not be considered as improper or useless if a word or two be said about this piece of inquiry.

"*Plague Expressions.*—The complexion of the face is somewhat of a darkish colour. Eyes wide open. Eyelids thrown backwards. Enlargement of the pupils. Standing and walking postures not erect, but rather bent (though many an attempt made by the patient to walk erect in order to avoid suspicion); and extreme fear and anxiety.

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"*New Suggestions for the Treatment of the Critical Cases of the Plague.*—It will not be amiss to throw a suggestion or two for treating plague patients in a very critical state when the hospital authorities do not find a single ray of hope for the recovery of such patients.

"1*st.*—First of all the region of the heart and the stomach and the intestines ought to be very thoroughly cleansed by means of a douche. Of course the preparation for this kind of douche must be of a bactericidal nature.

"2*nd.*—That hot and fresh blood of some vultures such as feed upon dead carcases of plague patients (especially those who died without any medical assistance) should be introduced directly into the bodies of the patients through an artery or a main nerve, at the same time keeping the chests of the patients rubbed either with cod-liver oil or oil of almonds in order to strengthen their hearts; but no strychnine or any other injection should be used at all.

"3*rd.*—That electric currents should be introduced right into their blood—of course at the same time keeping their hearts strengthened as already described above.

"*About Clinical Thermometers.* — That many an innocent patient becomes a victim of the diseases of some other patients through the instrumentality of the clinical thermometer on account of the germs of the diseases found, in the febrile moisture of the skin, sticking to it.

"Again one thing must be remembered very thoroughly, that the natives are in the habit of getting their armpits shaved by the barbers. Now arm-pits, like cheeks, are not flat parts of the body for an easy shaving. So it is probable that razor-cuts might be found therein.

"Therefore it can be remarked that it is absolutely necessary either to disinfect the thermometer or allow it to remain into some warm water for a few minutes especially after taking the temperature of any patients suffering from any infectious disease."

"*The Bubonic Plague.*

"I.—What is it?—It is a fever.

"II.—*Its Antiquity.*—The plague is of a very remote antiquity. It seems to have visited Egypt, Messopotamia, Asia Minor, Arabia, China, Persia, India, and some other parts of the globe, on different occasions, and in different forms ever since the times of the Pharaoes of Egypt, 1490 B.C., and the Shepherd Kings of the East and lately in Europe, especially England.

"III.—*Its Name.*—The proper name of the fever is Typhus (Gravior).

"IV.—*Its Nature.*—The fever is of a very putrid, contagious, and malignant nature. The temperature of the body of the patient is from 104 to 106 degrees, and the pulse beating very quickly and heavily.

"V.—*Its Causes.*—Among the most obvious causes which contribute to its contagion, the following may be enumerated—(*a*) Noxious exhalations from stagnant waters of the gutters; (*b*) slimy mud and rubbishes taken from the gutters and exposed upon the roads to be dried up by the sun; (*c*) corrupt, damaged, or noxious grains; (*d*) putrid fish, meat, fruit or such other eatables; (*e*) residence in confined situations where the atmosphere is unwholesome and the rooms not properly ventilated; (*f*) intemperance and immoral habits; (*g*) tropical or very hot climate, especially after a very heavy rain; (*h*) and the want of pure water and due cleanliness.

"VI.—*How it enters the Human Body.*—The contagion of the plague enters the human body through cutaneous lymphatics, and thence produces the disorder of the lymphatic glands.

"VII.—*Does the Plague make use of any Animal Agency for the Propagation of its Contagion?*—Yes, the rat, the fly, the gnat, and such other creatures have their share in conveying the miasms and other poisonous elements by means of microbes, bacteria, bacilli, protozoa, and so on.

"How does it do so? Its manner of working is thus:—According to my opinion, that there is a *worm* of a certain kind, lodged in the belly of rats of a certain kind (perhaps such as live in the fields and gutters). The worm in the belly of the rat either becomes ill, or dies away when the rat partakes of some putrid grain, meat or such things or inhales the noxious exhalations from the gutters; and thus the rat is infected with a poisonous fever. Now, men contract this poisonous fever in any of these ways: (*a*) that the rat, after entering the godowns, the shops or houses, makes some dirtiness there, and afterwards the fly, the gnat, and such things sit upon it, and then they sit again upon human bodies; (*b*) or through the bites of such infected rats; (*c*) through the application of the febrile moisture of their skins by their jumping upon or dashing against the human bodies while they are asleep; (*d*) or by hiding themselves into their clothes for the fear of cats; and thus men cause inflammations upon their own bodies by scrubbing it with their nails, which may, perhaps, afterwards take the form of buboes and such other tumours.

"VIII.—*Its Symptoms.*—Fatigue, extreme debility, unquenchable thirst or desire for thirst, uneasiness, appearance of buboes, carbuncles or other inflammations, extreme irritations in the arm-pits and the groins, extraordinary heat in the body and in the head, and so on.

"IX.—*Is the patient aware of his Situation when he is infected with the Epidemic?*—No, he comes to know of it two or three days after the setting in of the disease.

"X.—*Is the Plague communicable? If so, how?*—Yes, it is communicable. Not by association, but only by contact alone. For if proper precautions be taken, then other individuals living in one and the same house may be free from it.

"XI.—*How many Species of the Plague are there? And what are they?*—There are three species of the Plague. (*a*) In the first species the energy of the brain and the nervous system is greatly impaired, indicating by a certain kind of change in the speech of the patient, such as low drawling and interrupted utterance, the tongue is more or less white in the centre and at the extremity, countenance pale, strength much impaired, the anxiety great, and the stomach extremely irritable. Rigors and pain in the lower parts of the back are found. (*b*) In the second species the state of the brain is horribly excited, intense pain in the head, thirst frequently considerable, though sometimes wanting, countenance flushed, and utterance hurried, frequent, epistaxis, appearance of glandular swellings and their disappearance without making a slight change in the system, appearance of buboes in the arm-pits and groins (right). Carbuncles appear on different parts of the body, and they rapidly show some disposition to become gangrenous, although the patient is attacked by a high delirium and thus brought to the termination of life within two or three days. Sometimes he lingers on to the seventh day, but seldom the eighth or ninth day. (*c*) The third species is somewhat akin to the last one save the symptoms are much milder and the brain comparatively a little affected. The buboes and carbuncles go on rapidly yet mildly to suppuration. And if prompt and early precautions be taken, the patient recovers from the epidemic.

"XII.—*The Duration of the Disease.*—The exact duration of the disease is uncertain; for it is various. Sometimes the effect of the contagion results in the immediate extinction of life. Sometimes the patient survives but a few hours, sometimes he lingers on till the thirteenth, fourteenth, or even the seventeenth day of the disease.

"XIII.—*What Changes are observed to have been wrought in the Body of the Patient on the* post-mortem *Examination?*—The omentum, the stomach and the intestines gangrenous in some places, the liver in a state of congestion and considerably enlarged, and the gall bladder filled with a black fetid bile and the pericardium with a bloody fluid. Proofs of inflammation and gangrene are also observed in the brain and its investing membranes, in the lungs and in the kidneys. In many instances the glandular system has been found in a diseased state, the blood black and loose in its texture. On a minute search, one, two, or three microbes are found in gall bladder, pericardium or in the glandular system. These colonies of microbes belong to the plague-propagating class. The complexion of the face as well as that of the whole body appears to be of a light dark colour.

"*My Method of the Electrical Treatment for the Cure of the Bubonic Plague.*

"I.—Injections.

"II.—Mixture.

"III.—Electro-chemical bath.

"IV.—Chemical application of electricity.

"V.—Applications of cupping-glasses, ice, and oily frictions.

"VI.—The uses of sea-breeze and sea-bathing.

"I.—*Injections*—(*a.*) *Chlorine or Gaseous Injection.*—Take some sea-water, or, if not, take some ordinary drinking-water and add a little quantity of chloride of sodium into it and separate the elements from the water by introducing electric currents into it. The following process is being done. The oxygen gas which is in the water mixes with the chlorine (*i.e.*, a greenish-yellow gas produced from the salt), and thus chloride of sodium becomes hypo-chloride of sodium. The same is the preparation for the injection. (*b.*) Oily injection which is nothing but the pure oil of almonds.

"II.—*The Mixture.*—It is composed of sulphate of quinine, antipyrin and tincture of camphor.

"III.—*Electro-chemical Bath.*—The electro-chemical bath is administered in the following way: According to my theory for the present epidemic, the patient is placed up to his neck in a large metallic tub which is filled with iced or cold water, added to it a little quantity of hypochlorite of sodium [as per the injection marked (A)] together with some hydrochloric acid, and insulated from the ground. The patient sits in a tub upon a bench of wood or wicker-work insulated from the tub, and having the length of the body. The tub is to be connected with an electric battery by means of a copper or brass wire, and the patient holds one electrode in his hand. Afterwards the battery should be worked on.

"IV.—*Chemical Application of Electricity.*—An oval piece of brass of medium thickness (*i.e.*, $\frac{1}{20}$th of an inch) and about 4 inches in length and 2 inches in breadth should be taken. Then a piece of fine linen previously saturated with hypochlorite of sodium [as per injection (A)] is placed upon the tumours (*i.e.*, buboes or carbuncles, whether hard and dry, or soft and gangrenous), then the piece of brass be placed on the linen. Another piece of brass which is about 4 inches in length and an inch in breadth, having round corners, should be placed upon the belly of the patient near the abdomen like the first piece of brass. Then they should be connected with a battery which should be set to work.

"V.—*Applications of Cupping-glasses, Ice, and Oily Frictions.*—If the inflammation be not very severe and the pain tolerable, cupping-glasses with previous scarifications may be used with much advantage. For the nature must be assisted in the work of throwing off the morbid matter by the pores; because sometimes the emetics, the laxatives, the purgatives, and the sweating regimens are of no avail at all.

"*Application of Ice.*—In order to check the strength of fever, the intolerable thirst for water, and for the burning inflammations and irritations, the use of ice is absolutely necessary. It is applied to the crown of the head, held into mouth, arm-pits and groins. It should be used in their bathing-water too.

"*Application of Oily Friction.*—Perhaps it will sound very strange into the ears of some medical gentlemen that it is essential to make use of oily frictions in this epidemic. But if we look at the statistics of the deaths occurred by this epidemic we shall hardly find three, four or five persons out of 100, whose occupation is that of a dealer or worker in oils. Besides these, a little experience will teach us that it is a death, not only to microbes, insects, or flies, but even to worms, mice, and little birds too, if they are entangled into a small quantity of oil. For this purpose I recommend the use of the oil of almonds, because it has a soothing and healing property in itself.

"VI.—*The Uses of Sea-Breeze and Sea-Bathing.*—It is very essential that such patients should have the advantage of sea-breeze and sea-bathing. If such a thing be not practicable, then let a linen shirt and a trouser be dipped into sea-water and wrung off and then worn by the patients, wet and cold, allowing them to walk here and there in the ward. Let them have cold baths, two, twice a day.

"I should here like to make a request that an anatomical examination be made upon some rats in order to establish my theory, that there is a worm in the belly of some rats; that the true origin of the epidemic be found out.

"It will be found out, after a careful reading of this paper, that my electrical treatment for the bubonic fever, which I have laid before the public, is perfectly harmless and will prove to be an efficacious one, if a trial be given to it. Nevertheless, if any doctor or doctors entertain any doubt about its efficacy, I shall, however, request them to try it upon some lower animals by inoculating them with the matter taken from the buboes or carbuncles from any patient: and then try an experiment upon them according to the directions I have given. If the experiment proves to be successful, then let patients be treated accordingly. If not, we will leave the matter off.

"*N.B.*—The present is the true copy of my *Paper on the Bubonic Plague*, which I had written in the middle of the month of October 1896, when, or a little after, the plague first broke out in Bombay. It had been shown to some doctors and public men of established reputation, whose names I cannot give without their permission. But their names may be given with permission, if required. The manuscript copy may be produced, if desired."

(Witness withdrew.)

Mr L. Godinho called and examined.

17,981. (*The President.*) You are Deputy Health Officer here?—Yes.

17,982. Of what district?—At present I am at E Ward East.

17,983. (*Mr. Hewett.*) What are the means by which plague is communicated?—First by personal contact, secondly by kit, thirdly by rats, and fourthly by aërial infection.

17,984. When you speak of personal contact do you refer to the contact of a person who has bubonic plague or pneumonic plague, or both?—Both.

17,985. Can you explain how you think a person with bubonic plague communicates it by personal contact?—I have seen, especially among the poorer Hindu classes, females attending plague cases with buboes and nursing them. The females never wash their hands after nursing the sick, and they carry their spittle to their own noses or mouths.

17,986. Do you think that was through the sputum?—Yes, and the saliva.

17,987. Would not that point to the cases being pneumonic?—The cases were not pneumonic.

17,988. Have you any instance in which a person with bubonic plague gave pneumonic plague to another person?—Only one.

17,989. Will you specify it?—In Carpenter Street a medical man was treating plague cases which were all bubonic. He got fever and then showed symptoms of pneumonic plague. On the third day he developed a bubo. He had both forms of plague, pneumonic and bubonic.

17,990. Have you any instances in which a pneumonic patient gave bubonic plague to another person?—Yes; in the Mahratta Hospital a Resident Surgeon suffered from pneumonic plague. His nephew and another relative attended upon him. When they went back home the nephew suffered from plague and died.

17,991. Is that the only instance you have in that form of communication?—In this special instance several suffered. Altogether about eight persons suffered, and all died. They got it from the Resident Surgeon. Some had the bubonic form and some the pneumonic.

17,992. Do you think that funeral ceremonies tend to communicate it?—Yes.

17,993. Do you mean from the body or the clothes of the deceased?—I would classify those separately. So far as the Hindus are concerned they have got special ceremonies which the female kind conduct. First they make an ablution, and then afterwards put some stuff into their mouths, and noses, and other

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orifices. The Muhammadans close the different orifices by a plug of cotton, camphor, and spices. A rupee or some coin is placed in the mouth; afterwards this is taken off, and replaced, and I have seen the women-kind suffer from plague through handling these coins.

17,994. Can you give us any instances of infection from clothes?—Yes. I know one specific instance in which a Christian suffered from plague at Lanauli. A month afterwards his nephew used his blanket and got the plague and died.

17,995. Was there any plague at Lanauli at the time?—Yes. The nephew used the blanket he had in Bombay in a house in which there was no plague.

17,996. Have you any instances in which plague has been communicated through rats?—Yes. In the present epidemic, towards the end of February 1898, sick rats were found in Dukar Gali in Tanaswadi. They made their appearance on the ground floors at the east end of the Gali, and then rapidly travelled to the west end, keeping to the rear of the Galis, where water could be easily got. About a week after the appearance of sick rats the inhabitants of Dukar Gali began to sicken and die of plague. The first cases happened in houses where dead rats had been found. From Dukar Gali the rats travelled north-west to Burrow's Lane. A listless rat was found on the 5th March in No. 3 Burrow's Lane, and eight days after a servant boy, who is said to have killed and burnt a rat, was attacked with plague. I know that the boy did play with the rat and did burn it.

17,997. Do you know for a fact that the boy was not subject to any other source of infection?—Yes.

17,998. Was there no plague patient with whom he might have communicated?—No. The residents of this house were living then in a tent at Kennedy Sea Face.

17,999. Under observation?—No; he was a medical man, and had his own tent.

18,000. Why could not the boy have gone to some infected portion of Bombay, and there got plague?—The boy was in the tent, and used to go back to the house to sweep it, and he brought the news that he saw a sick rat there. The boy was in the tent all the while.

18,001. What security is there that the boy did not go to the town?—I cannot answer that.

18,002. I mean can you exclude all other possible means of infection?—I am almost certain the boy did not go anywhere else, because this refers to my own servant boy.

18,003. Have you had experience of mortality among rats before cases of plague occurred in human beings?—Yes, and also afterwards.

18,004. Have you observed mortality among rats to precede mortality among human beings, in any case except the one which you have just specified?—I have observed it in all the three epidemics.

18,005. That is to say you have observed mortality in rats in individual houses to precede attacks among human beings in those houses?—Yes.

18,006. I presume that when you made these observations plague was existing in the neighbourhood?—These were in different streets.

18,007. Has plague been present within a mile of those streets?—Within half a mile. I have seen rats sicken where the plague cases occurred only among men; first men suffered, and then on the eighth or tenth day rats began to sicken.

18,008. What do you wish to say about aërial infection?—I believe that in stuffy rooms, where a pneumonic patient is being treated, the air becomes infected and the nurses and other attendants suffer from plague afterwards.

18,009. Have there not been in Bombay some very marked instances in which, although there has been pneumonic plague, the attendants in the hospital and also the relatives of sick persons in the hospital have escaped entirely?—Quite so. I put that down to disinfection, and free ventilation in the hospitals. The perflation of the air reduces the infection to a minimum, and I believe also the attendants and medical men in the hospitals get immune against plague in a certain time in some way.

18,010. You say that in two instances the attendants on parsons suffering from pneumonic plague rapidly got the disease?—Yes.

18,011. Is it not possible that they got it from the sputum of the patient?—In these two instances I think not, because these people were in separate rooms; they did not occupy the same room. They did so many hours in the room, then went to the next room, and passed the time there. Of course it is just a remote possibility they *might* have got it from the sputum as they knew the value of disinfection.

18,012. Have you any experience with Yersin's curative serum?—I only know two cases which were treated by Dr. Yersin's serum sufficiently early; the results were negative.

18,013. Have you inoculated any persons?—Yes, about 400.

18,014. Have you observed the effects of inoculation?—Yes. The action depended upon the dose I gave.

18,015. Have these 400 persons remained under your observation after inoculation?—No, not all.

18,016. Therefore you are unable to see how far they have been attacked by plague?—Yes.

18,017. Have any instances come to your notice in which persons were inoculated and subsequently got plague?—Yes, a few. Dr. Davar suffered from plague in December 1896; he was inoculated a few months afterwards, and subsequently suffered from plague and died. Dr. Davar was a special officer working in the Kamatipura district.

18,018. Have you any other instances of persons who suffered twice from plague?—Yes. Mrs. Ezra suffered from plague at Hong Kong on the 24th July 1894, and on the 6th December 1896 she got a second attack in Bombay—Bellasis Road.

18,019. Did you attend her during the second attack?—Yes.

18,020. Did she recover?—Yes.

18,021. Have any other instances come to your notice?—Yes, one Narayan Mulji suffered from plague three times. I saw him the third time. The first time he was treated by Dr. Ismail Jan Mahomad.

18,022. When did you attend this Narayan Mulji?—In the second epidemic in July or August 1897.

18,023. What form of plague did he have?—Bubonic plague. The bubo was in the axilla accompanied with diffuse cellulitis.

18,024. Was that his second or third attack?—His third attack.

18,025. Will you give us the history he gave you?—He said he suffered first in 1896, when he had a bubo in the groin.

18,026. Who attended him then?—Dr. Ismail Jan Mahomad treated him the second time he had plague; that was in the second epidemic, about November.

18,027. Do you know who attended him the first time?—I do not know.

18,028. Did he recover from the third attack?—Yes. Then he was also treated by Dr. Ismail Jan Mahomad.

18,029. Is this man to be seen?—Yes, I think so.

18,030. Could you bring him here on the 20th?—I am sure he would be willing to come, because he offered himself, but I should like the Secretary to write to him; that might induce him to come. (Mr. Narayan Muljee was examined on February 20th.)

18,031. Have you any other cases of double attack?—Yes, a Customs' clerk suffered twice from the disease, the first time in November and the second time in April 1897.

18,032. Did you attend him on either occasion?—I saw him at the hospital; he was at the Lal Bag Hospital the second time.

18,033. Did he have bubonic plague both times?—The first time he mentioned to me that he had a bubo in the left groin. The second time I saw him, and the bubo suppurated, and he recovered.

18,034. Was the bubo in the same position?—I do not remember exactly.

18,035. Could you produce this man also?—I could not say that, but you could get the facts from Mr. Green, the Secretary of the Plague Committee. If you wrote to the Collector of the Customs you might get at that man. (The man in question, Mr. Champaklal Thakurdas, was subsequently examined on the 20th February).

18,036. Does that complete the list?—There are two more mentioned in my précis, but I am doubtful, because they might have been recurrences.

18,037. It is possible the disease was continued the whole time in one case?—I do not think so, because there was no rise of temperature, and the buboes had disappeared. It might have been a recurrence. I cannot say.

18,038. Have you observed any cases of mild form of plague?—Yes.

18,039. Can you describe the symptoms in these cases?—A man generally suffers from a small chill and headache in the front of the head; he then feels general *malaise*, pains all over the body and joints and small of the back. His temperature rises to about 100, and he is ill for a day or two and then he is again better.

18,040. Do these people go about their usual business while suffering in this manner?—Some do. There was a man named Mascarenhas, who lived at No. 18, Church Street. He had three days very slight fever, and on the fourth day a small shotty gland appeared; all the while he was all right, that is, he continued his work for three days, and on the fourth day the gland appeared, and on the fifth and sixth days his temperature was 100.

18,041. Did you attend him?—Yes; I showed this case to Colonel Wilkins.

18,042. When was that?—In October—the 6th to the 12th October 1898.

18,043. Can you tell us whether the cases with mild form of plague occurred mainly at the beginning, middle, or end of an epidemic?—Towards the end I saw people suffering from fever of a relapsing character, and then, afterwards, on the 14th or 15th day, they developed buboes; in one or two instances they died. On the other hand I have seen plague cases develop buboes on the third or fourth day, and then proceed towards recovery, and afterwards have the relapsing kind. It is a sort of mixture of the two. They had fever of the relapsing kind and a bubo appeared afterwards.

18,044. Have you observed this mild form occurring among young persons?—Young adults.

18,045. Not among children?—No; I have not seen them have it.

18,046. Do you think you could find any cases of it now?—Perhaps.

18,047. Will you try and find some cases?—Yes.

18,048. Do you think that inoculation is the chief means by which plague is caught?—Yes, through an abrasion or something of that kind.

18,049. Do you think that the practice of cleaning brass vessels with mud has anything to do with it?—From my observation in C Ward I found that in certain houses in the central street, Kumbarwada, the women suffered more from axillary bubos than men. Men suffered from femoral bubo.

18,050. The women are the persons engaged in cleaning the brass utensils?—Yes, in the mud-making operations.

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18,051. Do you think that explains the frequent occurrence of the axillary bubo among women?—Yes, I think so.

18,052. (*The President.*) Have you treated many cases yourself?—No. In the first epidemic I did treat a few.

18,053. Have you latterly seen many cases throughout the course of the illness?—Yes. During my tenure of office I must have removed over 1,000 to hospital. I have gone to hospital and seen them, but I have not treated them.

18,054. Were there many pneumonic cases among those you have seen?—Yes.

18,055. Have you observed in any case any swelling of the anterior part of the body?—I found dulness on percussion.

18,056. Did you see anything in the nature of œdema in the skin?—Yes, I could mention cases, and they had also bubo. For instance, Dr. Mehta, who attended the Banniah Hospital at Pinjgrapole, got plague, and had a diffuse swelling all over, anterior and posterior, so far as the lower angle of the scapula.

18,057. That is, on one side of the thorax?—Yes.

18,058. But have you seen anything like a general swelling of the whole of the anterior surface?—No. In cervical cases in children, where the buboes were in the neck, I have seen the swelling come down so far as the middle of the sternum.

18,059. In the cases of mild plague have you any instances in which they infected any other persons?—I could mention only one instance, but the second case in that instance, a woman, to my mind was not suffering from plague, but from fever. Mascarenhas had plague, I believe—he had a bubo in the right groin—and his mother suffered from slight fever which lasted three days.

18,060. She became affected afterwards?—Yes. Dr. Wilkins said the enlargement of the gland in Mascarenhas' case might have been due to other causes, but I could not find any abrasion, no cut, nothing on the foot, and I put that case down as one of mild plague.

18,061. The second case was fever?—Yes.

18,062. Then you have not seen any cases of mild plague infecting any other persons?—No, I have not.

18,063. (*Mr. Hewett.*) You have seen a good many of the people employed upon plague duty here?—Yes.

18,064. Have you noticed any of them with symptoms of plague, enlarged glands, or anything of that sort, without their actually getting plague?—Yes.

18,065. Is that common?—I think it is frequent among workers on the plague. Several times the glands get enlarged, especially the femoral glands, and then the enlargement passes away.

(Witness withdrew.)

Mr. A. H. Simcox, I.C.S., called and examined.

Mr. A. H. Simcox I.C.S.

18,066. (*The President.*) You are in the Indian Civil Service?—Yes.

18,067. You are Assistant Collector of the Malegaon sub-division of the Nasik district?—Yes. Malegaon is a large town of about 20,000 to 25,000 people. It also has another village which, although not included in the Municipal limits, is practically a part of the town: it is separated by a river which is nearly dry.

18,068. When did plague appear there?—The first known case was on the 11th November 1897. I do not think there was anything before that, because I traced the infection directly to an old woman who sneaked in from Bombay.

18,069. It was imported from Bombay?—Yes.

18,070. What measures did you adopt?—I was out in the districts. I got back the next day, and we took immediate steps. We got a large dharamsala for a hospital, and segregated the cases as they occurred. There were two other cases on the first day. The Municipal people suspected it on account of their having been three deaths very close together. The old woman died and two other people died at the same time in the same house. At that time there was only a native Hospital Assistant there, and I am sorry to say that I suspect he took bribes; we only got one or two declared cases a day at that time, all in the same neighbourhood. We began by clearing out the house in which a case occurred and the two houses on each side. After some time we built regular lines of huts for contacts and for the cases, for the people who nursed the patients and the contacts, and the people from the neighbouring houses. We got into regular swing, but never succeeded in stamping out the disease with this partial evacuation, which lasted from 12th November till the 19th January.

18,071. In that period how many plague cases occurred?—About 350.

18,072. You were not satisfied with the success of this measure?—No. On the 5th January we got 18 cases, our maximum for one day.

18,073. Besides this partial evacuation, did you disinfect the houses?—Yes, we disinfected all houses which were evacuated.

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18,074. When did you adopt complete evacuation?—On the 20th January I issued a notice that all the town was to turn out, in four quarters, allowing one week for each quarter, but it really came to three weeks, because towards the end the last two quarters went out together. They were all out on the 10th February. I got them into the maidan, about a mile from the town, where there was a dharamsala, a big rest-house for travellers, built of stone. I would not allow the people to go anywhere else but into certain lines. We made lines in the fields like a military camp, and gave each man so many square feet for building his own hut on, and made lanes between. We got the whole town out in this place. The population had largely diminished. A great many people ran away at first, but I reckoned I had about 12,000 people in the camp.

18,075. How many cases occurred after this complete evacuation on 10th February?—After that we had one or two cases. Almost all the population are weavers, and we put up the strongest huts we could to support their hand-looms, but we found we could not completely satisfy the people's requirements. They lived from hand to mouth by their daily weaving, and I had to select a certain number of large weaving shops in the town—not living houses—disinfect them, and let the people go into them in the day-time. There were a few cases of those people stealing into their houses at night, and it was among them we had further cases of plague. We did not have a single case which could be proved to originate in the health camp.

18,076. Do you know where the people who fled from the town went to?—Perfectly. I had a complete watch kept on all the country round, and I can practically tell you where every man went to. None of them went far away.

18,077. Did they convey the infection to anybody else?—No, not a single village was infected.

18,078. That disposes of the 25,000 people, does it not?—Yes. Each village in this country has a large area of fields belonging to it, and by far the greater number of people were within Malegaon boundaries.

18,079. How long were the people kept out?—We let them in again on the 2nd April.

18,080. Previously to that, what had you done to the evacuated houses?—I had disinfected every house all through the town, the greater portion with perchloride solution, according to Government orders. There was a man in the town who wanted to get rid of a lot of sulphuric acid, so I wrote to Dr. Hay, and got the opinion of the Surgeon-General that sulphuric acid, 1 in 500, would do instead of perchloride. Then I bought some sulphuric acid and did a certain number of the houses with that. We have not had a case since. The people have gone back, and there has been no recrudescence.

18,081. Are you free now?—Yes.

18,082. What was the effective measure which you adopted, in your opinion?—Turning out the town. Towards the end of our partial evacuations we turned out the whole block in which there was a case. Some people were bound to get into the neighbouring houses where they had relations. We found a great many dead and dying rats also.

18,083. Partial evacuation did not succeed?—No.

(Witness withdrew.)

(Adjourned till to-morrow.)

At The Secretariat, Bombay.

FORTY-EIGHTH DAY.

Wednesday, 15th February 1899.

PRESENT:

PROF. T. R. FRASER, M.D., LL.D., F.R.S. (*President*).

Mr. J. P. HEWETT. | Mr. A. CUMINE.

Mr. C. J. HALLIFAX (*Secretary*).

Lieut.-Col. W. G. H. Henderson, I.M.S.

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Lieut.-Col. W. G. H. HENDERSON, I.M.S., called and examined.

18,084. (*The President.*) You are in the Indian Medical Service?—Yes.

18,085. You are prepared to give evidence on the outbreak of plague in Hyderabad, Sind?—Yes.

18,086. (*Mr. Hewett.*) When did the first imported cases occur in Hyderabad?—The first imported cases occurred in Hyderabad in the first week of January 1897, shortly after the appearance of plague in Karachi.

18,087. How many imported cases were there before the disease became indigenous?—There were 34 or 35 imported cases.

18,088. When did the first local case occur?—In the last week in February.

18,089. So that there was an interval of about seven or eight weeks?—Yes, between the first imported case and the appearance of the plague locally.

18,090. After the indigenous cases began to occur, how long did the disease prevail?—After the local cases, until the end of July.

18,091. Can you tell us how many cases there were during the whole time?—Roughly speaking, just under 600.

18,092. Have you any idea of the proportion that died?—There were 400 treated in hospital, and 35 per cent. of those recovered.

18,093. Was the prevalent form pneumonic or bubonic?—Bubonic.

18,094. Were there any pneumonic cases?—There were what I thought pneumonic cases, but in several of these there were buboes as well.

18,095. I suppose you had no opportunity of observing whether a bubonic case gave pneumonic plague to another person?—No, I could not state that.

18,096. Did you find any instances of *pestis minor* or of the non-typical forms of plague?—Of those which had very slight symptoms, and which really suffered at all there were some cases, and I think there were some in the town, but we did not discover all who actually suffered from plague.

18,097. About how many of those cases came under your observation?—About 20 of them. They were really very mild cases; they came into hospital and rested there and recovered.

18,098. Did they have high fever?—No, they did not. Their temperature was never above, perhaps, 100 or 101.

18,099. Were they prevented through personal incapacity from doing their ordinary business?—They were incapacitated from doing their ordinary business, so far as the people in the town refused to have anything to say to them.

18,100. I mean physically incapacitated?—They were not physically incapacitated; they might have walked through the plague.

18,101. How long did the fever last in the case of these people?—For a week or 10 days. I had no reason for detaining them longer.

18,102. Did you find these cases more at the end or at the beginning of the epidemic?—More at the beginning.

18,103. Did you observe that they occurred among any particular class of persons, according to age?—They occurred among young adults.

18,104. Not among children?—There were some cases among children, but the ones that I recollect most were among the young adults.

18,105. After the imported cases, did you observe mortality among rats?—Yes.

18,106. That was subsequent to the arrival of the imported cases?—Yes, and immediately prior to the occurrence of local cases.

18,107. What inference do you draw from that?—That the rats were infected by the imported cases, and when they were infected, that they infected the locality; and that the infection of the locality was the means of infecting others.

18,108. Have you anything you would like to say regarding the manner in which people get inoculated with plague?—I have looked to the prevalence of fissures and cracks and wounds, and although there are some cases in which these occurred, the numbers were not sufficient to let me draw any decided inference from it. Although it is merely a supposition, my idea is that on a locality becoming affected, the people living in it, and eating in it, and cooking their food in it, may be affected through these means; especially through the food. I have seen people in infected localities sitting on the floor, and eating their food virtually off the floor itself, and I think if the locality is so seriously affected, as I am sure it was, that this may be a means of propagating the disease.

18,109. Have you anything more definite than the information that you have just given us, regarding intestinal infection?—No, nothing more.

18,110. Has it come under your observation that rats and mice fed on cultures or infected food have developed plague and died?—No. That is what I have been told by a bacteriologist.

18,111. Do you think it possible that the dust flying about in the air communicates the disease?—I think it possible that it may produce the pneumatic form of disease.

18,112. Do you think it possible that the custom of the females in this country of cleaning their cooking pots with mud might tend to give them plague?—I think it might decidedly have a tendency.

18,113. Do you think that the practice of the people after they have performed the necessities of nature is likely to give inoculation?—I have not thought of the inoculation being propagated in that form.

18,114. What is the population of Hyderabad?—65,000.

18,115. What proportion of the 65,000 left the town between the date of the imported cases and the occurrence of the local cases?—The population was increased rather between the 1st January and the latter week of March because the people still flocked up from Karachi to Hyderabad.

18,116. After the plague began to be indigenous at Hyderabad did the population remain at its normal figure, or did it decline at all?—It declined to a certain extent, but not to a very great extent, as we took these very stringent measures to prevent the people flocking to the districts unless they had been under observation.

18,117. You think that the whole number of the population were dealt with?—Yes, except that numbers did go that were allowed to go.

18,118. When you evacuated the town how many people remained who were not provided for in the camps?—About three-quarters of the population left, I should say, the most congested parts of the city.

18,119. Were they all taken charge of in camps, or were they allowed to hut themselves?—They were allowed to hut themselves under supervision.

18,120. Were they allowed to go back to the town?—They were allowed to go back to the town for business purposes, but even there they were under supervision.

18,121. Could you give us a statement showing how the plague declined week by week after you got the people out of the town?—(Witness subsequently intimated that the Collector of Hyderabad, on being asked for the figures, stated that no reliable information could be supplied.)

18,122. Did you carry out disinfection on a large scale?—Yes, we carried out the disinfection of houses on a large scale.

18,123. Did you disinfect only those houses in which cases of plague were known to have occurred?—No, we disinfected houses in the quarters which were infected.

18,124. Did you have any means for disinfecting the clothes of people that went into the segregation health camp?—Only boiling.

18,125. Did you have any cases of plague in the health camp?—19 cases in the contact camp and eight or nine cases in the other camps.

18,126. Are you of opinion that plague is conveyed by means of the infected clothing of plague patients?—I think it is.

18,127. Did any instance come to your notice of plague being conveyed by clothes alone without personal contact?—By clothes taken from an infected person, yes. In some of the villages of Hyderabad, in the district, I think that the clothes were the means of infection.

18,128. The facts that came to your notice showed that?—Yes.

18,129. Can you give us the particulars of that?—In the village of Allahyar I think the infection was carried by clothing, especially because in that case the large percentage of the people who went there had been kept under observation. Of course, some may have escaped, we cannot say that. Our disinfection of clothing, as it was simply boiling, was very inconvenient for the people, and they tried to elude it as far as possible, and although we kept a strict observation, I have no doubt that some clothing was taken away, in fact I am certain it was, to this village, and that the clothing was the means of infecting some of the people.

18,130. How do you think that the clothing of patients who suffered from bubonic plague got infected?—Either from the discharges from the bubo, or from the excreta, or from the sputum.

18,131. You think from sputum in bubonic cases?—Yes; but in the pneumonic form especially.

18,132. Did you have corpse inspection?—We had corpse inspection in Hyderabad when plague was prevalent at Karachi in the second epidemic, and at Kotri, which is just across the river from Hyderabad, during the epidemic there.

18,133. Did the people show any objection to it?—No, not at all; they were entirely with us, I think, after the first flight of the plague in the first scare, and they helped us in every way that they could.

18,134. Who examined the female corpses?—A lady doctor.

18,135. Do you think you can ascertain by the examination of corpses whether pneumonic plague is present or not?—I do not think so.

18,136. Have there been any other cases of plague since the epidemic ceased in July 1897?—There have been no local cases in Hyderabad or in the district.

18,137. Was the town of Kotri infected three or four months afterwards?—Yes.

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18,138. Does it lie immediately on the other side of the river to Hyderabad?—Immediately opposite, on the other side of the river.

18,139. Has any further infection come from Karachi?—Cases have come, but we discovered them at once, and we put them into the camps so that neither from Kotri nor from Karachi had we any other infection of localities.

18,140. You have had particularly favourable opportunities for stopping people, have you not, by means of the camp at Malir?—Very favourable indeed. There were only a very few cases escaped the camp, and occurred in Hyderabad during the Karachi epidemic.

18,141. Have you any information about inoculation?—No, I have no information about inoculation. We did no inoculation at Hyderabad, except in a few cases.

18,142. Have you anything to say as to the effect of climate on plague?—I think that we were helped considerably in Hyderabad by the very hot and the very dry climate there, and the nature of the soil which is rocky and sandy.

18,143. Did you find that the disease declined as the temperature increased?—From the first the temperature was very high. In March, the temperature goes up to 115 and 120, perhaps not as high as that, but in April the temperature is as high, probably, as it is in June and July.

18,144. Then your experience in Hyderabad is contrary to the opinion which some people have held that very high temperature will destroy the plague?—Well, it did not last very long in Hyderabad.

18,145. But did not the epidemic actually grow in Hyderabad when the temperature was at its highest?—From April, May, and June, the weather was very hot indeed, and the plague rose and declined during that time.

18,146. (*The President.*) From these cases of mild plague, did it come under your observation that any other persons were affected?—From my experience of my hospital, I think that the person to person infection is very rare indeed. I think that it is the locality which is the cause of the infection.

18,147. You did not observe any cases?—No.

18,148. What exactly were the symptoms of this *pestis minor*?—There was *malaise*, a feeling of indisposition and loss of appetite, and in some cases a small bubo which did not suppurate.

18,149. And a rise in temperature?—And a rise in temperature to 100 and 101.

18,150. Were there any bacteriological observations made?—I made none.

18,151. Are you aware of any having been made?—I am not a bacteriologist, and we had no means of making them at all.

18,152. Did you follow the course of any patients after you treated them?—Not after I treated them.

18,153. You had a considerable number of pneumonic cases?—No, not a considerable number; it was rare in Hyderabad.

18,154. How many do you think you saw?—I forget the numbers; there were very few.

18,155. Were they very fatal?—Yes, very fatal.

18,156. Did you see many of them shortly before death?—Yes, shortly before death.

18,157. And did you see their corpses afterwards?—Yes.

18,158. Did you observe in any of these cases whether there was any swelling in the anterior wall of the chest?—No. In some cases there was swelling in the walls of the abdomen.

18,159. Do you mean of the actual walls, or of the whole abdomen?—Of the whole abdomen.

18,160. Not restricted to the muscle or skin?—Not so far as I could make out restricted to the skin or muscle; there was a tenseness of the whole abdomen.

18,161. There was no œdema?—There was no œdema.

18,162. You speak of certain cases occurring in the contact camps?—Yes, in the camp where those actually in contact with the sick were located, or who came from houses in which cases had occurred. These were put into a separate camp.

18,163. There were a few cases in the health camp?—They were fewer than in the other camp.

18,164. Did these cases in the health camp originate some time after the patients had been segregated in these health camps?—All within 10 days.

18,165. They might have been infected in the town before that?—Yes, they might have been infected in the town.

(Witness withdrew.)

Mr. K. B. Shroff.

Mr. K. B. Shroff called and examined.

18,166. (*The President.*) You are acting as Deputy Health Officer here?—Yes.

18,167. To what places is your evidence in reference?—Malabar and Cumballa Hills.

18,168. I believe you also wish to speak of some other place?—Yes, Cutch Mandvi.

18,169. You followed the course of plague in Cutch Mandvi?—Yes.

18,170. When did it first appear there?—We were sent there on the 27th of April 1897, and I was there for 2½ months along with Dr. Wilkins and Dr. Mason.

18,171. When did the plague commence?—The plague had commenced about a month previous to our going there.

18,172. You do not therefore know much about its introduction?—No, nothing positive.

18,173. You have formed some opinion in regard to the communication of plague?—Yes.

18,174. Will you state your opinion?—I think plague was communicated in three different ways. First by the air, causing the primary plague pneumonia, which is very infectious. The second is by the rupture or abrasions of the skin, which might account for so many cases of buboes in the groin. We find almost all the poor classes, and all the servants, going about with naked feet, and they might have imperceptible abrasions between the toes, and that might also account for the great majority of people getting buboes in the groin.

18,175. Did you find many abrasions; did you search for them?—Yes.

18,176. Do they represent a large or a small proportion of the total cases?—A great many of the cases.

18,177. What kind of abrasions, and where were they chiefly?—Between the toes; there was a slight rupture of the superficial layer of the skin. We cannot find it unless we widen out the toes, and then we find a rupture of the superficial layer of the skin between them.

18,178. Assuming the virus enters one of these lesions, were there any local signs distinguishing them from other lesions? Was the actual lesion modified in any way?—No.

18,179. There was no inflammation or swelling?—No inflammation; nothing whatever.

18,180. You merely infer that the virus had thus entered because there were lesions?—Yes.

18,181. Did you find corresponding lesions to account for buboes in the axilla?—No.

18,182. Did you search the hands?—Yes.

18,183. How do you account for buboes in the axilla?—There might have been, but we did not find any.

18,184. And in the case of enlargements of the neck glands?—There we did not find any either.

18,185. Then, how do you suppose the buboes were caused there?—There was a case of a man who complained of caries in the tooth, and subsequently there was a bubo in the neck.

18,186. On which side?—On the corresponding side.

18,187. You have one case of that description?—Yes, one case recently. The patient was suffering from caries of the tooth, and after three or four days the man died, and there was swelling on the corresponding side of the neck.

18,188. You mentioned a third method of communication; what is that?—Propagation by rats.

18,189. What are your observations with regard to propagation by rats?—Last year the plague came on in Malabar Hill, and previous to that we had reports of dead rats found in all the servants' quarters, and in spite of the warning given to the servants not to sleep in those quarters, they persisted in sleeping there, and within a week we found a case of plague in those servants' quarters.

18,190. Where dead rats had previously been found?—Yes. Although the quarters were disinfected, and I had warned the owners not to allow the servants to sleep in the room, they persisted in sleeping there, and within a week I found dead persons, and cases removed. Most of those bungalows were on the seaside, and I think that the rats travelled from the drains to the foreshore.

18,191. From where did these rats travel by drains?—From the infected parts.

18,192. What would be the nearest infected parts?—Khetwadi, Girgaum, Tardeo, and other places.

18,193. Quite near?—Yes.

18,194. Have you any other observation?—We had one bungalow on the other side of the Hill where we had three cases, and dead rats were found for two or three days consecutively.

18,195. Before or after the cases?—Before the cases occurred.

18,196. How long before?—Ten or 12 days, not more than that; and the butler who was a very robust and strong man said he was very bold, and he persisted in sleeping in those rooms where the dead rats were found. After three or four days he was found lying in the compound quite helpless, with a bubo.

18,197. That was one case?—We had numerous cases like that.

18,198. How do you think rats actually communicated the plague to these patients?—I believe that rats brought the infection with them, and the germs were lying in the room.

18,199. And they infected the room?—Yes, and although we took all possible precautions to disinfect, still the cases occurred in these rooms.

18,200. Have you followed the progress of many cases of plague yourself, from the commencement to death, or recovery?—Many cases, but I had no opportunities of seeing the patients after their removal to hospital, except to Cutch Mandvi, where about 2,000 cases passed through my hands.

18,201. Did you inquire into their histories in most cases?—We had hardly any time to go into the history of all these cases. They were brought into the hospitals in a moribund condition—and we had great difficulty—we had no staff in the beginning, and we had to organise the whole thing ourselves after we reached Cutch Mandvi.

18,202. Did you see among these cases many pneumonic cases?—Yes, many pneumonic cases.

18,203. Did they occur throughout the epidemic, or at any particular part of it?—I think it was most prevalent at the time we went there in April.

18,204. Was that some time before the epidemic ceased?—In the middle of the epidemic; when we went there of course the decline commenced by seven to ten every week.

18,205. Was that after evacuation?—After evacuation, segregation, and disinfection; the whole town was evacuated.

18,206. Have you any facts to show whether a pneumonic case might obtain infection from a bubonic case?—I cannot say that.

18,207. Or *vice versâ*?—I cannot say. The pneumonic type gives infection.

18,208. Have you any cases?—Yes, we have.

18,209. Could you illustrate you answer by instances?—I can give three instances. There was one, a Hospital Assistant, who was suffering from the pneumonic type of the disease. Subsequently to that we had two persons who were attendants on this man who got plague and died.

18,210. What form of plague was it, pneumonic or bubonic?—Pneumonic.

18,211. Have you any other cases?—Altogether there were nearly ten cases from this one case of pneumonia.

18,212. Will you explain how they originated?—There were four attendants, two friends, and three or four other friends who accompanied the corpse—ten altogether.

18,213. Can you follow up the histories of these ten, and determine whether they were pneumonic or bubonic?—I find that these ten cases were of the pneumonic type. Then there was another case in another chawl. A lady died of pneumonic plague, and one of her relatives who attended her died of pneumonic plague, and a third also died of pneumonic plague—three altogether.

18,214. Do you recollect any case in which a pneumonic case produced bubonic plague, or a case in which bubonic plague produced pneumonic?—Yes, I had a case a few days back. One pneumonic case I removed to the hospital, and subsequently there have been two other cases both bubonic, in the same house.

18,215. What was the means of contact between these cases?—They were lying in the same room, and one was lying along with the patient. He was his uncle and the other was his friend. He only visited him, and did not reside in the same room. The man only died yesterday, and he had a bubo in the neck.

18,216. These were all the cases which occurred in connection with this case you have mentioned?—Yes.

18,217. You have had some experience with curative serum, have you not?—Yes.

18,218. Would you describe your experience?—When I was in Cutch Mandvi we had nearly 30 cases inoculated with Dr. Yersin's serum, and out of these only 11 recovered.

18,219. Did you take charge of the patients in these cases in which curative serum was employed?—Yes, I myself inoculated them.

18,220. Were you following the history of these cases?—Yes, with Dr. Mason.

18,221. You were doing it in conjunction with Dr. Mason?—I was under Dr. Mason, and I was in charge of those wards.

18,222. There is a clinical record of these cases?—Yes, but I differed from Dr. Mason in some of the cases he put down as plague. I took them to be malarial fever. That is the reason why I give 11, and I believe that Dr. Mason has shown a higher figure. Dr. Mason's theory was that these were plague cases that were inoculated with Dr. Yersin's serum, and who subsequently recovered, but my opinion was that they were malarial cases.

18,223. How many cases does this apply to?—I have no figures.

18,224. Were they a small or a considerable number?—Only a difference of two or three.

18,225. It does not affect the general result?—No.

18,226. What is your opinion of the value of Dr. Yersin's curative serum?—I do not think it is of any value; I am against it.

18,227. You had experience also of preventive inoculation?—Yes, I inoculated 1,242 persons up to the 12th instant, of which 999 were for the first time, and 243 for the second time.

18,228. At what period of the epidemic was this?—Since the last 14 months.

18,229. What was the result of your inoculations on the whole?—I have no case with any bad after effect, excepting one case after the second inoculation, where there was severe purging, vomiting, fainting, and urticarial rash lasting for about an hour-and-a-half.

18,230. In so far as you can estimate the value of the inoculation in preventing plague, what was your opinion?—I think it was a great safeguard.

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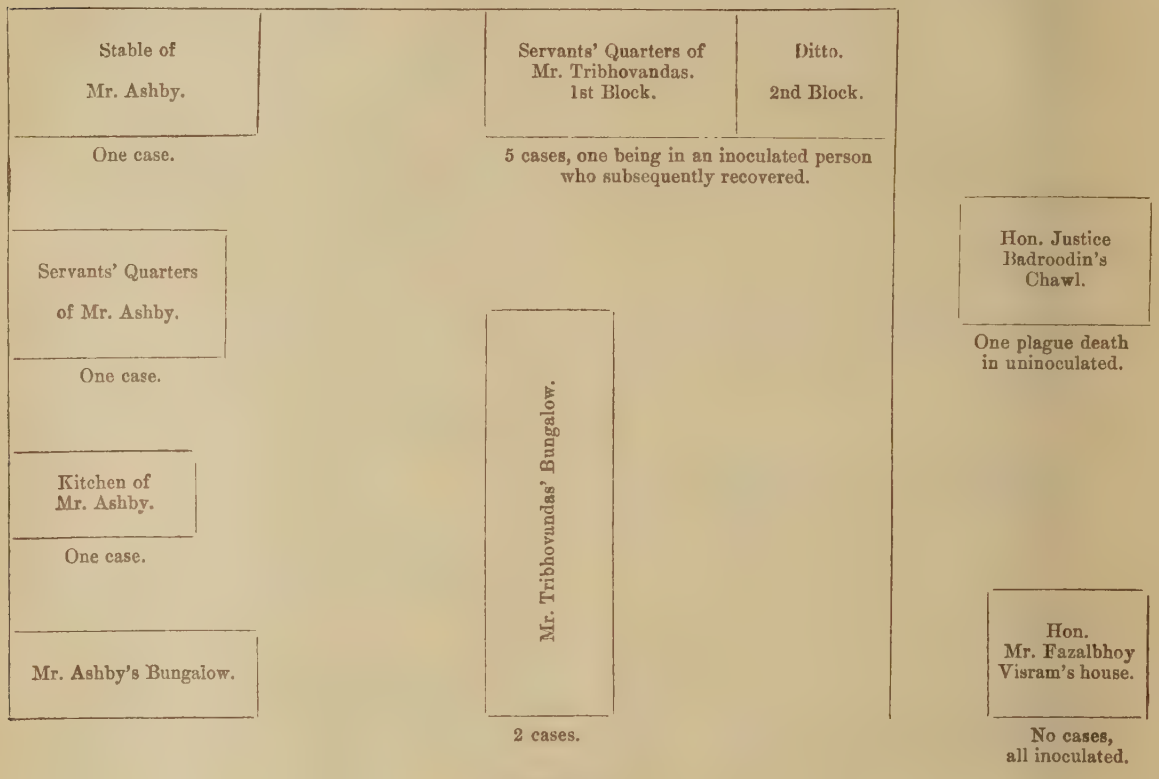
18,231. Have you got any facts and figures which support that view?—Yes. In Warden Road, just near the Battery, there is a big compound containing three bungalows; behind these, and separated only by a 6 foot wall, there are two chawls, and two other bungalows, and adjoining those again there is another bungalow, and servants' quarters. In the front bungalow, containing altogether 75 servants, only one man was inoculated. Here there were altogether 11 cases of plague, including the one inoculated, who subsequently recovered. This man was inoculated on 30th January 1898, and got plague on 10th March 1898, *i.e.*, 40 days after.

18,232. The only man who was inoculated also acquired plague?—Yes, 40 days after inoculation; he subsequently recovered in the hospital, but died later on of some other disease.

18,233. How long afterwards did he die?—About two months after his discharge from the hospital. Behind these bungalows almost all the people were inoculated, and here there was only one case amongst the uninoculated who subsequently died. In the neighbouring house all the servants were inoculated, and there was no other case there. I have a sketch here which illustrates it, as follows:—

A Rough Sketch of the Position of the Bungalows in Warden Road near the Battery.


Stable of Mr. Ashby.
One case.
Servants' Quarters of Mr. Tribhovandas. 1st Block.
Ditto. 2nd Block.
5 cases, one being in an inoculated person who subsequently recovered.
Hon. Justice Badroodin's Chawl.
One plague death in uninoculated.
Servants' Quarters of Mr. Ashby.
One case.
Mr. Tribhovandas' Bungalow.
Kitchen of Mr. Ashby.
One case.
Mr. Ashby's Bungalow.
2 cases.
Hon. Mr. Fazalbhoy Visram's house.
No cases, all inoculated.
Warden Road.

18,234. Have you any statement which shows the results in all the cases you inoculated?—No, I have only two or three instances.

18,235. Are these selected instances?—Yes.

18,236. You cannot tell me the result regarding the whole of these 1,242 persons, as to who acquired plague afterwards?—No, I cannot say that, but most of them reside in my ward, and I would have known about it if they had acquired plague.

18,237. Supposing they went away from your ward?—Then I could not trace them, but in that case the other wards would be making inquiries about them.

18,238. It is just within the bounds of possibility that you would have heard?—Of course I could not be positive about it.

18,239. (*Mr. Hewett.*) Do you think that the practice of using earth for the purpose of cleaning cooking utensils and other things may lead to people being inoculated on the hands with plague?—Yes, it might.

18,240. Have you noticed any difference in the proportion of males and females who get plague?—I find a greater proportion of females than males. I found the same thing in Cutch Mandvi also, not a very great proportion, but a little more than in the males.

18,241. Did you notice whether the buboes in the case of females were generally femoral or not?—They were mostly femoral.

(Witness withdrew.)

Mr. K. A. Dodihalkar

Mr. K. A. Dodihalkar called and examined.

18,242. (*The President.*) You are a Licentiate of the College of Surgeons, Edinburgh?—Yes.

18,243. (*Mr. Hewett.*) Are you employed in the Health Department in the Byculla Division of the town?—Yes.

18,244. Are the inhabitants of that chiefly Hindus or Muhammadans?—Half Muhammadans and half Hindus.

18,245. Is it a poor district?—Yes, a very poor district.

18,246. Is it very congested?—Yes, very congested.

18,247. Has plague been very bad there?—Yes.

18,248. Is it very bad there now?—Yes.

18,249. What is the population of the Division?—50,000.

18,250. How many cases of plague occur there now daily?—At present, I believe, it comes to nearly 25 a day.

18,251. Are they mainly pneumonic or bubonic?—Mainly bubonic.

18,252. Have pneumonic cases come under your observation too?—Yes, they have.

18,253. Have you ever noticed any case in which a patient suffering from bubonic plague has communicated pneumonic plague to another person?—I have not traced it, but one person in the same house

has got pneumonic plague while the other persons have had bubonic plague.

18,254. Have you had many instances of that?—No, very few.

18,255. Do you think that you get the majority of plague cases now?—Yes, we do.

18,256. Do the people report them?—The people do not report so much.

18,257. But you find them out yourself?—They are found in the cemetery, by the extra number of deaths.

18,258. Then you find them after the people are dead, not while they are sick?—Not while they are sick.

18,259. Do the people object to go to the hospital?—They do.

18,260. I see that you say in your précis of evidence that if you can assure them of their recovery they would go; that is rather a difficult thing to do, is it not?—They do not see the recoveries, and they do not like to go; they are ignorant people.

18,261. Do they object to going to segregation camps?—Yes.

18,262. What is their objection?—They are put to great trouble in removing their luggage and baggage, and at the same time they say that these huts will take fire at any time, and there is no certainty about their safety.

18,263. Are you at present employed on plague work?—No, I am on sanitation work.

18,264. On what sanitation work?—The removal of filth from the streets and houses, getting the w.c.'s cleaned, and, where the houses are dirty and insanitary, giving notice to the landlords to improve them.

18,265. Are you employed on disinfection only?—Not at present. I was last year.

18,266. (*The President.*) You said that when the houses are insanitary you give notice?—Yes.

18,267. Who is the judge of whether the houses are insanitary or not?—I am.

18,268. On what grounds do you proceed?—If the houses are dark and have no means of light, no windows, no ventilation, if they are damp, and the walls dirty, then I give notice to the landlord.

18,269. Do you give notice yourself directly, or through some other official?—I write to the Municipal Commissioner through the Divisional Health Officer.

18,270. Have you had occasion to give such notice in the case of a large number of houses or not?—I have.

18,271. In how many instances, roughly, have you given such notices?—In the last two years I have been working with the Health Department, and I may have given about 1,000 notices.

18,272. Have you any recollection as to the number of instances in those 1,000 in which sufficient action was taken as the result of your notice?—Some houses are demolished, and in some houses windows are made to admit more light and ventilation.

18,273. In what proportion of the 1,000 has action been taken?—About one-fifth.

18,274. What has happened to the remainder?—Their cases have been referred to the Engineering Department, and action will be taken in due course.

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18,275. In regard to the insanitary conditions, to what extent do you take notice of overcrowding?—We do not take any steps to remove overcrowding. May I mention a few of my ideas with regard to the origin of plague?

18,276. So far as you have got facts, we shall be very pleased to hear them?—In the year 1893, I was living in the Main Road in Khetwadi. My cat got a bubo on the neck which suppurated after one month, and the cat recovered. I was not aware of plague at that time, and I did not try to find out what it was. In the same month my brother got a bubo in the axilla, with high fever of 105 degrees. The fever was continuous for about a month, and then the bubo suppurated, and discharged the pus; he recovered, and he is living now. At the same time there was a little child about four years old living in front of my house. He had a small inguinal gland, with very slight fever, about 100 degrees, without any treatment it disappeared. I lay these facts before you to show that plague is not new in Bombay, and in India; but it is one of the old diseases, and the present severe form is only a new type. In Malad, a place about 30 miles from Bombay, the people are quite aware of this disease, they call it rat fever, and they describe it like this: that the rats fall from the roofs dead, and then if the people live in the house they get the fever themselves, and they die.

18,277. When did this happen?—About 15 years back. I do not know myself, but friends of mine who have been in Malad told me of this.

18,278. Do you know whether this rat fever is now present in Malad?—It is not now. 10 or 15 years back it was there, and they get it every 10 or 15 years; it is a very marshy place. About the first of this month one instance occurred. In my neighbourhood there is an institution called the Gouraki Press. There were some rats found dead in the compound, and there were two white rats that were kept in a cage in the institution for amusement. They died one after the other, and then one of the compositors got plague, and he was removed to the hospital and died there. This fact I want to place before you to show that it is not by contagion, it may be by infection, but the rats themselves got the disease in the vitiated air.

18,279. These rats were in a cage?—Yes, I went there every day, and I used to notice them.

18,280. Where was the cage?—It was on the roof.

18,281. It was sometimes taken down?—Yes, at dinner time.

18,282. Were the rats allowed to come out of the cage sometimes and run about?—No, they only placed the cage near the dish and gave them food.

18,283. Were there any cases of plague in human beings in this house or room?—Last year there was a case of plague.

18,284. I mean at the time?—Yes, one person got plague and was removed to the hospital.

18,285. At the same time or previous to the rats?—Previous to the white rats, but some rats had been dead before.

(Witness withdrew.)

MR. K. S. ENGINEER, called and examined.

Mr. K. S. Engineer.

18,286. (*The President.*) I believe you are a medical man?—Yes.

18,287. What are your qualifications?—I am a Fellow of the Chemical Society, London, and a Licentiate of Medicine and Surgery of the Bombay University.

18,288. What has been your association with plague?—As a medical practitioner.

18,289. You have no official connection?—No, only private connection with plague cases.

18,290. You come here as a private practitioner to give us your experience of plague?—Yes.

18,291. I think you have some opinions as to how plague entered Bombay?—Yes.

18,292. What are your opinions?—My opinion is that plague was imported into Bombay from Hong Kong.

18,293. On what grounds do you hold that opinion?—Because plague first occurred over those parts where articles of merchanise are landed from China.

18,294. What is the connection with China?—At that time plague was raging in Hong Kong.

18,295. When did the first case of plague come under your observation?—On the 16th of July 1896

18,296. Could you give us some account of that case?—A Parsee gentleman, a Dubash, *i.e.*, a stevedore by occupation, brought a Hindu Superintendent (muccadum, *i.e.*, overseer of coolies) to me who was working

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on the Port Trust Estates. The patient was suffering from high fever, and when I examined him I found a bubo in the left groin. Of course, at the time I could not decide whether it was a real case of plague or not; but subsequently from the observation I made, and from the experience I gathered, I can now safely vouch that the case I treated was one of plague.

18,297. How did that case originate?—The man came to me and told me that he was working somewhere near the docks, and had got a high fever, accompanied with chills.

18,298. Were there any similar cases at the same time?—On that very day I had none.

18,299. How soon afterwards did you see cases of a similar kind?—After a fortnight a similar case occurred.

18,300. Were these identified as true cases of plague?—The symptoms in both the cases were different. This second case was a pneumonic case. This case was seen with me by the late Dr. Manser. We treated it as pneumonia. Now, however, I have come to the conclusion that this case which I saw in consultation with Dr. Manser was a pneumonic case.

18,301. On what grounds have you come to that conclusion?—The pneumonic symptoms differed. They puzzled us at the time.

18,302. What difference?—The pneumonic symptoms were gradually running; they did not appear all of a sudden. When I saw the patient I found slight congestion at the left base—it was a left pneumonic case. When I inquired into the history of the case, from the symptoms one could not conclude that it was a typical pneumonic case—it was not a typical case of pneumonia; the sputum was not free, it was very scanty, and not rust coloured, but accompanied with blood. The symptoms merged suddenly, in such a way that we wondered whether it was a real pneumonic case or not. We found out subsequently, however, that it was a real pneumonic case.

18,303. You had definite symptoms of pneumonia?—Yes.

18,304. What definite symptoms?—Dullness at the left base, accompanied with tubular breathing, and increased voice sounds, and crepitations. This case lasted three days. The symptoms merged according to the grades—from one grade to another.

18,305. The patient was ill for three days?—Yes.

18,306. Did the patient die, or did he recover?—He recovered in three days.

18,307. But these symptoms were sudden?—Yes, the stages merged from one to another in such a way that we could not decide whether it was a real pneumonic case or not. I have come to the conclusion now that it was a pneumonic case.

18,308. At the time you had no evidence that it was a pneumonic case?—On the first day there was no evidence of pneumonia.

18,309. The whole time you saw this patient you were not satisfied that the symptoms pointed to pneumonia?—No.

18,310. But now you think they probably did?—Yes.

18,311. State why you think so?—To my mind it seems to me that it was a pneumonic case—only from the conclusive symptoms.

18,312 They appear to be inconclusive?—The symptoms were inconclusive in the beginning, but conclusive in the long run.

18,313. In what way conclusive?—In ordinary pneumonic cases the symptoms run from one grade to another, but in this case the symptoms were sudden, and disappeared all of a sudden.

18,314. Did you have any bacteriological examination?—No, we did not suspect anything.

18,315. To my mind it looks doubtful whether it was plague or not?—I have seen lots of cases, and I have now come to the conclusion that it was a case of plague pneumonia.

18,316. There is nothing more than you have stated?—No.

18,317. I believe you have some statement to give us with regard to the chief factors which propagate the plague?—Yes; there are three factors. 1st. Overcrowding; 2ndly. Increase in the sub-soil water; and 3rdly, Bad drains. These, in my opinion, are the factors which cause plague to flourish.

18,318. What evidence have you which enables you to say that overcrowding causes plague to flourish?—Because in overcrowded districts we find cases occurring.

18,319. Will you give us some facts?—Supposing a family occupies a room 4 feet by 6 feet, there may be 10 or 20 persons there. If a plague case were to occur there, the other members would be found to suffer as well.

18,320. Have you any examples of that?—I have seen many.

18,321. Will you state them?—I know the case of a Parsee who was sick with plague. He occupied a very small room, and he had three children by his side. He died. The next day his son was attacked, and he recovered after a time. But in that very room, within a period of a week or so, three patients were affected with plague.

18,322. How many people occupied this room?—Five.

18,323. Were they all affected?—Out of these five three were affected. They were removed from the locality. Wherever there is overcrowding we meet with many cases.

18,324. Does your experience embrace an overcrowded district as well as an overcrowded house?—Yes.

18,325. Can you illustrate that by the case of Bombay?—Of course, Bombay is an overcrowded place.

18,326. But the proportion of overcrowding differs in different parts of Bombay, does it not?—Yes.

18,327. What do you consider the most overcrowded district, and has plague been most prevalent there?—Mandvi, in my practice, I find.

18,328. Is that the most overcrowded district?—I should think so. I call Mandvi an overcrowded place.

18,329. I asked which was the most overcrowded district; are all the districts overcrowded?—I should say Dongri and Mandvi are the most overcrowded districts in my practice.

18,330. Is it within your knowledge that the largest percentage of cases to the population has occurred in these two districts?—I cannot give you the exact ratio.

18,331. Is it within your knowledge or not?—I cannot tell you.

18,332. You cannot tell us from personal experience whether an overcrowded district is productive of the greatest amount of plague?—From my personal experience, taking the localities I practise in, I consider Mandvi an overcrowded place.

18,333. But you have told me that all Bombay is overcrowded; I ask you which is the most overcrowded?—Mandvi, so far as I know. In certain chawls many persons were affected one after the other.

18,334. That, again, bears upon the overcrowding of special houses. Then with regard to the next point, what have you to say with regard to the increase in the sub-soil water?—An increase in the sub-soil water fructifies the germs.

18,335. That is theory, I want facts. Why do you say that? Can you mention a district where the sub-soil water is in a condition which you think is especially favourable for the increase of plague, and can you tell me whether plague is especially prevalent there?—At Mandvi I find the sub-soil increasing, and I find that the level of the water in certain wells had increased in those localities where I practise. If you were to examine the water of certain wells in Bombay you would find nothing but filthy water from the surrounding drains.

18,336. It is not merely the fact that the water is high, but the fact that there is filth in the sub-soil water?—The sub-soil water has a certain effect upon the propagation of plague, that is my argument. Wherever there is an increase in the sub-soil water there plague cases are found in abundance.

18,337. Can you tell me where there is a special increase, and whether plague has increased in that district?—In Mandvi there has been an increase in the sub-soil water, and there cases are still running on, and

have been doing so from the time of their commencement.

18,338. Is Mandvi the only part of Bombay where the sub-soil water has increased?—The sub-soil water has increased in Girgaum and Dhobika Talao.

18,339. Is the plague worse there?—Plague cases are found in abundance there as well.

18,340. They are found, I understand, all over Bombay; but is the total number larger in places where the soil is waterlogged?—Yes, where the soil is waterlogged.

18,341. What have you to say with regard to bad drains?—In most parts of Bombay we find the drains are badly laid, badly cemented, and badly paved, the consequence being that there is oozing from the joint connections. This tends also to increase the sub-soil water. I have noticed that wherever there is a smell from the drains plague cases occur.

18,342. Do you mean to say that in every place where you have smelt bad drains you have found cases of plague?—I have seen plague cases in those very houses.

18,343. Is there any other point bearing upon the diffusion or increase of plague which you would care to bring before us?—These factors, in my opinion, increase the propagation of plague.

18,344. I wish you had been able to give more facts, because we can get opinions to any extent. Have you had any experience of preventive inoculation?—Yes.

18,345. What experience?—I think inoculations immunise persons.

18,346. What is your experience with regard to inoculated persons?—All the persons who have been inoculated are now living, though they are scattered all over Bombay, and living in the worst possible localities.

18,347. They are all living?—Yes, they are all living.

18,348. When did you inoculate these persons?—I inoculated in 1897, 1898, and 1890.

18,349. What record have you kept?—I jot down the names, ages, and addresses according to the form prescribed by Prof. Haffkine. We send a return of these people every week.

18,350. You trace them out weekly?—Yes. We inoculate those persons who come for inoculation.

18,351. After inoculating them what do you do? How do you trace them?—After I have inoculated them I have my eye on these patients. I know them very well; they are not outsiders. I have inoculated about 600 families.

18,352. How often do you see these 1,800 persons?—Every now and then.

18,353. What do you mean by every now and then?—If any person died I should know it at once.

18,354. How?—Because they are within my range. I have to treat them—treat their families, and so on.

18,355. You have no other means of tracing the after history of these persons?—In addition to that I read the daily mortuary returns, and I do not see any of the names of these people in the returns.

18,356. These daily mortuary returns give the names, do they?—Yes.

18,357. Anything more?—Nothing more.

18,358. I should fancy there would be many persons who had not been inoculated by you who had the same name?—I could find out at once from the surname.

18,359. But there must be several surnames the same?—I do not think so.

18,360. In all Bombay?—I do not think so. I have inoculated most of our Parsees.

18,361. That is the only way in which you can identify them. I suppose?—No.

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18,362. (*Mr. Cumine.*) Have you measured the height of the sub-soil water in the various quarters of Bombay?—I have not measured it, but I have seen it with my own eyes where the borings were taken by the Municipality.

18,363. Do you know what the *relative* height of the sub-soil water is in different parts of Bombay?—Yes.

18,364. What is the height of the sub-soil water in Girgaum?—About 12 feet.

18,365. What is it in Nagpada?—I cannot say. I am not interested in that locality.

18,366. Then you do not know the relative height of the sub-soil in each quarter of Bombay?—No.

18,367. If you do not know the relative height of the sub-soil water in the different quarters of Bombay, how can you say that there is a connection, a relation, between the height of the sub-soil water in the different places, and the extent of plague in those places?—Where there is an increase I find many more cases cropping up. In certain parts of the town, wherever I have seen the relative height increasing, there I have found many cases. According to your statement I cannot tell you.

18,368. You spoke about leakage from the drains; have you inspected all the drains of Bombay to find out which leaked most?—I cannot say in which locality the drains leak most, but from my own personal observation I can say that in most parts of the town there is a leakage.

18,369. But have you inspected them all to find out which of them leaked most?—I cannot answer that question.

18,370. How many of these 1,800 people have died since you inoculated them?—Not one.

18,371. How long is it since you inoculated them?—I commenced my inoculations in January 1897.

18,372. So that in two years out of 1,800 people no one at all has died?—Not one of them has died.

18,373. You say that these 1,800 people represented 600 families; and that if anybody had died you would have heard of it, because there is always somebody from the 600 families coming to be treated. Do you mean to say that there is always someone ill amongst them?—I generally see them once a fortnight.

18,374. Even if one member came to be treated, the whole family would not come, would they?—I generally inquire how they are after inoculation.

18,375. (*The President.*) Is there much sickness among these 600 families?—In any community where there is a large number of men, every now and then we find certain seasonal diseases cropping up.

18,376. Is there much sickness amongst the 600 families?—Not much, I should say.

18,377. How can you see them frequently if there is not much sickness?—Some come for ordinary treatment.

18,378. Do you mean when they are not ill?—When they are casually ill.

18,379. If they are not ill, and there is very little sickness among these families, they can only come to you very seldom?—At the same time I generally inquire after them.

18,380. Do you go to see them?—No. I inquire after the relatives when they come to see me. I see them once a fortnight.

18,381. What do they come to see you for?—For treatment.

18,382. But you say there is very little sickness?—In cases where I have performed inoculations I inquire of the relatives.

(Witness withdrew.)

Major Richard Baker, I.M.S., called and examined.

Major R. Baker, I.M.S.

18,383. (*The President.*) I believe you are in the Indian Medical Service, and have the degrees of M.A. and M.D.?—Yes.

18,384. You have had experience in Upper Sind and Satara?—Yes.

18,385. In the first place, what have you to tell us with regard to the measures which were adopted?—The measures adopted in Sind were for the early detection of cases, for the isolation of the sick, for the segregation of contacts, and health camps. In addition to that in

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Sukkur we had a fourth camp, which was used as a detention camp. It was an observation camp for people coming in and going out of infected quarters. The measures for the detection of the sick at an early stage were various. Small rewards were offered for information; we made private inquiries, and we had search parties.

18,386. What rewards did you find necessary; what sum did you offer?—Very small sums—eight annas and one rupee. Private inquiries too, were made. A number of reliable officers were always deputed where plague had broken out, and we made as many inquiries as possible ourselves, also through our Government servants and neighbours. In addition to that there were search parties.

18,387. Was it a voluntary or paid organisation?—We had both. Speaking for myself I am inclined to think that the voluntary organisations were more useful in their results than military search parties.

18,388. I believe you employed these measures in a large number of towns and villages?—They were employed in Sukkur. In Shikarpur, which was curiously exempt from plague, I told the people that if they used all possible precautions themselves they would possibly avoid the necessity for sending military search parties. I am inclined to think that that had a very good effect, because the natives did not care for military search parties.

18,389. You were employed in Hyderabad?—For a short time.

18,390. And you were also employed in Daharki, Tatta, Karad, Lanauli, and Igatpuri?—Yes.

18,391. And you adopted the measures which you have described at all these places?—Yes. I may say that my functions were more supervisory and advisory than actually executive, because Mr. Wingate employed me in that capacity very largely. He sent me all about the country. In Tatta I found that this elaborate method of having so many camps was by no means necessary. The great dread was that we should have a camp epidemic in the segregation camp, but according to my experience there never was a camp epidemic in the segregation camp. If a segregation camp is properly organised and looked after, and each inmate inspected every day, and each case as it occurs at once sent to the isolation hospital, and proper methods taken for disinfection, I have never, in my experience, seen any symptoms of even the beginning of a camp epidemic.

18,392. What course did you there adopt?—I combined the segregation, health, and detention camps.

18,393. What did that leave you with?—A hospital for isolation, and one large camp. I found that workable for this reason, that when a disease breaks out in a place there is an exodus from that place, and as the disease abates there is a return of the fugitives. The amount of camp accommodation for both is practically constant, otherwise it would not have been workable. Of course it was much more economical, and much more easily managed than a series of separate camps.

18,394. Did you usually get very early information of the first imported case?—In Hyderabad we did, and in Sukkur at the beginning of the disease we did not: but when the people realised the nature of the disease, many of them came to inform us themselves—even the relatives came to inform us.

18,395. There were some villages in which you got early information of the first importation?—Yes.

18,396. What measures did you adopt in those villages?—The imported case was removed to hospital, and the people in the house were removed to the segregation camp, and the house itself was disinfected.

18,397. What was the result?—Mr. Dodgson, the Collector of Hyderabad, and myself, are distinctly under the impression that our efforts in that direction did stall off the occurrence of local plague in Hyderabad for some time, because there were at least five cases of imported plague spread over a long time before we had a local case, and we can trace the reason why we got that local case.

18,398. In any of these places did you actually prevent plague from becoming local?—Yes, for a time, that is our belief.

18,399. Finally plague became indigenous?—Yes.

18,400. When plague became indigenous what did you do?—When it became indigenous we cleared out larger areas.

18,401. Did you find that effective?—I cannot speak for Hyderabad. I was transferred from Hyderabad. I was doing work as Civil Surgeon of Hyderabad before I took up my special plague duties. Subsequently I was busily engaged in Sukkur.

18,402. Was Hyderabad the only place in which you partially evacuated areas?—I may mention that when I first went to Sukkur I found one imported case, and had it removed. Apparently, however, there was another imported case which I failed to move, and within a week or ten days I was sent to go to Sukkur again. I found that this second case had established a large centre in a temple, and measures had to be taken.

18,403. What was done; was the area evacuated?—This case occurred in a temple, and many other cases occurred there. People have a tendency to go to temples to die. This man went to the temple to die, and he infected the people in the temple. A large number of people were found in the temple dying or dead. The temple was dealt with the aid of a gang of 20 coolies, of whom 5 died of plague in a week, owing to the fact that there was no previous disinfection of the temple.

18,404. Did you evacuate the area?—That area was evacuated, but not properly evacuated.

18,405. Partial evacuation took place?—I would prefer to call it ill-directed evacuation. The result was unsatisfactory.

18,406. Was there entire evacuation afterwards?—Afterwards the disease spread over the place. I can give you a very successful instance of evacuating a large portion of a town. The part of Sukkur known as Gharibabad was infected. There were supposed to be 1,000 people in that part of the town, and in 16 days they died, at the rate of 25 a day, leaving 601 people in the town. During those 16 days, with the aid of the Railway Company, we erected a very complete camp—the most complete I have ever known. On the 17th day the 601 people were moved out.

18,407. With what result?—With the result that the 601 people escaped plague, except one who developed the disease within 24 hours of his removal to camp.

18,408. You did not have another case in the segregation camp?—No.

18,409. What happened in the portion of the town from which they were evacuated?—The people went back gradually.

18,410. Plague stopped there?—There was nobody to get plague. Absolutely every individual was segregated. The whole of this outlying portion of the town, which was quite separate, was evacuated.

18,411. The other parts of the town were not infected?—Not to that extent at all.

18,412. Did you have any case in which you at once evacuated a whole town or village without having first carried out partial evacuation?—No, I do not think so.

18,413. Your experience has been of cases in which there has been partial evacuation, afterwards followed by complete evacuation?—Yes.

18,414. As a rule, you found partial evacuation sufficient?—Yes, temporarily.

18,415. But on recourse to complete evacuation it was successful?—Yes.

18,416. How long did you generally keep people out of a town or village?—In Sukkur the people were kept out of the town longer than would otherwise be necessary, to admit of complete disinfection of their houses.

18,417. How long?—They were kept out of the town for about a month.

18,418. In the meantime the houses were disinfected?—Yes.

18,419. And the people were then allowed to go back?—Yes.

18,420. Was there any recrudescence of plague in the disinfected houses?—No.

18,421. Total evacuation combined with disinfection have been very satisfactory in your experience?—Evidently so.

18,422. In what position do you put these measures in regard to other measures?—In the first position.

18,423. I believe you have had some experience with regard to disinfection alone, apart from anything else?—Yes.

18,424. What are your views as to the value of disinfection?—Perhaps the best way that I can point that out to you is the danger of the want of disinfection, and the value of it in other places. There are cases as to the value of disinfection in Karachi, which came under my notice incidentally, where the plague wards were daily drenched with perchloride of mercury.

18,425. Plague wards?—Wards of the Plague Hospital —the floors.

18,426. But at the hospital there were many other conditions besides; I would rather you would take ordinary habitations, because hospitals are not comparable with ordinary habitations?—It would be easier to give you an example of the failure of disinfection where segregation is incomplete, that is to say, where the unaffected inmates are still left in the houses.

18,427. What is you experience?—At Igatpuri, during the rains, it was extremely difficult to segregate, I was told, because of the want of places in which to put the segregated people. They had no accommodation, and consequently segregation was not attempted, but each case was removed to hospital as it occurred, and each house in which plague occurred was disinfected. The result of that was that other cases which were still in the incubation stage at the period of the isolation of the infected person developed, and fresh cases continued to develop in the houses, each case being followed by the isolation of the patient and the disinfection of the quarters. In one instance what was found to be necessary as often as seven times.

18,428. Your view is that disinfection alone is inefficient?—Certainly.

18,429. Have you any experience of protective inoculation?—I have no statistics to offer. My impression is a general one.

18,430. What is your impression based upon?—Merely upon observation but without figures, because I had nothing to do with inoculation myself. In the course of my inspection work I had occasion to observe what the effects had been in a general way. The most striking was at Lanauli, where the people living on one side of the railway line had been subjected to inoculation, and the people on the other side of the line had not been subjected to inoculation. In the place where inoculation had been made use of the town was thriving and full of people, and the other side of the line was absolutely empty. I came upon this merely by accident.

18,431. One side had a greater amount of plague than the other?—One side had plague and the other had none.

18,432. How much plague was imported on each side of the line?—I made inquiries and was told there was no difference, but I do not know about that.

18,433. Is that your whole knowledge with regard to inoculation?—Yes. I have no special knowledge of it.

18,434. (*Mr. Hewett.*) I understood you to say that you considered that at Hydérabad you practically stopped the possibility of infection by dealing with the imported cases as they occurred?—Yes.

18,435. Dr. Henderson, who was there after you, said that the infection of rats followed the imported cases and occurred before the local cases; have you observed that anywhere?—When it does it occurs almost simultaneously with the occurrence of the first indigenous case.

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18,436. Do you mean that it occurs simultaneously with the first indigenous case that is detected?—Yes. I have never heard of a distinct case of rats having been found dead any length of time before the detection of the indigenous case.

18,437. Do you think it possible that an imported case occurs and infects the place and then the rats become infected, and that simultaneously with the infection of the rats human beings become locally infected?—That is the way in which I think the disease becomes local.

18,438. Whatever you do with the imported case you have always that danger?—Yes.

18,439. Did you have in Sukkur much pneumonic plague, or was it mainly bubonic?—My experience was in walking through the hospitals, which I did every morning, although I was not in charge of them. I do not know the normal proportion, but I should say there was a fairly large number of pneumonic cases.

18,440. Did any example come to your notice of a bubonic case communicating pneumonic plague or *vice versâ*?—No, I have no information upon that point

18,441. The temperature at Sukkur was extremely high, I think?—Yes.

18,442. Did you notice that the disease gained strength as the temperature rose?—Up to a certain point it did.

18,443. The rise of temperature did not seem to check it?—No.

18,444. What is the population of Sukkur?—There are three towns really—Sukkur, Rohri and New Sukkur, and I should say the total population was about 40,000.

18,445. What number of people had you in the segregation and health camps there?—600 people.

18,446. Only 600?—Only 600 in the only camp worth considering, the others were very small.

18,447. Therefore there was very little evacuation of the town?—The evacuation was of an outlying portion of the town.

18,448. The main portion of the town remained unevacuated?—Yes.

18,449. When a plague case occurred in the main portion of the town, what measures did you take?—Isolating the case and segregating the people, but the numbers in the town itself were very small compared with the numbers at Gharibabad.

18,450. You left the neighbours alone?—No, we put the neighbours into a segregation camp, but the numbers there were very small compared with the numbers at Gharibabad, so that that would hardly affect the question.

18,451. (*Mr. Cumine.*) Did you disinfect the clothing and effects of those 601 Gharibabad people when you took them out into the health camp?—Yes.

18,452. Do you think that is necessary to make the plague stop on evacuation?—Yes, I think it is safer at any rate.

18,453. With regard to Lanauli, does the railway line divide it equally into two parts?—Fairly equally, probably the greater portion were on the healthy side.

18,454. Is there any difference between the classes of people who live on the two sides?—No.

(Witness withdrew.)

Mr. G. W. ROUGHTON called and examined.

Mr. G. W. Roughton.

18,455. (*The President.*) What district are you prepared to speak about?—Kolaba and Fort Northern.

18,456. (*Mr. Cumine.*) You began plague work, I think, on the 31st December 1896?—So far as the Volunteer Artillery was concerned, I did.

18,457. With whom did you begin the work?—With the members of my corps, the Bombay Volunteer Artillery; but that was nothing to do with the house-to-house inspection, which was also worked. That was when we started doing actual plague work at the request of Dr. Weir. That was on the 31st December 1896.

18,458. What did you begin to do in March 1897?—I was appointed President of the Justices, who had been asked by Lord Sandhurst to look after the work, and I had the whole of the district from the Victoria Terminus to Kolaba under my control.

18,459. That would include the two parts called Fort and Kolaba?—Yes.

18,460. Did you retain the whole district throughout?—No; as a matter of fact during the first three or four weeks the work was confined, as far as I had anything to do with it—Mr. Raikes being my assistant—to work at Kolaba. Fort Northern was then a separate district. Although I was appointed plague authority for both districts, really we confined our attention to Kolaba.

18,461. The Upper or the Lower?—Both. From Sassoon Dock to Kolaba was not much trouble.

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18,462. When you began this house-to-house visitation in March 1897 who formed the search parties?—They were entirely members of the corps with the addition of Mr. Raikes and Mr. Howard, Mr. MacDonald (not Mr. James McDonald), and one or two officers of the Preventive Department.

18,463. The plan adopted, I think, was to meet every day at a given rendezvous, and decide what houses were to be searched?—That is so. I have here a map* of the Kolaba district, and I have marked on it with a red cross every house which is the centre of infection.

18,464. Was it possible for cases to escape detection or were the search parties too numerous for that?—At first it was possible, but eventually it was absolutely impossible. Then there was another thing; as it was seen that the whole agency was European, it was impossible for cases to be imported. That is what we had to struggle with at first. The result was that in something less than a month we had driven the whole of the plague out of our district. Of course it went to others, but that we cannot help.

18,465. In the first epidemic from March to June 1897, I believe some resistance was encountered?—Yes, a great deal; but in the second, from November 1897 to March 1898, there was no resistance encountered at all, except in Hamal Lane where there were five Muhammadan houses. That was the only place.

18,466. Did you find the inhabitants willingly present themselves for examination?—Yes. I never made so many friends in my life. You must remember that there is a great deal of zulm (oppression) in this sort of thing, but the remark which was commonly made to us was "Come every day, and then there will be no zulm." They were quite pleased to see us. We got to know pretty well everybody in the district, and everybody in the district was seen once at least in three days. I had such a large staff that, the place being not very large, there was not enough work for us. After the first three weeks we knocked off into double parties. You have to bear this in mind, that the Europeans working there knew the language, more or less, and could explain the matter to the people, and they did not attempt to interfere in any way with their caste prejudices. We knew what they were, and at the same time while enforcing the law we did what to our minds was right.

18,467. I think some houses were visited every day, and some not more than twice a week?—That is so.

18,468. Which were the ones visited every day?—Those in Hamal Street and Borah Lane.

18,469. At first were the houses in which plague cases had occurred vacated?—No, not in the first epidemic.

18,470. Later when the second epidemic occurred were they all evacuated?—Every one, if a second case occurred therein.

18,471. Had plague at that time become virulent?—There was a most extraordinary difference in the two epidemics. During the house-to-house visitation in the first epidemic you could tell by a man's eyes whether he had the plague or not. In the second epidemic there was often nothing of the sort; there were none of the usual symptoms. There was no furred tongue, and no *facies pestica* in a man's appearance, no swellings in the groin or neck or anything. That was a thing which struck me very much. In the first epidemic I could identify plague at once, although, of course, I am only an amateur.

18,472. Did you find that more people died in the second epidemic or less?—I have not considered that question, but I am quite certain of this, that the third epidemic is much more virulent than the other two. I hope to resume my work again to-morrow.

18,473. Did you find that cases occurred in fresh houses in the second epidemic, or in the houses in which it had occurred the year before?—It was practically entirely confined to those in which the epidemic had occurred the year before; they occurred even in the same rooms. After that the rooms were absolutely stopped from further occupation.

18,474. For how long?—For one month. They were the orders we got from the District Medical Officer.

18,475. Did you find when they were re-occupied after a month that cases occurred in them again?—In one instance only.

18,476. When did the first epidemic end in Lower Kolaba?—Practically a month after we started. We started, I think, in March, and it ended about a month after. We had the whole thing under control then, and there were none but sporadic cases.

18,477. When did the second epidemic begin?—In the following November.

18,478. Was the same system adopted?—No, because we had not the same staff. We did not have the European troops, and we did not have the Native soldiers to assist us, but so far as we could we adopted the same system. I have brought for the inspection of the Commission a photograph of the Staff on the first occasion (handed to President). We had 13 soldiers and a certain number of Native soldiers and some Policemen in the first epidemic; in the second epidemic we did not have them.

18,479. What did you have in the second epidemic?—Nothing but a few Policemen. The rest of the work we did ourselves.

18,480. When did you get Lower Kolaba practically free of plague in the second epidemic?—About a month after we started work. Then I wrote to the Chairman (Sir James Campbell) stating that it was useless to waste our time in Lower Kolaba, because there was nothing to do. I said that he had better transfer the rest of us to Fort Northern where we could do work, and leave four men at Kolaba, which was done. They were quite sufficient; we were so much in touch with the whole district that every man who was engaged on the work knew every house.

18,481. What results have you observed as to the effect of light and air on the disease?—My belief is that it does not matter a bit whether you have light, or whether you have air. I have formed that opinion on three particular houses which were all upper-storeyed houses, in which not a single case occurred on the first, second, or third floor, but in which several people died on the top floor. Of course, the tiles had not been removed, but that is the fact. I have said in my précis of evidence that I cannot understand how otherwise the disease can be anything but air-borne, unless, of course, the unfortunate persons went outside and contracted the disease, and then came back to their homes to go through with it. But that is incompatible with one particular case. There were three barbers living in the same house, one working in the Fort, one at Kolaba and one in Fort Northern. Those three men were perfectly well one night and were dead the next morning.

18,482. Do you know whether it was pneumonic plague or not?—No, that I canot tell you, but Dr. Barry can, because he was there.

18,483. Do you know whether the other inhabitants of the house were in the habit of going to the bazars to earn their living?—Undoubtedly; they were all Ghatis (mill-hands). They all worked in the local mills at Kolaba.

18,484. Was there any plague in the local mills at Kolaba?—Yes; I took two cases out.

18,485. Was there much plague there?—No, I do not think so.

18,486. May it have been that those mill-workers were exposed to less infection than the three barbers; that the barbers may have gone into more highly infected bazars?—One of the barbers shaved nobody but the officers of the Royal Artillery at Kolaba.

18,487. After having had experience of two epidemics what is your general conclusion as to the way in which plague can be overcome?—My opinion is that it cannot be overcome except by the strictest supervision by Europeans—Europeans who know the natives and know their habits and customs. Secondly, if segregation is not to take place, the people must be segregated in their own houses and not allowed to go out, so as to let the plague die out in each house. That is my opinion. I should adopt exactly the same system as they adopted in London during the great plague epidemic in 1666 as related by De Foe. I would lock the house up and not let a single person come out; but I would give them the option of going to a segregation camp if they chose. If you do that you have a chance of stamping the plague out, but not otherwise. It must be remembered that all the people are fatalists.

* Not published with the Proceedings of the Commission.

18,488. If plague is carried by rats how would locking the people up in a house prevent it spreading to another house?—It would die out in time, but of course not so quickly. It must be remembered that these people are not afraid of death in any way, so far as I can make out. What they want is not to be interfered with, and, if they have got to die, to be let die. That is the only way to prevent plague spreading. I have had 20 years' experience of natives in this country, and know something about what their prejudices are.

18,489. (*The President.*) Have you anything else you would like to add?—My evidence has related hitherto entirely to Kolaba. I should like to say with regard to Fort Northern that it is a very much more difficult district to work. In the first place it is not, like Kolaba, a district where you could cordon a certain section and deal with in that way. The inhabitants are of a very different character. Therefore it was not possible to adopt the same stringent measures in Fort Northern as we did in Kolaba. We did not succeed in quelling the epidemic there in anything like the same time we did in Kolaba, but we did it eventually. I submit that if you are going to endeavour to work on the present lines at all you must have a very strong hand, a hand not afraid to carry out measures but at the same time one which is not prepared to quarrel with the people.

(Witness withdrew.)

Mr. A. Britto, called and examined.

18,490. (*The President.*) What are your medical qualifications?—I am Licentiate of Medicine and Surgery of the Bombay University, and Deputy Health Officer C ward, Bombay Municipality.

18,491. I believe you have been engaged in plague operations?—Yes. At present I am in C ward, but at the commencement of 1896 I was appointed to Mazagaon and Tarwadi.

18,492. Have you any information as to the origin of plague in Bombay?—With regard to the first epidemic there is very little information of a definite character, but there is very little doubt that the disease has been imported into Bombay from outside, because prior to the outbreak there was an epidemic at Hong Kong and in Asia Minor. The conditions existing in Bombay are favourable for subsequent outbreaks once the disease has been introduced.

18,493. Do you mean the plague has never disappeared?—Yes.

18,494. There have been recrudescences?—Yes.

18,495. What knowledge have you as to the contagiousness of plague?—Plague is contagious as well as infectious.

18,496. How do you distinguish between the words contagious and infectious?—By contagious I mean by touch, and infectious by introduction into the blood actually by injection.

18,497. What do you say with regard to contagion?—In contagion by human agency, a plague-stricken person acts as a sort of centre of infection by infecting the room or rooms in which he may be moved, and then there is the careless disposal of excretions, chiefly the vomits and the dejecta, all of which contain plague bacilli.

18,498. Do you know if the vomited matter contains plague bacilli?—Yes.

18,499. What is your knowledge about that?—I have not made a bacteriological examination.

18,500. Now, with regard to clothes. What is your knowledge with regard to their being infectious or contagious?—I go on house-to-house visitation, and very often clothes worn by individuals suffering from plague are kept away from us, and these clothes are subsequently used by healthy persons. I really believe the infection is taken by wearing these clothes.

18,501. Do you know any case in which clothes have conveyed infection?—I cannot give you a case, but I can give you a case in which the bedding used by the patient has caused it.

18,502. You withdraw the statement regarding clothes?—Yes. With regard to bedding there was a case at Mazagaon in which the bedding which had been used by the patient was kept by the people; they were not given to us to be destroyed. That bedding was afterwards used, and we had other members of the family who got plague.

18,503. Are you aware whether the other members of the family came in contact with the original patient?—Yes, they did, except the younger members.

18,504. How can you exclude infection by other means? Why do you restrict it to the bedding? Have you any other instance?—No.

18,505. What are you prepared to say with regard to infection?—Infection is taken by wounds and abrasions in the surface of the body and the mucous membrane.

18,506. What is your knowledge about infection through wounds and abrasions in the skin; have you seen cases?—Yes. There have been cases in which people have had slight eruptions. It is not uncommon to see people in Bombay with scabs. Then, also, the natives of Bombay have a kind of eruption which is more of an eczematous character, generally about the loins. I have met with cases of plague in which these conditions existed, that is, the conditions of eczema and scabs, and I have no doubt the infection has been through these.

18,507. Have you had any experience of infection by food?—No.

18,508. Do you know of any case of plague in which the infection was introduced by the digestive system?—I do not know of any direct case.

18,509. What is your knowledge with regard to infection by the respiratory system?—There was a case of pneumonic plague which occurred in a house in the month of August, in the section in which I work. There was one pneumonic case which was concealed from us. Subsequently I found two cases in the same house, one was the manager of the firm, and the other was a woman who was also connected with the firm. Both she and the manager got pneumonic plague. I examined them both at the very commencement, and pronounced the cases to be pneumonic plague. Both of them died. In the case of the manager, one of the attendants subsequently developed plague and died in another house.

18,510. You observed one case of pneumonic plague from which two cases directly proceeded, and then a third indirectly?—Yes.

18,511. How do you know that in any of these cases the plague was introduced by the respiratory passages?—From my observation. The man was lying on a bed on the floor, and was being held by the attendant. The attendant had a little chatty which contained the plague sputum. They generally have a small copper vessel into which the man spits, and this is generally held up to the man's mouth by the attendant. Besides being in such close attendance, it is natural he may have inspired the same air.

18,512. The attendant took plague?—Yes, and he died.

18,513. Might he not have soiled his hands?—Yes; he was holding the patient besides.

18,514. Might not the sputum have been introduced into his own mouth and face by coughing?—Yes, but that would be direct.

18,515. That need not be by the respiratory passages; it might be by the alimentary canal?—If it went to the alimentary canal the form would be enteric.

18,516. Not necessarily. You have had some experience of preventive inoculation, I think?—Yes. When I was in charge of Mazagaon, I made a number of inoculations in company with Major Bannerman, I.M.S., in the most infected centres. This was in March 1897. During the next recrudescence in November up to the end—that is March—I did not observe any cases of plague amongst those who were inoculated.

18,517. How many did you inoculate?—There were about 200 inoculated.

18,518. What record did you keep of those inoculations?—They were in the same locality, and I had occasion to visit them every day in house-to-house visitation.

18,519. For what period did you keep them under observation?—During the time the epidemic raged, which was five months.

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18,520. Did none of those people leave the place during those five months?—No; they are low-class people, and could not afford to leave unless we took them ourselves into camp.

18,521. Did any of them die in that time?—Not to my knowledge, not those who were inoculated, because we directed special attention in cases of plague to observe whether an individual who got plague had been inoculated, and by whom.

18,522. Have you any other 200 people in similar conditions of life and locality who were not inoculated to compare with them?—The contrast was obvious from the fact that we did not get any cases among the inoculated, whereas we did get plague among others living in the same houses.

18,523. Will you give us some examples of that?—There is a place in Mazagaon, known as Telang's property, in which the chawls are very large. In one of the houses there we did as many as 26 inoculations. We had a number of plague cases there, but we did not have one among those persons who were inoculated. There is plague there even now. Another instance is a place known as Bawan's chawl. I do not know the exact number we inoculated, but we did not observe any cases among those, whereas we got plague among the other residents of the place, so much so that the place had to be evacuated, and the people removed to a health camp.

18,524. Are these inoculated people kept under observation now?—I do not know; I am not in charge of the district now. I have been away since June, and am now in charge of C ward.

18,525. You do not know whether they have had plague since?—No.

18,526. Did you make any selection of the persons inoculated?—No, unfortunately we have no choice in Bombay, because the people are very loth to be inoculated.

18,527. You take all you can induce to be inoculated?—Yes. We try and induce them to be inoculated, and then if they consent we inoculate them.

18,528. Even very young persons?—Not very young persons. I have not inoculated babies of one or two years.

18,529. What is the youngest?—Five years.

18,530. If you saw a person obviously unwell, would you inoculate that person?—No, and I would not inoculate contacts.

18,531. Have you given much attention to the clinical study of plague?—Yes, I was in charge of two hospitals in Bombay.

18,532. The symptoms have been fully described in your précis of evidence: have you encountered any symptoms different from the ordinary classic symptoms of plague?—No.

18,533. What do you mean by the cerebral variety?—That variety in which the head symptoms, that is, delirium and coma, are most marked, and where the temperature is not reduced by any treatment—where the temperature rises and the patient dies.

18,534. They are always fatal?—Yes.

18,535. Have they buboes?—No. These cases are rare. I have not noticed any with buboes.

18,536. How do you diagnose the cerebral variety?—First by the facial expression. There is a stolid anxious expression of the face, knitting of the brow, and drowsiness, and then the peculiar condition of the tongue, and the delirium which follows. The cases are rapid, and terminate in death.

18,537. Supposing you had all these symptoms and death did not occur, would you come to the conclusion that they were not cases of plague?—I have not come across any such case, but I should take into account the rapidity with which the symptoms came on.

18,538. What do you mean by the enteric form?—That is where the symptoms simulate enteric fever, but in which the symptoms are more rapid.

18,539. What symptoms of enteric fever are similar?—The first is diarrhœa with the characteristic stool.

18,540. Enteric stool?—Yes.

18,541. Pea-soupy?—Not exactly—yellowish.

18,542. A biliary stool?—Yes.

18,543. Not therefore characteristically enteric?—Not entirely pea-soupy. We do not get any cases of real enteric among the Indians. Then there is great tenderness in the right iliac, tympanitis, and delirium, which goes on from the second to the fourth day.

18,544. What is the form of the temperature curve? Does it resemble the ordinary temperature curve of enteric fever?—I have not noticed the chart very carefully.

18,545. Are there spots?—No.

18,546. What is there in addition which brings them into the category of plague cases?—In some of these enteric cases the mesenteric glands are enlarged.

18,547. Is this observed before or after death?—Previous to death. There is excessive tenderness in the right side.

18,548. The abdomen is generally tympanitic?—Yes but not at the commencement. At the commencement there is gurgling and pain, but not intense tenderness. There is a feeling, though not in all cases, of swollen glands.

18,549. In what region of the abdomen do you feel swelling of the glands?—In the right iliac.

18,550. Then you have specified the hæmaturic variety?—Yes; I only saw one case of that. The man passed bloody urine. Then he got a fit which I put down to uræmic symptoms.

18,551. Was this blood in the urine due to hæmaturia or hæmaglobinuria?—I did not examine by the microscope.

18,552. Have you ever found albumen in the urine of plague cases?—Yes.

18,553. Very often?—In most of the cases.

18,554. Have you seen tube casts?—I have made no microscopical examination.

18,555. Have you made any *post-mortem* examinations?—No, but I have seen the appearances.

18,556. What are the characteristic appearances?—That refers to the position in which the patient lies. We are often called out to see a man who is found dead in the street, and then we have to give our opinion as to whether it was a plague case or not. In the absence of glandular enlargement we look to the position of the corpse; the thumb is generally flexed into the palm, the eye-lids are half open, and the cornea is glistening. Then there is the appearance of the face. There is generaliy a livid, bluish appearance of the whole body. That is how we come to the conclusion that it is a case of plague in the absence of enlarged glands.

18,557. Putting aside the hæmaturic case, have you seen any œdema of the surface in other cases, either after death or immediately before death?—No.

18,558. If there had been œdema it would not have escaped your notice?—It would not have escaped my observation.

(Witness withdrew.)

(Adjourned till Friday next.)

At The Secretariat, Bombay.

FORTY-NINTH DAY.

Friday, 17th February 1899.

PRESENT:

PROF. T. R. FRASER, M.D., LL.D., F.R.S. (*President*).

Mr. J. P. HEWETT.
Prof. A. M. WRIGHT, M.D.
Mr. A. CUMINE.
Dr. M. A. RUFFER.

Mr. C. J. HALLIFAX (*Secretary*).

Mr. REGINALD GILBERT called and examined.

Mr. R. Gilbert.
17 Feb. 1899.

18,559. (*The President.*) You are a solicitor of the High Court of Bombay?—I am.

18,560. Where do you reside?—I reside in Bandra, a suburb of Bombay.

18,561. What is the population of that suburb?—About 20,000.

18,562. What is the nationality of the inhabitants?—About half are native Roman Catholic Christians.

18,563. When did plague occur in Bandra?—At the end of 1896.

18,564. When did your experience of the plague there commence?—My actual work did not commence till the 12th of February 1897.

18,565. What was the condition of matters with regard to plague generally when you commenced work?—I returned from Australia on that date, and the epidemic was very bad. Many members of the Municipality had left Bandra, and the Municipality was in a state of chaos, and almost bankrupt. I made the following offer to Government, namely, that I should have entire and sole charge of plague operations in Bandra, Government placing in my hands all moneys required, Government to appoint me President of the Bandra Municipality, I not being accountable or responsible to that body for my plague work, but responsible and accountable to Government only. This was accepted. When I commenced work a large number of people had voluntarily left their houses, and were residing in huts in the fields. I at once turned all the people out of their houses and got them to live in huts.

18,566. The whole of the inhabitants of Bandra do you mean?—Nearly all; the better classes did not turn out. Those that lived in detached houses, for instance, all remained in.

18,567. About how many of the inhabitants were included in this evacuation?—About three-quarters of the inhabitants were hutted: certainly more than half.

18,568. About 10,000?—Yes.

18,569. Had you much difficulty in effecting this removal?—Yes. The following extract from a letter which I wrote to the Collector of Thana on the 8th April 1897, illustrates some of the difficulties I had: "I beg "to say that there have been a large number of cases "of plague here amongst persons who have been living "in huts and segregation camps for ten days and "upwards. I believe I have personally taken or "assisted in taking approximately 70 cases of plague "from huts in which the residents have been living "for at least ten days away from their villages. I "think I can at least place the number at 80, and the "only reason why I do not give more is that a large "number of Danda villagers left their camp and went "back to the village, thus getting infected again, and I "cannot safely trust my memory as to what number "of these were taken to hospital. A large number of "native Christians went into huts. These seldom "returned to the villages, and I found cases amongst "such native Christians who have been encamped for "more than a month. One case of a Christian I "especially investigated, as he was taken with plague "at least 14 days after removing into a segregation hut. "The only way he could have got the contagion was "when he went into Danda with his bullock cart and "removed furniture from the houses of infected people. "I should mention that in many of the above 80 cases "the camps were very dirty, and the fæces of those "encamped there were often quite close to the huts. "On one or two occasions I found persons affected "with plague engaged in making chappattis for the "family so as to distract my attention. So often has "this trick been practised on me that I make a "habit of examining very carefully persons engaged "in cooking when I am doing inspection work. I was "not aware that anyone dare allege now that the "disease is not communicable by contagion. I am not "a medical man, and only give my views with great "deference. I have formed a strong opinion upon the "matter, and that is the disease is very contagious at "first, and less so afterwards; that people who live "in upper storeys escape it most; that it is most contagious in houses which are free of ventilation. I "also believe that those who sleep in very open places "will never catch it." I had great difficulty in getting the people of Danda, a village of about 3,000 fishing people, to turn out. They suffered very badly from plague. They abandoned their dead, and would not assist in their cremation. They also left children with their plague-stricken or dead parents. I had a hospital shed built and properly equipped with a proper staff. Most of the native Christians voluntarily came to hospital at first, but afterwards they concealed their cases. This was, I believe, because so few lived. A few Hindus came voluntarily and one or two Muhammadans. The rest we had to take by force.

18,570. What do you mean by taking them by force?—Actually taking them by force.

18,571. By what instrumentality?—I had to do it myself once or twice, and I had orderlies and police.

18,572. Police and soldiers?—Yes, I had two or three native soldiers with me, and also the Municipal Inspectors and so on.

18,573. What was their opposition due to?—They would not come, so we had to take them on charpoys, or something.

18,574. On what grounds did they oppose the removal?—They thought they were going to be poisoned, and they objected altogether to going to hospital. They believed we intended to poison them, and in many instances they would take no food or drink, or any medicine whatsoever. We had one case of a little Hindu boy who lived for many days, and I believe would have recovered if his parents would have allowed us to give the child food, stimulants, and medicine. They would not yield to all the persuasion we showered on them. Bandra is surrounded by fields, and it is easy to find places for the people to camp out. By the end of April 1897 plague was virtually stamped out of Bandra. I reckon nearly 1,000 people died from it.

Mr. R. Gilbert.
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18,575. How long did you take to effect this evacuation?—A great deal had been done voluntarily between Christmas and the 12th of February, when I took charge. Directly I took charge I would not allow anybody to stay in. They all went out—virtually all of those that lived in huts or houses that were crowded.

18,576. How long did it take?—Some were out before the 12th of February, when I took charge. It was then about its highest point. I might mention the following facts. A native family was camped in the fields in hut A for upwards of five weeks free of plague. A case of plague occurred on the 31st March, which they concealed and placed in a small hut B, 50 yards off A. This case died 31st March 1897. Huts A and B were burnt, and the family moved to hut C. On 4th April another case occurred in hut C, and the family erected a fresh hut D, to which they moved. On 9th April another case occurred in hut D, and the family moved to hut E. No further cases occurred. Everybody was out by the middle or end of February, and then by the end of April plague had disappeared from Bandra.

18,577. Can you tell us how many cases of plague had occurred before the end of February?—It would be difficult to say with accuracy, because there was no record kept. Most of the people had absconded. The Municipal staff had run away, and there are no proper statistics which we can go on. I could find out by writing to the Municipal authorities at Bandra, and ascertain how many died previous to the 12th of February.

18,578. Will you do so?—Yes. (Note by witness on correcting the proof of his evidence:—I am sorry to say I have not been able to secure this, and figures are not reliable made at this time, as most of the staff had run away.)

18,579. After the evacuation had been effected how many died?—Quite half, certainly.

18,580. Half the total?—Yes, quite half. I am speaking from memory, but I believe there were more than that, because plague had not got to its height when I took over charge; it was worse for a little time.

18,581. At what time after the evacuation had been effected?—It gradually began to cease when I got them into huts. Of cause cases often occurred in the huts, and so I pulled down those huts, and made them go out and build other huts somewhere else, and the plague gradually lessened.

18,582. Did the number of cases commence to fall rapidly after evacuation had been effected?—Yes, soon after the evacuation. Of course I used to take it village by village. One or two villages did not get plague at once, so I did not move the people out where there was no plague. There are small hamlets at Bandra—small collections of huts, and I never disturbed people unless plague broke out in them. I can give you one notable instance. I found about 200 Mahars living in huts, and on the first day, the 12th of February, plague broke out, and I found a corpse there, and one other person with plague. I had them all out, and hutted, that afternoon. I gave them some material which was handy to build huts, and they were all out by the afternoon. They were absolutely free from plague for the rest of the time. I camped them close to the sea shore.

18,583. That was a very rapidly effected and successful evacuation?—Yes. I have also other cases.

18,584. But the evacuation was not so complete as in this case?—No, not so complete as this one, but I think this was because they were under better control than the others.

18,585. What is your opinion about the value of evacuation and segregation from the experience you have had?—I think it is very successful in small places, but it would be absolutely useless in Bombay.

18,586. What is the maximum population which you think can be moved out?—If you have a town with open country all round, where you can make the people go out, I should not like to say what the limit might be.

18,587. It might be a very large number?—Yes, but in Bombay which is surrounded by sea on all except one side, there is no place to do it, and it would be absolutely foolish to try it, in my opinion.

18,588. You had a good deal of experience in regard to inoculation, I think?—I fought the question very strongly here. I am very much in favour of it.

18,589. Can you tell us what your actual experience of inoculation has been?—I have no actual experience, except more than an intelligent man who reads statistics.

18,590. You had no direct experience?—I was inoculated myself, but it was not done largely in Bandra.

18,591. Your opinion is founded on what you have read?—Yes, and what I have seen of it here.

18,592. And did that lead you to form the opinion that it was a valuable measure for restricting plague?—Yes, I consider it a most important thing.

18,593. Have you got any example which came under your personal observation, in which you found it was very beneficial?—No, none at all.

18,594. Have you found any difficulty in inducing people to be inoculated, or have you had an opportunity of observing whether there was a difficulty or not?—I have not tried it, and I have had no experience. In the little experience I had the people were against it.

18,595. As much against it as against evacuation?—Yes, quite as much, but it was never worked properly.

18,596. (*Dr. Ruffer.*) When you were in charge of the plague operations in Bandra did you have a medical man to advise you, or were you in sole charge?—I was in sole charge, but there were several medical men who assisted me as volunteers.

18,597. Native medical men?—Two natives and a European; they were all volunteers.

18,598. What provision had you for disinfection at Bandra?—I went in for it rather largely. I had a staff of seven men in the Royal Artillery, and I had some native inspectors, and I thoroughly disinfected most of the houses; for instance, at Danda I did every single house. The disinfectant I used was perchloride of mercury.

18,599. In what strength?—I forget now, but it is the same strength as was used in the Hong Kong epidemic. A plague doctor from Hong Kong advised me as to the strength.

18,600. Did you disinfect the clothing of people before they were moved into camp?—No, I did not.

18,601. When you had a plague case in camp did you disinfect the clothing of the people?—No, we had not time.

18,602. What was the staff in your hospital?—I had two European nuns, and two or three native Christians who came from the Bandra Convent, also several native hospital boys, a Hospital Assistant, a compounder, and sweepers. It was very well equipped.

18,603. What was the maximum number of patients you had?—We had altogether in hospital about 150.

18,604. The other cases died in their houses?—Yes, we often found them dead.

18,605. You say in your précis of evidence, "I reckon "nearly 1,000 people died from plague at Bandra"; how did you ascertain that number?—I can give you the official numbers, but they are incorrect.

18,606. Why are they incorrect?—They are incorrect in this way, because I used to take the normal. The normal death-rate in Bandra is one or two a day, and all over that I consider were plague. As a matter of fact, there were officially 800 returned as having died of plague, and I found that that was not correct.

18,607. You also say, "I think in small towns plague "can be stamped out by segregation and evacuation"?—Yes.

18,608. On what evidence do you base that statement?—On practical experience of my own.

18,609. Can you give us an example of that from your own personal observation?—For instance, plague left these people so soon after I turned them out.

18,610. How long after you turned them out?—It began to decrease gradually, and then it disappeared.

18,611. How long did it take to disappear?—It had all disappeared by the end of April. Of course it is difficult to say, as there were two or three hundred huts, and cases kept occurring in the camps.

18,612. It may have gone on much longer than you think?—When they were turned out it left them, it got gradually less, and by the end of April it virtually disappeared. There were only a few cases after the

end of April, but I do not say they were all in the camps, because at the beginning of May I let the people go back.

18,613. And did the epidemic grow?—No, then cholera came.

18,614. Can you give me some information about quarantine regulations at Bandra?—People were not allowed to come in except with passes, and then when they got out at Bandra they were stopped on the causeway and they were examined at the railway station.

18,615. How did you prevent people coming in without passes?—That was done by the Plague Committee.

18,616. You have no information about that?—No.

18,617. Do you think people got in?—Oh yes, it was a mere question of getting a pass, and they were allowed to go in. People passing over the causeway were examined by a doctor, and were allowed to pass in.

18,618. Do you think the people passed in unknown to the authorities?—I have no doubt they did.

18,619. And you think people got out?—Yes, I am sure.

18,620. How many people did you inoculate at Bandra?—I believe there were only about a couple of hundred done.

18,621. Do you know how many died?—I do not think any died.

(Witness withdrew.)

Prof. T. K. Gajjar called and examined.

18,622. (*The President.*) You are a Professor of Chemistry?—Yes—Professor of Chemistry at the Wilson College.

18,623. (*Prof. Wright.*) You have formed some views, have not you, about the causation of plague?—Yes, I have.

18,624. Would you explain your views to us?—In the first paragraph of my précis of evidence I say that the origin is due to a combination of two causes—the presence in the air of germs, and the presence of environment. When these two are combined, plague breaks out. I have got several statements giving authorities.

18,625. Do you think these germs are of a specific nature; do you think that a particular kind of germ is necessary?—They must be of a particular genus of cocco-bacilli.

18,626. What sort of cocco-bacilli must be present?—Of a certain morphological shape.

18,627. You think they must be short rods?—Yes, in a way.

18,628. Do you think any short rod-shaped bacillus will develop into a plague germ?—I am not sure of that, but it must be of a particular form as well. The germs are generally styled species; but in the same species you might have pathogenic germs and non-pathogenic germs, or saprophytic and parasitic.

18,629. In the classification of bacteria which is generally adopted, do you not think that the physiological properties are taken into consideration as well as the morphological shape?—Yes.

18,630. You do not differ from ordinary bacteriologists in this matter. You think with them that the plague germs are bacilli which have a particular shape, and which are endowed with certain special physiological properties?—No, I do not think so. They are not endowed with specific and constant characters. If we accept the general view of the "species" of germs, even then there is a variation in the same "species." When we say there is a "species" of germs which we call plague germs, in that "species" will lie, as well, germs of a non-pathogenic nature and germs of different degrees of pathogenic virulence.

18,631. What are the circumstances that you think gave virulence to the bacteria?—The environment—the media.

18,632. You think, then, that, in addition to the germs, some other influence must be at work to produce plague. What environment do you think is necessary; do you not think that a plague germ of sufficient virulence will infect an animal in any part of the world?—Yes, that is what I think.

18,633. You mean, when you have a weak and attenuated germ, some further circumstances are required to cause the development of plague?—Yes.

18,634. What kind of environment do you think is required?—Some filth.

18,635. You think, under filthy circumstances, the bacteria acquire more virulence?—Yes.

18,636. Do you think that they require certain other conditions?—Yes.

18,637. You have, in your précis, laid some stress on temperature and moisture of the air?—Yes.

18,638. Do you think that plague bacilli flourish most luxuriantly when the air is moist?—Yes.

18,639. Can you account for the fact that plague bacilli do not flourish in Calcutta, where there is both moisture and filth, while they flourish in Bombay?—The sewage decomposition may not have reached the particular stage we have it in Bombay.

18,640. I understand that Calcutta is extremely filthy, and that the smells are extremely bad, and that in that respect Calcutta is worse than Bombay. Have you any theory to account for the fact that plague is very active in Bombay and very quiescent in Calcutta?—Either germs of a sufficiently virulent degree may not be present in Calcutta, or the particular stage of decomposition may not have been reached, although it may appear very filthy and very foul. Further, we do not know what particular products and gases of decomposition are necessary for this purpose. Unless we analyse the air comparatively, we cannot determine that.

18,641. Then a combination of sewage decomposition and of germs is not by itself sufficient to cause an epidemic. Is that your opinion?—Sewage decomposition of a particular kind at a particular stage can develop plague in combination with germs in a particular stage of evolution.

18,642. Are you able to be more precise about this; can you say what particular kind of sewage decomposition is required for the development of plague? For instance, can you explain how it is that the sweepers who carry away the ordure of Bombay suffer very little from plague, although they come in contact with every kind of sewage?—Decomposition of long-standing sewage matter on a sufficiently large scale, leading to the evolution of such poisonous gases as carbonic acid, sulphuretted hydrogen, ammonium sulphide, and different toxic amines in a concentrated form would be necessary for developing plague. The sweepers acquire immunity from being accustomed to work in filthy surroundings; their immunity is due to that fact.

18,643. In view of the great sewage pollution which exists in Bombay, do not you think that everybody in Bombay must have acquired such immunity?—I do not think all people live in such surroundings.

18,644. But all the people who live in filthy surroundings would have acquired this immunity, would they not?—Their occupations are not such, and the surroundings in which they live are not habitually such as the sweepers are brought to in their work. They were not exposed to such filthy conditions before the evils of the sewage system were developed, I am glad to say.

18,645. You say that sewer gases probably exert an influence on the development of plague. One of the arguments you adduce for that is that more "attacks of plague" occur at night?—Yes.

18,646. What do you mean by "attacks of plague;" do you mean people begin to sicken of plague during the night?—Yes.

18,647. Have you any statistics to show that people come into the hospitals more at night than in the daytime?—They are not generally sent to the hospitals at night; they are sent during the daytime.

18,648. What evidence can you adduce of the fact that people begin to sicken of plague during the night, and not during the day?—In the localities with which I am acquainted I heard of more attacks at night; and if you require a statement, I will furnish a comparative

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statement, where information of night-attacks was obtained, and information of attacks during the day.

18,649. Have you evidence on that point, sufficient to satisfy you that more people begin to sicken of plague at night than in the daytime?—Yes, I am satisfied. Further, I have got several statements ready in support of the view I put forward. It is not an original theory; it is the pithogenic theory of Murchison. I am not here to suggest any new theory, such as the millet theory of Dr. Waters.

18,650. You have expressed yourself dissatisfied with the antiseptics at present in use—mercuric chloride, for instance. Do you doubt that mercuric chloride will kill germs?—Yes, I doubt that.

18,651. Have you made experiments on that point?—I have not actually experimented with plague bacteria.

18,652. Have you made experiments with other bacteria?—Yes.

18,653. Have you found that bacteria can grow in mercuric chloride solution?—No, because they are rendered inert, inasmuch as an insoluble casing is formed by coagulated albumen.

18,654. What is the albumen which becomes coagulated?—If the germs are in an albuminous medium, the albumen will be coagulated by the action of mercuric chloride, and this coagulated albumen will protect them.

18,655. Eggs, I understand, consist of albuminous material. If you introduce an egg into mercuric chloride, and if you thereby coagulate it, do you not take away its vitality?—Yes.

18,656. Does not the same thing, then, hold true of an albuminous microbe?—Yes.

18,657. Why do you think that the microbe retains its vitality in mercuric chloride solution?—The albuminous medium in which bacteria are held is attacked first, and precipitated, and thus the destruction of bacteria is prevented.

18,658. Supposing they are held in suspension in water?—Then they would be destroyed; but if they are in an albuminous medium they will not be destroyed.

18,659. You do not think bacteria will be killed if mercuric chloride is applied to a wall?—In such circumstances it would act.

18,660. If mercuric chloride solution was poured on a floor which contained bacteria, you do not doubt, do you, that it would destroy the bacteria?—It would, in the absence of albuminous material; but it should not be used in the ordinary brass or metallic syringes.

18,661. That is another objection?—Yes.

18,662. You do not doubt that these disinfectants have bactericidal power?—No, they have this power, provided no albuminous medium interferes.

18,663. You have an objection to Prof. Haffkine's vaccine?—Yes.

18,664. On the ground that bacteria in the vaccine are not dead, but dormant?—Yes.

18,665. I understand that you think the vaccine contains living plague microbes?—Yes, in a dormant state.

18,666. Have you taken steps to ascertain that it actually does contain these living plague microbes?—Yes, I have. I went to Prof. Haffkine's laboratory several times for the purpose. I was introduced to him a month back, and I was discussing the question with him. Prof. Haffkine calls them dead, inasmuch as their cultures in artificial media are found extinct. He has not satisfied me that they cannot crop up into activity in living animal systems.

18,667. You have, in your précis, made an assertion that the bacteria are still alive in the vaccine. What is the evidence upon which you base that statement?—The process adopted is not sufficient to kill bacteria.

18,668. What is the process adopted?—The serum is heated to about 50 or 60 degrees.

18,669. We have it in evidence that the fluid is heated to 65 degrees for an hour. Does that not suffice?—There are certain microbes that are not killed at that temperature.

18,670. Do you know as a fact that plague microbes are not killed at that temperature?—No, possibly they are not.

18,671. I thought you had made yourself responsible for the stronger assertion that the plague bacilli were actually alive in the vaccine?—Yes, alive in a dormant state. There are other circumstances also confirming this belief. The only explanation of propagation and communication by inoculation could be given only on the supposition of microbes not being killed.

18,672. What is the other evidence that makes you think the plague vaccine contains living plague germs?—The symptoms we observe in inoculated persons. There is a period of incubation.

18,673. What is the period of incubation?—About six hours or one day.

18,674. Do you mean that the symptoms which are produced by the vaccine do not come on for six hours?—Yes, the fever of inoculation; there must be some living microbes, and they would multiply.

18,675. That is how you explain the fact that six hours elapse before you get any symptoms?—If it was merely a toxine, or a chemical substance, the symptoms would probably supervene within an hour two.

18,676. Do you think that if you put a slowly soluble poison under the skin, you would have symptoms developed immediately?—I am talking of a readily soluble chemical substance.

18,677. Are you aware that the toxines which are contained in Haffkine's vaccine are not all in solution in that vaccine? Are you aware that many of the toxic substances are contained in the bodies of the bacteria? Do you think this fact might not readily account for the slow supervention of the symptoms of poisoning?—You mean they are insoluble?

18,678. Do you know what the contents of Haffkine's vaccine are?—Yes, they are the supposed dead germs and toxines.

18,679. Dead germs, I understand, require to be dissolved before their constituent substance could exert a poisonous effect on the system; is that so?—The so-called "dead" germs will act, without being dissolved, on their enveloping casing being dissolved.

18,680. Do you think that solid particles can act poisonously without being dissolved?—If the germs are dormant they might grow and multiply.

18,681. I understand that you think that the germs are living, and that the poisons are introduced in a soluble condition, and that it is therefore not a question of their requiring to be dissolved?—No, but if you separate the serum into two portions, we find that the supernatant fluid gives all the febrile symptoms. That is what I find in Prof. Haffkine's own statement.

18,682. I think we had it from Prof. Haffkine that he had not made experiments on the effects of the supernatant fluid alone?—I have got it here. The sediment is the cause of local inflammation, and the supernatant fluid is the cause of febrile symptoms. I find it in Dr. Bhalchandra's printed lecture, at page 4.*

18,683. Have you got your facts from Prof. Haffkine?—In my conversation I got this very explanation.

18,684. Have you any other reasons beyond the fact that the advent of the symptoms is delayed for six hours, for thinking that Prof. Haffkine's prophylactic contains living plague germs?—Yes; the communicability—the increased mortality among the uninoculated.

18,685. What evidence have you of increased mortality among the uninoculated?—The published statements of Prof. Haffkine show that uninoculated died in very large proportions, and such was not the case in any town where inoculation was not introduced.

18,686. Will you give us an instance? I think you refer to the case of Dharwar and Hubli. Do you know what the total mortality among the uninoculated was in those towns? What, for instance, was the total mortality in Hubli among the uninoculated?—I have not got the total.

18,687. Would you be surprised to hear that the total mortality among the uninoculated up to December was only 14 per cent.? Have you added the figures up?—No, I have not added the figures up. I think that is high enough.

18,688. Do you think that the 14 per cent. mortality which occurred among the uninoculated in Hubli is a higher percentage of mortality than has occurred in other places?—Yes.

* Not published with the Proceedings of the Commission.

18,689. Would you be surprised to hear that in villages round about Hubli where no inoculation had been done, mortality amounted to 28, 29, and 30 per cent.?—I am told several people went to Hubli to get themselves inoculated.

18,690. That is quite true. The total mortality among uninoculated in Hubli, which you said established your case, was 14 per cent. We have had it in evidence that round about Hubli, in the villages, mortalities of 28 per cent. were not uncommon where no inoculation was done. How does that agree with your theory that inoculation causes increased mortality among the uninoculated?—You are citing villages round Hubli, and there inoculations were performed.

18,691. We have had it in evidence that no inoculations were performed in the villages which are in question?—Captain Leumann states in his report* that he inoculated several hundreds and thousands coming from different villages round Hubli.

18,692. Do you think there is no greater mortality than 14 per cent. has been observed to occur from plague?—Sometimes it may be. I have not come across any instance of higher mortality in places untainted by inoculation.

18,693. Do you know what the mortality was in Dharwar among the uninoculated? You say there was an extraordinary high mortality; would you be surprised to hear it was 9·5 per cent.? The facts which you have in view are, I understand, the data which refer to the individual weeks. Those facts were taken upon a census of the inoculated and uninoculated, and you think there was a large mortality among the uninoculated there?—Yes, I would be; because the Government Weekly Returns show a much higher rate than 9·5 per cent. This higher rate can be reduced to any extent by simply distributing the mortality over long periods.

18,694. Have you any evidence that the numbers of uninoculated are correctly given in the weekly report?—Approximately they must be true.

18,695. How do you know?—They are the census returns.

18,696. You cannot vouch, can you, for the accuracy of the figures of uninoculated given in the weekly returns?—They are supplied by Government Officers.

18,697. You have not totalled the figures up to find out what was the mortality among uninoculated in a sequence of weeks; the total as thus arrived at is very low, is it not?—Of course, the extraordinarily high mortality of those weeks during which inoculations have been performed in very large numbers, may be disguised by totalling up and taking the mean.

18,698. Have you totalled those figures up?—No, I have not totalled them up. The results arrived at by totalling would fail to give an idea of the true state of things. When the numbers of inoculated increase rapidly, we find the mortality percentage increasing.

18,699. Does it occur to you that many inoculations were performed where plague was very virulent, and that a large mortality of plague causes a large afflux of people to the inoculation stations?—The inoculations at Dharwar and Hubli were more or less of a compulsory character. There a rise in the epidemic did not lead to a rise in the inoculation figures, but the inoculation was the cause of the rise of the epidemic. I will give you an instance of Daman.

18,700. Where did you obtain your facts about Daman?—I have Prof. Haffkine's Report† here.

18,701. Would you tell us the facts that strike you about the mortality in Daman? What do the facts you propose to quote purport to show?—An increase of mortality among the uninoculated.

18,702. An increase as compared with what?—Compared with stations where no inoculations were introduced.

18,703. You mean there was a larger mortality in Daman among uninoculated than in any other place in India. Before you give us your facts, will you tell us what your facts are going to prove?—That an increase in inoculations produces an increase in mortality among the uninoculated.

18,704. Do you not think where an epidemic is severe more and more people would naturally offer themselves for inoculation?—How can such conservative people of this country know it for certain that inoculation would save them, and rush to avail themselves of such a novel and experimental measure? When an epidemic is violently raging they would, on the contrary, refuse to oppose the decrees of such a visitation of the Plague Deity.

18,705. If the epidemic increases in severity, do not you think that that increase will cause more people to be inoculated?—The epidemic cannot increase so very rapidly. In Hubli there was a jump.

18,706. Would you tell us the facts which you are going to give about Daman, and what they are going to prove?—Whenever inoculations were done, the mortality, which was on the decrease, jumped up. Twice it has been the case at Daman.

18,707. Would you prepare a table showing these facts, and send it in to us for a consideration?—Yes, the table is as follows:—

The number of inhabitants in Lower Daman were 10,900. Plague first appeared there in February 1897. In February and March nearly 2,000 people left the city, a cordon was established on the 23rd March, and the British territories were closed on the 30th March.

The First Series of Inoculations.—Between the 23rd and 25th of March 1897, 846 persons were inoculated, and 171 more between the 4th and 7th of April.

This was followed by the *worst period* of the epidemic. The climax was reached in the middle of April, the daily mortality rising to 80.

The Second Series of Inoculations.—529 persons were inoculated between the 17th and 27th April, and 99 more between the 24th April and 2nd May. It may be noted that these inoculations also had the effect of raising the mortality, the number of deaths on the 30th April being 38, and on the 4th May, 47.

The mortality began to decline in May, and dropped down to 6 and 8 on the 17th and 18th May.

18,708. You had two reasons for saying that bacteria were alive in Haffkine's fluid. One was that the symptoms do not appear till after an interval of six hours; the second reason you gave for your belief was the fact relative to the mortality among the uninoculated in Hubli and Dharwar. You now add Daman. Have you any further reasons for your belief?—Inoculation was also tried at Undhera, a village in the Baroda territory.

18,709. What was established by the experiments in Undhera?—All the deaths among the uninoculated took place within a week or so after the inoculation.

18,710. Does it occur to you, if you vaccinated against small-pox, and a small-pox epidemic was going on, you would not get your immunity immediately?—It may be so. But I am not referring to deaths among the inoculated. The many deaths that took place among the uninoculated were within a week or so after the inoculation, and although the uninoculated remained uninoculated, there were no deaths afterwards. The epidemic practically ceased after a fortnight after inoculation date.

18,711. You say all the deaths occurred among the uninoculated?—I say all the deaths among the uninoculated took place within a week after the inoculations were introduced—most of them, I do not say all; there were some few deaths after the first week or fortnight.

18,712. What do you deduce from that?—That there must be some connection between inoculation and the spread of plague.

18,713. Could not you account for the facts just as well by referring them to the epidemic that was raging at the time?—It could not rage in this particular way—that it goes up to its climax when the inoculations are in the highest proportions. I have noticed in several instances that when the number of inoculations was largest the epidemic was at its climax.

18,714. Would you pass on from that question. Have you any other reasons than those you have given us for thinking that there are live germs in the vaccine?—Yes, I have got several other facts. There are many houses in Bombay where no plague cases occurred before or after inoculation; but there were some cases just after the inoculation.

18,715. Does that prove that these cases were referable to the inoculation?—I think that these cases should be

* *See* App. No. XV. in Volume I. of the Proceedings of the Commission.

† *See* App. No. IV. in Volume I. of these Proceedings.

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referred to inoculation. There are many such occurrences.

18,716. If there was a wedding in a house, and a case of plague occurred afterwards in that house, would you infer that the wedding would have produced the plague?—It is not like that. This causal connection can be explained on a scientific basis.

18,717. You say that the facts compel to this conclusion. I say that a similar train of reasons would also compel you to the conclusion that the wedding in the house was the cause of the plague. You have an inoculation in a house, and a case of plague in another inhabitant of that house, and you argue that the case of plague is due to the inoculation. That is your argument?—That is not my argument.

18,718. Will you, then, state your argument?—I have reasoned this out on a scientific basis, and I have corroborated it by statistical data.

18,719. The facts, you say, force this conclusion upon you, that when an inoculation is performed upon one person, some other person in the house gets plague. Is the evidence you adduce any more conclusive than the evidence that can be adduced to prove that a wedding in a house is necessarily followed up by a case of plague in that house?—If there were a lakh of weddings followed by plague in every instance, I should certainly take it upon myself to see if any connection could be traced on a scientific basis.

18,720. But you did not say you had a lakh of cases in which inoculations in a house were followed up by the occurrence of plague cases; you said that there were *some* houses in which that sequence had been observed; have you made a census of those houses?—I have got it, but it is not complete. But, as far as it goes, it is sufficient to convince any reasonable man.

18,721. Have you any more *such* arguments which you wish to adduce in favour of your views that the inoculations are a source of danger to the uninoculated?—In Hubli we find that there was an increase in total mortality. That is another way of looking at the same figures. The increase of the total mortality increased with the number of inoculations.

18,722. I asked you whether you thought that inoculation would not increase *pari passu* with the growth of an epidemic. Is not that the natural way to look at it? The more the danger from plague, the more the people would recur to inoculation. Is not that a simpler way of explaining these facts than that which you adopt?—The epidemic would not keep *pari passu* with the inoculations.

18,723. Do you think the vaccinations would not keep pace with the rise of an epidemic? Do you think if the mortality from plague to-morrow in Bombay was 1,000, and the day after that 2,000, that more people would not recur to inoculation?—No, I did not think so.

18,724. Do you think that it is the danger of being attacked by plague which causes people to come for inoculation? Do you think that if an epidemic of small-pox breaks out, it does not drive people to vaccination?—No. If it is produced by the vaccination, people would not come in for inoculation.

18,725. If what is produced?—If the rise in the epidemic was due to vaccination.

18,726. How do you know that? There are two explanations, either that the epidemic produces vaccination, or vaccination produces epidemic?—You mean to say the epidemic and the inoculation went up in the same proportion?

18,727. That is the fact which we have to explain. As plague increases, inoculations increase. You have that in every case of epidemic. For instance, when there is a large epidemic of small-pox, people get vaccinated in large numbers. Do you think that vaccination produces an epidemic of small-pox, or that the epidemic of small-pox produces vaccination? You think, do you not, that vaccination is effective in stopping small-pox?—Yes, I think it is.

18,728. Let us pass on to the question of iodine terchloride, which you mention in your précis. You have an idea that the antiseptics in use are useless?—More or less.

18,729. That they are useless therapeutically, I mean?—Yes.

18,730. You think the terchloride of iodine would be a better antiseptic than any of those which are at present in use?—Yes.

18,731. In support of this opinion, you can produce data showing that one in 1,000 of this terchloride would kill bacteria in a body?—Yes.

18,732. Have you calculated how much iodine terchloride would be required to produce a concentration of one in 1,000 in the body? What doses would you have to give to effect the destruction of microbes in the body?—Doses of 20 minims repeated every hour.

18,733. I have calculated it out, and I find that an ordinary man weighs 70 kilos, that is, 70,000 grammes: a concentration of one in 1,000 would only be obtained by administering to him 70 grammes of iodine terchloride?—Yes, if you would include also bones, muscles, &c. in your weight.

18,734. Seventy grammes is 2½ ounces. What is the sense of giving a dose of 20 minims when you want 2½ ounces to produce a bactericidal effect?—If we take the weight of the blood only, the quantity required would be 5 grammes to produce the full germicidal effect in one single dose; but we cannot give such large doses; but small doses will exert inhibitory action.

18,735. Have you any evidence to show that?—I have several letters with me in the vernacular to say that they used this, and that people were cured of plague in Belgaum, and other infected places.

18,736. You have two cases where marvellous effects were, you say, obtained in the Parsee Hospital. Do you think many marvellous cures are not effected also by Mother Siegel's Syrup?—That was the opinion of a medical man of a very good reputation.

18,737. Most medical men attribute marvellous results to most drugs?—I do not know. The late Dr. Bahadhurji tried many experiments, and Prof. Behring also used it. Iodine terchloride is his favourite drug.

18,738. This iodine terchloride was not discovered by you; it was discovered, was it not, in Germany?—Yes, it was.

18,739. Do you know whether it is used in the treatment of fevers in Germany?—I do not know that. It is used in dyspepsia and flatulence, and in diseases of gastric fermentation.

18,740. I am speaking about fevers. Did its inventor recommend it as a therapeutical agent which was applicable to the treatment of fevers?—It is on trial there. Prof. Behring uses it in diphtheria. I have got a standard authority for this information, namely, Stephenson and Murphy's Hygiene.

18,741. Are you aware that Prof. Behring, so far from prescribing iodine terchloride as a cure for diphtheria, has brought out an anti-toxine as a cure for diphtheria? If iodine terchloride was effective in diphtheria, do you think that Prof. Behring would have taken the trouble to seek out a new therapeutical method for diphtheria?—It is stated in the standard books. He is using it along with his serum for germicidal purposes.

18,742. (*Dr. Ruffer.*) You say that one of the arguments against using Haffkine's prophylactic is that there is a long incubation period between the time of injection and the first symptoms. Supposing you inoculate an animal with a small quantity of snake venom, how long is it before the first symptoms appear?—Probably an hour or two.

18,743. Are you aware that sometimes it takes as long as six or seven hours? I will take you to another point. I believe naphthaline is not a bacterial poison?—But it is an insoluble substance.

18,744. It is, but supposing you give an animal large doses of naphthaline, the animal goes blind after a time, does it not?—Yes, but it will take some time. I do not know how long.

18,745. You may take it from me that it takes several days; months even?—It is a most difficult substance to dissolve.

18,746. Are you aware that all bacterial toxines require a long incubation period before they act? Supposing you take a tetanus culture, and filter off all the bacteria, and inject the pure toxine into an animal, how long does it take before the first symptom shows itself?—From the authorities I find that these symptoms

must take place in an hour or two if the toxines are in a soluble form.

18,747. My experience is that it takes at least eight or ten hours, and that if you give an animal a thousand times the dose that would kill the animal, you see no symptoms for seven or eight hours. Supposing you inject a filtered culture of diphtheria toxine, how long is the incubation period?—I have not studied all these toxines.

18,748. I simply point them out.—You mean there are incubation periods for toxines.

18,749. Do not the bacteriological facts we know absolutely prove the opposite of your theory?—I have Klein's authority to prove that toxines should act within an hour or so, while sometimes Prof. Haffkine's fluid requires a day.

18,750. Does not the fact that a poison takes a long incubation period simply show that it is a bacterial poison?—Yes.

18,751. (*The President.*) Have you anything else you wish to lay before the Commission?—I have a chart here* showing the barometrical pressure and plague mortality. The mortality goes *pari passu* with the rise in barometric pressure. That explains that environment (the sewer gases) has much to do with the plague. In summer we find the barometric pressure very low, and then the soil and sewer gases do not come up; while in winter, when the barometric pressure is at its highest, the warm moist gases come up. Owing to these emanations the germs acquire their pathogenic functions, and the virulence is greater.

18,752. Have you taken into account the fact that in cold weather people crowd into their rooms, and that, therefore, infection may be greater?—Crowding is rife throughout the year.

18,753. But it is greater in cold weather?—Yes, people shut up their windows and doors, but then that probably explains my theory as well, that gases are accumulated in those places, and that the air is injurious.

*Not published in the Proceedings of the Commission.

(Witness withdrew.)

Mr. W. C. Shepherd, I.C.S., called and examined.

18,754. (*The President.*) I believe you are Assistant Collector at Thana?—Yes.

18,755. Have you acquired your plague experience entirely there?—Yes.

18,756. What is your experience with regard to the source of infection?—Generally in Thana, as far as I have been able to make out, the source of infection has been some infected person coming from Bombay.

18,757. You think it has been carried by human agency?—Generally by human agency; but in some cases rats appear to have died before any case occurred in the village.

18,758. In some cases it has been introduced into the locality not by human agency, but by rats?—It seems so in one or two instances.

18,759. How far have rats proceeded from the nearest infected area in the cases you have referred to?—There was one case in which the nearest infected area was certainly three miles away, possibly four.

18,760. Can you entirely exclude human agency in that instance?—No, I could not in any instance. Our information was so inaccurate at the beginning of the epidemic that I could not lay any stress upon it. I do not know anything about the way rats travel. I believe they travel long distances.

18,761. There are, at any rate, human conduction and rat conduction?—Yes.

18,762. Have you any evidence with regard to the way plague can be conveyed by clothing or dead matter?—No.

18,763. I think you have had a good deal of experience with regard to plague measures?—Yes.

18,764. Firstly, with reference to evacuation, what is the opinion you have formed from your personal experience?—In every instance where evacuation has been carried out the plague has afterwards decreased. The effect of evacuation appeared to depend upon whether it was immediate or whether it was delayed. When it was delayed the plague mortality was greater than when it was carried out immediately.

18,765. Will you give us the number of cases before total immediate evacuation, and the number after; also, if possible, the number which occurred 10 days after evacuation, so as to exclude those who had been already infected?—(Witness subsequently explained that he could not give any reliable statistics.)

18,766. The general result has been that wherever total evacuation has been speedily accomplished in a growing epidemic the epidemic has decreased rapidly, and then has ceased in a short time?—Yes.

18,767. Have you largely made use of disinfection?—Yes.

18,768. And what views have you formed with regard to its value as a plague measure?—I have stated in my précis of evidence that I had no reason to suppose that it produced any good result; but I do not think I ought to have said that, because, as a matter of fact, disinfection has been carried out in almost every case I have had anything to do with. I can point to no instance in which disinfection clearly produced an effect upon plague, but as disinfection was always resorted to, I am not in a position to say what would happen if it were dispensed with. My objections were based largely on the ground that it was so badly done that it could not be of any value.

18,769. It was insufficient because it was done so badly?—Yes.

18,770. Can you describe how it was done?—The disinfection was almost entirely done with perchloride of mercury. We used syringes of different kinds, metal and wood. We washed the walls and floors of the houses with the perchloride of mercury as well as we could. That is the disinfection I saw myself. I have reason to believe that when there was no officer to supervise it it was not properly done, *i.e.*, the walls and floors were not thoroughly washed with the solution.

18,771. But I suppose you saw it sometimes properly done?—Yes.

18,772. What was the result in those cases?—I have never had an instance in which I could say it produced any result at all.

18,773. Did plague re-appear in properly disinfected houses?—In the next epidemic. I do not wish it to be assumed that I call the system of disinfection I used "proper" disinfection, all I mean is that my object was to thoroughly wash the inside of an infected house with the disinfectant, and when I was present I saw that it was thoroughly washed.

18,774. But plague did not reappear quickly, not until after a considerable interval?—As a rule, nobody was allowed to go into the houses for two or three months after they had been disinfected.

18,775. You disinfected the houses; did you do anything more to the houses before the people were allowed to enter them?—We took off a good many tiles, and opened the windows where we could.

18,776. Had you any recurrence of plague in the houses you treated in that way?—I know of no case of recrudescence except as part of a general recrudescence.

18,777. You had some experience of this matter, I believe, in Bassein town?—Yes.

18,778. What?—Bassein town is an exceptional case, because it appears that, in Bassein, there was a permanent source of infection, that is to say, it appears that there were two or three epidemics, and in each epidemic the first infection came from one particular place.

18,779. Had this particular place been disinfected?—Yes, it had been disinfected over and over again.

18,780. You bring that forward as an example of the inefficacy of disinfection?—Yes, I do.

18,781. Have you any opinions to offer with regard to segregation?—In dealing with villages I think it is unnecessary, except to the extent of putting the people who are sick and their immediate relatives in huts at some distance from the others.

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18,782. Therefore you think that partial evacuation may succeed?—It may succeed where the infected block is separated from the rest of the village; but, as a rule, I have found that plague cases occur in all parts of the village almost directly after it has been first infected.

18,783. Does the case of the Bassein Koliwada which you refer to have any bearing upon this matter?—I referred to the Koliwada as being an infected locality.

18,784. What do you think is the maximum size of a town in which complete evacuation could be effectively carried out?—It depends entirely upon the locality. If you had a wide plain which you could take up without much expense, you could evacuate a whole town of almost any size. There is no absolute impossibility about it.

18,785. Practically, there is no limit?—Practically, I should say there is no limit except that of expense.

18,786. Taking the towns one finds in India, about what is the maximum sized town that could be evacuated?—In the Thana district it could be very difficult to evacuate a large town because the country does not admit of it, it is all cut up into small fields, there is much broken ground and wooded country.

18,787. What is the largest town?—You could evacuate a town of 10,000 or 11,000 inhabitants in most parts of the district.

18,788. Did you find much resistance offered by the people?—Not so far as evacuation is concerned. We used to have considerable resistance to segregation. It was very unpopular indeed. That is one reason upon which I base my objections to it.

18,789. (*Mr. Hewett.*) What is your reason for thinking that dissemination by contact with infected persons is so rare that it may almost be ignored?—If dissemination was chiefly by contact, considering the way the people live in the villages, we should get a certain class of people infected first. If it began among a certain caste, several members of that caste would be infected before others; but, as a matter of fact we do not find that to be the case.

18,790. Is it your experience that people living close together, and having dealings with one another, do not get infected?—No, they do get infected, but people who do not have dealings with one another also get infected in the same town.

18,791. Do you find that neighbouring houses get infected in the same town?—Sometimes yes, and sometimes no. It varies. I could not give any general rule.

18,792. You said that mortality among rats generally occurred before you got a local case of plague?—That is my belief.

18,793. But are you certain that it occurred before you had an imported case?—I am certain that in some cases mortality among rats occurred before an imported case was reported.

18,794. Can you exclude the possibility of there having been an imported case?—Our information is so inaccurate before an epidemic that it is impossible to exclude that.

18,795. Do you think that it is likely that rats get infected from an imported case and then move plague about in a local area?—That seems to be the probable way in which it is spread in most of these villages, because it comes up in all sorts of places instead of following a regular course.

18,796. (*Mr. Cumins.*) You spoke of recrudescence, and said that you do not know of any recrudescence except as part of a general recrudescence. Did any case come under your notice where, apparently, an old germ, after lying quiescent for some time, woke up into activity again? Is that what you mean by general recrudescence?—No; by general recrudescence I mean when plague had stopped in a village it did not begin again except in the course of a general recrudescence of plague all over that district.

18,797. When plague began again in a village was that due to a secondinfection or to an old germ?—As a rule it was possible to trace it to the second infection, except in the case of Bassein which I quoted.

18,798. When villages or towns became infected a second time did the plague seem to reappear in the same houses?—Sometimes it did actually in the same houses where it appeared at first. Sometimes it did not. I can think of one case where it did undoubtedly, and another case where it began in the same part of a town, though perhaps not exactly in the same houses.

18,799. In this case where it began in the same house, or in the same part of the house, could you trace any re-infection from outside? Could you be certain that there was no re-infection?—No, I could not possibly be certain.

(Witness withdrew.)

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Mr. Ernest L. Marsh called and examined.

18,800. (*The President.*) I believe you have been engaged in bacteriological work?—Yes.

18,801. (*Dr. Ruffer.*) I think you have been making some investigations as to the approximate numbers and varieties of micro-organisms recovered from samples of soil and dust from native dwellings?—Yes.

18,802. How did you take the samples of the soil in rooms?—I took samples from the surface of the earthen floor.

18,803. How deep?—The immediate surface, *i.e.*, scrapings from the surface. I collected the samples in sterile tubes, and carried the tubes immediately to the laboratory. Then one hundredth of gramme of the sample was inoculated into melted agar.

18,804. How did you weigh it?—I did not weigh the quantity actually examined. I first weighed a control quantity, and from its bulk I estimated the amount to use for the purpose of bacteriological examination. This amount was removed on the end of a sterile platinum rod furnished with a scoop of known capacity. I found that the method of estimation was sufficiently accurate. The estimated one-hundredth gramme was immediately added to a test tube containing 10 c.c. of melted agar. By shaking it it was distributed as evenly as possible through the agar, which was then spread over the surface of a Petri plate to solidify. The plates were incubated at the temperature of the room for 24–48 hours, when the number of colonies which had developed in the agar in the plate, and in the agar remaining in the emptied test tube were counted. Agar was employed because of its suitability as a solid nutrient material at the comparatively high temperature of the laboratory (80°–86° F.). For this reason alone the number of colonies counted is probably much lower than might have been obtained with gelatine had it been practicable to use that medium.

18,805. Can you give us the general results of your investigations in unoccupied and occupied rooms?—The houses examined were not necessarily plague infected houses; some of them were. I took samples from two unoccupied houses. One of these houses was in the servants' quarters in a bungalow on Malabar Hill, the other was in a plague district in Bombay. The highest number of colonies in one sample was 5 million odd (calculated for 1 gramme of the sample); the lowest number in a sample from these two houses was 8 thousand odd, while the average of 22 cultures from 22 samples was one million odd. These results were obtained for unoccupied houses. From occupied houses the highest number from any one sample was 8 million odd, and the lowest number from any one sample was 126 thousand odd. The average was 3 million odd.

18,806. So that there is a rather larger number of micro-organisms in the occupied houses than in the unoccupied houses?—Yes.

18,807. Were they the same kind of floors?—Yes.

18,808. In the same quarter of the town?—One of the unoccupied houses was in the servants' quarters in a bungalow on Malabar Hill. The other house was in a plague district.

18,809. What did you find in the disinfected and unroofed houses?—In the two disinfected houses examined the highest number of colonies in any one

sample was 4 million odd. Two samples were sterile. The average of 25 cultures from 25 samples was 1 million odd.

18,810. So that it was very much lower than in the occupied houses?—Yes.

18,811. How had the houses been disinfected?—With perchloride of mercury.

18,812. Did you take any steps to neutralize the perchloride when you made your agar plates?—No.

18,813. There might have been some perchloride of mercury sticking to the sample?—There might have been. I thought I would give the sample all the advantage of the fact that it had been treated by perchloride of mercury.

18,814. How long after disinfection were your examinations made?—The samples were collected and the examinations made from within a few hours to four days after disinfection.

18,815. What did you find in unroofed houses?—I examined one, and the highest number of colonies in one sample was 1 million odd, and the lowest number was 2,000. The average was 500,000.

18,816. What conclusion do you draw from that?—That the average number of colonies of micro-organisms in 25 cultures from 25 samples from disinfected houses was 25 per cent. less than the average number of colonies in 22 cultures from 22 samples from unoccupied houses, and 65 per cent. less than the average of 20 cultures from 20 samples from occupied houses.

18,817. What was the nature of the microbes you found?—They were in great variety. The spore-bearing organisms were the commonest.

18,818. In all the samples could you give us the exact result of what you found?—The majority of bacteria recovered from the various samples were obligate saprophytes, dealing with oxidation, nitrification, and formation of carbonic acid of the dead and decaying matter in the filth of the houses. A large number of the samples contained species of the more sensitive facultative parasites. Divided into spore-bearing and non-spore-bearing forms the following account may be given:—Spore-bearing: B. subtilis, B. mycoides, B. vulgatus, B. mesentericus, B. butyricus; Non-spore bearing: B. coli communis, and other species of the coli group; Bac. vulgare, and other species of proteus; Bac. acidi lactici, Bac. fluorescens, Bac. syncyaneum, Bac. prodigiosum, Bac. janthinum, Bac. pyocyaneum, Micro. candidans, Micro. agilis, Micro. luteus, Micro. pyogenes aureus, citreus, and albus, streptococcus pyogenes, staphylococci, sarcinæ, moulds, yeasts. The spore-bearing species were ubiquitous, and in a great preponderance.

18,819. Did you find any difference between the number of the spore-bearing and other organisms in disinfected houses?—I cannot give you exact figures.

18,820. What is your impression?—My impression is that I found a greater number of spore-bearing organisms in disinfected houses.

18,821. That is simply an impression; you have no actual numbers?—It is a very strong impression.

18,822. So that perchloride of mercury and whitewash would kill off most of the non-spore-bearing organisms and leave the spore-bearing organisms?—Yes.

18,823. Are the spore-bearing organisms mostly non-pathogenic?—Yes.

18,824. It would not matter very much whether they were left alive?—No; my method of culture, however, only allowed me to recover the aerobic micro-organisms; it only deals with the aerobic micro-organisms.

18,825. How did you collect the dust on the floors or walls?—The samples from the floors included dust, cow dung, and dirt.

18,826. What result did you get from the dust on the walls of unoccupied and occupied houses?—The results are included in the statement I have given.

18,827. You mixed the two together, the soil and dust?—Yes. I took 14 samples, as a rule, from each house. The majority of the samples were from the floor, but three or four samples would be taken from the walls.

18,828. I believe you have made experiments with bubonic microbes and cow dung; could you give us the particulars of your experiments?—In carrying out this series of experiments the object was to reply to a letter received by M. Haffkine from Mr. Cumine, in August, with regard to the action of perchloride of mercury on cow dung and cow dung floors. In conducting the series of investigations, I tested the samples of perchloride of mercury against sterile cow dung to discover what effect sterile cow dung had in neutralizing a solution of perchloride of mercury. In the experiments of Mr. Hankin and Dr. Pitchford, perchloride of mercury dissolved in sterile water in the proportion of 1 in 5,000 caused the death of a pure culture of the plague micro-organism after one minute's contact. I found that by adding to 10 c.c. of perchloride of this strength a quantity of cow dung equal to 2 grammes or more, the disinfectant no longer existed; it was neutralized.

18,829. Will you give us the details of your experiments upon that point?—A series of test-tubes containing from ·5 gramme to 5 grammes of sterilized fresh cow dung in the form of dust was prepared. A measured quantity of sterile distilled water containing perchloride of mercury in solution was added to the tubes, so that each tube held the test amount of cow dung mixed with 10 c.c. of water containing ·002 gramme of perchloride of mercury, or a proportion of 1 in 5,000. At the same time each tube received a glass bristle wetted in its lower third by a thin film of living culture obtained from the surface of a three days old agar growth of plague. The experimental tubes were kept at the temperature of the laboratory for 18 hours. At the end of this interval the glass bristles infected with plague were withdrawn from the paste of cow-dung and perchloride solution in the different tubes, washed in a weak solution of carbonate of soda, and inoculated *seriatim* into tubes containing 20 c.c. of nutrient broth. The culture tubes were incubated at the temperature of the laboratory for ten days, and their ability to produce fresh culture of the test organism was as follows:—

Gramme of Perchloride of Mercury in 10 c.c. of Distilled Water.	Amount of sterilized Cow Dung acted upon.	Growth in Broth after 18 Hours attempted Disinfection.
·002	·5 gramme.	−
,,	1·0 ,,	−
,,	1·5 ,,	−
,,	2·0 ,,	−
,,	2·5 ,,	+
,,	3·0 ,,	+
,,	3·5 ,,	+
,,	4·0 ,,	+
,,	4·5 ,,	+
,,	5· ,,	+

I found that tubes containing quantities of cow dung of 2 grammes and less were disinfected, but those containing quantities of cow dung above 2 grammes were not disinfected of the test organism.

18,830. That was a neutral solution of perchloride of mercury?—Yes, an aqueous neutral solution.

18,831. Did you make any experiments with an acidified solution?—Yes.

18,832. Could you give us the particulars?—I have not done an experiment in which the results might be compared with the results just stated, but in my next experiment I used a solution of the formula and the strength commonly used in Bombay, viz., one in 725, 1015, and 152 of perchloride of mercury, chloride of ammonia, and hydrochloric acid respectively.

18,833. You made it up yourself?—I made a control sample myself, and I got a sample from one of the disinfection officers in Bombay and compared them one with the other.

18,834. What were the results?—They are detailed in the following experiment. Two series of test tubes containing from ·2 gramme to ·3 gramme of cow dung (weighed dry) were prepared. A measured quantity of the test solution was poured into the tubes so that each tube held the test amount of cow dung wetted with 10 c.c. of the disinfectant solution. Care was taken to thoroughly wet the cow dung with the solution, 10 c.c. of which should moisten dry cow dung in a fine state of division up to five grammes. At the end of 18 hours a minute quantity of the cow dung was

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abstracted and inoculated into nutrient broth. The results obtained were as follows:—

Gramme of Perchloride of Mercury in Solution with Chloride of Ammonia and Hydrochloric Acid in 10 c.c. Water.	Amount of unsterilized Cow Dung acted upon.	Growth in Broth after 18 Hours' attempted Sterilization.	
		Control Solution.	Bombay Solution.
	Grammes.		
·014	·2	−	−
,,	·3	−	−
,,	·4	−	−
,,	·5	− − −	− − +*
,,	·8	− − +*	+ +
,,	1·0	+ +	+
,,	1·5	+	+
,,	2·0	+	+
,,	2·5	+	+
,,	3·0	+	+

* Spores of moulds and of bacillus megatherium, B. subtilis, and B. mycoides obtained from the unsterilized cow dung. Whenever equivocal results seemed to be obtained the tests were repeated.

This experiment also shows the comparative results obtained with a laboratory prepared solution and a sample of the solution obtained from disinfection officers in Bombay.

18,835. Kindly give us that?—It is expressed in the statement that ·5 gramme of ordinary cow dung can be efficiently sterilized by 10 c.c. of the laboratory solution, but not by the other solution. The sample from the plague officers showed a very slight difference however.

18,836. What was the difference? — A difference measured by the fact that the laboratory solution sterilized ·5 gramme. The plague officers' solution did not sterilize that but sterilized ·4 gramme.

18,837. A difference of 20 per cent.?—Yes.

18,838. What other experiments have you made?—The experiment which has just been mentioned was repeated in all its details with living cultures of the plague bacillus as the test organisms. Each tube received in addition to the cow dung and disinfectant solution a glass bristle infected with a culture of the plague microbe recovered from a broth culture 15 days old. Care was taken to wash the bristles in sterile water to remove the adherent broth before inoculating them into the test tubes. On withdrawing the bristles at the end of the experiment they were first washed in sterile water made faintly alkaline with carbonate of soda, then agitated in melted agar, and finally plated from a second tube of melted agar for observation and recovery of the test organisms. Although this method of differentiating and identifying the test organism is not a delicate one, still, in the absence of animal inoculation tests, it appeared the most satisfactory one to employ. The use of broth is very unsatisfactory, owing to the rapid vegetation of the common saprophytic micro-organisms. In those instances where the results seemed to fall short of accurate demonstration the tests were repeated. The results obtained were as follows:—

Gramme of Perchloride of Mercury in Solution with Chloride of Ammmonia and Hydrochloric Acid in 10 c.c. Water.	Amount of unsterilized Cow Dung acted upon.	Growth of the Plague Bacillus after 18 Hours attempted Disinfection.	
		Control Solution.	Bombay Solution.
·014	·2	−	−
,,	·3	−	−
,,	·4	−	−
,,	·5	−	−
,,	1·0	−	−
,,	1·5	− −	− − −
,,	1·75	− − −	+ − +
,,	2·0	− − −	− + −
,,	2·2	+ − +	− + +
,,	2·5	− + −	+ + +
,,	2·8	+ + +	+ + +

The results show a small success of the control solution over the Bombay solution, the latter being capable of a germicidal action on 1·5 grammes of cow dung experimentally infected with the organism of plague, the former a similar action on 2 grammes.

18,839. You refer to the action on the plague bacillus?—Yes, as the test organism in this experiment. The results were attained by 10 c.c. of a solution of 1 in 725 strength, an acidified solution.

18,840. In that case the sublimate would have to go through a considerable layer of cow dung before it got to plague bacillus? That is not quite the condition of nature, is it? There would be a layer of, say, a quarter of an inch of cow dung between the plague bacillus and the sublimate?—Not so much as that. The glass bristle was inserted into the mass of cow dung, and the cow dung was thoroughly moistened with the disinfectant. It is just as likely that the disinfection was in contact with the glass bristle as with any of the cow dung particles.

18,841. Do you moisten the cow dung before you put the bristle in?—No. I put the bristle into the cow dung first, so that the results may not be varied by the action of the cow dung in neutralizing part of the disinfectant. Then immediately afterwards it was moistened with the disinfectant.

18,842. It was simply moistened? — It was made wet. 10 c.c. very thoroughly moisten quantities up to 2 grammes of cow dung. In fact, 10 c.c. will moisten 5 grammes.

18,843. Have you any other experiments on that point?—I took samples scraped from the surface of native floors and used them as the test material.

18,844. In plague houses?—Yes, houses which had been notified to me by a plague officer.

18,845. Do you put in all those experiments?—Yes. In these experiments the Bombay solution was further tested. Instead of ordinary cow dung the test material employed was dried mud and cow dung dust scraped from the floors of native dwellings. The results obtained are detailed in the following experiment on the germicidal value of a sample of perchloride of mercury used in Bombay for disinfecting native houses:—

Amount of Earth and Cow Dung acted upon.	Growth in Broth after 18 hours attempted Sterilization.
·2 gramme	− −
·3 ,,	− −
·4 ,,	− − +*
·5 ,,	+ + +
·6 ,,	+ +
·7 ,,	+ +
·8 ,,	+ +
·9 ,,	+ +
1·0 ,,	+ +
1·5 grammes	+ +

* Spores or moulds and of B. megatherium, subtilis, and mycoides, were commonly obtained from the unsterilized material.

From the results it would appear that an amount of earth and cow dung from the floor of a native dwelling over ·3 gramme is not efficiently sterilized by 10 c.c. of a solution containing perchloride of mercury in the proportion of 1 in 725.

18,846. Is it sterilized as far as non-sporing organisms are concerned?—I failed to recover any non-sporing organisms in less than ·7 gramme of the test material.

18,847. Have you ever tried the following experiment, to put plague microbes in cow dung floor, pour the solution on it, and see then whether the plague is not destroyed? — I have not done an experiment actually in that way, but I have done one with pieces of a cow dung floor kept as samples in the laboratory.

18,848. Will you give us that experiment?—I took a 4-inch square of dry cow dung floor, and inoculated the surface with plague micro-organisms contained in a watery emulsion made from an agar culture. Then I poured on the surface of the piece of floor 10 c.c. of the 1 in 725 solution, and after 20 minutes took a scraping from the treated surface, which I inoculated into agar for plate cultivation of the plague bacillus, but failed to recover the organism. I also inoculated an animal with negative results.

18,849. In all these cases I do not quite see how you neutralized the perchloride of mercury; was it simply by a carbonate of soda solution?—Yes.

18,850. By a weak alkali?—Yes.

18,851. By that weak alkali you would neutralize the acid, but you would not neutralize the mercury, would you?—Yes.

18,852. My difficulty is, that perhaps your results are too good. I should have thought that if you put a certain amount of mercury with a plague microbe into a tube, unless you took some special measures to neutralize the disinfecting action of the perchloride of mercury, you would have a sort of film of perchloride of mercury round your microbe, which might prevent it growing?—I tried by means of a weak alkaline solution of carbonate of soda to neutralize the mercury.

18,853. Have you any experiments bearing on that?—No personal experiments.

18,854. My objection is probably quite fallacious; I only suggest it?—I think that experiments have been done, because when perchloride of mercury is used in very strong solution as a disinfectant it is the practice of the disinfectors in certain places to follow up the use of the mercury salt with a solution of carbonate of soda with which to neutralize it.

18,855. What other experiments have you got?—The last experiment was repeated with living cultures of the plague organism, the test material being scrapings of the floors of native houses. The results showed that ·8 gramme of earth and cow dung from the floor of a native dwelling is not efficiently sterilized by an acid solution of perchloride of mercury of the strength of 1 in 725.

18,856. As far as the plague microbe is concerned?—Yes.

18,857. How did you do it in the experiment with the dust; did you ram the dust down to the bottom of your tube? How did you mix the plague microbe with the dust?—I still used the glass-bristle method.

18,858. Was the dust loose or firmly bound together?—It was loose cow dung dust; it was ground down in the form of a powder.

18,859. Have you any other experiments?—I took the reaction of the disinfectant solution after contact with several quantities of cow dung and earth floor, and found that, while a control sample of the disinfectant solution always gave a strongly acid reaction, within two minutes' contact with 2 grammes cow dung and earth the reaction was affected to such an extent that it was only acid, no more than acid, with ·5 gramme the reaction was neutral, and with ·9 gramme the reaction was alkaline.

18,860. You want to add a considerable amount of acid to your solution?—Yes. From half to three-quarter per cent.

18,861. Have you any further experiments on cow dung?—No.

18,862. What conclusion do you draw from your experiments?—I concluded that 10 c.c. of an acid solution of the strength of 1 in 725 (Bombay formula) efficiently disinfects 1·5 grammes of cow dung experimentally infected with the organism of plague, but does not suppress the more resistant saprophytic bacteria. I also concluded that 10 c.c. of the 1 in 725 acid solution efficiently sterilizes ·7 gramme of cow dung floor experimentally infected with the organism of plague. It also reduces the number, but does not suppress the more resistant saprophytic bacteria. Also that as the effective limit of the 1 in 725 acid solution, in regard to disinfection against plague, is represented by the action of 10 c.c. of the solution on ·7 gramme of cow dung floor, and as ·7 gramme of cow dung floor is the amount commonly recoverable from 4 square inches of the earthen floor of a native house when the immediate surface for a depth of $\frac{1}{16}$ of an inch is removed, then 100 square feet (14,400 square inches) would yield 2,520 grammes, which amount of surface material would require approximately 8 gallons (three bucketfuls, the buckets being of a capacity of $2\frac{3}{4}$ gallons—many of the wooden buckets in Bombay have a capacity of only $2\frac{1}{2}$ gallons) of the 1 in 725 acid solution to vitally damage any contaminating plague micro-organism. But if the infectious material were below the immediate surface under disinfection, or on the surface protected by leaves, bits of rag, &c., then with such an application it could hardly be hurt at all. In a test observation in which 8 samples of dirt and cow dung from a room were examined before disinfection, and 12 samples from the same room immediately after disinfection, and in which care was taken that the quality and amount of perchloride of mercury used and its method of application allowed a 1 in 725 acid solution to produce its maximum effect, a comparison of result showed that while 4,057,800 was the average number of colonies of micro-organisms recovered from 1 gramme of each sample before disinfection, the average number after was 228,300—a reduction of about 94 per cent.

18,863. That is counting both sporing and non-sporing organisms?—Yes.

18,864. Then you have some experiments on the microbes of bubonic plague in relation to earth?—Yes.

18,865. What are your experiments with regard to the microbe of bubonic plague and sterilized garden mould?—That in sterilized mould it can have a continued existence up to 13 days.

18,866. How did you perform that experiment?—I took quantities of garden mould and sterilized it in test tubes in an autoclave, and made sure that it was sterile by control experiments. Then I inoculated the sterile earth with a culture of plague micro-organisms taken from the surface of an agar tube, and I also introduced a glass bristle infected in the same way as I have already described in the former experiments. At the end of intervals of from 1 to 13 days or more a glass bristle was removed from the sterile earth and inoculated into a culture tube of broth, and also a quantity of the earth itself was removed at the end of a platinum loop and inoculated into another series of broth tubes, and the results obtained culturally.

18,867. Did you make any experiments in unsterilized garden mould?—Yes. I got the plague micro-organisms to live in unsterilized garden mould from 4 to 6 days.

18,868. Do you think your experiment is quite a fair one? Supposing you put the plague bacillus sticking to the bristle, it is not in the same condition as if it were mixed with garden earth, is it?—With sterile media it really did not matter. I got the plague micro-organism from particles of the sterile media. With unsterilized media it was very difficult to recover the organism unless I adopted that method along with other methods. While the glass bristle method yielded these results, the results when I took a particle of non-sterile medium were very unsatisfactory. Owing to the difficulties that had to be overcome in the bacteriological proof of a few plague germs where there were crowds of saprophytic organisms, I could not be sure of them.

18,869. Were you able to isolate the plague bacillus again when you mixed it with unsterilized garden mould?—Only up to the length of time mentioned.

18,870. When you mixed it thoroughly with the garden mould, not with a glass bristle, but in a dish or tube, and powdered it all up together, could you then isolate the plague bacillus again?—I have done so up to short intervals.

18,871. It is a very difficult thing to do, is it not?—Yes.

18,872. Is it a matter of weeks?—Yes; it is very difficult.

18,873. Then it is very doubtful that you would get it in all cases, is it not?—I never used that method, because of its difficulty, when I wished to test if the organisms were alive for longer periods than 24 hours.

18,874. Have you given us all your experiments on earth and cow dung?—No. I have made laboratory experiments on sterile earth, sterile tap water, and sterile cow dung. I found when the plague microbes were inoculated into these materials that the conditions were favourable to their continued existence up to 13 days, 44 days, and several months respectively.

18,875. How much culture did you add to the sterilized tap water?—I did it in the way already described, with a glass bristle—the amount that would adhere as a delicate film to about one-third of the length of a glass bristle about 3 inches long.

18,876. Bombay tap water?—Yes, sterilized.

18,877. (*Prof. Wright.*) How long did the plague bacillus live in cow dung?—It lived several months in sterile cow dung.

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18,878. Is that moist cow dung?—Yes.

18,879. Wetted with Bombay water? —Yes, and sterilized.

18,880. (*Dr. Ruffer.*) How long did it live in non-sterilized cow dung?—Never after six days.

18,881. How about sterilized tap water?—The period was 44 days.

18,882. And in ordinary tap water?—Never after six days.

18,883. When you recovered it from water how did you make sure it was plague bacillus in the ordinary water?—I did animal control experiments.

18,884. (*Prof. Wright.*) Was the plague bacillus virulent after that time?—Yes. It was not virulent in the sense that it always killed the animal, but of a series of animals some sickened, and some of the sick died of plague.

18,885. (*Dr. Ruffer.*) Have you any other experiments on water?—No.

18,886. Have you any experiments with the microbe of bubonic plague in relation to dessication in sterilized air and ordinary air?—I did some experiments simply to find out how long the plague micro-organisms would live on glass bristles in ordinary air and in sterile air. I found in exceptional cases they lived as long as 48 hours on the glass rods. I inferred that that was due to the fact that there was probably a thicker film on the glass rod than usual.

18,887. Did you try sterilized articles of clothing inoculated with the plague microbe?—Yes.

18,888. How long does it live in them?—Never longer than 18 hours.

18,889. How did you expose your sterile clothing: did you put it away in the dark or hang it in a room?—I exposed it in a sterile chamber.

18,890. Did you dry it or keep it moist?—I kept it dry.

18,891. Then the desiccation may have acted?—Yes.

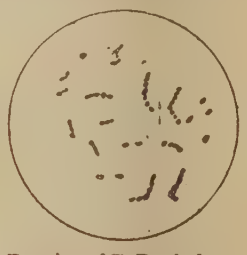
18,892. Have you made any experiments such as contaminating a shirt or a vest with the plague, and then rolling it up in the usual way in which you would send it to the wash, and then put them away in a drawer?—No, I have not.

18,893. Have you any facts with regard to the growth of the plague micro-organism in ground air, and in artificial admixture of certain gases and ordinary air?—Yes. A series of six ordinary test tubes containing a four days' agar growth of plague (obtained from broth culture three days old, from a previous broth culture 10 days old, from an agar culture direct from bubo in a case of plague) were buried 9 to 12 inches below the surface of the soil in Bombay on January 3rd 1899.* The place of burial for two of the tubes was the earth of the floor of a native house, the remaining four tubes being buried in garden mould in the open. Control tubes containing cultures of the same age and pedigree as the above were kept in a dark cupboard in the laboratory. The tubes buried in the garden mould were allowed to remain undisturbed for 9, 23, and 28 days respectively. Of two tubes removed at the end of nine days one was discovered to be invaded by mould-fungi. The other tube was pure. This tube presented a slightly exaggerated growth of plague when contrasted with the control tubes kept in the cupboard. In the tubes removed after 23 and 28 days burial the growths were notably exaggerated. The appearance of the culture viewed by obliquely transmitted light was not the simple ground glass appearance of the control tubes, but that of a thick opaque growth. At a number of points the growth had developed what looked like secondary colonies, of a dull waxy appearance. These colonies had regular margins, and stood out well from the original arrangement of the growth. Stained specimens from the last two tubes showed under the microscope (× 800) bacteria of consistent shape and much larger dimensions than the average. The bipolar arrangement of the protoplasm was defined with unusual distinctness for an agar culture. A still more noteworthy feature was the presence of chain formations. As many as 20 to 30 individual bacteria were commonly counted in the chain fragments visible in the field. The chains were not exactly comparable to the short streptococcus-like growths obtained from a bouillon culture of plague made alkaline with carbonate of soda. They were stouter, and showed the bipolar markings of the individual bacteria in a prominent manner. A number of the chains presented an appearance of striation due to the presence of vacuoles. In the two growths buried in the floor of a native house for 23 days similar cultural and microscopical appearances were obtained. The occurrence of vacuolation in the elements of the chains was perhaps less marked.

Inoculated into broth, typical and vigorous growths occurred in each instance.

One drop of a broth sub-culture (24 hours old), from one of the agar cultures buried in the soil of a native floor, injected subcutaneously into a white mouse caused death within 18 hours. A similar dose of a broth sub-culture from a control tube was fatal after 50 hours.

Drawing of B. Pestis from an agar culture buried 23 days, 9 inches below surface of soil.



Drawing of B. Pestis from from a four days' broth culture made alkaline with carbonate of soda.

The above series of facts would seem to indicate that soil-air is possessed of conditions that are at least favourable to the growth and multiplication of the plague micro-organism. As the results obtained suggest that the dilution of the oxygen in ground air by carbonic acid is advantageous to the vegetation of the organism, a series of experiments was done in order to reproduce roughly similar conditions in the laboratory. The amount of carbonic acid in ground air being on an average from 2 to 3 per cent., or about 1 per cent. less than the amount in air expired from the lungs, an admixture of air and carbonic acid in the proportion of 3 per cent. carbonic acid was used in an initial experiment on agar test-tube growths of plague (one day old). The test tubes (four in number) were allowed to remain in this atmosphere in the dark at the ordinary temperature of the laboratory (82–88° F.) for two to six days. As a result of this treatment the growths were found to be much more luxuriant than growths obtained in control tubes kept under the ordinary atmospheric conditions in a cupboard. Under the microscope the individual bacteria were large and showed a clear outline with prominent bipolar arrangement of their protoplasm. The chain formation characteristic of the underground growths was evidenced by short chains of several bacteria. Owing to the occurrence of vacuolation in many of the chains, the appearance of striation sometimes so well seen in the diphtheria bacillus was observable.

In the next experiment the fact of there being (probably) a larger amount of carbonic acid than 3 per cent. in the air in the superficial soil of native houses was considered. The abundance of organic matter, the comparatively high ground temperature, the presence of moisture, and absence of ventilation, would probably make the per-centage of carbonic acid present approach the maximum limit of carbonic acid found in ground air, namely, 14 per cent. Three test-tube growths of plague on agar, one day old, were accordingly exposed to an admixture of moist air and carbonic acid con-

* Readings of a Thermometer the Bulb of which was 9 inches below the Surface of the Ground. Government Observatory, Bombay.

January.		January.		January.	
	Fahr.		Fahr.		Fahr.
3	76°·8	13	76°·5	22	78°·1
4	76	14	76·4	23	77·8
5	76·8	15	76·4	24	77·4
6	76·9	16	76·8	25	77·2
7	75·3	17	77·0	26	76·8
8	75·8	18	77·0	27	76·7
9	76·1	19	77·2	28	76·4
10	76·6	20	77·6	29	76·2
11	76·2	21	77·9	30	77
12	76·6				

taining the latter in the proportion of about 14 per cent. The growths exposed to this condition of atmosphere at a temperature of 92° F. for three to nine days were found to thrive particularly well.

In an experiment in which a cent. per cent. admixture of air and carbonic acid was used at the laboratory temperature the growths appeared to do no better than in ordinary air. In an atmosphere of almost pure carbonic acid the cultures appeared to be inhibited in their growth, but otherwise not seriously interfered with. Stained specimens examined microscopically showed a number of involution forms.

From the above, it would appear that an admixture of 14 per cent. of carbonic acid and 86 per cent. of ordinary air at 92° F. favours and enhances the growth and reproduction of the plague micro-organism.

As has been already mentioned, the advantage of the presence of carbonic acid seems to exist in the fact that it acts as a diluent of the oxygen, and not for any special favouring influence it might have on the plague bacillus. When nitrogen was used instead of carbonic acid, so that 100 volumes of air, instead of containing 79 volumes of nitrogen, were made to contain 85 volumes, a very similar increase of development was noticeable. In this case, however, the microscope did not show the excellent specimens obtained with carbonic acid. Air containing a proportion of nitrogen equal to 90 per cent. did not appear to help the vegetation of the micro-organism.

With hydrogen gas in 25 and 50 per cent. admixture with air the growths did not appear to be specially benefitted. Under the microscope large numbers of involution forms were obtained from an agar culture (two days old) that had been subjected for four days to an atmosphere containing 50 per cent. hydrogen. In a series of agar test-tube growths of plague (two days old) the cultures were found to produce involution forms in abundance and rapidity as the amount of hydrogen was increased. When hydrogen gas was made to pass through nutrient broth containing a four days' growth of the plague bacillus the growth was not sterilized, but large and exaggerated involution forms were obtained in 24 hours with an abundance and similarity of shape to those obtained from a culture of the plague bacillus in sterile tap water at the end of 44 days' growth at ordinary temperature.

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This circumstance would indicate that the conservation of the plague micro-organisms in a living condition is favoured by the production of involution forms.

The shapes of the involution forms appeared to be correlated with the nature—whether solid or liquid—of the medium in which they happened to be present. On solid media the tolerably fixed dimensions and shape of the normal bacilli were confused among the involution forms. They had no certain standard of size, or consistent shape: the usual shapes had a bulbous and a tenuous end, with the protoplasm massed on one side. In broth and sterile water the involution forms appeared to be able to retain the shape commonly characteristic of the normal organism. They were usually spherical or swollen-up masses of protoplasm several times the size of the normal form. It was possible to cultivate the involution forms obtained from the agar hydrogen-treated tubes during three generations of sub-cultures.

Drawing of B. Pestis (showing involution forms) from an agar H-treated culture.

Drawing of B. Pestis (involution forms) from a broth hydrogen gas treated culture.

Drawing of B. Pestis (showing involution forms) from a sterile tap water culture 44 days old.

The use of an atmosphere diluted as regards its oxygen by the use of 10 to 15 per cent. of carbonic acid was found to be specially serviceable in occasioning the vegetation on agar of plague micro-organisms existing in small numbers in the blood or tissues of animals dead of plague. It was found that when the infected blood or tissue pulp were inoculated on to agar vigorous growths of the plague micro-organisms were invariably obtained within 24 hours when carbonic acid was used, whereas tubes not so treated, but exposed to the ordinary air in an incubator, either did not develop the characteristic agar growths of plague at all, or did so very slowly.

It was also found that the dilution of the oxygen by the use of 10 to 15 per cent. carbonic acid was helpful in retaining the virulence of cultures of the plague micro-organisms growing in artificial media. Cultures which had made several passages through rats, and experienced growth on artificial nutrient media, such as agar and broth, in the intervals of their existence outside the bodies of the experimental animals, were occasionally found to have lost their virulence, whereas similar cultures placed in an atmosphere containing a diminished amount of oxygen did not lose in virulence.

18,894. Have you any other experiments you wish to put before us?—No. I have made other experiments, but the results are still under investigation.

18,895. You have some clinical memoranda of two cases admitted to hospital at Poona with bubonic plague eight and nine months respectively after inoculation with Dr. Haffkine's prophylactic?—Yes.

18,896. What is the general result of your observations in the first case, the one admitted on the 5th January 1898?—There is nothing special about it except that the man had been in frequent contact with plague for several months. In fact both cases had been in close association with plague and plague-infected spots for some considerable time before becoming infected. I wished to draw attention to their succumbing to the infection only late after inoculation.

18,897. Eight and nine months after inoculation?—Yes. They were ordinary cases of bubonic plague, there was nothing special about them. They both recovered.

18,898. There is nothing more you wish to especially call attention to?—No.

18,899. Do you think they ran a milder course?—Perhaps one of them did, but there were many equally mild cases of plague among uninoculated cases.

18,900. Who inoculated them?—I could get no precise information about that, except that one of the district officers in Bombay had inoculated them.

18,901. How did you know they were inoculated?—I saw their certificates of inoculation.

18,902. Had they been inoculated once or twice?—One twice and the other once.

18,903. Do you know whether either of them reacted to the inoculation?—Yes, one was ill for two days and the other felt ill for 24 hours after the two occasions on which he was inoculated.

18,904. You have had some clinical experience of plague?—Yes.

18,905. Do you ever find high rises and falls in the temperature of plague?—Yes, regularly on the fourth or fifth day; any pyrexia later on in the illness was usually due to the fever of suppuration.

18,906. You have also some records of bacteriological examination for diagnostic purposes of the circulating blood or material from a painful gland in 25 of the patients admitted to the observation wards of the Poona Plague Hospital?—Yes. Of 1,593 cases admitted for observation to the Poona Plague Hospital, 650, or 41 per cent., were diagnosed as cases of plague without much difficulty. The 25 cases under remark were

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doubtful cases. Where these was no doubt the case was either sent into the hospital as plague or kept in the observation camp till better of the condition that simulated plague. Of the 25 doubtful cases examined bacteriologically four were in the second day of illness, 15 in the third day of illness, 3 in the fourth day, 2 in the fifth, and one in the sixth day.

18,907. Had they any other symptoms besides?—Yes. Some of them had symptoms of malarial fever, and that helped to make them doubtful. Examination of the circulating blood in the 25 cases by smear inoculations on agar culture media gave positive results in only three cases (all fatal), a proportion equal to 12 per cent. The examination of glandular material in 18 of the cases ——

18,908. What do you mean by glandular material; did you put a needle into the gland?—I put a needle in some cases, in others a very thin glass pipette, into the gland, and got some lymph and blood from the gland. An examination of this material in 18 cases presenting some glandular symptoms or condition such as hardness, tenderness, or slight enlargement, gave positive results in 14 cases, a proportion equal to 79 per cent. Thus the 25 cases yielded positive results to bacteriological examination in 17 cases, a proportion equal to 68 per cent.

18,909. Did the clinical symptoms afterwards bear out your bacteriological diagnosis?—There were only one or two cases in which doubt continued to exist.

18,910. As a rule have you got typical plague afterwards?—As a rule they were found to have plague.

18,911. You got other symptoms showing they had plague?—Yes; these examinations were made a few hours after admission, and the clinical evidence later on showed they had plague.

18,912. Even the cases in which the bacteriological examination gave a negative result turned out to be plague?—I found that several (two or three) of them turned out to be plague.

18,913. Have you any facts with regard to the presence of plague bacilli in the blood of patients who ultimately recovered?—I enclose two clinical charts* giving short record and character of fever in two cases of plague septicæmia, in which the organism was cultivated from the blood on the fifth and sixth days of the disease respectively. The patients recovered. Of 18 cases of plague septicæmia in which the organism was recovered from the blood, only two (the present cases) ultimately recovered.

18,914. (*Mr. Cumine.*) Have you any reason to believe that the floor of which you took scrapings had already infected some human being with plague?—I cannot say definitely that it had. It was, however, in a plague-infected house.

18,915. (*The President.*) Did you examine any food substances?—No.

18,916. Did you examine milk?—No.

* *See* Appendix No. LVI. (i.) and (ii.) in this Volume.

(Witness withdrew.)

Mr. E. F. Allum.

Mr. E. F. Allum, called and examined.

18,917. (*The President*). What is your occupation?—I am a merchant of Bombay. I have been doing plague work in Bandra.

18,918. (*Mr. Hewett.*) You are a member of the Bandra Municipality and Chairman of the Sanitary Committee?—Yes.

18,919. Were you employed in the first plague epidemic in Bandra?—No, I commenced in December 1897, and have continued since.

18,920. How did the place get infected the second time?—It broke out in the village of Khari at the end of December 1897. There were two cases imported from Santa Cruz, which is about two miles from Khari, on the north side.

18,921. Did it get into Bandra from the village of Khari?—Khari was the first village attacked in the whole district of Bandra. There are about half a dozen villages—in addition to the town of Bandra—in the district.

18,922. How did the town of Bandra itself become infected?—The first case in Bandra was in February; I think that was a compositor in Bombay who was living in Bandra. A large portion of the population of Bandra go to Bombay for their occupation.

18,923. During this time there was no interference with their moving between Bombay and Bandra?—No.

18,924. How many cases occurred in the second epidemic at Bandra?—The number of cases for the whole year was 358 attacks and 322 deaths. That is from December 1897 to November 1898.

18,925. Were the majority of these cases in Bandra itself or in the villages?—The majority were in Bandra itself, and occurred mostly in July and August.

18,926. You have never been able to stamp plague out entirely in the town of Bandra?—In November we came down to two cases for the month; in December there was a recrudescence, and this year it is worse than ever.

18,927. Is it bad at present?—In this last December there were 17 attacks and 12 deaths. For the previous December there were only 2 deaths. In January this year there were 56 deaths from plague, and in January last year only 9 deaths. From the 1st to the 15th February this year there were 34 deaths from plague; in February last year there were only 17 deaths for the whole month.

18,928. During the second epidemic in Bandra itself, did you take the people to hospital and isolate the contacts?—In Bandra we had a case in February, and there were isolated or sporadic cases till July; then there was an epidemic at the south end of the town.

18,929. Did you turn the people out of any portion of the town?—When the epidemic came to Bandra it was worst in July and August and that was in the middle of the rains so we could do nothing. In addition to that, it was principally in the Muhammadan quarter of the city, and one could do little with these people. We got some of the Hindus to go out, but we could do little with the Muhammadans.

18,930. Is that still the case?—There is the greatest difficulty in getting the Bandra people out of their houses.

18,931. Is there a difficulty in ascertaining cases in Bandra?—Yes, there is a great deal of concealment.

18,932. How do you endeavour to ascertain the cases?—We have inspectors going round, we have no house-to-house visitation, simply Sanitary Inspectors going around.

18,933. Did you find most cases while the patient was still alive or after death?—We generally get intimation just before the patient is dying.

18,934. In such cases, do you find that some of the contacts have disappeared, or are they generally all present?—Sometimes they are there, and sometimes they are not. It is difficult to trace them in a large place like Bandra, it is easier to trace the people in the villages.

18,935. I believe you treated some of the infected villages in the neighbourhood of Bandra in one way and some in another, please specify the different methods of treatment and the results?—In Khari, in the outbreak of the epidemic, all the people were moved out from the village into huts outside, and we insisted upon all patients being removed to hospital immediately on detection.

18,936. What is the population of Khari?—Roughly speaking from 800 to 1,000. About 12 cases had occurred before the people camped out. All cases immediately on detection were removed to the hospital. Some member of the family was allowed to go with the patient, and the rest were isolated in our segregation camp for six or seven days.

18,937. How long did it take to stop the plague in this village?—It died out about the end of February—about two months.

18,938. Did you have any cases among the segregated people?—Not one, but I should like to add that we were very strict as far as that village was concerned. We burnt all the old clothing, and gave the people

new clothing and blankets, and also fed them for the six or seven days.

18,939. Were the persons who were segregated as contacts permitted to communicate with other infected areas in the pursuit of their ordinary avocations?—No.

18,940. In the village of Pali you adopted a different method, I believe?—Yes; there we had a different class of population to deal with.

18,941. What is the population there?—In Khari the population is mostly very poor, and consequently we could recompense them with money for their loss of labour; but in Pali the people are of a better class, and it was difficult to deal with them in a similar way. I think the population of Pali is about 600. There the people made a temporary hospital in the fields, and removed their sick to it, and all the people camped out of their own accord.

18,942. Did the people look after their own sick?—Yes.

18,943. How were the persons in segregation treated?—There was no segregation.

18,944. How long did it take for the disease to die out in this village?—I should think about six weeks.

18,945. Was there the same opportunity for communicating with infected places as at Khari?—Just the same.

18,946. Was the course of the disease the same?—Yes: it lasted about six weeks.

18,947. Have you any other village you would like to refer to? Danda, for instance?—In July we removed the people out of Dandâ, which is a village of from 3,500 to 4,000 people. About 1,500 came into Bandra. We took a census and kept them under surveillance, and within the next 10 days some of the people removed were attacked, but no cases occurred among the Bandra people in the neighbourhood.

18,948. Did you at any time during the epidemic at Danda have the contacts segregated for a certain time?—No. It was only in the village of Khari that we insisted upon segregation.

18,949. Is your experience that when you segregate contacts, but fail to keep a check upon the remainder of the population, that you put down the disease more rapidly than if you leave the contacts alone?—I do not think the disease is put down more rapidly, and I should not recommend the segregation of contacts.

18,950. Did you notice any mortality among rats before plague cases occurred in these villages?—Personally, no, but I had reports of it.

18,951. Did those reports satisfy you that mortality among rats occurred before there were any imported cases of plague?—Yes.

18,952. Do you feel sure of that?—Perfectly certain. During this present outbreak this year I have been taking particular notice of it. The village of Pali in this present month of February is an example.

18,953. Do you think that, prior to the mortality among rats in Pali, it was impossible for a plague infected person to have come from any outside area into that village?—No; it was quite possible.

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18,954. So that there may have been a person suffering in that village before the rats began to die?—They were mixing in the whole neighbourhood.

18,955. I mean that you cannot exclude the possibility of rats having been infected by some outside human being?—That is so.

18,956. Did you find that people wer as disinclined to give you information in the villages, as they were in the town of Bandra?—The people in the villages are more amenable. The smaller the village, the easier it is to get information, and the larger the more difficult; there is a general disinclination to give information.

18,957. In the villages you could get much more accurate information than in the town?—We find out cases in the villages more easily, but there is a general inclination to conceal cases.

18,958. Did they generally object to European treatment?—Yes; very few take this treatment. They are generally treated in the hospital by the native hakims.

18,959. Did you resort to disinfection in the villages?—Yes; in the village of Khari particularly.

18,960. What disinfectant was used?—We used perchloride of mercury for the interior of the houses, and carbolic acid for the narrow galis outside.

18,961. Did you observe that after a house had been disinfected, the people did not get plague in it, unless they had had opportunities for contracting it outside?—The people moved out of all the houses which were disinfected, and the houses were left vacant for several months afterwards.

18,962. Upon reoccupation, did cases of plague occur in any house which had been disinfected?—In Khari there were cases of plague last month in houses which were disinfected a year ago, and similarly in Bandra.

18,963. Might those cases have been due to outside disinfection?—Yes, it is quite possible.

18,964. You have nothing to show that the disinfection was inefficient?—No.

18,965. (*Dr. Ruffer.*) Do you know of any villages in which there has been an epidemic of plague among rats and not among human beings?—Yes.

18,966. Will you give us the names of those villages?—There is only one village where rats were reported to have died, and that is the village of Sherli. There was plague in the villages on each side, but Sherli escaped altogether.

18,967. How far off were the neighbouring villages?—Perhaps a quarter to half a mile apart.

18,968. At Kantwadi, Malla, and Rajan, you had very few deaths, I think?—Only five deaths.

18,969. There was a noticeable mortality among rats?—I do not know that the mortality was large. It is very difficult to get statistics about the numbers of rats. We get intimation that rates are dying and that is all.

(Witness withdrew.)

Mr. R. J. C. Lord, I.C.S., called and examined.

Mr. R. J. C. Lord, I.C.S.

18,970. (*The President.*) You were Assistant Judge at Broach?—Yes, and am now acting as Registrar of the High Court, Bombay.

18,971. (*Mr. Cumine.*) Where has your experience of plague been?—I saw a certain amount in Ahmednagar as Assistant Judge, and afterwards as Assistant Collector in charge of the Nagar taluka, and as Personal Assistant to the Collector I saw the reports concerning the whole of the district and had an opportunity of noticing their contents.

18,972. Is Nagar city a walled town?—Yes; it is a walled city which used to be the capital of the Nizám Sháhi dynasty.

18,973. What is the population?—Over 40,000.

18,974. Are the people crowded together?—Very crowded. The houses are closely jammed together. In one corner of the town you can see about an acre of flat mud roof with tiny openings in it. The tops of the houses are like a table, as it were, with large holes here and there, which are the only means of getting light and air.

18,975. While the people were in the town you had an establishment with a supervisor for every 100 houses in each ward and a certain number of Superintendents to look after the supervisors?—Yes; there were 12 wards.

18,976. And you had a Hospital Assistant to diagnose plague cases?—Yes; we had one, or at one time, I think, two.

18,977. Did this system enable the supervisors to become perfectly well acquainted with all the dwellers in their sections in a short time?—Yes, and thereby search parties were obviated.

18,978. Do you think it is a good thing that search parties should be unnecessary?—I consider search parties do a lot more harm than good, and should be avoided if the other system can be introduced, because a sick woman does not like to be looked at, even though she has not got plague. Nothing is gained by search parties which can outweigh the discomfort and disquiet they cause to the inhabitants.

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18,979. After the occurrence of a certain number of indigenous cases in a ward, I think you evacuated that particular ward, did you not?—Yes, we turned them out by the agency of the police.

18,980. The result was that, by a certain time, the town was completely emptied?—Yes, fairly quickly.

18,981. I understand the people who went out of this town to live, and who were not taken to actual contact camps, lived in two kinds of camps, the Savedi health camp and another one?—Yes. The Savedi health camp was erected by the Municipality, and the other camp was made by the people themselves in the adjoining fields. It is hardly fair to call it a camp, because it just consisted of little huts dotted about in the people's own fields, especially where there was a tree, or well, or something like that.

18,982. Did you have any inspection of these people living in their own huts?—Yes, by the agency of the Abkari police.

18,983. Did you find many cases occurring among them?—Latterly I think there were a good many.

18,984. What did you do when several cases occurred?—We evacuated the field, and segregated everybody in the segregation ward of the Savedi camp. That was the first step in the way of segregation which was taken in the operations.

18,985. Did the infection appear to be brought from the town, or to spread from one hut to another in the fields?—I think it must have spread from one hut to another in the fields after having originally come from the town, because we found that one case in a mala or field was followed by several others. It was not as if the cases were sporadic, but one case nearly always brought on more in the same locality.

18,986. How did you manage with regard to weavers and other handicraftsmen who had to earn their bread?—If they would live in the Savedi camp, we allowed them to have a pass to take them by day into the city, returning at night. Thus we were able to inspect them every morning and evening.

18,987. Did you find that any of them caught plague by going into the town in this way?—Only one, as far as I can remember; that was a weaver who was taken sick at his loom. He died at once, and was buried by his sons in his back yard. He was missed that night, but owing to the ingenuity with which his grave was concealed, it was not detected for, I think, a whole day, though the house and vicinity were searched for it.

18,988. Did the cases appear to get milder towards the end of the epidemic?—No, they appeared to be of a more virulent type. That is simply my impression; it struck me that, as a rule, towards the end of the epidemic, when a man got ill of plague he died within a few hours.

18,989. Did you see any cases of black plague?—One or two were reported to me, but I did not see any myself. I believe that information regarding them can be found upon the record. There were returns of a case which occurred, I think, in Nagardeola.

18,990. Did any cases come to your notice where cats appeared to be attacked with plague?—Nagar was overrun with cats, and in the opinion of the Civil Surgeon there they did get plague. They were looking very miserable, with generally a pink line about their neck. Some were seen by the Civil Surgeon, I believe, in one particular quarter of the town running about with open buboes in their necks, and they were destroyed.

18,991. Do you know whether they were bacteriologically examined or not?—No, they were not, as far as I can remember. The great thing was to kill them and get them out of the way, to prevent the re-infection by them of the houses we had just disinfected. We did not go any further into the matter. The Civil Surgeon may know more about it, but I do not.

18,992. Were there many rats in Nagar?—Very few, perhaps on account of the number of cats.

18,993. Did the plague appear to take any particular course through the town of Nagar?—From east to west, practically, along the three main roads. It seemed to go by a line and then diverge and then pass out through the different gates. It passed in a very regular line, but I dare say that was owing to the fact that evacuation followed that line; but we did not find that the few people whose removal would have endangered their lives who were still allowed to remain got plague behind the line of march.

18,994. I suppose you treated the villages in much the same way as the town?—Yes, practically.

18,995. With all these people going out of Nagar town, did the people in the villages appreciate the danger of admitting refugees who might possibly be infected?—At first they did not; afterwards they did.

18,996. Did you find that although people might be averse to going out in the first instance, still when they had lived a little time in their huts they were perfectly willing to stay there?—Yes, they settled down to it more or less. Of course they preferred to live in the city. Until the middle of February they used to complain of the cold, and then in the third week of February they complained of the heat: it was only really that they preferred their houses; but afterwards, when it came to the time for closing the Savedi camp, there was a considerable difficulty in getting some of the people out of it.

18,997. Did you have many cases in the Nagar district of plague imported from Bombay not taking root in the Nagar district?—According to my recollection there were a great many such cases. So far as I can remember, I think there was only one case at Sathral, in the Sangamner taluka, from Bombay, which was followed by indigenous cases, but I am not certain if even it came from Bombay. Others may have occurred before my connection with plague operations, but I do not think they did.

18,998. Did you find that cases of infection imported from Poona seemed to take root more easily than imported cases from Bombay?—Very much more easily; in fact, I believe it will be found that every case imported from Poona or Sirur led to an outbreak.

18,999. Where did the infection come from to Nagar town itself?—It came from Nagar Cantonment, to which it came from Sirur. Sirur was between Poona and Nagar.

19,000. Where did the infection in the villages come from?—It mostly came from Poona, especially in the Parner and Shrigonda talukas, and to the best of my recollection a certain amount came from Igatpuri.

19,001. How was the infection taken from one village to another?—By sick people.

19,002. By human agency?—Yes.

19,003. How did it appear to be carried from one house to another within a town?—I suppose by human agency, but I must say that as far as I could see in big blocks of houses it did not pass from one house to other houses adjoining it in other streets (*e.g.*, touching back to back, with doors in different streets), but travelled up the street, *i.e.*, it probably was brought through the doors and not the walls.

19,004. (*Prof. Wright.*) Did it surprise you to find every case from Poona affecting the village?—That is more or less a generalization on looking back; I mean it did not strike me at first, and it was only latterly that I came to think about the matter. It did not attract my attention at the time.

19,005. Can you suggest any explanation of the fact?—No. My private opinion is that the bacillus from below the ghats had got to be acclimatized in a damp place, because Poona is a damp place, and so is Igatpuri, more or less, and then Sirur was a little drier, and so the infection was brought gradually to a place in a condition in which it could live. I believe the same thing has been noticed in the Broach district, because cases taken straight from Bombay to Broach did not seem to spread. But when cases were brought into Broach from Ankleshwar, which is just south of Broach, the plague spread at once. I believe there had been several imported cases from Bombay direct into Broach which did not spread.

19,006. (*Dr. Ruffer.*) How long does it take to go from Bombay to Broach?—Just one night.

19,007. How long to Ankleshwar?—Pretty much the same time. Ankleshwar is about 6 miles from Broach.

19,008. How long does it take to go from Ahmednagar to Bombay?—About half a day.

19,009. How long does it take to Poona?—A quarter of a day.

19,010. (*The President.*) Do you know what is meant by the "black plague?"—No. We called it "black plague" because the people used to turn black.

19,011. After death or before death?—They were black after death; I do not know when the exact change took place.

(Witness withdrew.)

The Hon. Mr. BHALCHANDRA KRISHNA, called and examined.

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19,012. (*The President.*) You a Licentiate in Medicine of the Bombay University?—Yes.

19,013. And a practitioner here?—Yes.

19,014. You take much interest in the affairs of the Municipality?—Yes; I am President of the Corporation.

19,015. And a member of His Excellency the Governor's Council?—Yes.

19,016. What is your opinion as to the origin of plague in Bombay?—I believe that the theory that it was introduced by fakirs from Garhwal and Kumaun is not tenable. The theory advanced was that some mendicants came from that place and brought with them the poison of plague. I am not prepared to accept that theory.

19,017. Why not?—Because these people came all the way from the north, and they must have halted at different places. They must have gone to Nasik, where there was an annual fair where thousands of people had assembled. If they went there, they were sure to have given the poison to those people, because they were in contact with them. Nasik was not first infected; in fact, it was during the second epidemic in Bombay that some cases occurred in the Nasik district. If the theory be correct, then Nasik and the places where those pilgrims halted should have had the plague to commence with, whereas we find that it commenced in a certain portion of Bombay called the Port Trust Estate.

19,018. How long does a person take to go from Garhwal to Nasik, as a rule?—At least 8 days.

19,019. What view do you entertain as to the origin of plague in this city?—Another theory has been advanced that the plague bacillus was generated *de novo* in Bombay on account of the insanitary condition of those places where it first originated. My own view is that it must have been introduced into Bombay from Hong Kong—first on the Port Trust Estate. My reasons for holding that as a working hypothesis—I will not call it a theory—is this. If we trace the earliest cases of plague in Bombay, we find that about the 15th August cases began to appear; at least, they were noticed at that time, although they were not diagnosed as cases of plague, but as a peculiar kind of fever. The medical men who attended those cases were not in a position to exactly diagnose them. I am informed that the first case occurred in a house situated in Broach Street, in the Port Trust Estate, belonging to Mr. Rowjee Heerji. That is about the 15th August 1896. After that, very close to it, a case was noticed by a medical practitioner on the 21st August 1896. Near the house of Mr. Rowjee Heerji there is a godown belonging to Mr. Dhomo Narsi, situated in Surat Street, on the Port Trust Estate. Close to this house there is a godown of grain, and this godown abounds in rats. I believe that the first infection was communicated from those houses to the rats which abound in this grain godown, and from thence it spread. Even up to this time it is quite circumscribed and confined to one side of the Port Trust Estate. From thence it spread to the other side of the Mandvi Bridge, what is called the Argyle Road and Clive Road, where there is a large mala, called Vakhatna Mala, situated on Clive Road. Then it spread to a house belonging to Mr. Lakhamsi Napu. It is here that Dr. Viegas, who first diagnosed it as plague, found his first case, and he announced it at a meeting of the Standing Committee, of which I was the Chairman, on the 21st September 1896. The history of this case seems to be that Mr. Lakhamsi Napu had some labourers connected with his firm, and they were residing at a place called Marwadis' Mala, where the second case occurred on the 21st August. Two labourers from this Marwadis' Mala used to be associated with the nephew of Mr. Lakhamsi Napu, and all of them died of plague. My argument is this, that all this portion of the Port Trust Estate has godowns, where Chinese goods are stored. There are three on Argyle Road, where Chinese crackers are stored, and there are two or three on Broach Street and Surat Street, where Chinese sugar and Chinese silk are stored. Chinese crackers are generally brought to Bombay in the early part of July, or the beginning of August, and I believe that the infection must have been imported into Bombay by these goods. Crackers are used in August, and then again in November. For nearly a month plague was confined to these Estates only. It did not make its progress beyond Mandvi Bridge towards the city. It was after a month that it entered the city, and then it spread to all its parts. From these facts, I am inclined to believe that it was imported into Bombay from China.

19,020. What is your opinion as to the way in which the disease is communicated?—I believe it is communicated in several ways. The first and most important is by infected and dead rats. I have known of cases where dead rats have communicated the disease.

19,021. Can you give us an example of these?—During the first epidemic in 1896 I saw four cases where the people had handled dead rats; they developed plague and died.

19,022. How soon after handling the rats did they acquire the disease?—Three or four days.

19,023. Had these persons any opportunity of obtaining the infection elsewhere?—So far as I could ascertain, there had not been any other source of infection. Then another case occurred lately in a Parsee lady, near Girgaum. She was taking out rice for cooking, and a dead rat came on to the palm of her hand, and next day she got plague. She was under my treatment, and she died in three days.

19,024. Was there any injury on her hand?—I could not trace any.

19,025. How do you think she acquired the disease—by mere contact?—Yes.

19,026. Through the epidermis?—I believe so.

19,027. Through the uninjured epidermis?—I believe so, because there was no wound on the hand, so far as I could see.

19,028. Besides communication through rats, what other means do you consider may be operative?—I consider the locality is the most powerful source of infection, where dead rats were found. Even if the rats are removed that locality retains the poison, and that is a fruitful source of the disease. Then infected clothing is also a fruitful source. I have seen one or two cases of this, especially one very marked case. It was the case of a young man who, simply through hardihood, slept in the bedclothes where a patient had lain. One case was removed from the bungalow of the Honourable Justice Ranade. The boy was removed to the Hindu Fever Hospital, with which I am connected, and the boy who brought him to the hospital slept in his bed clothes. The result was that three days afterwards he had a very violent attack of plague, and he died on the third day, whereas the boy who was removed previously recovered.

19,029. Had not the boy who died any opportunity of coming into contact with the plague patient who had been removed, before he slept in these bed clothes?—He brought that first boy to the hospital, and there were others with him who were not affected.

19,030. I mean before this boy was taken to the hospital?—They were living in the same house. I made a note of this, because there were others who came with him who all escaped. They lived in the same house, under the same conditions, and they escaped; whereas this boy, in order to try the experiment, slept in the bed clothes one night, and on the third day he got attacked.

19,031. In addition to that, what other means of conveyance are there?—Contact with plague patients.

19,032. Do you think that this contact can introduce plague into the body through an unbroken skin? Have you any observations which bear upon that point?—I cannot distinctly say that, because it is a very disputed point; in fact, I think I have come in contact with plague cases several times myself, both in private practice and in the Hindu Fever Hospital. Although they are few, at the same time cases have occurred of medical men and ward-boys who were in contact with plague patients who have died of the disease. The medical man in charge of the Hindu Fever Hospital, after three months, all of a sudden, developed plague. So far as I could see, there was no injury to his hand or to his body, and there was nothing to account for it.

19,033. Did you search specially for any abrasions?—Yes; he was under my treatment.

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19,034. You looked at the skin carefully with that object?—Yes, and so far as I could discover, there was nothing.

19,035. What kind of plague was it?—Ordinary pneumonic plague.

19,036. Further than that, do you think discharges from the patients might cause plague?—Yes; discharges from patients, especially patients affected with pneumonic plague, and their sputum. I have noticed cases occurring from the contact of discharges from the body of a plague patient after death.

19,037. What discharges?—Fæcal discharges, or urine, which, falling upon the corpse bearers, gave them plague. Amongst us, the corpse is borne on the shoulders of men; it is not carried in hearses, and so on, but the people carry the dead bodies. Some four cases came to my notice, where the discharge fell from the dead body upon the bearers, and gave them plague. Other cases must have occurred, but I only allude to those cases which I have seen.

19,038. Have you particulars of those cases?—One is a gentleman named Munshi. During the first epidemic in 1896, he assisted in carrying a dead body, and about four days afterwards he developed plague and died.

19,039. What is the evidence that any discharge fell upon him? How do you know that he came in contact with anything proceeding from the corpse?—There is no particular evidence to show that. I was not able to examine the body at the time.

19,040. Now, the second case?—The second case is of Mr. Lokhande, who was similarly infected, but I cannot give any positive evidence that the discharges fell upon his body. These people were not living in an infected place, and they were not living where dead rats were found, and they did not come in contact with any other plague cases except those dead bodies. That much I can say. I am not able to produce direct evidence as to discharge falling from the dead body.

19,041. What is your opinion as to the value of curative serums?—There are three curative serums which have been tried. With regard to Haffkine's curative serum, I think it failed. With regard to Dr. Yersin's, I saw one case which was treated with Yersin's serum and was cured. That was the only case of plague I saw cured by Yersin's serum. In order to give further trial to the serum, I requested Dr. Yersin, together with Dr. Blaney, to go round the Hindu Fever Hospital. He examined all the patients, and said that several patients were already recovering, so that there was no necessity to inject the serum. With regard to three cases which were almost moribund, he said it would not be advisible to waste his serum. But, fortunately, those three moribund cases recovered under hospital treatment, and a note has been made of that by Dr. Lowson in his report*.

19,042. The evidence which came under your observation does not show much?—No.

19,043. You have given a great deal of attention to the value of Haffkine's prophylactic fluid have you not?—Yes.

19,044. I believe you yourself have been inoculated?—Yes.

19,045. Will you describe to the Commission what effects the inoculation produced upon you?—I was inoculated twice, in February and March 1897. On the first occasion I was given 2 c.c., which was injected into my left arm. That was in the evening about 6 o'clock. I was able to attend to my work up to 9 o'clock. After that I had fever which lasted throughout the whole night, and my temperature rose to 101. I was restless all night and had disturbed sleep on account of dreams. I had copious diuresis and severe headache. In the morning I perspired, but I was able to take my bath and attend to my duties, only I felt exhaustion and inability to work.

19,046. When did the temperature go down?—In the morning. I had perspiration in the morning at 6 o'clock and the temperature went down, and I was able to take a bath.

19,047. Now with regard to the local phenomena?—There was swelling of the left arm at the seat of inoculation.

19,048. When?—At the same time that I had the fever, at 9 o'clock, and there was pain. The pain on the first day was not very severe, but on the second day I was not able to move my arm because it was painful. That lasted for three days and left a very thick nodule at the seat of inoculation, but no other symptoms. In fact, after three or four days I felt better than I had felt previous to inoculation. That is say experience of the first inoculation. In March I was inoculated a second time with 2½ c.c. and the symptoms were nearly the same, but on this occasion the temperature rose to 102, and I had disturbed sleep, as I have mentioned. There was no inconvenience of any kind. The sequence of symptoms and the effect on my body were the same as on the first occasion. Then I was inoculated again in February 1899, that is this month.

19,049. A third time?—Yes, about the commencement of February—and I had the same symptoms. I have yet a slight pain left in my arm, but beyond that there is no inconvenience. There is one thing I noticed, and that was a little erysipelatous inflammation running down the lower portion of the arm to the elbow. Beyond that I had nothing to complain of.

19,050. There was no diminution in the severity of the symptoms in the subsequent inoculations?—There was no diminution.

19,051. Is it in conformity with your general experience that the injections have not done much harm?—I have seen several cases, and so far as I have been able to gather it has not done much harm in the majority of cases. I have come across cases where it has produced general malaise, inability to work, or very slight rise of temperature; but beyond that I have not come across any case in which it has produced absolutely serious symptoms. There is one case which I must place before the Commission, and that is the case of a Khoja girl who was shown to me six months after inoculation. She is about 12 years old, and her parents told me that she developed the symptoms a month after inoculation. Those symptoms were the second stage of phthisis. The girl is living now. I treated her and she is all right. I must mention that the family has an hereditary taint of phthisis. Her elder sister died of consumption, but the parents said that this girl developed the symptoms after inoculation. This is a case which occurred in my practice and I must mention it, but I am not prepared to believe that the symptoms were due to inoculation. Very likely if the girl had not been inoculated she would have developed the same symptoms. There is no evidence to show that it was the inoculation which brought on the symptoms. I referred the case to Dr. Haffkine and he also agreed with me.

19,052. Have you been able to follow the after-history of many of the cases which have been inoculated?—Yes.

19,053. Have many of them died to your knowledge?—I have known of one case personally which succumbed after inoculation. It was the case of a Vakil, a strong young man who was living in a house where dead rats were found. It seems that he felt symptoms of fever and plague coming on, and ran up to Bombay to get himself inoculated. On the same day he returned to Thana, and developed very severe plague and died. This is the only case which came to my notice.

19,054. To what do you attribute the plague in this instance?—I attribute it entirely to the previous infection, and not to inoculation. This case has been made much of by the opponents of inoculation, but I know the circumstances thoroughly, and the Commission may take them for what they are worth. The second case is of a Parsee boy, but that was not in my practice. He died after inoculation, but I do not know the circumstances of the case.

19,055. On the other hand have you observed any good effects, outside of the prevention of plague, to follow inoculation?—Yes, I have come across some cases. I mentioned those in a paper which I read in November 1898 before the Grant College Medical Society on M. Haffkine's prophylactic inoculations as follows:—"In certain cases inoculation has undoubtedly produced a beneficial effect on the general health of the inoculated. I have personally seen two cases in whom the anti-plague inoculation produced remarkable results. A medical gentleman, whom many of you know, and who was habitually strong and robust, began, some time back, to lose flesh and strength, and in the course of some months became anæmic and weak. An eminent

* Report on the Epidemic of Plague, from 22nd February to 16th July 1897, by James A. Lowson, Plague Commissioner, Bombay.

physician of our city whom he consulted, pronounced him to be phthisical, but he was not supported by another, also a very competent gentleman of the profession, who declared that he was suffering from an exhausting disease, the nature of which he was not able to diagnose definitely. The patient was treated, but without any beneficial results. While in this condition he was advised to get himself protected against the plague. The result upon his general health was most striking. He began to improve almost immediately after inoculation, and in a few months gained up to 15 lbs. in weight. He dropped all other treatment, and has now completely regained his health and weight. The second case which came within my own observation, is that of the wife of an eminent citizen of Bombay, a Hindu additional member of His Excellency the Governor's Council. She was suffering for a long time from intermittent fever which was not amenable to any treatment. When the rest of the family was inoculated against plague she was inoculated too, at first cautiously, and then with another dose. The two inoculations had the unexpected effect of subduing entirely her habitual fever. Prof. Haffkine has kindly placed at my disposal four other cases of analogous nature. One is the case of Mr. K., residing at Nepean Sea Road, Bombay, who was suffering for a long time from a daily rise of temperature which defied all treatment. Mr. K. desired to undergo inoculation against plague, and requested Dr. Temulji B. Nariman, his medical attendant, to consult Prof. Haffkine. The answer was :—" It is advisable to abstain from inocula- " tion in cases of acute or sudden attacks of fever " the nature and the issue of which is not taken, but " there is no contra-indication in cases of chronic " fevers; only the inoculation should be done in such " cases with reduced doses." Under this advice Mr. K. underwent inoculation by Prof. Haffkine himself, with the result that the inoculation produced the usual rise of temperature, which subsided within 24 hours; but with that Mr. K.'s usual fever was entirely cured. Prof. Haffkine visited the gentleman a fortnight later, found him in perfect health, and inoculated him again with the full standard dose, to which he re-acted as a perfectly normal case. The other cases were those of Captain MacDonald, I.M.S., and his servant, who were suffering from continuous attacks of malarious fever defying all treatment. They were inoculated against plague with the result that their chronic fever left them entirely. Another similar case is that of Dr. K. N. Gokhale, L. M. and S., Chief Medical Officer, Bhor State. Dr. Gokhale, when he first joined his appointment, suffered from fortnightly attacks of fever. When plague broke out in the State he went to Poona and got himself inoculated in the David Sassoon Hospital. From that time he never had another attack of fever." These and other similar cases to a great extent bear out Prof. Haffkine's view that the inoculation against plague seems to influence favourably the resistance to some other diseases than plague, and especially fevers.

19,056. Will you state generally the result?—The result has been beneficial in those cases, although the inoculation was not done with the object of curing them, but simply as a preventive of plague. In the case of the Parsee gentleman whom I have mentioned, he was a medical man himself, suffering from a wasting disease, and the physicians of Bombay were not able to diagnose his case. Some said it was phthisis, and others said it was a wasting disease.

19,057. It was a vague affection?—Yes.

19,058. What were the others?—They were malarious fevers. They have distinctly been benefited. There was the case of a friend of mine, a member of Council also, and his wife. She suffered from malarious fevers and she was inoculated for plague, and the result has been that she has shaken off the fever.

19,059. Have you never found cases of malarial fever recover rather unexpectedly?—They do.

19,060. Likewise in cases of vague illnesses, such as a wasting disease, sometimes a most unexpected improvement takes place?—That is so. I have said that it may not be due to inoculation.

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19,061. Have you had much experience of the effects of the prophylactic in actually preventing plague?—Yes.

19,062. Under your own observation?—Yes.

19,063. Have you the statistics?—I have mentioned them in the pamphlet* which I have put in as an accompaniment to my précis of evidence.

19,064. Have you any collection of cases?—I have collected a few cases.

19,065. Will you put them in and tabulate them?—I have not put them in tabular form, but they are all included in these statistics.

19,066. Individual cases are not likely to be of much value unless they are very definite. Are there any which you would like to state particularly?—I will mention two or three cases. For instance; in the family of a Parsee friend of mine several members were inoculated. He was living at Parel. His servants, some four or five Bhyas, refused to be inoculated, and when plague spread to Parel two of those four Bhyas died. Then the remaining two insisted upon being inoculated, which was done, and they did not get plague. Then in the case of a Muhammadan friend of mine, also a member of the Governor's Council, all the members of his family were inoculated, but some three servants refused to be inoculated. During the first epidemic those three servants succumbed, while all the rest escaped the disease. In another family, in the same way all the members were inoculated except one girl, and that girl died, while the others escaped. These cases I mention for what they may be worth.

19,067. (*Mr. Hewett.*) What were the main types of the disease in the three different epidemics?—Firstly, bubonic plague, then pneumonic plague.

19,068. When was that?—That was in the second epidemic.

19,069. Did this type prevail throughout the second epidemic?—It was in the middle part of the second epidemic.

19,070. Can you give us the months approximately?—I think it was in the months of February and March.

19,071. What is the chief type of the third epidemic?—The third type has been almost pneumonic; it is a mixed type. Sometimes there were not enlarged glands at all, but simply fever and delirious symptoms, but in others only the pneumonic type.

19,072. What is it mainly at present?—At present it is bubonic.

19,073. Is it very fatal now?—Yes, it is.

19,074. Is the bubonic form now more fatal than it was in the first epidemic?—Yes, it is.

19,075. Have you noticed any instances of people catching pneumonic plague from a bubonic patient?—No.

19,076. Have you noticed any cases of people catching bubonic plague from a pneumonic patient?—No.

19,077. (*Dr. Ruffer.*) Do you think a great many of the patients in this epidemic get lung symptoms?—Yes.

19,078. Do you consider that a bad sign in plague?—I do.

19,079. With reference to the case that travelled up from Thana in order to get inoculated, how long afterwards did he get plague?—Six hours afterwards. He was inoculated, and immediately developed plague after going to Thana.

* Not published in the Proceedings of the Commission.

(Witness withdrew.)

Dr. D. A. D'Monte called and examined.

Dr. D. A. D'Monte.

19,080. (*Mr. Cumine.*) You are a non-official, I think?—Yes.

19,081. You are Vice-President and Plague Authority at Bandra?—Yes.

19,082. I think that Bandra has had an epidemic of plague during each of the three last years?—Yes.

19,083. Can you say which of the epidemics was the worst?—The first one was the worst; but the third one is not yet finished.

19,084. I mean with regard to the virulence of the germ?—This one, I think, is more virulent.

19,085. Was the second one milder?—It was mild, I think, compared with the others.

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19,086. In your précis of evidence you mention that the first case during the present epidemic occurred in a hut where the first imported case from Bombay died in 1896?—Yes. This seems to show that the germs remained dormant for a period of 24 months.

19,087. Might not the present infection have been imported from outside by the person attacked?—I do not think so. This was an indigenous case; it occurred in a person who never went to Bombay, and could not have got the infection from elsewhere.

19,088. Had anybody from an infected place come to him?—I do not think so.

19,089. Had there been any cases in this hut in the previous epidemic?—Not in the second epidemic.

19,090. Have you noticed whether, in the succeeding epidemics, the cases seem to occur in the same houses?—Yes.

19,091. Do they occur in those houses only?—They occur in other houses as well; but, generally, in those houses where plague occurred in the first and second epidemics, cases have occurred again this year.

19,092. Are you able to say that the infection was not brought from other houses into these houses in the present epidemics?—In some cases they were not brought from other houses.

19,093. You think it was a resuscitation of the old germs in the house itself?—Yes.

19,094. How are you able to say positively that in certain cases there was no re-infection from other houses?—I could not say positively.

19,095. You mention, in your précis of evidence, two cases of buboes in cats, and also fowls getting plague. How do you know the fowls had the plague?—Because just before some cases occurred in some of the houses in a village, lots of fowls died.

19,096. How do you know that they died of plague?—Because they actually had buboes in the neck and groin.

19,097. Have you any instance where a person has certainly caught plague from clothing?—No.

19,098. Have you any instance where persons caught plague from an article of food?—No.

19,099. Have you any instance where people have caught plague from the corpse of a person who has died of pneumonic plague?—Yes.

19,100. Is it within your knowledge that the people living on the ground floor of houses are more susceptible to the disease than those living in the top floors?—Those living in huts and on the ground floors are more susceptible.

19,101. Is the nature of the floor different in the ground-floor rooms from what it is in the upper rooms?—The top floors are made of wood, and the ground floors are of earth.

19,102. When people live in sheds in the fields, does the plague spread much from hut to hut, or only a little?—Only a little.

19,103. Have you had any experience of inoculation with Prof. Haffkine's prophylactic serum?—Yes.

19,104. How many cases have come under your notice?—About 27.

19,105. Did you observe any bad effects in any cases?—In one case. That was not inoculated by myself, but by Prof. Haffkine himself.

19,106. What was the bad effect?—I did not mean a bad effect of inoculation; but this man was inoculated, and he got a bubo after inoculation, and died.

19,107. Did you see him?—Yes.

19,108. Have you any instance where a man has caught plague three times?—There is one case.

19,109. Did you see the man each time?—Yes.

19,110. What was the interval between the three attacks; could it not have been a relapse?—Three months the first time, and six months the second time.

19,111. (*The President.*) We had better have the exact date of each occurrence?—I have not them here, but I will give them. (Witness intimated later that he was not able to get the exact dates.)

19,112. (*Mr. Cumine.*) How many cases of Haffkine's curative serum have come under your notice?—Four.

19,113. What happened in each case?—They all died.

19,114. How many instances of Yersin's serum?—One, and that patient recovered.

19,115. How many cases of Lustig's serum?—Two, and both recovered.

19,116. (*Prof. Wright.*) Did you examine those fowls which died of plague?—Yes.

19,117. How many *post mortems* did you make?—I did not make any. I saw the fowls myself.

19,118. How did you see they had buboes?—I could feel them.

19,119. How many buboes had each fowl?—Some had one, and some had two.

19,120. They were not examined bacteriologically?—No.

19,121. In the case which you report, in which a man had plague three times, can you tell us whether he had buboes each time?—The first time he had a bubo; the second time he had no bubo; and the third time he had a bubo.

19,122. How was it known that he had plague a second time?—He had a very high temperature, rigors, and drowsiness—the same symptoms, almost.

19,123. Where were the buboes in the first and third times?—In the groin, both times.

19,124. The same groin?—Yes

19,125. Did he recover from the third attack also?—He died of the third attack.

19,126. (*Dr. Ruffer.*) What class of people live in the ground floor, as compared to top floors; are they a better class of people?—No, they are poorer.

19,127. (*The President.*) To what do you attribute the difference; how do you explain the difference in mortality in the ground floors, as distinguished from the higher floors?—To the dampness of the floor.

19,128. Is there any difference in ventilation?—Yes; the top floors are better ventilated.

19,129. The conditions there are, in that respect, similar to those which prevail in the huts in the fields?—Yes.

(Witness withdrew.)

(Adjourned till to-morrow.)

At The Secretariat, Bombay.

FIFTIETH DAY.

Saturday, 18th February, 1899.

PRESENT:

PROF. T. R. FRASER, M.D., LL.D., F.R.S., (*President*).

Mr. J. P. HEWETT.
Mr. A. CUMINE.
Dr. M. A. RUFFER.

Mr. C. J. HALLIFAX (*Secretary*).

Dr. C. H. FREEMAN UNDERWOOD called and examined.

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19,130. (*The President.*) You are a doctor of medicine, I think?—Yes, and a licenciate of the King's and Queen's College of Physicians, Ireland.

19,131. Have you any official position in regard to plague?—No. I am in private practice in Bombay.

19,132. (*Dr. Ruffer.*) Can you give us any evidence as to the origin of plague in Bombay?—To the best of my knowledge the plague has been in Bombay in 1690, 1700, and also in 1804–5.

19,133. I mean the present epidemic?—In the epidemic of 1896 my initial cases were observed in the latter part of July or the early part of August 1896. Following on these I managed to obtain three photographs which I took of what appeared to me a peculiar type of disease. Two were natives, and one is an European.

19,134. When were these photographs taken?—These were taken as soon as I saw the cases in the latter part of July, or early days of August 1896. The generality of the cases I observed were rapid in incidence and fatal in termination without glandular developments; the course of the disease being terminated within 24 or 48 hours of its incidence. Following on these I managed to obtain photographs of three peculiar cases, two natives and one European, the former being cases of bilateral parotid enlargement, an intumescence of the thyroid gland and the tissues of the neck, the pharynx and tonsils being deeply injected, and aphonia, *tinnitus aurium*, and occasional flashes of light before the eyes being complained of; the maximum temperature in the Muhammadan case was 104·8, whilst the Hindu showed 105-F; both recovered.

19,135. I want to know first about the cases which terminated fatally in two days. First of all what evidence have you got to show that they were cases of plague?—From the rapid course, and, to say the least, the unique and peculiar clinical features of these cases, I was led to diagnose them as *typhus siderans*. No previous cases had been reported, and as it was very obscure at the time I put these down as *typhus siderans*.

19,136. What evidence have you got to show that these were cases of plague?—The rapid course, the high fever, and the glandular enlargements. My subsequent experience of plague proved beyond doubt that they must have been cases of plague.

19,137. Why?—Because you get, even in the present day, cases that commence with very high fever. The nervous system seems completely to be shattered by the virulence of the poison, and in fact sufficient time does not remain for the development of glandular enlargements.

19,138. But do not you get similar symptoms from other microbes, for instance in septicæmia from post-mortem wounds?—In such cases there is always the history of pre-existing wound infection.

19,139. Did you look for an infection wound?—Yes. It was said here by some medical men that the glandular cases were generally the result of infection.

19,140. I am not speaking of the glandular cases; I am speaking of the other cases?—I have not been able to trace any infection through external wounds or abrasions.

19,141. The only evidence you have got that these cases were plague was the presence of profound intoxication?—Yes, and that they died quickly.

19,142. You made no bacteriological examination of the rapid cases?—No, not of rapid cases. When I saw them coming on I made bacteriological examinations, I made an agar culture, and I then found the plague bacillus.

19,143. What month was that?—It was September 1896, or the latter part of August. The cases were not so numerous.

19,144. You did not make any bacteriological examination of these rapid cases in July or August?—No.

19,145. When was the first case in which you made a bacteriological examination?—I cannot rely on it, but I believe at the latter end of August.

19,146. There you found the plague bacillus?—Yes.

19,147. Did you recognise it at the time?—Yes.

19,148. Did you notify it to the authorities?—Having no voice in the councils of the city, I sought the Health Officer, but on each occasion missed both him and his Assistant, in consequence of their multifarious duties demanding their presence elsewhere, and it was left to Dr. Viegas to bring the matter before the Standing Committee of the Municipal Corporation.

19,149. Have you seen similar cases since?—Very many.

19,150. Now you are able to diagnose them as plague cases?—With certainty.

19,151. Are these acute plague cases which you observe now similar to the cases which you saw in July, or do they differ in any way?—They seem very similar and present the same acute septicæmic intoxication.

19,152. Can you give us an account of the acute symptoms noticed by you in July?—I was called to see these patients when they were suffering from high fever. Many of them were in a comatose condition, some of them with a low muttering delirium: others could be roused when loudly called to, relapsing again into this apathetic, or semi-conscious state; the fever ran high; there were no glands at the time, and there seemed to be no pulmonary, cardiac, or any other complication as far as could be made out. Some of them had bronchitic complications, but nothing much to speak of.

19,153. Did you examine the eyes?—Their eyes were injected.

19,154. What was the tongue like?—Coated and dry.

19,155. You held no *post-mortem* examination?—No, it was impossible, as a private practitioner, to do so.

19,156. Can you tell me where these cases occurred; in a certain district, or scattered through the town?—In a certain district—in Mandvi.

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19,157. At that time, had you already found dead rats at Mandvi?—It was I who first drew attention to the rat mortality.

19,158. When did you first draw attention to the mortality among rats?—In 1896.

19,159. What month?—I believe in the month of September. Previous to that rats died here and there, but in September, when the plague increased in virulence, rats died in very large numbers.

19,160. In all bubonic cases you observed in July or August, in what part of the body were the buboes?—The larger percentage was in the right inguinal.

19,161. The photographs you have shown us are mostly cases of cervical enlargements?—Yes, but those were seen by me also about the early part of the epidemic of 1896.

19,162. When did you observe these cases of enlargements in the neck?—I believe in August, I am not quite sure.

19,163. When did you see the cases with enlargements in the groin?—In the latter part of August and September.

19,164. How are you certain that these cases were plague and not enlargement of the glands from other causes?—From the rapid incidence of the disease, the high temperature and the absence of any syphilitic or other contamination.

19,165. Anything else?—The dirty and filthy condition of the houses that they lived in.

19,166. Did these cases terminate fatally, or did they not?—Not all.

19,167. What was the proportion?—In 1896 I was fairly successful.

19,168. About how many cases did you see in August?—Between 10 and 15.

19,169. How many died?—Most of them died.

19,170. Did you notice any cases of pneumonia at that time?—I did. I was the first who drew attention to that in Bombay.

19,171. In August?—No, in September.

19,172. Did you consider these cases of pneumonia at that time as plague, or as ordinary pneumonia?—I had my suspicion that it was not the ordinary pneumonia on account of the very exaggerated symptoms and the rapid development of hæmorrhagic expectoration.

19,173. In the cases of pneumonia did you find bubonic cases in the same house?—I cannot say that I did.

19,174. Did you ever see several cases of pneumonia occur in the same house at that time?—I have noticed as many as two, not more.

19,175. Were these cases in the worst part of the town?—At Mandvi. There is very little to choose between the worst parts.

19,176. They are all bad?—Yes.

19,177. (*The President.*) Was it in the worst part of Bombay?—Yes, Mandvi is a very bad part. It was built on the former sweepings of the town.

19,178. (*Dr. Ruffer.*) How do you think the disease got into Bombay? Have you any facts bearing on that?—Yes, I believe that the disease has been hanging about Bombay, on and off, for some centuries.

19,179. What facts have you observed to make you think that?—I quote from authorities.

19,180. I do not want quotations, I want facts. Did you ever, before July, 1895, see a case of plague, or a suspicious case of plague?—I could not say that.

19,181. Do you know any medical man who has seen a case of plague, or anything like plague during that time?—I do not think so. There is a medical man in Bombay, who, I believe, saw cases; on my asking him to give his evidence here, he replied that he "did not care to waste his time—his time was too valuable."

19,182. Then you have practically no evidence on that point. Have you any other reasons for thinking that plague was present in Bombay?—Not recently, not within any short period before its development in 1896.

19,183. Say within the last two years before 1896?—I have evidence of it being here in 1804 and 1805, when it ruined the city completely.

19,184. Within the last 10 years?—No, not within the last 10 years.

19,185. Do you think the disease was imported into Bombay?—No, I do not.

19,186. Why do you think that it was not imported?—Because there is a history of it having been about Thana Daman and Bombay before, extending over a period of two centuries.

19,187. I mean within the last 10 years?—Not within the last 10 years. The last I heard of it was in 1836 and 1837, in the Jodhpur State and at Pali.

19,188. Have you any evidence to show that plague was present in Bombay or parts round Bombay between 1836 and 1896?—I have not, but what I would wish to suggest is this: that the microbe has been in the soil.

19,189. Have you any facts to show that the microbe was in the soil?—The rat mortality points to that.

19,190. The rat mortality broke out after you had seen suspicious cases of plague. Have you any other facts to show that the plague bacillus was present in the soil in India between 1836 and 1896?—No, I have not. The rat mortality was coincident with the appearance of plague.

19,191. Can you give us some information as to the course of the disease?—The disease was spread by human agency chiefly. Mandvi is a business centre, and many people from various parts of the town find employment there during the day. It being at that time an infected spot, it is possible that they may carry away the poison in their systems to the other localities of the city, and there, after the period of incubation had been completed, the disease developed itself. It spread first in a westerly, then in a southerly, and lastly in a northerly direction.

19,192. How do you know that?—The spread was slow, and I traced it on to Bhuleshwar and Girgaum, and other parts of the town.

19,193. How do you know that there were not other cases in other parts of the town? All you know is that there were certain cases in that direction, but when you say it spread in one direction you mean that it spread in that direction alone?—We get the mortality tables from Dr. Weir.

19,194. We have had the tables from Dr. Weir. You have no other facts?—No.

19,195. In your précis, you say you think there were "influences, meteorological and telluric, which acted as "determining factors on the disease"?—We had a water-logged soil which was consequent upon the heavy monsoons that we had, and an obstruction at the outfall of the sewage system, which led to a damming up of the whole of the sewers and overflow of the sewers on to the low lying districts of the town. Add to this, the retention in the soil of the balance of 10 million gallons, the daily water supply of the city, and the water-logged condition is accounted for.

19,196. What evidence have you got to show that these conditions would spread plague, or would influence plague in any way?—The soil pollution and dampness would influence the development of the plague bacillus.

19,197. Why?—Because an organically polluted soil is a very favourable medium on which to grow any sort of bacterial entity whatever.

19,198. Have you any other reason to believe that the sewage or damp contaminated soil would spread the plague?—The exhalations from these polluted areas of course would vitiate the atmosphere and be determining influences, especially where the air was bad in stagnated localities—from aërial stagnation.

19,199. You think overcrowding would act in the same way?—Yes.

19,200. Do you think it would act in any other way?—It would lead to a vitiation of air, and the air would be re-breathed. It would have a devitalising influence on the human economy, and render it prone to the inception of any disease whether zymotic, or other.

19,201. Could you give us any evidence as to the pneumonic form spreading plague? Have you noticed that the pneumonic form is more contagious than any other?—I have.

19,202. Could you give us any facts, for instance, instances where there has been several pneumonic

cases in the same house or in the same room?—In the same room, yes; but I have not got the facts.

19,203. You have seen that?—I have seen two cases in the same room develop one after the other, first the son and then the mother was attacked.

19,204. That is one case; can you give us the others?—I cannot isolate them now.

19,205. Have you seen pneumonic or bubonic cases occurring in the same house?—Yes.

19,206. Could you give us examples of that?—In the same room in the same house? I could give you numerous examples. Had I known before, I could have given you the numbers of the houses in Mandvi that have developed and redeveloped plague in each epidemic.

19,207. That is not what I mean. Have you ever seen it in one family; at one and the same time, one person with plague pneumonia, and one or two or more persons with glandular disease? Can you show, for instance, that a person got bubonic plague from a pneumonic patient?—I could not trace the connection.

19,208. Can you trace the connection where a person got pneumonic plague from a bubonic patient?—I cannot.

19,209. Have you any example showing that a bubonic patient got bubonic plague from a bubonic patient?—Yes.

19,210. Can you give us some examples of that?—There is a case down at Abd-ul-Rahman Street. A boy, aged 14, was attacked with bubonic plague. He had four buboes, two on the right side and two on the left, in the groin, out of which the one on the left groin suppurated, and the entire gland was discharged. On account of the acuteness of the inflammation it was thrown out. Subsequent to that the mother got attacked, and then the aunt got attacked.

19,211. How long after the first attack?—I should say about a fortnight or three weeks.

19,212. She might have infected herself from someone else?—She was in attendance on the boy while he was ill. I do not believe she left the house; and in that very house there were eight or ten cases.

19,213. Did the boy die?—No; he is alive now. He had a temperature of 108½°.

19,214. Might she not have infected herself from the eight or ten cases?—They were subsequent to the mother.

19,215. In that house did they all have the bubonic form?—All bubonic; not a single pneumonic case.

19,216. Did any of the cases die?—Yes, several of them.

19,217. (*The President.*) Did this house consist of one or several chambers?—Several chambers.

19,218. (*Dr. Ruffer.*) What do you consider is the most infective form of plague?—The pneumonic.

19,219. Why do you think the pneumonic form is the most contagious?—For the simple reason that I have seen this one particular case occur, and I believe that it was owing to infection hanging over the patient. Then there is the case of Dr. Manser; he got the disease from a pneumonic case.

19,220. In all these cases the pneumonic form always produced another pneumonia case?—Yes, as far as I know.

19,221. The bubonic form must be infectious too?—Yes, but not to the same degree as the pneumonic form.

19,222. Why do you think the bubonic is not infectious? You have only given us two examples?—There is the case of the English nurse in Poona, who was attending a case.

19,223. We have had that case. Have you any others under your own observation?—No, I cannot mention any.

19,224. Do you think rats have much to do with the spread of plague?—I do.

19,225. Could you give us any facts bearing on that?—Rats, being dirty feeders on organic débris of every description, especially in damp localities with organic pollution, where conditions are favourable for the development of the lower grades of organic life, are from their peculiar proclivities prone to bacterial and other infection. In the recognition of a rat mortality as a criterion of an infected or plague area, the fact of their tenacity for life and well-being must not be overlooked, and a sudden exodus to other localities has followed.

19,226. How do you know that?—I have seen them.

19,227. Have you seen an army of rats walking about from place to place?—People have told me. At one period of the plague there were no rats at Kolaba.

19,228. Who told you?—Mrs. Guidera, who got the plague.

19,229. Did you observe these facts yourself or is it only hearsay evidence?—Some I have observed, and others I have been told by my patients.

19,230. How do you know there were no rats in the place?—She said there were no rats, and she was surprised to see rats in the garden. Subsequently to that dead rats were found in her house and she got plague.

19,231. That does not show that there was an exodus. It simply shows that dead rats were found in the garden. You say they migrate from place to place. What facts have you got to show that?—People themselves have told me. They say the moment plague occurs in a house every rat leaves the place; they were found on the opposite side of the street where they were not before.

19,232. How do you know they are the same rats?—It would be very difficult to say.

19,233. How do you know, for instance, that the rats who lived on the left side of the street went over to the right side of the street?—Generally the sides that are infected are deserted by rats.

19,234. That simply shows that the rats have died?—Not necessarily.

19,235. That is one explanation?—No; they carried the infection from here to Bandra.

19,236. You say that you know rats migrate from the side of the street because people told you on the other side of the street that the rats were there, and none were seen on this side of the street. That does not show that the rats on this side of the street went to the other—at least it does not in my opinion?—It is very difficult to observe each individual rat.

19,237. I quite agree with you. I want to know how you can say there was an exodus of rats, and that the rats migrate from place to place?—Because I have seen them travelling over the causeways. I heard that they were migrating and I went to verify the fact.

19,238. Have you seen them travelling along the streets?—Yes, I have seen them running along the causeways.

19,239. Have not you seen them before the plague occurred?—I cannot say I ever went to see them then.

19,240. Your attention was drawn to it, and then you observed them going across the street. Have you any evidence that more rats travelled across the street during the epidemic than before?—It is impossible for me to know that.

19,241. What other evidence have you got to show that rats migrate from place to place?—They were found in localities in which they did not exist previously.

19,242. How do you know that?—Because people have told me.

19,243. But is it not the fact that when rats are dying of plague they, in a dazed condition, leave their holes, so that more would be seen than in ordinary times?—I was the first that pointed that out. They were in a state of plague intoxication and they did not care where they went; and they did not seem to notice human beings if they came near them or even kicked them.

19,244. Would not that account for the fact of their being seen in places where they never have been seen before? Take Watson's Annexe, for instance? I have never seen a rat there; but supposing rats got plague in Watson's Annexe, I should probably see them, because they would come out of their holes. You, yourself, have observed it. Would not that account for the fact?—They seem to die so rapidly that a few hours finishes them off.

19,245. Does not this simply show that the disease is very contagious? Does the fact of a rat being seen where it never was seen before show that it comes from another place?—Not necessarily,

19,246. Have you any other facts to show that rats migrate from place to place?—I have noticed them, as

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I said, travelling across the causeways during the still hours of the night, when traffic was suspended. They have, in some instances of the occurrence of plague on the lower floors, escaped to the higher ones, and even across the streets to other, and, to them safer, localities.

19,247. I will put it in another way to you. Do rats get on board ship?—They do in the usual course, but since 1896 they have been practically exterminated in the locality of the docks.

19,248. Are not you losing a good many rats now in Mandvi?—Occasionally in the town, but not about the granaries, as formerly.

19,249. There are no rats there?—Very few.

19,250. If rats carried the disease, how do you account for people on board ships not getting the disease? Are not rats continually going from ships ashore, and from the shore on to the ships?—The docks are kept particularly clean, no organic débris lying about, and no filth, of any description.

19,251. What was the causeway where you saw the rats?—The Mahim Causeway and the Sion Causeway.

19,252. Is that near the harbour?—No; on the other side of the city altogether.

19,253. Have you any other facts to show that rats spread the disease?—The disease has invariably occurred where rats have been found dead—within, at least, 60 to 80 per cent. of cases.

19,254. Anything else?—Under favourable conditions I have noticed that the bacillus has steadily increased; and if you keep the rat at a warm temperature, you find that the bacillus increases for the first 24 hours in the body of the rat: afterwards there is no increase.

19,255. It multiplies?—Yes.

19,256. Anything else?—No.

19,257. Can you give us any facts showing that plague is carried by air?—People from different parts of the city have always worked at Mandvi during the greater part of the day, it being an important business centre, and used to have their meals at their homes. They used to be at Mandvi during certain hours of the day, and only those hours. After that they used to go home, and it was not possible that otherwise than through the air they imbibed the disease; because many of them had no wounds, and to all intents and purposes were in perfect health, and perhaps within two or three hours after their return home they developed plague.

19,258. Have you any other facts besides that?—There was the case of the very first European that got plague, Mr. Orford Sherman. He had been photographing the cases at Mandvi, and subsequent to that he developed plague, and died of it.

19,259. How does that show that he got it through air?—He was otherwise perfectly healthy; a man at his business.

19,260. You have no other facts?—No.

19,261. I believe you have done a good deal of inoculation work?—Rather more than 2,000 cases.

19,262. Did you take the temperature of these cases?—Previous to inoculation, yes.

19,263. After inoculation?—I had no opportunity.

19,264. Did you ever see any evil effects from inoculation?—I cannot say that I have seen any evil effects.

19,265. Have you ever seen any abscesses?—Not as a result of my inoculation, but from neglect of due asepsis.

19,266. Have you ever seen very high temperature after inoculation?—Yes; 105°.

19,267. You said just now that you took no temperatures after inoculation?—The patients told me.

19,268. Did they take their temperatures themselves? Yes.

19,269. Have you any other evidence as to the rise of temperature?—The majority of them came round, and told me it was about 102, 103, and so on.

19,270. How did they know?—The Parsees are an educated class.

19,271. You refer specially to the Parsees?—Yes.

19,272. How did you standardise the dose you used?—I judged the dose according to the individual I had to deal with, rejecting those in whom I found an increased temperature.

19,273. You said you took no temperatures?—Previously to inoculation I did.

19,274. Previous to inoculation; but we have been told that one way of standardising the prophylactic fluid is to take the temperature of a certain number of people after the fluid has been injected. I understand you did not do that?—It was not possible.

19,275. How did you judge the dose you were to use? For instance, supposing the Plague Commission came to you to be inoculated, how would you judge what dose you were to give each Commissioner?—It would be the case of the eminent French physician, who judged according to the exigencies of the hour, and the individual susceptibilities of his patient.

19,276. But you could not possibly know the individual susceptibility of the 2,000 persons who came to be inoculated?—It is impossible to judge idiosyncracies.

19,277. Supposing 100 people presented themselves, how would you judge the dose you inoculated into each one of them: did you inject all with the same dose?—No.

12,278. What guided you in the choice of the injection?—First, their individual physical condition.

19,279. What points did you attend to in their physical condition?—Whether they were well nourished, whether they were frail, whether they had recently recovered from illness, whether they were by nature weakly, whether there was any physical tendency; and generally, whether there were any other militating conditions, such as the existence of old disease, scrofula, or anything else.

19,280. Then you reduced or increased the dose according to these conditions?—According to the individual condition of each man.

19,281. You must have started with a standard dose; how did you ascertain this standard dose? Did you simply take what was on the bottle?—I did not. The doses written on the bottles vary, and at that time I took it tentatively. I graduated my doses. I started with the dose marked down as the standard, which was 5 c.c., in all these cases. I did not believe in initial and only inoculation: I believe in a succession of doses rapidly increased from the minimum dose.

19,282. Did you inoculate these people several times?—Yes, twice and three times.

19,283. With gradually increasing doses?—Yes.

19,284. What guided you in the number of times you inoculated the people, why did you inoculate one person once, and another person three times?—I inoculated as many as I could three times.

19,285. At what interval?—At intervals of a week or ten days.

19,286. What dose did you inoculate each time?—According to the individual I had to deal with I would give him two c.c., then three, and afterwards, five.

19,287. Why did you adopt this method in preference to M. Haffkine's method?—I adopted this method thinking of obtaining a gradual, but permanent, immunization; that was my idea.

19,288. But have you any facts to show that you can produce a gradual and permanent immunisation against plague by repeated doses?—I should have said, as perfect an immunization as possible.

19,289. What facts have you got to show that you do get a permanent immunization by this method?—Because "a perfect immunization" is a scientific fraud. In some cases—I will not say all—I found the initial dose produced a re-active fever, in the second inoculation the reaction was less.

19,290. What height of fever?—102 perhaps.

19,291. Is not that the very thing that you want to get?—Yes, for the development of an anti-toxine in the blood.

19,292. We have no evidence as to the development of the anti-toxine. Professor Haffkine told us he wanted a temperature of 102. You got that temperature, and then you inoculated people again. What facts have you got to show that three inoculations are better than one?—Because, as far as I am able to judge from the experience of others, those that were inoculated three times seemed to have a greater resistent power against plague than those who were inoculated only once.

19,293. But who else has inoculated people three times? We have not had it in evidence yet, and I should like to know who the people are who inoculated three times. It is an interesting point. Can you tell me anyone else in Bombay who has inoculated three times?—I believe Professor Haffkine has inoculated twice.

19,294. Yes, but you said three times?—There were so many inoculating in Bombay at the time, that it is difficult for me to get facts.

19,295. How can you know that three inoculations are better than one if you have no facts?—People used to come and get inoculated, and we lost sight of them after that, and there was no time to get complete results.

19,296. Why do you think it is better?—Because instead of one, you get three re-actions.

19,297. What evidence have you got to show that three reactions are better than one?—Out of all these people who were infected, I believe a small percentage of those were inoculated three times were infected within a period of two or three months.

19,298. Can you give us any statistics as to that?—It is impossible for me to give you statistics.

19,299. How many of the 2,000 people did you see again after inoculation?—With regard to the three inoculations, my object in these cases originally was to get, if possible, a view of these people again after they had been inoculated, to bring them back and get the results from them.

19,300. How many of the 2,000 people have you seen since, as a matter of fact?—I have seen numbers; I could not approximately say.

19,301. Do you know how many of them died of plague?—I could not say, off-hand, not knowing the localities where they lived in, but very few of them died as far as I could ascertain.

19,302. How many of them died of other causes besides plague?—I could not say. You could not control the population as they came and went.

19,303. I take it that you saw no evil effects from Haffkine's inoculations, and that you do not know what became of the majority of the 2,000 people you inoculated. Have you observed any after-effects from the inoculation?—Among the sequelæ of its reactive developments, idiosyncratic and other, may be mentioned neurasthenia, dizziness, and, in females, hysteria, and an asthmatic condition, varying in degree.

19,304. How long after inoculation?—A month afterwards, one particular woman tells me.

19,305. Only one woman?—No, two women. One told me she had asthma occasionally up to the age of five. After that she had never had any asthma till the age of 40. She is 40 now, and she was inoculated and got asthma and she gets asthma now. The other was a young woman of 24 years of age. There was a history of asthma in that case up to about eight years ago. She has been free from asthma for eight years, but subsequent to inoculation she developed this asthma, which continued for about four months. She was treated by several physicians, not by me alone, but by other people, and the asthma has practically passed away, but she suffers from occasional attacks of mastitis, swelling in the breasts. I cannot understand the thing myself, but there it stands. I do not put it down to that at all, I cannot trace and will not trace the connection.

19,306. Have you noticed anything else?—Aphonia, with occasional exaggeration of hysterical conditions, recurrent attacks of mastitis, articular pains, and the development of rashes, erythematous, urticarial, and one case of petechial rash.

19,307. How long after the inoculation?—Varying times; the erythematous and the urticarial within a few days of inoculation.

19,308. Were they painful?—Yes, but they passed off; it was only a matter of 24 or 48 hours.

19,309. Anything else?—And a lichenoid rash which came later on, about a week after.

19,310. Did these rashes spread from the point of inoculation?—No.

19,311. They appeared anywhere?—Anywhere.

19,312. Have you any evidence to show that these people did not suffer from these rashes before? You know well enough that when children are vaccinated every rash the child gets afterwards is put down to vaccination. Do not you think this might be a similar case? Have you any evidence to show that these rashes were due to inoculation?—They were attributed to the inoculation.

19,313. Are you satisfied yourself that they were due to inoculation?—I have seen erythema and an erysipaloid condition after inoculation, but that I put down to want of careful asepsis.

19,314. Have you seen anything else?—Isolated cases of leucoderma and leprosy have been reported I have two cases, but they are not my cases.

19,315. Then you had better not mention them. You have not observed them yourself?—No.

19,316. Anything else?—I cannot say anything else. Practically I do not believe that these results are consequent upon inoculation, or in any way influenced by inoculation.

19,317. (*The President.*) All of them; the skin eruptions and so on?—The skin eruptions may be determined.

19,318. Were the last two—leprosy and leucoderma?—No, I could not say they were.

19,319. (*Dr. Ruffer.*) You say in your précis that Haffkine's fluid is prone to putrefication: have you any facts fearing on that point?—If it stands in a bottle, it is liable to putrefaction.

19,320. It contains half per cent. of carbolic acid?—I cannot say; I am speaking of some months ago.

19,321. It putrified easily?—Yes it putrified easily. There is another curious thing in connection with this Haffkine's prophylactic fluid. We all know that perchloride of mercury is a powerful germicide, but it has no action on the toxine itself. I had used a serum bottle, and half was left. I took a strong solution of perchloride of mercury and poured it into this bottle and put this bottle inside a cage with rats, securing the bottle so that they could not by any chance drink the contents of the bottle, and over that I put a deal-wood box, and some of the rats were dead the next morning, but not with plague.

19,322. Did they drink it?—They could not drink it.

19,323. How could they get plague from the prophylactic fluid?—I did not say it was plague; it might have been toxinal poisoning.

19,324. How could they—are there any volatile substances—have you ever smelt one of Haffkine's bottles? I have frequently, but never found any smell?—I have frequently smelt them myself.

19,325. (*The President.*) What do you suggest,—carbolic acid poisoning?—No, I do not think so. Volatile toxine, I think.

19,326. (*Mr. Hewett.*) You say you were the first person to discover the mortality among rats in September 1896?—I drew the attention of the authorities to it.

19,327. Whose attention?—I mentioned the matter to Dr. Weir. He may have seen the rats himself, as he was continually down there.

19,328. Did you report it in writing to Dr. Weir?—No, I saw him personally.

19,329. (*Mr. Cumine.*) Perhaps the Commission did not understand the full significance of what you said about rats crossing the causeway. When you said that they crossed the causeway, you did not mean, I fancy, that they went from one side of the roadway to the other side, but that they ran along from one end of the causeway to the other end. Is that so?—Yes.

19,330. Are those causeways the causeways that cross the creek north of Bombay Island?—Yes.

19,331. What would be the length of the Bandra causeway?—I could not say,—some hundreds of yards.

19,332. You say you went there at night, and stopped to see if rats were passing?—I casually passed by.

19,333. You did not go specially?—I was told about it, and I casually went there.

19,334. What did you see?—I saw some rats running along.

19,335. Which way?—Towards the Bandra side.

19,336. Which way were you going?—I stood towards the Mahim side.

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19,337. Where did you stand in the roadway?—On the side of the roadway. I was in a carriage on the side of the road, not in the centre of the road.

19,338. How long did you remain there?—About three quarters of an hour.

19,339. How many rats passed?—I did not count them; several passed.

19,340. Did you follow them up to see if they went over to Bandra or were they only playing about on the causeway and did they return again to the Mahim side?—I did not follow them up, but I was told that, subsequently to this, other people had noticed the rat migration.

19,341. I want your rats please. When you had observed them for three quarters of an hour, did you go on to Bandra, or come back again?—I came back again.

19,342. You do not know whether they went right across the causeway?—I could not say.

19,343. Was there plague in Bandra at that time?—Yes, there was.

19,344. You cannot say whether these rats took plague across, or not?—I was told that they did. There was no plague up to the time of the rats going across.

19,345. Your rats?—Not my rats, but rats noticed by others.

19,346. I want to know about your rats?—Plague had been already over there.

19,347. Do you know whether rats go across every year, when there is plague and when there is no plague?—I could not tell you that. My attention was drawn to it, and I simply went there out of curiosity.

19,348. (*Dr. Ruffer.*) Did you ever see plague twice in the same person?—Yes.

19,349. Can you give us the details of the case or cases?—Yes. There is one case of a man who died in the Modikhana Hospital after three attacks.

19,350. Can you give us evidence of that case?—He was the grandson of Dr. Byramjee. He got over two attacks and succumbed to the third.

19,351. Did you see him after the two attacks?—No, I was told about it.

19,352. Who told you?—I asked Dr. Byramjee himself.

19,353. You yourself did not observe the case?—No.

19,354. Dr. Byramjee would be able to give us the details?—Yes.

19,355. Have you any other cases?—There was another case of a man by the name of Ghella, a Hindu. First of all he had axillary plague, and then four or five months afterwards he got parotid plague.

19,356. How do you know it was parotid plague?—By the symptoms, and he had high fever.

19,357. How do you know it was not mumps?—It was not mumps.

19,358. Did the man die?—No, he is alive now.

19,359. Can you find him and bring him here?—Yes, I will try and bring him here, unless he has run away to his country.

19,360. Have you any other cases?—I could not think of any just now. Some people have run away from Bombay, and the average native is very loth to come and appear before any Commission. They will tell you a good many things, but if you ask them to come and give evidence, they will not.

19,361. In the case in which you inoculated a person three times, did you find that the second inoculation produced more or less reaction?—It varied; there is no hard and fast rule.

19,362. Sometimes more and sometimes less?—Yes.

19,363. And the third inoculation?—The result was the same there, sometimes more and sometimes less.

19,364. You never inoculated a person more than three times?—Not more than three times, although some came and asked for a fourth inoculation, but I refused.

19,365. (*The President.*) I did not understand why you inoculated three times. I have no doubt you had some satisfactory reason. What was your object?—The object was this, that many of the people I inoculated complained that they were not able to rest in their houses during the period of this reaction, and they wanted a smaller dose, so that the reaction would ensue, but not so very pronounced a reaction.

19,366. Your object was to introduce by several small doses, each insufficient to produce inconvenience, a total quantity sufficient to cause immunity?—Yes, that is so.

19,367. In regard to the cases which you thought were plague cases, and where there was no glandular enlargement, had you any evidence of infection from one case of that kind to another?—No, not at that time.

19,368. Have you had any instance of infection from one person to another in the cases of glandular enlargement that you refer to?—From one to another I have.

19,369. Have you had cases where you had evidence that a patient suffering from plague with glandular enlargements conveyed that form to another patient?—I could not say definitely that it was directly from that man to another man, because people go in and out. The native customs are very peculiar. About 10 or 12 come and huddle up together in a room to see one man, and remain some hours, and then go away again to their houses.

19,370. So far, you have only had evidence that a man who has been infected in that way has had an opportunity of contracting the disease from another person similarly infected?—Yes, I have seen some cases like that in the latter years of the plague, but not in the initial cases.

19,371. In the early years that you refer to what prevalent diseases were there; were there any parexic diseases prevalent? Was tonsilitis common?—Yes, tonsilitis was present, but not to any great extent.

19,372. Did you satisfy yourself that these were not cases of acute tonsilitis? What I heard you say does not make it clear to me that they might not have been cases of acute tonsilitis?—The symptoms were so exaggerated. You do not get such exaggerated symptoms with tonsilitis.

19,373. You get a very high temperature, and prostration?—Intense prostration follows in those cases.

19,374. I do not see anything in what you have mentioned which would not be consistent with tonsilitis?—There was the general intumescence of the tissues extending right down to the clavicles.

19,375. That is the point, is it?—Yes.

19,376. You gave evidence about telluric conditions and you thought moisture of the soil might be an inducing, or an adding cause, at any rate?—A determining cause.

19,377. Is it within your knowledge that the plague bacillus thrives better in a moist surrounding than in a dry one? That is what you probably mean to say?—That is so.

19,378. What do you think is the manner in which rats infect human beings?—Rats die in a house, and perhaps, it is the exhalations from their bodies, like the exhalations from any corpse which has died of zymotic disease, and which is always prone to carry disease to others—small-pox, for instance.

19,379. By exhalation do you mean the volatile material that proceeds from a corpse?—Yes.

19,380. What volatile material proceeding from a corpse, whether that of a human being or an animal, would in itself produce plague?—If it is in a close atmosphere the possibilities are that there are exhalations from the body.

19,381. What volatile material do you refer to? Is it something different from the bacillus?—It is.

19,382. Is it possible that any other substance, excepting the plague bacillus, would cause plague?—No, it is not possible.

19,383. Then what is it you mean?—You might get intoxication.

19,384. You do not mean to say that anything but the plague bacillus, which is not volatile, would cause plague?—I do not.

19,385. With regard to Haffkine's prophylactic fluid, do you think that it is the want of "scientific potential value" which is the objection?—No, it is not an objection. It has not been determined, and I do not

advance that as an objection against the prophylactic at all.

19,386. It is a question to be investigated?—Yes.

19,387. Could you determine the potential value of an unit of this fluid?—Yes, by injecting, according to body-weight, a certain amount of the toxine or anti-toxine.

19,388. What would you do in this case? What would you test each against, and how?—By injecting rabbits or guinea pigs, and seeing the amount of resistance they may aquire, according to body-weight, against the virulent bacillus itself.

19,389. (*Dr. Ruffer.*) What do you mean by the potential value?—The resisting value in units to body-weight.

19,390. In units of what?—According to body-weight.

19,391. Body-weight of what?—For instance, you take a guinea pig and you give it a certain dose of anti-toxine. *Dr. C. H. Freeman Underwood.* 18 Feb. 1899.

19,392. Is there any anti-toxine to Haffkine's fluid?—No. I am not talking about that; I am talking about anti-toxine in general.

19,393. You say anti-toxine. I want to know whether there is an anti-toxine to Haffkine's fluid. I have not heard of it?—I have not heard of it either. The amount of resistance or immunity conferred was not determined.

19,394. On animals?—Yes; it has not been determined. That is what I meant to infer. The other matter associating anti-toxines with Haffkine's fluid was far away from my ideas altogether, knowing, as I do, that it is a solution of plague toxine in neutral mutton broth.

(Witness withdrew.)

Mr. N. Futehally called and examined. *Mr. N. Futehally.*

19,395. (*The President.*) You reside in Bandra?—Yes.

19,396. Are you a medical man?—No.

19,397. What occupation do you follow?—A merchant.

19,398. I believe you are Vice-President of the Bandra Municipality?—Yes, I was so until March last.

19,399. (*Mr. Hewett.*) Were you in charge of plague operations at Bandra at any time?—Yes.

19,400. From what date to what date were you in charge?—From October 1897 to April 1898.

19,401. During that period was plague continuously in Bandra?—There was plague in certain districts only—in two districts.

19,402. You mean to say in the villages round Bandra?—Yes, the villages included within the Municipal limits of Bandra.

19,403. When did it exist in those villages?—I first heard of a case in December.

19,404. What village was that?—In Khari village.

19,405. Did you have an outbreak in Khari then?—Yes.

19,406. How long did it last?—It lasted for 2½ months, I believe.

19,407. Did you evacuate the village?—Yes.

19,408. Did the plague spread from Khari to Bandra itself?—No, it did not.

19,409. What other village caught infection?—A village called Naupada.

19,410. How long did plague exist there?—About two months.

19,411. Did you evacuate that village?—No.

19,412. Did plague spread from Naupada to Bandra?—No.

19,413. When did you get your first case of plague in Bandra during the time you were in charge?—We had no case in Bandra then.

19,414. When was the first case in Bandra?—During the time I was in charge, there was no case in any other place except Khari and Naupada.

19,415. When did you actually give over charge?—On the 31st of March. I believe there were one or two cases in the middle of March.

19,416. Were there no cases in February?—No, not in any other part of Bandra, except the two villages of Khari and Naupada.

19,417. When did the first sporadic case take place, in March?—The first case in Bandra was in about the third week in March. There were three cases.

19,418. Do you know anything of the measures then taken in Bandra?—Yes, the measures were all taken by myself.

19,419. What measures were taken?—There were two sorts of measures, One sort had reference to the whole of Bandra generally, and the other had reference only to plague-stricken districts. With regard to Bandra town, including the plague districts, we did take some measures; that is to say, we had house-to-house visitation, examination at the station, and we issued notices that all new arrivals should report their arrival to the Municipal Officers, and we called upon the house-owners to give intimation to the Municipality of all new arrivals in their houses. Those were the general measures in all Bandra.

19,420. Whether as a result of those measures or not, plague did not touch Bandra while you were in charge?—That is so.

19,421. Since then it has broken out there?—Yes.

19,422. And has existed continuously?—Yes, it has existed continuously. Sometimes there have been great numbers, and sometimes there have only been a few attacks.

19,423. It is still there now?—Yes; it is rather bad.

19,424. Is Bandra subject to constant infection from Bombay?—Yes, it is subject to that.

19,425. When plague is bad in Bombay, are people in the habit of going to Bandra?—Yes, many people come to reside there and go daily to Bombay for business, and so are the Bandra people in the habit of going to Bombay for business.

19,426. Then the Bandra people are constantly liable to infection while doing their work in Bombay?—Yes.

19,427. Do you think that Bandra got infected by the residents of Bandra coming to Bombay and catching plague in Bombay?—The first case that occurred in Bandra was that of a woman who was resident in Bombay. She came to Bandra and brought plague with her. That was at the very first outbreak, not in my time.

19,428. I am now asking you what your opinion is as to how the place got infected after you gave over charge of operations?—I think the examination was not sufficiently strict.

19,429. The examination of what?—The examination of the people. We disbanded several of our Municipal Inspectors. The examination at the station was also discontinued, and so was the house-to-house visitation. As I have said in my précis, I believe it was through slackness of measures.

19,430. During your time were the people who went from Bandra to do their business in Bombay inspected?—Yes.

19,431. Was every individual regularly inspected?—Yes. We used to issue passes to the permanent inhabitants of Bandra who had business in Bombay. Every man had a pass, and before the pass was given to him he was examined. If he was found to be all right a pass was given to him. The pass would enable him to return to Bandra and it would say that he was a permament resident. Passes were also given to distinguished people who were not permanent residents at Bandra.

19,432. You contend that it was the abandonment of this system which led to plague getting into Bandra?—Yes.

19,433. Is that all you wish to say?—Yes.

(Witness withdrew.)

Prof. O. V. Muller.

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Prof. O. V. Muller called and examined.

19,434. (*The President.*) Please to inform us what you are a Professor of?—History and Political Economy at the Elphinstone College, Bombay, the Government College.

19,435. (*Mr. Cumine.*) You are a volunteer witness, are you not?—Yes, I have been a volunteer all through.

19,436. When did your experience of plague begin?—I was present as a member of the Standing Committee the day Dr. A. Yiegas announced to us that there was plague in Mandvi. As soon as the meeting was over I got on my horse and rode down to Mandvi. I have been looking on and taking part in almost every sort of operation ever since,—taking notes and photographs from the very beginning. Having a historical knowledge of plague, I tried hard to discover how the plague spread, and how it could be stopped. I studied every possible detail.

19,437. How do you think it did spread from house to house in Bombay?—When I came down to Mandvi I found the plague was existing in some six or seven houses. I noticed that the houses where the plague was were very good houses, and very often in breezy situations, and facing the docks. Many of the rooms were very excellent. I should have had no objection to living in some of the rooms myself, especially the one where the plague was so bad in the beginning—No. 45, Argyle Road. Inspector Hallams and myself disinfected that house over and over again according to the methods then in vogue, with a fire engine and phenyle and water. We cleaned it from top to bottom and whitewashed it twice. As long as there were any people left in it the plague continued to attack them. Various theories were started. The natives said it was due to the European drains that had been put down in the town. I inquired into this matter, and found that although the drains were being cleaned in Mandvi in the infected district, and the silt taken out, not a single coolie suffered from any disease whatever, although they were handing the filth supposed to contain the plague. One person started the idea that plague was due to the grain stored in godowns under each of the houses where plague occurred. I volunteered to clear out one godown at the bottom of Argyle Road, No. 45; and I was given thirty coolies to do it. I saw these coolies for many days afterwards and not one was taken ill nor was I, although at the time I was looked upon as very foolhardy. I was one of the people who showed the public that a healthy European who took reasonable care was immune. My friends in Bombay treated me as if I were a leper, and society practically cut me as it was known I was on plague duty. I noticed immediately I came down that the plague was confined to a very small district on the east of the railway, and bounded at the two ends by two railway bridges. For some days it remained there, and then it gradually spread to the other side of the railway. In every godown we opened in the morning we would find twenty or thirty dead rats. To give you an example of the abundance of dead rats, when Mr. Hankin arrived he drove down to Mandvi, and asked me for some dead rats. In five minutes I got him twenty in and around the houses in Dongri Street. He said he wanted some freshly-killed rats. I went into a house and turned over some bales. Rats ran out in all directions, and I pursued and killed some with my riding whip. They were all sick. I noticed rats on the Masjid Bunder Bridge running about as if they were drunk. You could see them lying about the streets in quantities. I have never in any other part of the town seen sick, dead, and dying rats in such quantities. Mandvi being the centre of the grain godowns, there are, perhaps, more rats than anywhere else. On hearing that this district (which contained good houses, and streets laid out at right angles) was attacked, and that rats were sick with plague, I went down to inquire from P. and O. Officers whether there was any possibility of rats coming from Hong Kong, and I was told that at Hong Kong the ships go up against the quay, so rats can both leave and go on to ships. The same applied to Bombay, but to no intervening port. From this I came to the conclusion (which I have always held, and always pointed out to foreign scientists) that plague was brought here by sea by sick rats from Hong Kong. Plague spread a week or two afterwards to Karachi—before it did to Thana or Poona. I thought my theory was confirmed by the fact, that it went by sea to Karachi much quicker than it moved by land to even neighbouring towns. Mr. Vincent, the Commissioner, has told me that he held the theory that the Sadhus from the Himalayas brought it, but personally I have always been convinced of the opinion that it came from Hong Kong in ships by sick rats. Every sailor will tell you that at every port where ships go up against the quay, rats will go on board, and also leave the ships. I should like to mention a fact which I think helped to spread plague in the beginning. Plague practically began in two houses, 45, Argyle Road, and 22, Clive Road, containing respectively 400 and 600 people; and the people fled all over the town. At first we could not get them to move. They did not care; they looked out of the windows when disinfection was going on, but afterwards they fled, and those two houses became quite empty. Then I noticed another thing. Down in the Masjid Bunder Siding Road several rich men subscribed three or four thousand rupees, and gave a huge caste dinner four days after plague broke out on the 28th of September. People were invited from all parts of the town, and were feasted in an infected house to propitiate the Deity. I urged the men rather to spend the money in white-washing these houses. The people however went and feasted there, and kept it up nearly the whole night, and then went back. Whether these people carried infection or not I was unable to ascertain.

19,438. Did you try any experiments to see whether the theory that grain spreads the plague was correct or not?—When Dr. Waters pointed out that certain grain godowns were "obviously (?)" causing plague, I got sterilised bottles; I used soda-water bottles; I went to each godown he had specially pointed out; I opened the bottles, poured the soda-water out, and took grain from where it would be most infected; I sealed the bottles, and disinfected my hands between each operation. I took the bottles to Mr. Hankin; he inoculated rats with the contents, but he failed to discover any trace of plague whatever.

19,439. Did any bad results occur to the coolies you got to assist in removing grain from the godowns?—No. I undertook to empty the godown he pointed out as particularly bad. I had 30 coolies given to me for the work; I took a careful note of them, and none of them got ill at all.

19,440. (*The President.*) How were these coolies dressed?—They were almost naked.

19,441. Were their feet covered?—No. It was very hot work. They took off what little clothes they had, all except a band.

19,442. (*Mr. Cumine.*) Will you kindly tell us whether there is any point which came to your notice, and which, so far as you know, has not been mentioned by any other witness?—I did notice that several people whose names I took down who fled from Mandvi afterwards caught the plague in other parts of the town. I know of seven people whose names I took down who left infected houses, and who afterwards died of or developed plague. With regard to rats dying before cases of plague occurred in a house I may mention I noticed that whenever dead rats were found in servants' quarters, and ladies told me of this at the Yacht Club, 10 days or a fortnight afterwards (almost with the regularity of clock work) their servants would arrive at my Servants' Plague Hospital. I have noticed too that cases have been very frequent in low-lying huts and houses, especially in houses with earthen floors; while houses with pukka floors, *i.e.*, tiles, cement, or the like remain immune. I have known fairly good houses with earthen floors which were not as immune as those with cement or stone-floors. I have also noticed that when plague comes into the neighbourhood of a village, if the whole village leave their houses and remain out until the rains there were no cases among them.

19,443. (*The President.*) It stopped plague?—Yes.

19,444. Can you mention any villages?—There is the village of Ghowari.

19,445. What is the population, large or small?—Not very large, some thousands.

19,446. Any other village?—I cannot speak with exactness except with regard to Ghowari. That I am quite certain of, because I was in charge of it. Then there is another point. I noticed that the Christian section of the village of Vadala did not suffer as much as the Hindus. They said, "We do not get plague

because we sleep on beds." I took a note of that, and I found that almost every Christian villager had a bed, whereas almost all the Hindus slept on the ground.

19,447. Why do you think that healthy Europeans are relatively immune?—I think because we wear boots and sleep on beds. Our houses are lighter, and more airy, and we keep our neighbourhoods clean.

19,448. You do not, I suppose, mean that the immunity is specially racial; if a Hindu lived under the same circumstances, he might also be relatively immune?—To a large extent. I believe it takes more plague virus to give a European plague than a native. Speaking personally, I have been scratched by plague patients and treated in various ways, I should think, that would have given a native plague.

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19,449. Were any of these rats that you have referred to bacteriologically examined?—Yes, I frequently took them to the laboratories. My friends took to sending them to me in biscuit tins to have them examined, a practice I soon stopped.

19,450. I am referring to the rats at the commencement of the outbreak: were they bacteriologcally examined?—Yes, by Mr. Hankin and others.

19,451. Those for example, which seemed to you to have been the immediate cause of the importation of plague, were they examined?—Yes, rats were examined, which came from Nenaz Kairaz's house in Dongri street in Mandvi, where there had been cases, and where I killed rats for Mr. Hankin. He showed me afterwards a section of the spleen of one of the rats with a colony of microbes in it.

(Witness withdrew.)

Mr. J. K. N. Kabraji, I. C. S., called and examined.

Mr. J. K. N. Kabraji, I.C.S.

19,452. (*The President.*) You are Acting Collector at Kaira?—Yes.

19,453. You have seen plague in a large number of villages, I think?—Yes.

19,454. With regard to the first village mentioned in your précis of evidence, Igatpuri, the importation, I understand, was from Bombay?—Yes; we had five cases in January 1897, five in February, one in March, and three in April, and then, after the middle of July suspicious cases began to occur in a line of houses on the Agra Road, and the presence of plague was recognised. The infection was believed to be introduced by a Muhammadan who came from Bombay about the middle of July and died soon after.

19,455. What were the measures taken when plague showed itself there?—In the first place the sick were taken to the hospital, but the other inmates of the infected houses could not be removed easily on account of the heavy rainfall. They were just taken out for a few days while their houses were disinfected, and then they were allowed to go in again.

19,456. None of the neighbours were removed, I suppose?—No.

19,457. What was done to the houses?—The houses were disinfected and lime-washed.

19,458. Therefore you had a very imperfect evacuation, but I suppose there was complete disinfection?—Yes, there was complete disinfection.

19,459. What is the result?—The result was that there was no improvement, that is, plague had its course for two or three months and then subsided.

19,460. The result was that the plague went on increasing?—Yes.

19,461. You thought you would anticipate plague by disinfecting the houses which had not been infected?—Exactly.

19,462. What is the result of that?—The result was that plague went there just the same.

19,463. You did not succeed in your object of preventing the extension of the plague?—No.

19,464. What was the disinfection you employed?—Perchloride of mercury only.

19,465. Was that done thoroughly?—I believe it was; there were men specially looking after it.

19,466. Was it under your observation?—Major Anderson was looking after the whole arrangements.

19,467. It was not under your observation?—No; there were special men told off.

19,468. What did you afterwards do there?—At Igatpuri nothing more was done except that we got a lot of waggons from the Railway Company. That was about the time when only railway employés were living in the place with their families, all the others having gone away. A number of waggons were obtained and the people were put in them, and then all the houses were vacated.

19,469. What was the further history of the plague?—After that the plague subsided about the end of October.

19,470. By that time, therefore, there had been a comple evacuation effected, following the incomplete one?—Yes.

19,471. And the result was that plague ceased?—Yes.

19,472. Have you had any experience in Bijapur?—Yes.

19,473. The measures you employed there were of a similar nature, were they not?—In the first place there was a small outbreak at Kaladgi in February 1898. There the measures were of a similar nature, but there was complete evacuation at the same time; that was in the dry season.

19,474. The great difference is that in this instance you were able to get complete evacuation carried out, as contrasted with the former village?—Yes.

19,475. How does the history of plague here contrast with the history of plague in the former village?—In Kaladgi, after evacuation, the plague disappeared in almost no time—in two weeks. We did not disinfect every house in this case, but only the infected houses and those in the neighbourhood. The other houses in the town were cleaned out and lime-washed.

19,476. Had you experience in any other villages?—After that the real epidemic began in August last year at Ilkal. There, again, it was in the rainy season, so in the first place the people were taken to the hospitals, but after a time, I think two or three weeks after it commenced, the whole town was evacuated, and people were made to go into their fields. There was not much rainfall, so we were able to do it.

19,477. What occurred with regard to the plague?—There, too, plague subsided.

19,478. And entirely stopped?—Yes, the last case being on 15th November, 1898.

19,479. Had you any satisfactory measures for discovering cases of plague?—I employed a lot of men in the surrounding villages, in fact, in the whole taluka. It was their business simply to go round and visit two or three villages a day, and inspect new arrivals, and every house where deaths had taken place. In that way we were able to detect a number of infected villages at the very beginning.

19,480. Do you think that very important?—I think so, because it happened in some of the cases that we detected the first or the second case of plague and the village was turned out, and after that the mortality was very little.

19,481. You have had other instances in which complete evacuation had been effected, I think?—Yes.

19,482. Was the result in them the same?—We have always noticed that plague went down after evacuation.

19,483. Where the evacuation was only partial it did not go down?—No.

19,484. What was the result when you adopted disinfection only?—I cannot say that disinfection did any harm, but I consider it was simply thrown away—all the money was thrown away on disinfection. We got just the same result without disinfection. I simply compare Igatpuri with Ilkal. At Igatpuri we disinfected every house, and in some cases we disinfected streets in advance of plague before they got infected. All the same, plague went there.

Mr. J. K. N. Kabraji, I.C.S.

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19,485. Into these very houses?—Yes. At Ilkal we did not do any such thing.

19,486. How soon after disinfection were the houses re-occupied at Igatpuri?—They were allowed to go in within three or four days. It sometimes took three or four days and sometimes a week. At Ilkal the people were turned out but only the infected houses were disinfected, and then the people were allowed to go in again, and the results have been the same with very little cost.

19,487. Can you tell me the result?—At Ilkal plague subsided two months after, practically, and it did so also at Igatpuri. In spite of disinfection it took two months before plague was got under there. At Igatpuri the people were allowed to go in after that time and also at Ilkal, and there has been nothing since at Ilkal.

19,488. In both cases it stopped?—Yes.

19,489. Can you contrast disinfection with complete evacuation?—There was no evacuation at Igatpuri for some time, but at Ilkal we started evacuation within a month of plague appearing there.

19,490. How soon after evacuation did plague disappear?—At Ilkal plague disappeared in three weeks after evacuation; at Igatpuri it went on for a long time in spite of disinfection.

19,491. Will you tell us what you mean by a "long time"; was it months or weeks only?—At Igatpuri we had the first recognised case on the 2nd August, and the last case on the 2nd November.

19,492. It went on for three months?—Yes.

19,493. Have you the details?—I have a statement for Ilkal also which shows that at Ilkal it subsided in somewhat less time. I put in those statements.

IGATPURI.

Date.	Attacks.	Deaths.	Date.	Attacks.	Deaths.
1897			1897		
Aug. 2	6	1	Sept. 21	11	6
„ 3	1	3	„ 22	8	7
„ 4	3	3	„ 23	6	4
„ 5	1	0	„ 24	7	6
„ 6	2	1	„ 25	9	4
„ 7	1	3	„ 26	2	5
„ 8	0	0	„ 27	6	3
„ 9	2	0	„ 28	3	9
„ 10	0	1	„ 29	1	3
„ 12	7	2	„ 30	6	7
„ 13	0	0	Oct. 1	0	0
„ 14	3	2	„ 2	8	7
„ 15	1	1	„ 3*	4	3
„ 16	3	1	„ 4	8	8
„ 17	2	2	„ 5	8	8
„ 18	4	4	„ 6	6	0
„ 19	6	1	„ 7	4	1
„ 20	3	5	„ 8	5	5
„ 21	1	2	„ 9	2	1
„ 22	3	1	„ 10	4	3
„ 23	1	2	„ 11	4	1
„ 24	9	2	„ 12	4	3
„ 25	10	3	„ 13	4	4
„ 26	8	10	„ 14	4	4
„ 27	6	5	„ 15	4	5
„ 28	6	3	„ 16	1	1
„ 29	8	7	„ 17	0	1
„ 30	14	13	„ 18	4	1
„ 31	15	9	„ 19	0	0
Sept. 1	16	18	„ 20	0	4
„ 2	13	10	„ 21	0	1
„ 3	21	19	„ 22	0	0
„ 4	10	11	„ 23	1	1
„ 5	20	16	„ 24	0	0
„ 6	14	12	„ 25	6	2
„ 7	8	12	„ 26	1	0
„ 8	20	11	„ 27	0	0
„ 9	2	4	„ 28	0	0
„ 10	6	8	„ 29	4	0
„ 11	12	5	„ 30	0	0
„ 12	12	6	„ 31	0	0
„ 13	18	12	Nov. 1	0	0
„ 14	9	6	„ 2	2	0
„ 15	6	12	„ 3	0	1
„ 16	14	10	„ 4	0	1
„ 17	13	8	„ 5	0	0
„ 18	11	5	„ 6	0	0
„ 19	8	5			
„ 20	6	9	Total	512	411

* Date waggons were used.

ILKAL.

Date.	Attacks.	Deaths.	Date.	Attacks.	Deaths.
1898			1891		
Sept. 17	5	2	Oct. 18	13	11
„ 18	1	0	„ 19	15	16
„ 19	0	2	„ 20	11	8
„ 20	4	0	„ 21	15	11
„ 21	3	0	„ 22	8	10
„ 22	1	2	„ 23	4	3
„ 23	4	4	„ 24	4	2
„ 24	4	3	„ 25	3	5
„ 25	3	3	„ 26	4	1
„ 26	1	3	„ 27	1	3
„ 27	0	0	„ 28	8	5
„ 28	1	0	„ 29	1	2
„ 29	0	1	„ 30	1	2
„ 30	4	1	„ 31	2	2
Oct. 1	2	1	Nov. 1	5	5
„ 2	1	0	„ 2	7	1
„ 3	5	4	„ 3	2	2
„ 4	5	4	„ 4	3	3
„ 5	10	4	„ 5	0	1
„ 6	2	4	„ 6	0	1
„ 7	6	8	„ 7	1	0
„ 8	4	3	„ 8	1	1
„ 9	11	6	„ 9	1	1
„ 10	11	10	„ 10	1	1
„ 11	9	7	„ 11	0	0
„ 12	12	10	„ 12	3	3
„ 13	16	9	„ 13	2	1
„ 14	7	15	„ 14	0	0
„ 15	13	14	„ 15	2	1
„ 16	5	7	„ 16	0	1
„ 17	16	8			
			Total	284	238

19,494. How soon after complete evacuation did it subside at Ilkal?—Evacuation at Ilkal commenced in the beginning of October and was completed about the 3rd week of October. By about the 20th of October the whole town was out, and completely evacuated. The last case occurred on the 15th November.

19,495. When did the majority of cases occur after evacuation?—The highest mortality was just about the middle of October.

19,496. How many days after evacuation is that?—I should think evacuation was just near finishing by that time.

19,497. How many cases were there after complete evacuation?—After that cases went down from 15 to eight, four, five, and so on, and on the 5th and 6th November there were no cases.

19,498. What do you think is the reason that complete evacuation is so efficacious?—I consider the chief reason is that people go into the fields, where they get plenty of light and air.

19,499. Why do you think so?—Because that is the only difference that I can see between huts in the field and the houses in the town.

19,500. Generally speaking, the houses with which you have been dealing were defective in those respects?—Native houses are always badly ventilated.

19,501. And always badly lit, do I understand?—Yes.

19,502. Were they overcrowded?—In many cases they were overcrowded.

19,503. Generally speaking, or not?—Yes, generally speaking, they were overcrowded for villages of that stamp, but not to the same extent as in Bombay.

19,504. Had you any experience of preventive inoculation?—I have not had much experience of preventive inoculation. At Ilkal we inoculated people after the town had been evacuated, and before they were allowed to go into their houses. A few families were allowed to stay in the town after being inoculated; they were not turned out.

19,505. Did any harm result?—No. There were a few cases. I think 12 deaths occurred among the inoculated people, but they occurred two or three days after inoculation, so that they were, perhaps, already infected.

19,506. These 12 cases might have carried on the infection in the village which they were allowe to remain in?—Yes, they might.

19,507. There is a certain amount of danger there?—Yes.

19,508. You have formed some opinion as to the time which elapses between the first importation of a plague case and the actual occurrence of an outbreak?—Yes.

19,509. Will you give us the facts?—The first time it attracted my notice was at Igatpuri, where I ascertained that some Muhammadans came from Bombay in the middle of July. The first death among them was on the 21st July. Then it was nearly 12 days before any further deaths took place in that locality.

19,510. You always received early information? Your machinery was adequate?—Yes; but we did not receive information about these arrivals from Bombay, because we had stopped medical inspection just about that time.

19,511. You found that out afterwards?—Yes.

19,512. Have you any other case?—I observed it more closely at Ilkal, where a man, who was suspected to have brought the infection, died on the 31st of July, and the first death which took place in that house afterwards was on the 20th August. On the 31st July a Marwari Banniah came from Hubli, and died at Ilkal. That death passed unnoticed. Then the man he had been living with died on the 20th August, and his wife on the next day. That started the plague at Ilkal.

19,513. That was the second case?—Yes. After that I have had some other instances. As one illustration I may say that in this way I detected what, I believe, was the first indigenous case of plague ot Bobleshwar, a large village in the Bijápur táluka. It is situate between two infected villages. I told the people they were in imminent danger, and advised them to live in their fields as a precautionary measure. They emphatically said they had had no communication with either of the infected villages. They, however, assented to my suggestion to thoroughly ventilate their houses. This they set to do, but while I was inspecting the progress made and also visiting the houses where deaths had taken place within the preceding four weeks, I came upon a house where that very morning (17th December) a man had died in a certain house and I was informed that his wife had died in the same house on 23rd November. After a deal of cross-examination it was brought out that the woman had been to a funeral at Kakhindki, one of the infected villages in question. Two days after her return she had died. Regarding this, therefore, as an imported case, and allowing three weeks for full incubation, I came to the conclusion that the husband must have died of plague. I also ascertained that three days before his death he had been seen going about. I therefore segregated that family as also those of the brother and the sister of the deceased—occupying adjoining houses. In another house, at the back of the first one, the wife of the occupant was absent, and was said "to be living at her father's house for some time." She was visited there and found to be feverish. She, as well as her parents, were segregated. On the third day following she died, apparently of pneumonic plague. Two of the sons of the first case, as well as his brother, also developed plague (as pronounced by a Hospital Assistant) in quick succession. By this time all the people of the village voluntarily cleared out in two days. After this there was no fresh case or death for whole week; and there had been only five cases and three deaths up to that time. Unfortunately, it so happened that I had allowed a woman, who was very near confinement, to stay in her house with her husband and children, as I thought there was no immediate risk of that locality being infected. Almost all the members of that family, however, got plague after a few days, and were the means of spreading the disease among others living in the fields. Barring this mishap, I consider that Bobleshwar is a notable instance of the success attending immediate evacuation if the first case of plague is spotted. It also shows that it is not enough to evacuate the portion of the village which is immediately infected, as other localities eventually get infected. This happened, although I had caused the roofs of all the houses in the infected block to be completely removed within three days. I understand, also, that dead rats were subsequently found in different localities, even after the people had quitted the houses.

19,514. Is the interval, elapsing between the first imported case and the first local case, the same in dry weather as it is in damp weather?—I think it is shorter in damp weather. I have given some instances and can give others to show that nearly three to four weeks elapse before an imported case, if left alone, causes indigenous plague in a hitherto uninfected locality. This fact helps to detect the first signs of plague. The period may be shorter in damp weather. This interval of time should be always borne in mind in calculating the effect of the admission of people from infected areas into another place, and raises important considerations as to the safety of the present surveillance and detention system. I have further observed that cases of plague are sometimes developed in the fields 10 to 20 days after evacuation, although the people have not mingled with infected persons.

19,515. What do you say with regard to disinfection?—Except at Ilkal, disinfection with perchoride of mercury was abandoned, and the infected houses were only ventilated, generally by removing the roofs altogether. This is easy to do, as in that part of the country the houses are only one-storeyed with flat mud roofs, which it costs little to renew. The very poor persons can be compensated. From my experience I think disinfection, considering its cost and other drawbacks, has not been proved to achieve the purpose for which it is undertaken. It is not proved that immediately a house is disinfected after an infected family is removed from it, another healthy family can safely go and live in it, much less the original family. Disinfection in advance, too, has failed, as I have said above. On the other hand, the very fact that a properly made Plague Hospital does not require to be disinfected every time a case dies, and that the medical and other attendants can safely live in it, shows that disinfection must be a superfluity in other infected houses, provided other conditions are the same, and I fail to see what they can be but the ventilation—the free admission of daylight and fresh air. With this view we insist that when people evacuate a village and go into the fields they should not create the same unhealthy conditions again by making air-tight and badly-lighted huts as they are prone to do, but should leave plenty of passage for light and air. If disinfection is authoritatively given up or condemned, it will not only result in a saving of nearly half of the cost of operations in rural areas, but will remove a highly fruitful source of irritation to the people, so that still another inducement to conceal will be taken away. It has been observed that by far the largest majority of plague cases, if not all, occur when the sleeping-rooms are badly or insufficiently ventilated or lighted. Even if there are windows or ventilators, the people are in the habit of closing them at night. This may sufficiently account for the occurrence of plague among the well-to-do classes. On the other hand, people living in open spaces, even in filthy surroundings, escape infection. I think ventilation should have precedence even over the cleaning up of houses and streets. It is advisable not to stir up the refuse of ages until some time had elapsed after the evacuation of a house or locality. This would also minimise the occasions for people to re-enter evacuated villages. Ventilation should immediately follow evacuation, but the cleaning may stand over for a month or so, and be carried out a few days before it is proposed to let the people in again.

19,516. (*Mr. Cumine.*) Have you had experience of two epidemics in one district?—No, not actual experience. I only know that at Igatpuri there has been a recrudescence lately.

19,517. From what place did the case come which infected the Bijapur district?—There were practically two epidemics in the Bijapur district, one at Kaladgi, which was stopped, and then, after some months, it broke out at Ilkal.

19,518. From what places did the two cases come which set up these two epidemics?—At Kaladgi, it was supposed that some people had come from Sholapur, which was badly infected at that time, and at Ilkal it was thought the man had come directly from Hubli, but they said he had also come from Bombay with a pass some days before. He came first from Bombay to Hubli and stayed there, and then came on to Ilkal and died there. Hubli was badly infected then.

19,519. Did you have instances of people coming from Bombay and developing plague in villages in the Bijapur district and not infecting the villages?—I have not come across any.

(Witness withdrew.)

(Adjourned till Monday next.)

At The Secretariat, Bombay.

FIFTY-FIRST DAY.

Monday, 20th February 1899.

PRESENT:

PROF. T. R. FRASER, M.D., LL.D., F.R.S. (*President*).

Mr. J. P. HEWETT. | Mr. A. CUMINE.
Prof. A. E. WRIGHT, M.D. | Dr. M. A. RUFFER.

Mr. C. J. HALLIFAX (*Secretary*).

Mr. B. N. Darabsett.

20 Feb. 1899.

Mr. BARAMJEE NAOROSJI DARABSETT called and examined.

9,520. (*The President.*) I believe you are a practitioner of medicine in Bombay?—Yes.

9,521. What are your medical qualifications?—I am a graduate of the Grant Medical College, and I have practised here for the last 39 years.

9,522. We chiefly want to have some information in regard to one special case which I believe you have had under your care, a man who we are told has had three attacks of plague. Would you be good enough to give us a narrative of the case of this man?—Yes. The notes I have here have been put down from memory. Munchershaw was a young Parsee, aged 26, residing in Modi Street.

9,523. In what district is that?—In the Fort. He was of fair complexion and sanguine temperament, strong, healthy, and hale. He was of fair stature. The first attack he had was in Poona. He had been there for a change for about 10 days. About the 10th of February he had shivering and an attack of fever, followed by swelling in the right inguinal gland. He was then treated with brandy, quinine, diaphoretics, and belladona plaster, the ordinary treatment for fever. He got well in about 15 days.

9,524. Had he been inoculated?—No.

9,525. That was the first attack?—Yes. He got over that. The second attack occurred about the middle of April 1897. The attack came on with shivering and high fever. After a time there was a rusty-coloured sputum, with crepitations in the lungs. He was treated similarly internally, and was frequently sponged with carbolic acid lotion.

9,526. On which side did the enlarged glands occur?—On the left side. He had an enlargement on the left side in the second attack. In the first attack it was on the right side. The inguinal glands on the left-hand side were enlarged, and he was treated with an inhalation of carbolic acid with steam, and frequent external spongings with carbolic lotion. This time he recovered in 20 days. On neither occasion was there any suppuration of the inguinal gland. The third attack was on the 25th of January 1898. It commenced with high fever and shivering, followed by hæmorrhage from the nose and lungs. No glands in the groin were enlarged, but enlargement of the gland in the neck and chest appeared the day previous to death. No inguinal glands were affected on the third occasion, but he again had pneumonia and hæmoptysis.

9,527. Were the glands within the thorax affected, the mediastinal glands?—Yes.

9,528. How did you find that out?—He complained of a very severe pain in the chest. We were obliged to inject morphia hypodermically.

9,529. How do you know that the glands within the chest were enlarged?—Because of the chain of glands extending down into the mediastinum.

9,530. Part of the neck was enlarged?—Probably the glands could not be felt at first. The man died on the 1st of February after an illness of about six or seven days.

9,531. Was there any *post mortem* examination?—Certainly not; he was a Parsee.

9,532. What was the man's occupation?—He was employed as a clerk in a photographic office.

9,533. Can you tell us to what extent he was exposed to plague before any of these attacks?—He was exposed to plague; he was living in a plague house.

9,534. On which of these occasions?—On all three occasions he was in the same house.

9,535. He came from Poona, did he not?—The first time he was in Poona. The very next day after the attack he returned to Bombay, to Bazar Gate Street, and he was treated in Modi Street for the last two attacks.

9,536. With regard to the first attack, he was at Poona and came to Bombay. Did he become ill in Poona or in Bombay?—The fever commenced in Poona.

9,537. With regard to that attack, what was the exposure to plague—the first time?—The first time, I should think, he exposed himself in Poona.

9,538. What was the exposure in the second attack. How was he exposed to your knowledge?—Plague cases occurred in the house he lived in.

9,539. And the third attack?—The same. Previous to his dying there had been two deaths from plague in the same house.

9,540. Had not the house been disinfected before he became ill?—Very perfunctorily—just whitewashing with lime. At that time strong disinfectants were not used. Latterly, however, they have been used more freely.

(Witness withdrew.)

Mr. Champaklal Thakurdas.

Mr. CHAMPAKLAL THAKURDAS called and examined.

9,541. (*The President.*) What is your occupation?—I am in Government service. I am an Inspector in the Bombay Custom House.

9,542. (*Dr. Ruffer.*) When did you first have plague?—On the 11th of October 1896.

9,543. Can you describe the symptoms you had at that time?—That day, being Sunday, in the evening I went to visit a friend of mine. At that time I was living in Kandawdi near Girgaum. My relative was living near Kalbadevi. On reaching half way I got some pain in the left groin. I did not care for the pain, and I at once proceeded to the house of my relative. I sat there for half-an-hour, but in the meantime I began to shiver from the effects of cold. I informed my relative about it, and he predicted that this would be followed by high fever.

19,544. Did a swelling appear in the groin?—It appeared afterwards.

19,545. When?—An hour or so after I got the fever.

19,546. How long did the swelling last?—It lasted for four days.

19,547. Did it suppurate, did matter come out of the bubo?—It was not operated upon. It subsided by the mere outward application of a plaster.

19,548. What was the highest fever you had?—My temperature went up to 105°.

19,549. Had you ever had an attack like that before?—No, not before that.

19,550. Are you a married man?—Yes.

19,551. Did you have any pain anywhere else at that time?—Only in the left groin.

19,552. You had no pain in the testicle?—No.

19,553. Or anywhere else in that region?—Nowhere else.

19,554. Did you have any pain in the abdomen?—At that time I had no pain anywhere else.

19,555. The swelling went down in five days?—Yes.

19,556. How long did the fever last?—Five days. For two days my temperature was 102°, and on the third day it was 102°. Then it began to subside, and on the fifth day I was all right.

19,557. Was your appetite good?—I did not take food all the time.

19,558. Did you want to eat, or not?—On the first day I did not want to eat, but on the second day I thought I was feeling hungry.

19,559. What was your tongue like, do you know?—I was very thirsty.

19,560. Do you know whether your eyes were red?—My eyes were red.

19,561. How do you know?—They told me so.

19,562. You got quite well of that?—Completely.

19,563. When was the second attack?—On the 29th of March 1897.

19,564. Where?—That day I attended office, and while going home in the evening I felt rather uneasy, and on the road I felt pain in the very same part.

19,565. Had the gland remained enlarged between the first and the second attack? Was the swelling you had the first time still to be felt when you had the second attack?—Although there was no pain there was a small gland inside the skin which could be felt, but it did not cause any pain.

19,566. How long did the second attack last?—Very nearly three months.

19,567. How did it begin, with fever?—First of all I felt some pain in the same place. That pain afterwards subsided when I got fever.

19,568. How long did the swelling last in the groin?—In the second attack it was cut open.

19,569. When was it cut open?—After 15 days.

19,570. Why was it cut open?—Because it was ripe.

19,571. There was matter in it?—Yes, there was matter in it.

19,572. Did you have a swelling anywhere else?—When the suppuration was going on I felt some electrical current passing from that part to the middle of my thigh, where I have another cut. On the next day there was another swelling.

19,573. What was that other cut in the middle of the thigh; why did you have another cut there?—They operated upon it in the hospital.

19,574. Afterwards?—Yes. On the next day there appeared some swelling on that part. That was also poulticed. After eight days that was also operated upon.

19,575. And pus came out?—Yes, and pus came out.

19,576. Did you have any swellings anywhere else?—The same swelling appeared in the groin.

19,577. When was that?—That was some 10 or 12 days after the operation.

19,578. How long did that swelling last?—That swelling lasted until I recovered.

19,579. How long was that?—I think some portion of it remains now.

19,580. How long after the first, second, and third swellings had been opened did they remain open?—The first healed in 15 days, and the second also took 15 days.

19,581. What about the third?—The third was not opened.

19,582. It healed up without being opened?—Yes.

19,583. Did you have headache the first time you had plague?—The first time I had no headache.

19,584. Did you have headache the second time?—No.

19,585. Did you have any delirium the first time?—No, I had no delirium the first time, but the second time I had.

19,586. How long did it last?—It lasted for nearly two days.

19,587. Were you seen by a medical man each time?—On both times I was seen by a medical man.

19,588. Who was the medical men who saw you the first time?—Mr. V. S. Divan.

19,589. Did he think you had plague?—Afterwards he took it to be a plague case.

19,590. He did not at first take it to be a plague case?—No.

19,591. The second time he had no doubt about its being a plague case?—No. He removed me to the Plague Hospital.

19,592. Did any of your relatives get plague either on the first or second occasion?—No.

19,593. Was there any plague where you lived?—No.

19,594. Did you feel very weak after the second illness?—Yes, I was very weak.

19,595. How long did the weakness last?—For three months.

19,596. Were your eyes injected—red—the second time?—I do not remember.

Mr. Champaklal Thakurdas.

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(Witness withdrew.)

Mr. NARAYAN MULJEE called and examined.

Mr. Narayan Muljee.

(*Dr. Godinho acted as interpreter.*)

19,597. (*The President.*) What was the date of your first attack of plague?—September 1896.

19,598. What glands were then affected?—The glands in the right groin.

19,599. How long did that attack last?—A month and a quarter.

19,600. By whom were you treated?—I was treated by Dr. Ismail Jan Mahomed.

19,601. Where?—I first went to him, and then he called upon me.

19,602. Where is your house?—Nos. 2, 4, and 6, Falkand Road, C. Ward, opposite Dr. Ismail Jan Mahomed's dispensary.

19,603. When was the second attack?—A month and a half after I recovered from the first attack I got the second attack.

19,604. That would be about November?—Yes.

19,605. Which glands were then affected?—The cervical gland on the left side.

19,606. How long were you ill?—About a fortnight.

19,607. Were you treated in the same house?—I did not take any medicine from the doctor; I was treated at home.

19,608. When was the third attack?—In the month of August 1897. First my brother was attacked and then my father was attacked.

19,609. What glands were affected?—The glands in the right axilla. I was operated upon for that.

19,610. How long did the third attack last?—A month and a half, six weeks.

19,611. On which occasion were the glands opened?—Only once; on the third occasion.

Mr. Narayan Muljee.

20 Feb. 1899.

19,612. Do you know if you had been specially exposed to plague infection?—I cannot give any cause for the first infection. I took the second infection from my tenants.

19,613. What tenants? How long had they been ill before you became ill?—I do not remember how long before.

19,614. Was it before you were ill?—They were ill before I was ill.

19,615. Was it a week or a month before?—Within 15 days.

19,616. Were you living in the same house?—Yes.

19,617. How did you get the third attack?—First, my brother was attacked. He died. Next, my father was attacked, and he died. Then I was attacked; and after me four others were attacked.

(*Dr. Godinho.*) I remember about those four cases.

19,618. (*The President.*) Had you been inoculated?—No.

19,619. I suppose there was no bacteriological examination?—(*Dr. Godinho.*) Nothing was done in 1896.

19,620. (*Dr. Ruffer.*) Did you have other symptoms besides enlarged glands, did you have headache?—First I had severe pains in the head, and next, I had nausea and vomiting.

19,621. Did you have fever?—Yes. Those symptoms were immediately followed by fever.

19,622. How long did the fever last on the first occasion?—About 16 or 17 days.

19,623. (*The President.*) How do you know that?—I was unconscious for seven days, and afterwards I became all right.

19,624. You said the fever lasted 16 or 17 days; how do you know?—I did not move from home, that is how I know it.

19,625. (*Dr. Ruffer.*) Did you have any fever on the second occasion?—The second attack of fever was mild.

19,626. Did you have headache then?—I had a chill.

19,627. Any vomiting?—No.

19,628. Did you have fever on the third occasion?—I went to the funeral ceremony of my brother, and afterwards I went to the tank and had a bath. I felt chilly there and I came home.

(*Dr. Ruffer.*) Is there anything to see under your axilla? Have you still got the scar? (*The Commissioner here examined the axilla of the witness.*) I see the scar is about half-an-inch long on the outer border of the *pectoralis major* muscle: it is not in the axilla.

(Witness withdrew.)

Mr. G. Monteath, I.C.S.

Mr. G. Monteath, I.C.S., called and examined.

19,629. (*The President.*) You are in the Indian Civil Service, and are Assistant Collector at Thana?—Yes.

19,630. (*Mr. Cumine.*) In what districts have you had experience of plague, and for what periods in each?—In Satara for six or seven months, and in Thana since June last.

19,631. In your précis of evidence you mentioned the town of Tasgaon. At first, was your evacuation confined to the infected houses, and those immediately surrounding them?—Yes, it was.

19,632. Did you find that that stopped the spread of plague in the town?—Not at once.

19,633. What did you go on to evacuate?—The whole of the infected quarters, by pets and blocks.

19,634. Did that stop the spread of plague in the town?—It did, in a little less than three months.

19,635. Why did you have finally to resort to complete evacuation of the whole town?—The town was never completely evacuated.

19,636. When there were fewest people in the town, about how many were there left?—Under 1,000.

19,637. As regards the infected villages, did you have some instances where, having got the first one or two cases, partial evacuation was successful in preventing the spread of plague in the village?—Yes, I remember two cases.

19,638. But in the majority of cases did it fail?—In the majority of cases the plague was not found out immediately on the first case occurring, and so partial evacuation failed.

19,639. In order to make evacuation produce the maximum of good, with what other measures must it be supplemented?—First, I think you must separate the sick, and also the inhabitants of the same houses, from the healthy population.

19,640. That would be one measure; what is the other?—And disinfect the houses themselves.

19,641. And prevent communication with the infected houses?—Yes, as far as possible.

19,642. If in a district there are a large number of villages infected, is it practicable for the Collector and his staff to effectively carry out, in the case of each village, the measure of evacuation with the two or three complementary measures that you have mentioned?—I do not think so, judging from the Satara district, unless all the village officers are to be depended on.

19,643. Have you anything to tell us, in connection with monkeys, as to the way in which the poison hangs about evacuated houses?—I give the instance for what it is worth. When the village was evacuated, large numbers of monkeys used to enter the houses by the gaps made in the roofs, seeking what they could steal I noticed, on riding into the village on one occasion, a monkey drop dead from a tree above the chavdi. On inquiry I learned that 10 or 12 had died similarly—some in the houses. Coming again to the village a few days later, I was told that five or six more had died.

19,644. Did you have any of them bacteriologically examined?—No.

19,645. You mention, in your précis of evidence, Parli Fort as a good instance of the way in which rats helped to spread the disease. Do you mean spread the disease from house to house within a village?—Yes, from house to house.

19,646. How do you know it was rats that were spreading it from house to house?—I went up there and found out for myself.

19,647. How did you satisfy yourself that it was rats?—I saw one or two, and all the people agreed that rats were dying in nearly every house in the Fort. It is a very confined place, of course.

19,648. I ask how you knew it was rats that were carrying it from house to house?—I think it was by rats, but I did not prove it.

19,649. Have you any notes to show how long after the arrival of an imported case, which infects the village, the first indigenous attack occurs?—I have no notes; but I remember that, in Vasadage, it seemed to take about 14 days.

19,650. Have you noticed whether rats are, or are not, always attacked between the imported case and the first indigenous case?—Yes, I think that is almost always so, according to what I have seen.

19,651. Would the imported case appear to infect the local person directly, or would it appear to infect the rats first, and then the rats infect the local person?—It seemed to me that it infected the rats indirectly, and then they spread the disease from house to house. I do not know, but that is what it seemed to me.

19,652. Do you think you quite understand my point? We have been told by one witness that there always appear to be attacks amongst rats between the imported case and the first indigenous case, as though the imported case were unable to infect the local people directly, but had to do it through the medium of rats. Have you noticed that at all?—I have noticed it, yes.

19,653. That rats always died between the imported case and the first local case? Can you say confidently that you have noticed that?—I have noticed that they almost always so die.

19,654. Would you read us what you say in your précis about Wasind?—Wasind, taluka Shahapur, was infected about August 20th, 1898. Fortunately the village was

discovered to be infected before the first indigenous case occurred, from the fact that rats had begun to die in several houses. A good many persons left the village of their own accord, and on the occurrence of the first case the whole of the inhabitants of the infected area (the bazar) were made to go and live in the fields. Two or three cases occurred among the railway hands, whose families lived in the bazar. The disease died out almost at once. In November a solitary case, apparently of plague, occurred at Wasind; the same measures were put in force, and there were no more cases. One case was reported on December 9th, but I do not believe it was plague, since it was neither preceded nor followed by other cases, nor by the discovery of dead rats.

19,655. Who said that the case on December the 9th was plague?—It was reported to me by the Mamlatdar as plague.

19,656. Since one case in November, and one case in December, appear to have occurred, would you say that the outbreak of August the 20th had died out, or that it appeared to be smouldering?—I think it had died out, there have been no more cases since.

19,657. Is there any plague in Wasind now?—No.

19,658. Has the death rate of Wasind been abnormally high lately?—No.

19,659. Is there any plague in Kalyan at present?—Yes, there is.

19,660. Has the segregation of contacts been abandoned in Kalyan?—Yes.

19,661. What has been the effect of that?—What happened immediately afterwards was that the number of cases a day were nearly doubled.

19,662. And then?—And then in about a fortnight they decreased again.

19,663. Do you leave any of the patients in their houses in Kalyan, if so, which?—Cases occurring among persons who pay Rs. 20 and more income tax are allowed to be segregated in their own houses, one or two members of the family remaining to attend the sick person, and the rest being directed to remove either to another house or out of the town.

19,664. Do you disinfect daily these rooms in which the patients are?—No.

19,665. You find that bad results have arisen from leaving the patients in their houses?—Directly or indirectly, altogether. The rest of the people want to follow their example, and it is rather difficult to deal with them.

19,666. Do you find that many other people have been infected by these patients?—No, I cannot say so.

19,667. Do you find that the people left to nurse them get plague a good deal from them?—I do not think so, anymore than they would if they were turned out.

19,668. Have you had any experience of inoculation?—No, none at all.

19,669. Has any instance come to your notice of plague having appeared in a village which was infected last year, and which has had, so far as you can make out, no fresh infection this year? Has, in fact, any instance of a real recrudescence, from the original germ, come to your notice?—Which was not re-infected this year?

19,670. Quite so?—I do not remember any.

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19,671. Did you allow these patients in Kalyan that you left in their rooms to be put on a bed, or did you allow them to lie on the floor?—They mostly had beds.

19,672. If they have not, do you put them on a bed?—No.

19,673. (*Mr. Hewett.*) Have you tried the kiln system of disinfection at all?—Dr. Dadima has tried it in Kalyan.

19,674. Is it expensive?—No, not as it is done.

19,675. Do you think, supposing that disinfection with perchloride of mercury is equally effective, that it is preferable to the kiln systems that are substituted for it?—I do not know, but I have seen no results from the kiln system.

19,676. Supposing they were equally effective, which would you prefer?—I think the kiln system would be better and cheaper probably.

19,677. You think it would be cheaper. Do you think that it would involve less interference with the people than disinfection by means of perchloride of mercury?—No, I do not think it would.

19,678. Do you think that it would interfere more with them?—Yes, I do, somewhat more.

19,679. You tried burning down huts once or twice, did you not?—Yes, I did twice.

19,680. Were they isolated huts?—They were.

19,681. People would not like that to be done on a large scale?—They would not.

19,682. Did you find it effective in thec ases where you burnt them down?—I did in those two instances.

19,683. Had you a single case in each hut?—Yes, a single case.

19,684. (*The President.*) What do you think, in your experience, is the most effective method of dealing with an epidemic of plague?—The most effective seems to be to turn all the people out, or as many of them as possible, into the fields.

19,685. Evacuation?—Yes.

19,686. What is the largest town or village in which you carried out that method?—Kalyan, I suppose; it is practically evacuated.

19,687. What is its population?—The normal population would be about 13,000.

19,688. Within what time did you effect the evacuation of the town, how long did it take?—It took about three months, gradually.

19,689. You did not try to do it all at once?—No.

19,690. What is the kind of house in the district you are now talking about?—In the villages most of them live in very poor huts—chappars.

19,691. How many chambers in each hut?—Sometimes two, sometimes three; they are only separated by chappar.

19,692. What outside openings have they got besides the door—have they any windows?—No, no windows.

19,693. Only a door?—Yes, only a door, or doors.

(Witness withdrew.)

Lieut.-Colonel A. W. F. Street, I.M.S., called and examined.

Lt.-Col. A.W.F. Street, I.M.S.

19,694. (*The President.*) You are in the Indian Medical Service and Sanitary Commissioner for the Presidency of Bombay and Sind?—Yes.

19,695. What are your medical qualifications?—M.R.C.S.E., and L.R.C.P.Lond.

19,696. (*Mr. Hewett.*) Can you give us the total mortality from plague, and the rate per mille per annum, first in Bombay, and secondly, in other parts of the Bombay Presidency and Sind, from 1896 to the present time, also the mortality from all causes at the same time and the rate per mille; also the mortality from cholera and the rate per mille; and also the mortality in all diseases classified as fever?—I put in a statement which gives the figures asked for.*

19,697. Have you any Assistant Surgeons employed under you in the Sanitary Department in the Bombay Presidency?—There is only one Assistant Surgeon in the Department, but he has no sanitary duties.

19,698. How many Deputy Sanitary Commissioners have you got?—Five.

19,699. Does a Civil Surgeon in the Bombay Presidency and Sind have any authority as regards sanitary matters?—Not any real authority; in his own town he interests himself to a certain extent.

19,700. Has he any responsibilty as regards th sanitation of the Municipalities in his district?—No, do not think so, except in places where there are dispensaries, and in his headquarter station.

19,701. Nor as regards the sanitation of rural areas in his district?—No, none.

* *See* App. No. LVII. in this Volume.

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19,702. The whole responsibility as regards the rural areas at all events rests with the peripatetic officers; the Deputy Sanitary Commissioners, and you?—Yes.

19,703. Are you Inspector-General of Vaccination, as well as Sanitary Commissioner?—Yes, vaccination is one of the duties of the Sanitary Commissioners.

19,704. Have you any establishment as Sanitary Commissioner, distinct from the establishment which you have as Inspector-General of Vaccination?—Only clerks.

19,705. What is your vaccination establishment?—Inspectors and vaccinators. The Inspectors of Vaccination are Inspectors of Sanitation as well.

19,706. What is the number of vaccinators employed under you?—388.

19,707. And of Inspectors of Vaccination?—One Superintendent of Vaccination, Bombay, one Superintendent of Vaccination, Karachi, one Assistant Superintendent, Bombay, 31 Inspectors of Sanitation and Vaccination.

19,708. You say these Inspectors are employed on sanitary matters?—To a certain extent.

19,709. Have they any sanitary qualifications?—No, no qualifications; no pass qualifications. They pick up a certain amount of knowledge when with the Deputies on tour.

19,710. They have not been trained in hygiene?—No.

19,711. What is the rate of their pay?—Rs. 55, 75, 100, and including travelling allowances Rs. 75, 100, 130.

19,712. Are any of them Assistant Surgeons?—No, except the one in Bombay, who has no sanitary duties, and he gets Rs. 380 pay and allowance.

19,713. Would it in your opinion be a good plan to substitute for your present Inspectors of Vaccination, Assistant Surgeons who have been trained in hygiene?—Do you mean in order to benefit vaccination?

19,714. I mean in order to benefit sanitation?—Yes, of course, but I do not know whether vaccination would be benefited.

19,715. Why do you think vaccination would not benefit?—Because I like the men who have gone through the mill and risen from vaccinators.

19,716. On what ground?—Because they know all the ins and outs of the work.

19,717. Do you think that an Assistant Surgeon could learn the ins and outs of vaccination?—I do not think he could as well, not unless he went through the mill himself.

19,718. Has the Inspector been through the mill?—Yes.

19,719. Could not the Assistant Surgeon acquire the necessary knowledge?—Yes. I do not doubt, if you got very good men, he might.

19,720. Can you suggest any means by which you can obtain earlier notification of cases of plague other than the improvement of the class of your subordinates?—Of course we have the village officers.

19,721. Do you think that the responsibility of the village officers is one which can ever be properly enforced? Can they know what is plague and what is not?—That is so.

19,722. Can you suggest any other means of improving the system of notification?—You cannot supervise every village with a fully qualified man in that way.

19,723. My idea was that a qualified Assistant Surgeon might, while on inspection work, be able to help you to some extent if he had to look round and inspect the village and examine its condition from a sanitary point of view; might he not assist you as regards the recognition of plague?—He might, but the villages are very numerous, and he would have a large number of villages. My vaccinators have done a little in that way. They have always been told, if they come across anything of that kind, to report it.

19,724. Is their utility in this respect not rather affected by the fact that they could not tell a case of plague?—Yes; but if the village officers knew that plague was in the village, and the vaccinator went there, they would not let him work, and would tell him that there was plague, and, therefore, he could not work, and then if plague was being concealed, its existence would come out.

19,725. You think that you could, under present arrangements, find out the existence of plague in places where it exists to a considerable extent?—Yes.

19,726. I was thinking more of trying to find the first cases?—That would be a very big business owing to the extent of the country and the number of villages.

19,727. I do not quite understand why an Assistant Surgeon, who had been properly trained, should not be able to check the ordinary vaccinators in their work?—I do not see why he should not, if we got good men; but I prefer a man who has gone through it all from being a vaccinator.

19,728. Have you any authority as Sanitary Commissioner over the city of Bombay?—No, except as regards vaccination, and to a certain extent, over the Health Officers of the Port.

19,729. (*Dr. Ruffer.*) Who appoints the Deputy Sanitary Commissioners?—Government appoints them, but there are generally candidates for the post, and the Sanitary Commissioner would choose and recommend a man to the Government.

19,730. Are they chosen from men who have got special qualifications in sanitary affairs?—I do not know. I suppose a man would be preferred if he had.

19,731. As a rule they are not?—I cannot say absolutely.

19,732. Have you got a voice in their choice?—Yes, I think so. If I knew a man I wanted, I should ask for him; and if a man wanted an appointment he would write to me and say he wanted to come to the Sanitary Department, and I would pick out the best man, and ask for him if there is a vacancy.

19,733. You have five Deputy Sanitary Commissioners?—Yes.

19,734. How many of the Deputy Sanitary Commissioners have any qualification in Sanitary Science?—I know one has; I do not think any of the others have.

19,735. What qualification has he got?—D.P.H.

19,736. Of Cambridge?—I think so; I am not sure.

19,737. Have you any authority over Civil Surgeons?—No, none.

19,738. How large is the District the Deputy Sanitary Commissioner has to visit?—I have only just taken up the office of Sanitary Commissioner. I have been a Deputy for years.

19,739. How large is your district?—It is as big as Scotland.

19,740. How often do you visit each village?—I could not visit each village.

19,741. You have never visited each village?—No. I have got five Collectorates, and they are divided into talukas, and it is as much as I can do to visit half my talukas in one year. I ought to have only half the district, if I had to visit each taluka. The average number of villages in one taluka is 154; the numbers range from 49 to 394.

19,742. The large majority of the villages are not visited from year to year by a Sanitary Commissioner, or a Deputy Sanitary Commissioner, or anyone holding a Sanitary qualification?—A very very small proportion. Of course the Inspectors visit a fair amount.

19,743. I believe there is a Provincial Sanitary Board?—Yes.

19,744. How often does that meet?—I cannot say; I have just taken over the duties.

19,745. How long have you been Sanitary Commissioner?—About a month.

19,746. Has it met during that time?—No.

19,747. Can you tell me how many formal meetings they have had in the last three years, and whether minutes were taken?—During the monsoon, once a week, or oftener if there is anything urgent. During the fair season, perhaps, once a month. No minutes are kept as the Board has no initiative, and is, therefore, only a consultative body to consider plans and proposals transmitted to it by Government and others.

19,748. Have you any disinfecting apparatus in Bombay apart from the plague work?—No.

19,749. None whatever?—No, not that I know of.

19,750. Have you any disinfecting arrangements at all for other diseases besides plague?—No, I do not think so. When the plague began, I do not think there was

a single disinfecting machine in Bombay, so far as I remember.

19,751. What are the number of villages the vaccinator goes through in the course of the year?—I generally like to give them about 80.

19,752. How often does he get into these 80 villages, once or twice?—Every one once, and the big ones twice, as many as he can of the big ones.

19,753. (*Mr. Cumine.*) Did you tell us how many Sanitary Inspectors you had? Take the district of Ahmednagar; how many are there in that district?—One to each Collectorate; I have got two in Satara.

19,754. In one Collectorate, how many hundred villages would there be?—A very very large number, 403 to 2,824.

19,755. (*Prof. Wright.*) How many members are there on the Sanitary Board?—Two members; the Sanitary Commissioner and the Sanitary Engineer.

19,756. Have you any other work beyond inspecting vaccination?—Sanitary work and vaccination.

19,757. What does the Sanitary work consist of, reporting on accumulations of dirt?—Inspecting the Municipal towns. The Sanitary Commissioner confines himself to the Municipal towns. The Deputies go everywhere.

19,758. What does the Sanitary Commissioner do?—You inspect a town and see what they have and what they want, what can be improved; and then you put it on paper in the form of a report.

19,759. Supposing there is an epidemic of cholera, do you take any practical measures, or do you simply write a report?—In most large towns, where you do anything, there would be a Municipality most likely, and they are put into motion. There would be a Civil Surgeon most likely.

19,760. Has the Sanitary Department anything to do with fighting cholera in a cholera epidemic, for instance?—It does what it can. You cannot do much except in the large towns. For the country we try to improve sanitary matters by a better water supply and that sort of thing. That is the only way to get over cholera in the districts.

19,761. There is a good deal of inspecting; but is there any practical action taken?—It is difficult to get any action taken. It is generally through want of funds.

19,762. Do you think the inspection does any good, short of taking practical steps as the result of that inspection?—Yes, it does a certain amount of good. It keeps Municipalities up to their work, and you may get some improvements even if there is no money to do it with, and then when the time comes you have got it down on record what is to be done. You can advise the Municipality what they ought to do, and when they get money they may spend a certain amount in the necessary improvements; and when they get more they may turn their hands to the other things.

19,763. I see the expenditure of the State upon the Sanitary Department: I do not see the return that accrues to the State, except in the case of vaccination?—If they give us money we are prepared to do more.

19,764. (*The President.*) What are the chief duties of the Deputy Sanitary Commissioner?—Vaccination and sanitation.

19,765. How much of the time is occupied in vaccination work?—The seven months that he can get out he travels in the districts and inspects. He does vaccination and sanitation work at the same time.

19,766. The chief work is vaccination?—Yes, we have money for it and we do it.

19,767. It is a sort of incident to his work that he does sanitary work?—There are certain sanitary duties to do when he is on tour. Every village I go to I see the water supply and that kind of thing; and in every Municipality I make a thorough inspection,

19,768. Do you go into the houses to see what the actual conditions in the houses are, or take any cognisance of overcrowding or ventilation?—No; not, as a rule, in the country, but we do in the towns, when necessary. We know what the conditions are.

19,769. Any cognisance into the conditions of milk supply?—I am speaking of the country now.

19,770. Any part of your district—country or town?—In the towns we do, but to a very limited extent, and only, as a rule, in the case of European supplies.

19,771. I thought you said the amount of vaccination work was so large that it practically occupies the whole time?—You have to go a certain number of miles to a village, and when you are there you can do both. If I had to go 20 miles in one direction to see sanitation, then 20 miles in another to see vaccination, it would take a lot of time, but I can do both in the same village.

19,772. What do you mean by both? You mean with regard to large questions like those of water supply?—Yes, water supply, the surface cleanliness, and surface drainage. Those are the things we chiefly want in villages, good water supplies and good wells.

19,773. I did not quite gather how long it took you, when you were Deputy, to go round your own district?—Two years. I could hardly do it in two years.

19,774. Did you visit every village of a fair size in two years?—No.

19,775. How long would that take you?—I could not do it.

19,776. Ten or 20 years?—Yes.

19,777. With regard to the Inspectors, who have smaller areas to deal with, what are they termed—what is their official title?—Inspectors of Sanitation and Vaccination.

19,778. Do they merely inspect the vaccination, or do they themselves vaccinate?—They inspect.

19,779. In regard to the work of vaccination, how often can they go through and inspect the district which they have to deal with?—They go through every part of it once a year or more.

19,780. It requires very nearly a year to inspect the vaccination of that area?—Yes.

19,781. Then there cannot be very much time left for examining the details of the sanitary conditions, I presume?—No; except, as I say, you can combine the work. I mean if you are in one place, your vaccination does not take you the whole day. You can do your vaccination work, and then you can do your sanitation work in the village. The distances are so great that the work must be combined.

19,782. (*Mr. Hewett.*) You have not mentioned anything with regard to vital statistics. Is it not a part of the duties of the Sanitary Commissioner and Deputy Sanitary Commissioner to supervise the collection of vital statistics?—We register all the births and deaths.

19,783. Do not you go round and check the correctness of the registers?—Yes.

19,784. Is not that one of the most important duties of your inspecting staff?—Yes, it is one.

19,785. Then you are not entirely occupied in vaccination?—Oh no, that is another part of it. They do that in the villages they go to: they examine, in fact, all the registers, the sanitary registers, the vaccination registers, and the cholera registers.

19,786. (*Mr. Cumine.*) You were asked by Professor Wright what share the Sanitary Department take in fighting an epidemic. Do they take any share at all in the actual fighting of it when it breaks out?—It depends; if it is a big Municipality they do.

(Witness withdrew.)

Lieutenant H. P. Keelan, I.S.C., called and examined.

19,787. (*The President.*) I believe you are a member of the Indian Staff Corps?—Yes.

19,788. And you were engaged in plague work in Hubli?—Yes.

19,789. (*Mr. Hewett.*) Could you state the dates between which you were employed at Hubli?—From the 25th of December 1897 to the 10th of October 1898. From the 10th of October 1898 to the 11th of November 1898 I was in Dharwar, a few miles from Hubli.

19,790. What is the normal population of Hubli?—About 55,000.

19,791. At the time you went there in December was there any plague?—There had been some cases in the railway chawls before we arrived there; and for about,

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I think, 25 days at a time, we used to find one solitary case now and again. There was not plague really for the first month after I arrived there.

19,792. What duties were you employed on first?—I was first employed in the railway station, disinfecting passengers.

19,793. When did plague actually begin to attract your attention in the town of Hubli?—About April, I think.

19,794. What measures did you take when it broke out in the town in April?—We had the ward system going on the whole time before that.

19,795. That is a system by which you hoped to discover cases, is it not?—Yes.

19,796. Please describe the system?—The city was divided into ten sub-divisions, and each sub-division was divided into wards. Each ward had a certain number of Ward Supervisors, and there were Ward Superintendents over the Ward Supervisors.

19,797. How many houses had each Ward Supervisor to look after?—From about 90 to 120.

19,798. How many Supervisors had the Ward Superintendents to look after?—About 10. They were not aequal. There were 10 in some, and 11 in others, and so on.

19,799. Did this arrangement give accurate and immediate reports of all cases of sickness?—As far as we could make out it did at the beginning, when the cases were few.

19,800. You think that you found the first case that occurred in Hubli?—I was not there when the first case occurred.

19,801. Do you think you found the first case that occurred when plague began for the second time in April?—That I cannot tell. I am doubtful about that myself.

19,802. When did the system cease to work satisfactorily?—When the panic set in.

19,803. When did the panic take place?—About May, when the cases began to increase. During the rains, generally, it went up. In May, June, and July the panic kept on increasing.

19,804. When you heard of a case of plague, did you remove the sick person out of his house?—Yes.

19,805. What did you do with the healthy persons residing in the house?—We segregated them.

19,806. How many camps had you?—Two camps.

19,807. How many people were in these camps at any one time?—We have had as many as 2,500. It was a bad time of the year.

19,808. What was the maximum you had in your camps at any one time?—Over 2,000. I was not running it myself.

19,809. Who was running it?—Dr. Leumann.

19,810. Did many of the people leave the town?—Yes.

19,811. In what months did they leave the town?—I think it was in July and August.

19,812. Had you a system of issuing passes before they left the town?—Yes.

19,813. When was that introduced?—From the time of my arrival there they had a system of passes.

19,814. Did you give passes to people who went by road as well as by rail?—Yes; but there was no check upon that at all.

19,815. Would any individual who came to you and asked for a pass be entitled to get it?—At first he was, if we knew if he lived in a fairly healthy locality where there had been no plague.

19,816. When did the system change?—The system was changed when inoculation came in.

19,817. Do you remember when that was?—I could not tell you exactly when that was.

19,818. After inoculation came in you only gave the certificates to persons who had been inoculated?—Yes.

19,819. Was it necessary that they should have been inoculated more than once in order to obtain a certificate?—As a rule the people were twice inoculated.

19,820. Were you then responsible for giving the certificates to the twice inoculated who went away by road?—Yes.

19,821. Do you think that the people continued to leave by road after this rule was introduced with regard to inoculation?—Yes.

19,822. Do you consider that as many of the inoculated left as of the uninoculated persons?—No. We found that out by asking our Ward Supervisors to send in lists of the number of persons inoculated, and the number of persons uninoculated every week.

19,823. Then you think that more uninoculated persons left than inoculated?—Yes.

19,824. In spite of the fact of your system of passes?—Yes.

19,825. Then the system of passes was practically useless?—Yes, as far as the road was concerned.

19,826. Were there persons who had been twice inoculated, and who were allowed to remain in their houses when plague occurred in them?—Yes.

19,827. What did you do to the houses in those cases?—We had them disinfected and put back those people who were taken out for the time being.

19,828. Leaving the patient in the house?—No; the patient was taken away to hospital.

19,829. Did you collect any figures to show that in these cases the inhabitants of the houses were not attacked by plague?—I collected particulars of about 200 cases where the uninoculated were attacked, and the inoculated were left in the town and got off.

19,830. Were the figures which were attached to Captain Leumann's Report,*—the 69 cases in the appendix—collected by you?—Yes, the majority of them; more than that, I collected about 200 cases.

19,831. Did you personally verify in each instance the names of the persons said to be inoculated?—Yes.

19,832. And the names of the persons said to have died from plague?—Yes. I went to the houses and asked them all to show their passes.

19,833. What I mean to say is this: supposing you went to house No. 744, did you test whether the five individuals who were said to have been inoculated and to have escaped plague were then present, and whether they had been actually inoculated?—Yes. I took away all the passes, and asked them their names separately.

19,834. Did the deaths from plague come to your notice in the ordinary business of plague administration?—Yes.

19,835. Then are you responsible for the accuracy of the Appendix of Captain Leumann's Report?—Some of the cases were got by him, but the majority were got by me; I do not know whether he has got mine or his.

19,836. Did you find that cases of plague were notified mainly before or after death?—Mainly after death.

19,837. When you had a case notified to you after death, how did you determine whether it was the body of an inoculated or uninoculated person?—We went to the house and simply asked if the person had been inoculated.

19,838. Supposing the residents said that he had not been inoculated, did you accept the statement?—The person's name was sent in, and Dr. Leumann proved whether the person had been inoculated or not in his records.

19,839. Did the records contain the the place of residence of each person inoculated as well as his name?—Yes.

19,840. Was Dr. Cardoz employed in the identification of bodies?—No.

19,841. Did you take the original census of Hubli?—No.

19,842. By whom was it taken?—It was taken before my time.

19,843. Were you responsible for ascertaining through the Superintendents how many persons arrived in Hubli and departed from Hubli each week?—Yes.

19,844. Did you then deduct from your assumed population the number of deaths and the number of persons who had departed, and add to it the number of persons who were reported to have arrived?—Yes.

* *Vide* App. No. XV, in the first Volume of the Commission's Proceedings.

They were not exactly accurate always. There was generally a discrepancy of one or two in each ward.

19,845. Do you think that the result was approximately accurate?—Yes.

19,846. Assuming that the original census figure was right?—Yes.

19,847. Do you rember any date from which a difference was made in recording the number of deaths of plague among the once inoculated?—No. I always put down whether they had been once inoculated or not. The people in the houses generally told me.

19,848. Do you think that cases of deaths from plague among the inoculated were actually recorded as having occurred amongst the uninoculated?—Yes, I think so; there may have been a few left out. I know for certain that some had gone away and died, because they were brought in sometimes, and we got accounts. But the inoculated almost all stopped in Hubli.

19,849. The assumed population of Hubli at the beginning was 58,000, and by the 13th September 1898 it had fallen to 38,408. Of that 38,408 what number do you think had been inoculted?—I should have said by the figures I had then that there were about 3,000 or 4,000 who had not been inoculated.

19,850. Do you continue to hold that opinion?—Yes.

19,851. You do not think there were more than that?—No.

19,852. When you were at Dharwar what privileges were given to the inoculated?—They were given passes. They were allowed to stay in the houses, just the same as in Hubli.

19,853. Notwithstanding permission to stay in their houses, did not a large proportion of the population of Dharwar run away?—They evacuated their houses and lived outside.

19,854. Do you mean that they left the town of Dharwar and that you did not know where they had gone to?—They would be in big camps round Dharwar.

19,855. About how many people were there in those big camps?—I should think in one of the camps there were 400 or 500 people.

19,856. Could you estimate the total population which you think might have been in those camps?—There were huts and camps all over the place, all round Dharwar.

19,857. Do you think there were 10,000 altogether?—Quite.

19,858. Do you think there were more?—I could not tell you that, but I should think there were quite 10,000 people.

19,859. How many were there left in Dharwar town?—I do not remember. I know that a great many went away by train when the first case or two occurred; they went through Hubli by train when there were no restrictions whatever on Dharwar.

19,860. Did a considerable number of inoculated people leave Dharwar?—Yes, they did.

19,861. Did more uninoculated people leave Dharwar than inoculated?—We never tested that.

19,862. You did not test it as you did in Hubli?—No.

19,863. Do you think that more uninoculated people went?—I think about an equal proportion of each. They simply went out into their huts outside, and used to come in from there to be inoculated.

19,864. Do you know how the uninoculated people got back into their houses in Dharwar?—I think they had huts outside and used to come in and sleep at night.

19,865. But when they went away you used to lock their houses up, did not you?—No, we did not.

19,866. You were not there when the houses were locked?—I was there in October and November.

19,867. The houses were locked when we were there?—They locked the houses themselves. There was a rule that the people must be twice inoculated before they were allowed to return to their houses.

19,868. How could you prevent them going back if they themselves had locked their houses?—The Ward Supervisor would go round every morning, and if he learned that a family had come back who were uninoculated, those people would either have to go to the health camp or else be turned out again.

19,869. How long was that rule in force?—From the beginning of plague there.

19,870. (*Dr. Ruffer.*) I do not quite understand how your census was taken. Did the Ward Supervisor go round all the houses each week and ascertain the number of people?—Yes; every Sunday morning he went round. That is the day we thought most people would be in because all the cotton mills and workshops were closed. A lot of people in the railway workshops lived in the town.

19,871. How did you ascertain the number of people? Did you get all the people out in the street and see them?—That was the order given.

19,872. Did the Supervisor do it?—I cannot say that, but it has been done in some cases. I used to go round some of the wards during the week.

19,873. Did you find that the previous statements of the Ward Supervisor were correct when you yourself went with him, or did you find any marked discrepancy?—There was never any marked discrepancy.

19,874. Supposing an inoculated person died, was the house treated in the same manner as when a non-inoculated person had died?—So far as disinfection was concerned it was treated in the same way, but otherwise the people were taken out of the houses and put back again.

19,875. The inoculated people?—Yes.

19,876. What was done with the uninoculated people in the same house?—They were taken to the segregation camp.

19,877. (*Mr. Cumine.*) Does this Appendix* take in all the houses in which inoculated and uninoculated people lived together, and in which plague cases occurred?—No, there are only 69 cases there, I think.

19,878. On what principle were those particular houses selected?—I was told to get good cases, houses where several had been inoculated, and one or two had not been inoculated. There were a lot of houses where perhaps one person had not been inoculated and the others had, and those I should call good cases, where only one of the uninoculated got the disease. I could not go round to see every case when they got heavy.

19,879. I want to know why you went to these particular houses?—Because they came in my beat. I used to take a division one day, and in other cases I would ask the Superintendant to report to me, and I would go and verify it.

19,880. Did you ever see the register kept of inoculated persons?—No, it was kept in the Plague Hospital.

19,881. You do not know whether the names and addresses of each individual were got down fully?—The Pet was put down.

19,882. Have you ever seen the inoculation process going on at the busiest time?—Yes.

19,883. How many people were done in one minute?—I should think about 2.

19,884. When you were making inquiries from house to house as to attacks among the inoculated and the uninoculated, and certain people came forward with inoculation certificates, saying that they were inoculated people who had escaped, did you make any endeavour to make certain as regards each person producing such a certificate that he was really the person named in the certificate?—My method was to take all passes away from the people. A certain number of people showed me their passes, and I asked each one what his or her name was. Then if the name given corresponded with the name on the certificate I gave it back.

19,885. Did you make any endeavour to find out whether the person was giving the correct name or a false name?—I do not see how they could find out what names were on the passes.

19,886. But did you make any endeavour?—No, the inoculation certificates were all written in English.

19,887. Supposing a person has a certificate and dies, and his brother annexes the pass, would he not know that his brother's name was on the pass?—Yes, he would. There is no way of ascertaining that.

19,888. (*Prof. Wright.*) What were the inducements offered to people to get inoculated?—The inducements were, that in case of plague occurring in the house, the inoculated persons would not be segregated or sent

* Vide App. No. XV. in the first volume.

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to the health camp. Also, in case of an area being turned out, inoculated persons would not be sent to either of those camps. They could also travel anywhere in the district with a pass, whereas people who were uninoculated would not be given a pass.

19,889. Do you think that the people felt that there was any pressure being applied?—At first they did.

19,890. Do you think, therefore, that uninoculated people would have tried to escape observation? For instance, would they have gone out by the back door when the census was taken and try to hide themselves?—Yes, they did.

19,891. How do you explain the enormous mortality of 65 per cent. there was in certain weeks among the uninoculated? Do you think that represents the actual mortality among them, or do you think there were more uninoculated people than was stated in the returns?—There may have been a few more uninoculated, but I do not think there were many more. I think that this census which was sent in was really correct for each division to within 50 or 100.

19,892. You think there was actually that enormous mortality among the uninoculated in those particular weeks?—Yes.

19,893. I undertand that the total mortality among the uninoculated was 14 per cent., while some weeks it rose to 65 per cent.?—Yes.

19,894. Can you account for that in any way?—No, I cannot. The only thing is, that plague got into a bad quarter where the people were not inoculated. It got into the Muhammadan quarter and carried them off right and left. The plague seemed to go from north to south, it started near the railway workshops, and went straight down to the south of the town. When it got to the congested quarters it took off more people.

19,895. You think the excessive mortality during those particular weeks may be accounted for by the fact that plague had spread into the densely populated and uninoculated quarters?—Yes, I think so. I cannot put any other construction upon it.

19,896. Do you know, as a matter of fact, of the existence of any such uninoculated areas in Hubli?—Yes, there were one or two uninoculated areas where people were most loth to come up for inoculation, but they gradually came up afterwards. I do not know what time the plague was there.

19,897. Can you suggest any means of finding out whether an excess of mortality occurred in those particular areas in those particular weeks?—The returns would show. I suppose they have got the returns in Hubli showing the number of deaths in each of those wards.

19,898. (*Dr. Ruffer.*) Did I understand you to say that the population of Hubli was 55,000?—When I arrived there I think the population, according to the last census, was 52,000, and I estimated there were about 3,000 more people since the census had been taken.

19,899. (*Mr. Cumine.*) With regard to the principle on which you selected those 69 houses, you said you took them because they were good cases?—Yes.

19,900. Do you mean good cases as showing the protective power of inoculation?—Yes. If I went to a house and saw that none of the people had been inoculated, and a person had died, I would not call that a good case. I had lots of cases where there was one person inoculated and four or five uninoculated in the same house, and an uninoculated person died there. I did not think so much of that case as I did where 10 persons were inoculated and the 11th was uninoculated, and the uninoculated person died.

19,901. Did you enter in this statement any cases where, there being both inoculated and uninoculated in a house, it was an inoculated person who was attacked, and all the uninoculated persons escaped?—All the time I was there I got three cases where every person in the house had been inoculated and one of them got the plague.

19,902. That is not my question. Have you got in this statement any house in which, there being both inoculated and uninoculated people, all the uninoculated escaped, and one or more of the inoculated people were attacked?—No.

19,903. Can you say that no such houses existed?—None came to my notice.

19,904. It is not the case that you rejected such cases and entered only those favourable to inoculation?—No.

19,905. When you had prepared this statement, who compared the names in it with the names in the inoculated register in the possession of the Inoculating Officer?—Dr. Leumann, I presume.

19,906. You did not?—I did not. Is imply sent in the cases with the number of the pass and the date on which the man was inoculated. They were sent to Dr. Leumann, and he selected them out of about 200 cases which I sent.

19,907. When a man did not produce a pass, did you assume he was uninoculated?—Yes.

19,908. Did you ever find cases where it was declared that a man had been inoculated, but where he had no pass to produce?—Very seldom. The people looked after the passes far too well.

19,909. When you did find such a case, did you return it as inoculated or uninoculated?—Uninoculated, if I did not see any pass. If they did not show me a pass I presumed they had not been inoculated, and I sent them either to the health camp or the segregation camp.

19,910. You had no extract from the inoculation register which is in the possession of the Inoculating Officer?—No.

19,911. You cannot tell us whether the entry in the inoculation register was so full as to enable you to identify the person entered?—No.

19,912. You cannot tell us whether it was a full entry or not, whether it gave the man's name and stated that he lived in such and such a house in such and such a street?—I cannot tell you that.

(Witness withdrew.)

Dr. W. Venis.

Dr. W. Venis called and examined.

19,913. (*The President.*) You are Divisional Health Officer of Bombay?—Yes.

19,914. Of what division?—No. 3.

19,915. What wards?—Byculla, Mazagaon, Tarwadi, Tardeo, Kamatipura, and 1st and 2nd Nagpada Sections.

19,916. (*Prof. Wright.*) You have had experience of corpse inspection in Poona?—Yes.

19,917. Is corpse inspection done in the houses after death?—Yes.

19,918. Was it never done at the cemeteries or burning ghats?—No.

19,919. Do you think it is better to do it in the house than at the cemetery?—Yes, because if it turns out to be a case of plague you know where the case occurred, as a rule.

19,920. Do the people object more to your doing it in the houses?—No, they do not object much in Poona.

19,921. Supposing the body was already dressed and prepared for burial when you arrived, was there any objection to allowing you to examine it?—In my own experience there were only objections on two, or, perhaps, three occasions.

19,922. Were you able to detect any other cases of plague besides the bubonic form by means of this corpse inspection?—In some cases we had a suspicion of pneumonic plague, but, of course, we could not be absolutely certain.

19,923. Do you think the system of discovering plague by corpse inspection is the best which can be adopted?—I think it is the best practicable means. It would be better if you could have all the cases notified and see them during life, but that is quite impossible.

19,924. Do you think that system would be applicable to a town like Bombay?—I should think probably not from what I have seen of Bombay.

19,925. What difference is there?—They are a different class of people; they are not so likely to be amenable to argument.

19,926. What do your duties consist of at present? Do you find out plague cases?—I have nothing to do with plague.

19,927. You are employed in the Health Department?—Yes.

19,928. Your only experience of plague is at Poona?—Yes.

19,929. Does your evidence bear upon anything else but evacuation and corpse inspection?—Simply corpse inspection and notification of illness. I was in charge of two of the camps at one time.

19,930. (*Dr. Ruffer.*) Were you ever able to examine the sick at Poona before they were removed from the house?—Yes.

19,931. How did you induce the people to let you see them?—There was no inducement required; they used to come and report the sick and the medical men used to report them.

19,932. Had they any objection to your seeing women?—No.

19,933. Did the Musalmans object at all?—Very rarely.

19,934. In Poona, did you always examine corpses in the houses or sometimes in the cemetery?—Never at the cemetery. The body could not be removed from the house until we saw it.

19,935. Do you think they removed the body from one house to another?—I think they did occasionally from one room to another, but we used to disinfect the whole house, and it did not matter.

19,936. Did you disinfect the people?—They were sent to the segregation camp and detained there for ten days. On arrival in camp they were given a bath, and their clothes were disinfected. This routine was observed while plague prevailed in epidemic form. Later on close segregation was abolished and the people simply slept in the camp at night and went about their work during the day, but the disinfection of the persons and clothing of contacts continued as before.

19,937. Did they have mercurial baths?—Yes, perchloride of mercury. I forget the exact strength.

19,938. Did you ever get a case of mercurial poisoning?—Never.

19,939. Did you ever see a case of mercurial poisoning among the disinfectors?—Never.

(Witness withdrew.)

Major T. E. Dyson, I.M.S., called and examined.

19,940. (*The President.*) Where did you acquire your experience of plague?—I was first of all in Bulsar in the early months of 1897; afterwards, at the latter end of the year and the early part of 1898 I was in Surat.

19,941. What do you think is the manner in which plague is spread?—I think it is spread by human agency from place to place. In Bulsar plague was undoubtedly introduced from Bombay, and I think in Surat also, but I cannot speak positively about that. There are many people in Bulsar who have dealings in Bombay, and have houses in Bombay; they fled from Bombay in thousands, and the first cases occurred among them.

19,942. Having been imported, how did it spread, do you think?—I think it spread by human agency again and by rats into different parts of the town.

19,943. Have you any distinct evidence of its being spread by rats?—I have no distinct evidence that the rats died of plague; they were never examined, but there was an epidemic among rats as there was among human beings. There is a village close to Bulsar, where the first evidence of the existence of plague, preliminary to the outbreak among the people, was the death of rats in a particular quarter, while the people were not affected until afterwards. They promptly turned out of their houses and went into the fields to live, and developed plague a week afterwards among themselves.

19,944. Could they not have acquired plague by being infected by persons suffering plague?—They might have got it from Bulsar itself.

19,945. From human subjects?—Yes.

19,946. You have no more distinct evidence of the conveyance by rats?—No, not of the actual conveyance, but simply that in Bulsar itself large numbers of dead rats were found in quarters before plague appeared among the people, and also concurrently with cases among human beings.

19,947. When you got an imported case what steps did you take?—Unfortunately no steps were taken at all. The imported cases ran through their course to death or recovery before they were found out in the first instance. When they were detected they were removed to hospital, and at that time nothing was done to the rest of the inhabitants of the place.

19,948. Have you any opinion as to the length of time which elapses between the entrance of infection and the outbreak of an epidemic?—As far as I can judge in Bulsar it must have been six weeks, it may have been eight weeks. Of course, it is very difficult to know exactly. We have to take the time from the time when the cases were detected, or at any rate when there was an appreciable increase in the mortality. That occurred in Bulsar about the end of January.

19,949. But in the meantime, of course, plague cases may have been occurring?—Yes.

19,950. With regard to the treatment of an infected area, what did you find was the most successful measure?—I tried in Bulsar first of all cleaning and disinfection with lime. Afterwards, through want of labour, we tried the perchloride of mercury solution as being simpler and easier of application. In the two areas I have quoted it seemed to have distinctly good effects.

19,951. I believe you did something more than mere disinfection?—Yes, that was later. One particular quarter of the town was evacuated.

19,952. What measure do you regard as successful in dealing with an infected area?—I think evacuation and disinfection.

19,953. Have you had any experience of evacuation without disinfection by chemical disinfectants?—Without any disinfection at all? In some of the villages there was very little done more than opening up the houses, removing the tiles, and letting a free percolation of air through them.

19,954. And after evacuation?—I do not know any instance in which the people were allowed to enter their houses again within a month, very often two or three months, and after the plague had ceased among them out in their sheds.

19,955. What followed from their return?—They had no more plague.

19,956. That is to say without chemical disinfection, and simply by opening up the houses you found those houses did not again infect people when they returned?—That is so, after a lengthened stay away from the houses.

19,957. Have you any experience of disinfection after very brief evacuation—say a week or a few days?—In Bulsar, in two of the quarters there, I allowed the people to re-occupy their houses within a fortnight, I think.

19,958. No briefer period?—No.

19,959. That was in the case of houses which had been disinfected by chemical agency, I suppose?—Yes.

19,960. What was the result?—They had no more plague after the fortnight.

19,961. What is your opinion as to the infectiveness of plague in its different forms?—The most common form—the bubonic form—does not seem to be very infectious. We had numerous instances of Hospital Assistants, and particularly children having to be taken in with their sick mothers, and they never were affected by plague afterwards.

19,962. What is your experience with regard to corpse bearers?—From some statistics I collected in Surat

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there does not seem to be any special danger attached to corpse bearing.

19,963. How many people were you dealing with?—I was dealing with nearly 400. In only two or three instances was there any suspicion that the plague might have been contracted in that way.

9,964. You think that the bubonic form is not very infectious?—Yes.

19,965. How do you account for the occurrence of a considerable number of cases of bubonic plague in one house?—I should say they contract the infection from the original source. The house is infected and the germs enter through the abrasions of the skin.

19,966. What is your experience with regard to the infectiveness of pneumonic plague?—I have had no experience at all of the pneumonic form.

19,967. You saw no cases?—I could not say whether the cases I have seen were primary pneumonia or secondary pneumonia following on plague.

19,968. You have no clear cases?—I have no clear evidence at all.

19,969. Have you any experience of curative sera?—None whatever.

19,970. Have you any experience of Haffkine's prophylactic fluid?—Yes, I had some experience, but it was not entirely my own. All the figures I have obtained were got from the Assistant-Surgeon in Bulsar.

19,971. They are not your own?—No, they are obtained from him.

19,972. Can you give us those figures?—Yes. In an isolated corner of the town there were 1,018 people inoculated, and I have the figures of a portion of those people, some Muhammadans who had plague very badly. There were 539 of these Muhammadan weavers, of whom 490 were inoculated, and of the remaining 49, 20 were not inoculated because they had had plague in the previous year, three were not inoculated because they were sick with plague at the time, and 17 were not inoculated because they were children under two years of age; nine persons escaped inoculation of whom we have no record. After the inoculation of these 490 persons 64 cases of plague occurred with 23 deaths.

19,973. Do you know the intervals which elapsed between inoculation and the occurrence of plague?—I can only give the weeks and the number of cases.

19,974. Do you know how many deaths occurred among them?—No, I do not.

During	1st	week	after	inoculation	there were	12 cases.
,,	2nd	,,	,,	,,	12	,,
,,	3rd	,,	,,	,,	15	,,
,,	4th	,,	,,	,,	11	,,
,,	5th	,,	,,	,,	8	,,
,,	6th	,,	,,	,,	3	,,
,,	7th	,,	,,	,,	3	,,
					64	

19,975. Were these inoculated persons left in the village or town?—They were left in their houses with their sick.

19,976. Previously to inoculation, had many cases of plague occurred there?—In an adjoining quarter there were some fishermen living, and there were about 20 or 30 cases among them.

19,977. Not in this actual quarter?—Not in this actual quarter.

19,978. Was any measure taken in the adjoining quarter?—The whole of the fisher people were inoculated as well as these Muhammadans, and some Muhammadans in the adjoining quarter, but I only have reliable figures about these particular Muhammadans which I have mentioned. Among the Muhammadans inoculated in the adjoining quarter who had not plague at the time no epidemic ever broke out.

19,979. (*Dr. Ruffer.*) How many times did you inoculate the Muhammadans?—Only once.

19,980. Did they all re-act?—I do not think they did. I did not do it, and I did not actually see it done.

19,981. Do you know the actual number of plague cases which occurred among those who had had plague before?—I know of one instance. A boy who had had plague the previous year got plague again the second year.

19,982. Then you stated you had 17 children under two years of age; do you know whether any of them got plague?—I am afraid I cannot say among whom the cases in the people left out occurred, whether they occurred among the people who had had plague the year before or among the children or those who escaped inoculation.

19,983. But you know of one case of a boy getting plague twice?—Yes.

19,984. (*Prof. Wright.*) Did you see any of these cases which occurred among the inoculated?—Yes.

19,985. You thought they were much milder than the cases among the others, did you not?—The mortality was considerably different. Some of the cases were very severe, but I think, on the whole, the epidemic was milder; the mortality was only half what it had been the previous year.

19,986. You state, in your précis, that other people refused to be inoculated?—In the rest of the town.

19,987. Because there was still plague?—Yes, among those who had been inoculated.

19,988. Were the symptoms in the inoculated persons so severe as to frighten the people?—No; I think the simple reason was that the inoculated people still continued to have plague among them, although they had been inoculated, and the other people did not see at the time there was any benefit to be derived from inoculation.

19,989. Do you think the inoculated were the same class of people as in the other quarters of the town?—There is another quarter containing exactly the same class of people, and they had plague afterwards. Their mortality was very much the same as in the previous year, over 70 per cent. They were exactly the same class, Muhammadan weavers, only they lived in a different part of the town.

19,990. 70 per cent. was the percentage of mortality among those attacked?—Yes.

19,991. Do you know what the percentage of attacks was among them?—Their numbers were altogether 492, and they had 65 cases with 47 deaths.

19,992. (*Dr. Ruffer.*) Are the two quarters about the same?—Very much the same. In one quarter there were 151 houses, and in the other 141 houses.

19,993. (*The President.*) Were they similar houses?—Yes, exactly similar, and a similar class of people. The population would be almost exactly the same.

19,994. (*Prof. Wright.*) It works out that there is about the same percentage of attacks among the uninoculated persons, but a greater mortality than among the inoculated?—Yes; the number of attacks among the uninoculated was about the same, but the mortality was greater.

(Witness withdrew.)

(Adjourned till to-morrow.)

At The Secretariat, Bombay.

FIFTY-SECOND DAY.

Tuesday, 21st February 1899.

PRESENT:

PROF. T. R. FRASER, M.D., LL.D., F.R.S. (*President*).

Mr. J. P. HEWETT.
Prof. A. E. WRIGHT, M.D.
Mr. A. CUMINE.
Dr. M. A. RUFFER.

Mr. C. J. HALLIFAX (*Secretary*).

Major M. A. T. COLLIE, I.M.S., called and examined.

Major M. A. T. Collie, I.M.S.

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19,995. (*The President.*) You are in the Indian Medical Service?—Yes.

19,996. And Superintendent of Matheran?—Yes. I have control of the station there, and complete control of plague matters.

19,997. (*Dr Ruffer.*) Will you tell us what has been your experience of plague?—My first experience in the first epidemic was chiefly administrative, for the Ratnagiri Collectorate. We simply tried to protect the district as far as possible from the introduction of plague by having passengers examined at the chief ports of call.

19,998. Where is that district?—It extends along the coast from Bankote Creek down to Vingorla.

19,999. How large is the district?—I forget how many miles it is.

20,000. Is that all the experience you have had?—No, after that I came up from Bombay, and I was placed in charge of the D. and E. wards in Byculla, in addition to my other work; and during that time I was Medical Officer of the Umarkhadi Jail.

20,001. I believe you say that there is difficulty in diagnosing certain cases of plague. Could you tell us what is the difficulty in diagnosis?—At first we were led to believe that the most stereotyped thing was simply an enlarged gland, and from that that there was no difficulty in diagnosing any case of plague. At first, however, I found extreme difficulty in diagnosing any case of plague at all. Even where we made *post mortem* examination of cases of enlarged gland, we could find none of the local signs of plague, even microscopically. If a case of sudden death was reported, or even if we saw people 48 hours before death, there was great difficulty in the diagnosis.

20,002. What was the chief difficulty in the diagnosis?—They appeared to be only suffering with fever.

20,003. Had the majority of cases an enlarged gland?—Not unless I saw them during life.

20,004. You saw a certain number of cases of fever without enlarged glands, which you diagnosed as plague?—Yes, after death.

20,005. Why did you diagnose them as plague?—Simply from the microscopical appearance.

20,006. Did you make a bacteriological examination?—I could not carry out the bacteriological examination, but I made a microscopical examination of every case I could.

20,007. In how many of the cases in your district did you diagnose the case after death?—In Hubli every case was diagnosed after death. I did not see more than three or four cases before death.

20,008. Were you in active service there, looking for plague cases?—Yes.

20,009. You think at the beginning most of the cases in Hubli were non-bubonic cases?—I cannot say that, for I did not see them till after death, except in about four cases.

20,010. Were most of the cases which you saw after death non-bubonic cases?—I could not find any bubo. When we came to look for the swollen glands that I saw during life, at the *post mortem* examination, I could not find them.

20,011. They had disappeared?—Yes.

20,012. Is that frequently the case?—I have not had sufficiently large experience to say.

20,013. How many *post mortems* did you make altogether?—During the time I was at Hubli I had 14 deaths, and I *post mortemed* every one.

20,014. How many had buboes?—I cannot give you that information. There was one case, for example, of a child who had several enlarged glands over the body, and I only found the ordinary *post mortem* plague appearances in one small gland of the neck, although the child had enlarged glands in the groin and the axilla, and there was none of that effusion of blood round the glands as we usually find, except in the cervical gland.

20,015. Was that a rapid case?—Yes, as far as we could gather.

20,016. Did you see any other cases like that?—I saw one woman which I had diagnosed as plague beforehand. She had an enlarged gland round the angle of the jaw, and she died within a few hours, but I could not find the glands after death.

20,017. It is your opinion that if you find a dead body in the street, unless you make a microscopical or a bacteriological examination, you cannot diagnose plague with certainty?—Certainly not.

20,018. In cases of plague pneumonia, did you find anything special after death?—No.

20,019. Did you see any cases of plague pneumonia at Hubli?—I got three cases there which showed *post mortem* signs, with ordinary complications.

20,020. Primary plague pneumonia?—I did not see them in life. In the obstetric hospital where I have been in charge for several months, I have seen cases come in there just moribund. They were brought in in a moribund condition, and as far as I could gather they had symptoms of plague.

20,021. Did you ever notice œdema of the front wall of the chest in cases of plague pneumonia?—No.

20,022. In the cases of plague without buboes, did you examine the other organs of the body for plague bacilli?—Always in the spleen or liver.

20,023. Did you find them?—If I did not find the glands, I found them in the spleen or liver before I gave my diagnosis.

20,024. Do you think there is much difficulty in diagnosing plague during life?—I think so.

20,025. Why?—It is very difficult to diagnose in many cases.

20,026. Why do you say in many cases?—I would not be prepared to swear to any case that I saw.

20,027. Unless you saw enlarged glands?—Or even then.

20,028. Why not?—Take the case which came up to Matheran, which is a small hill station four hours

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from Bombay. He is the son of a Parsee gentleman. He travels first class. He was seized with rigors, and his temperature rose to 105. He gives me a history of chronic eczema of six years' standing. He had an enlargement of the femoral and inguinal glands in both groins, one was especially large, but he is convalescent now.

20,029. Is that a case of plague?—I cannot say, but I believe it to be, because he came from a house where dead rats were found and because another person was taken from that house to the Parsee Plague Hospital and died. I telegraphed down, as soon as the case came in, for pipettes and culture tubes, &c., but before they arrived he was convalescent, and Dr. Galeotti had injected 15 c.c. of Lustig's serum.

20,030. I suppose the diagnosis is difficult also in septicæmic cases?—Yes.

20,031. Do you think it is possible to diagnose a septicæmic case of plague in many cases, unless a bacteriological or a microscopical examination is made?—I think not.

20,032. Do you think the ordinary person who is in charge of a plague ward, who is not a medical man, would be able to diagnose a case of septicæmic or pnuemonic plague either during life or after death?—The first few cases I sent down from the Umarkhadi prison, I was very doubtful about. They were the first cases that occurred, and I think the first man had been 56 days in prison. I got Dr. Leon, who was working in my district, to come and take a little serum from the gland, and we sent the case down as one of plague. I sent the man to the Plague Hospital, and went round the next morning, and was told that he had had the glands for several years, and that it was not a case of plague at all, and I assumed that the patient was better. I asked how he was, but he was dead. This was the Arthur Road Hospital. We got our culture tubes there, we got characteristic cultures, and had microscopical examination in life, and yet it was pronounced by one of the staff of the Plague Hospital not to be plague.

20,033. Have you had recently any hospital experience of plague?—Not recently.

20,034. You have been to Plague Hospitals?—Yes.

20,035. Have you got any data to show that other patients besides plague are admitted in Plague Hospitals?—Certainly.

20,036. Could you give us examples?—When I had charged of the Narelwadi segration camp, people came down from Cutch. It was in October or November. One man had an enlarged gland which, on careful enquiry, I found had existed probably since childhood. After satisfying myself that it was not plague, but simply intermittent fever he was suffering from, I discharged him from the segregation camp after keeping him five days under observation. Within three hours one of the spies, or whatever they were, who were employed by the Plague Commission, seized this man while he was eating his food, and sent him to the Plague Hospital.

20,037. Had he fever at the time?—No.

20,038. The diagnosis was based simply on the enlarged gland?—Yes. I traced him to the Plague Hospital and turned up the case. He was treated entirely as a case of *pestis ambulans*. The man went to work within a day or so after his discharge from the hospital, after being there for only four days. He had no plague.

20,039. Have you seen cases of relapsing fever in Plague Hospitals?—I have seen cases of relapsing fever, but not in hospitals.

20,040. Have you seen any cases of malarious enlargements of glands admitted as plague patients?—Only that one that I mentioned just now, but not any others to my knowledge.

20,041. Have you ever seen cases of syphilitic glands or gonorrheal glands admitted as plague?—I have not seen them; I have heard of them. I have no personal experience.

20,042. I believe you are a strong advocate of "the "importance of quarantine versus surveillance and "inspection as examplified in the occurrence of plague "throughout the Ratnagiri Collectorate in the first "year's epidemic." Can you give us any evidence as to why you prefer quarantine to surveillance and inspection?—It is based entirely on the fact that unless you can observe a person for 10 or 12 days you do not know whether he is in the incubation stage or not. He passes up-country, and in the Ratnagiri Collectorate there were 300 odd cases of plague and nearly half of them were indigenous.

20,043. How could you establish a perfect system of quarantine in the Ratnagiri Collectorate, for instance?—Only by allowing the people to land at certain ports.

20,044. Could you prevent them landing in other places?—If the authorities in Bombay Harbour only give clearances to specified ports. I think it is one of the districts that could be most easily managed. It is entirely a coastal service, and people get down by steamer or by native craft, and there are very few roads from the other side of the Ghats.

20,045. Cannot they go down by land?—It is very inaccessible; all travel by sea.

20,046. Supposing they knew that the sea route was closed, would not they come by land?—The journey is so long that they would not do it. It practically embraces quarantine. The nearest station by land is Kolhapur, which is 40 miles from Ratnagiri frontier.

20,047. That is a day's journey?—You will not do it in a day.

20,048. Say two days?—They have to go up and down the Ghats.

20,049. I want to know how you can protect a large tract of country like a Collectorate. How many men would you want to protect a Collectorate by quarantine?—The proposal was that native craft should only get clearing papers for certain ports.

20,050. What prevents them from going into other ports?—The Customs authorities along the coast.

20,051. I suppose there is a certain amount of smuggling there like everything else. Why could they not smuggle plague cases?—They might do it. They might land them on the shore.

20,052. Supposing you had quarantine, do you think the people would try to avoid the quarantine measures?—I think they probably would.

20,053. You say you have seen "the occurrence of cases in quarantine camps up to eight days, and in one case 12 days"?—Yes.

20,054. Were the people disinfected before they entered quarantine camp?—Yes.

20,055. Were there any other cases of plague in the quarantine camp at the time?—One was in what you might call an "observation camp" where the people had been taken from an infected area. The other case of 12 days was a case that came up from Goa and was put into a quarantine camp because we did not know the condition of Goa at the time.

20,056. Did you disinfect the clothing?—Yes.

20,057. Did you disinfect every person's clothing?—Every person's clothing was disinfected by the steam steriliser.

20,058. There have been no cases of plague in the camp?—No.

20,059. You advocate "early removal of people from "infected areas with liberty to carry on their daily "avocations out of the health camps"?—Yes.

20,060. You also say there are "difficulties in pronouncing when an area or town may be declared free, as exemplified in the case of Hubli"?—Yes. There was a very careful census taken there, and a very careful system of inspection of houses &c., and as far as we could find out no cases occurred except at intervals. First there was the 9th and 10th of January, then you pass on to the 18th, which is a doubtful case. It is put in as a case, but it was not recognised as occurring until the 30th, that is 20 days.

20,061. May not that case have got plague somewhere else and not in Hubli?—No, we are certain about that.

20,062. Why?—Because he was not out of Hubli.

20,063. Supposing a case of plague had come to Hubli and died, could you exclude any possibility of Hubli being infected?—He was known by the census; he was in the roll-call every morning.

20,064. But supposing a man had come from outside to Hubli, and died from septicæmic plague, which is difficult to recognise, could you exclude that possibility?—Except that all the roads were controlled. Of course, it is impossible to say that they did not get there; you cannot exclude it.

20,065. Can you give us any examples of plague being introduced into a community by means of clothing?—Yes.

20,066. Would you give us the details?—The first case that came to my notice was at a village of Ladghar, which is about eight miles from Hurnai in the Ratnagiri collectorate. A Bhandari, one of the fisher class, had lost his wife, and he came to Bombay and took down her jewels and her clothing. Within a few days after his arrival in the village dead rats were first observed, and then the first person to suffer was an aunt who lived in the same house, and then a nephew, and then the son, and finally the Bhandari himself who had brought the infected clothing. He died, and altogether in that village we had 47 attacks and 39 deaths.

20,067. How long after the clothing was brought down was this first attack?—I had to inquire from a Hospital Assistant, who was on duty there, and he says in an indefinite way that dead rats were first observed two or three days after the man came down from Bombay.

20,068. Is there no possibility of plague having been introduced into that village in some other way?—They are all people who live by fishing and toddy drawing and no one had left the place except this man.

20,069. Can you tell me what the total mortality of that village had been in the preceding weeks?—I cannot.

20,070. Was the total mortality of the village raised?—There was no report of it. Inquiries were made in all these cases, and there was no unusual mortality.

20,071. Rats were found two or three days afterwards?—Yes.

20,072. The clothing must have infected an enormous number of rats very quickly?—I cannot tell you that.

20,073. I can understand a great many rats being found dead a week afterwards, but it is difficult to believe it was found within three days?—You know what a native's statement is; it is possible that plague had existed for some time before we had intimation of it. A native is always very indefinite with regard to his time statements.

20,074. Have you any other evidence of plague being carried by clothing?—At a village called Guhagar, down the coast, a boy who had recovered from plague was brought down by his friends, and stayed at an aunt's house. They had a feast of some kind there, and it was said that he brought down the clothing with which he was discharged from the hospital. Ten days afterwards his aunt sickened with plague and died and then one of her children.

20,075. There was no plague in that village?—No.

20,076. Did not any rats die?—No. There was another case at Hubli. The whole of the infected area had been cleared out, and this man had been 20 days up in the health camp. The whole of this area was under the charge of the police. He had heard that I wanted to destroy his house, which was next to the first infected house in that area, and he went back to his hut to remove some clothing from there, and within 11 days from that occurrence he sickened from plague in the health camp and died. He had been 20 days altogether (supposed to be) in the health camp.

20,077. There was no other case of plague in that health camp?—No, the whole of that had been cleared out.

20,078. Were the health camps under the observation of medical men?—Yes.

20,079. Every day?—I went up every day; there was a roll-call every evening and morning.

20,080. Had the clothing of the people going into that health camp been disinfected?—Yes, all through the steam steriliser, as far as I could say.

20,081. Are you certain of that?—Officers were appointed to see that it was done.

20,082. Who are the officers?—Lieutenant Keelan had charge of the disinfection, and there was a Hospital Assistant who would not allow things into the camp unless a chit was given stating that the things were disinfected.

20,083. Have you any other examples of plague being communicated by clothing?—No.

20,084. You say you have "examples of plague being "introduced into a community by means of clothing, "and of a man contracting plague by surreptitiously "revisiting a cleared area"?—Yes. This man, as far as I could gather from his wife, had removed a pagri from that place, and whether it was from the pagri or not I cannot say.

20,085. You think it is important that railway authorities should look after their own employés?—Certainly.

20,086. So as to prevent the spread of plague from one district to another, because they travel through large tracts of country?—Yes. As an example, I may say that a railway guard was found at Hubli station travelling from Belgaum to Bezwada. His father and his brother and his sister-in-law had all died of plague, which was not diagnosed as plague, at Belgaum. At the railway station at Hubli he removed his wife and his mother and they died of plague in a few hours. This man travelled down from Belgaum with a pass to Bezwada.

20,087. Did he himself suffer from plague?—No, and the children did not suffer.

20,088. How did the disease reach his mother and wife?—His mother and wife died in the railway quarters.

20,089. You believe in "the importance and necessity "for the medical officer being alone the organiser and "director of all plague measures." Can you tell us what you base that on?—The whole training of the medical officer is to deal with disease. He knows the disease and he knows how best to deal with it, and he knows the people. At the time I was working here in Bombay I was constantly interfered with; for example, I got a hard and fast rule laid down to me that if persons had a temperature of 102 degrees in the segregation camp, I was to send them to the Plague Hospital, whether I thought they had plague or not. I do not think that is an order that should be issued to a qualified person.

20,090. You think that was a mischievous rule?—Most certainly. In many cases, if I had acted upon it, I should have sent malarial fever there; but I never did act upon it.

20,091. Can you tell us any other rules that you consider unnecessary?—When they first introduced the order as to clearing out persons from areas in which dead rats were found, I regarded it as a most rational thing to clear the whole of that area out. I cleared out from one chawl near Bombay about 80 people, and I got an order the next morning that I was not to do these things in such a wholesale manner; that I should simply take the room in which the dead rats were found, and the two neighbouring rooms.

20,092. Where were the two neighbouring rooms?—One on each side of the room on which the dead rats were found.

20,093. Do you know the reasons why they said the two neighbouring rooms?—I cannot give you the reason.

20,094. Can you give us any other instances?—I think the medical officer should be allowed to control all these things himself.

20,095. And you think, probably, that the difficulty of diagnosing cases of plague is also in favour of your contention that the medical officer should be in charge?—Certainly.

20,096. (*Mr. Hewett.*) Were you at Hubli when the census was taken there?—No.

20,097. Who took it?—Dr. Meyer.

20,098. You refer to a case in January 1898, which took place 23 days after the one immediately before it. Do you attribute the infection in this instance to the man having got infected in a house which contained the poison?—After careful inquiry, I found that he went back there with his wife.

20,099. Is not that the man that went back from the segregation camp?—Yes, the same man.

20,100. Is that how you think he got infected?—I believe he stayed there all that night. The evidence is that he was drunk that night, and he must have slept there.

20,101. Raga Appaji is the man?—Yes.

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20,102. (*Mr. Cumine.*) You spoke of a Bhandari boy. Are they fisher people, or toddy tappers?—I think they do both.

20,103. What was the interval that elapsed between the arrival of the boy and your hearing of the death of the woman?—We heard of the death of the woman about a week after her death. We had a system of weekly reports.

20,104. Who brought the boy down from Bombay? Did some friends come with him, and deposit him and go away again?—Some Bhandaris came with him, and they went up the coast, and came back again. They were owners of a native craft, and they brought him down from Bombay and deposited him at his aunt's house, and went away.

20,105. Can you exclude the possibility of their having plague in their bodies when they deposited him and went away?—No, I cannot exclude that.

20,106. Is not there an observation ward for doubtful cases attached to each of the public hospitals in Bombay?—I cannot say.

20,107. As regards the man whom you released from the Narelwadi camp, and who was sent to the hospital, can you say for certain whether he was put into the plague ward, or into the observation ward?—I was told that he was put into the plague ward, and that he was treated as a case of *pestis ambulans*. I went to the hospital and saw the charts and notes of how he was treated.

20,108. What did you think it was at first?—I had no doubt about it being malaria, and I kept him in the Narelwadi observation camp. He had fever one day, and then an intermission, and then he had another attack of fever. I discharged him as soon as I was satisfied that it was malaria.

20,109. When a district is very heavily infected like Satara, with many infected villages, how could a medical officer manage everything there himself?—He must be the organiser; he cannot be everywhere himself. He must be the administrator in all these things, and, as far as possible, he should have skilled Hospital Assistants—the same as in cholera; you cannot be in every village when cholera breaks out.

20,110. I think you said that medical men are not invariably able to diagnose a case as to whether it is plague or not?—I think it is very difficult.

20,111. What are the other respects in which you think they are superior to, say the Revenue Officers or the Police Superintendents?—Their whole medical training is to deal with disease; they are not likely to act rashly, like a person belonging to the laity, who is not accustomed to deal with it.

20,112. I think you said there was a man from Goa who developed plague in the quarantine camp after 12 days?—Yes.

20,113. Were there any plague cases then in Goa?—We could not find out. I do not know whether anybody knows yet.

20,114. Has plague ever been officially declared in Goa?—No. I heard there was a great deal of sickness and large mortality. The Goanese told me that themselves.

20,115. (*Prof. Wright.*) Were you in charge of the Umarkhadi Jail when the inoculations were done in January last year?—Yes.

20,116. How long did you continue in charge of the jail?—I gave up the appointment on the 4th January, but I had been in charge of it for three months.

20,117. The inoculations were done about the 1st?—Yes.

20,118. You have no personal knowledge of what happened afterwards?—No.

20,119. Did you supervise the selection of the candidates for inoculation?—I was present there. A bell was rung, and the prisoners marched in. They were mustered in a row, and there was no selection made.

20,120. Were all the prisoners allowed to be inoculated if they so desired?—They were told they were going to be inoculated, and that if any prisoner objected very strongly he would not be inoculated.

20,121. Was every second man inoculated?—Every second man, unless they had fever, or were very old and debilitated, or objected. I think there were not two objectors in the whole lot.

20,122. I understand that some of the prisoners worked at outdoor, and some at indoor occupations?—No, not in Umarkhadi.

20,123. Were they all engaged on indoor occupations?—All indoor.

20,124. Were they all employed in the same occupations?—Not all the same. I had not charge of the employment of prisoners, but I know that some do grinding, some do the teasing of oakum, some treadmill, and some the making of mats, and such like.

20,125. Were the occupations of the inoculated different to the occupations of the uninoculated?—No. There was no distinction; the occupations are changed very frequently, so as not to allow them to mingle too much together.

20,126. Did you see the febrile reactions that followed upon the inoculation?—I took the temperature in every case.

20,127. Did you find any very high temperatures after inoculation?—I think the average temperature was 101 and 102; some went up to 104.

20,128. Are there any records of these temperatures?—Yes, there must be on the files in the hospital.

20,129. Did you take the temperature of every person inoculated?—Every temperature.

20,130. Did you find much difference in the reactions?—Yes, some had very little reaction, and some very strong reaction.

20,131. Where are these records to be got?—They should be on the files of the hospital. I told the Hospital Assistant to keep them, but he is changed now, and I do not know whether they would be accessible now.

20,132. (*Dr. Ruffer.*) Do you think plague is a more easily diagnosed disease than any other acute infectious disease?—Do you mean small-pox or measles?

20,133. Yes, or any other?—I consider it is far more difficult to diagnose plague.

20,134. (*The President.*) If it be more difficult to diagnose, is it not your opinion that the work of diagnosing should be done by trained diagnosers, such as medical men?—Yes.

20,135. (*Dr. Ruffer.*) You think most men now negaged in plague work are absolutely incapable of diagnosing a case of septicæmic plague?—I think so.

(Witness withdrew.)

Capt. C. H. L Meyer, I.M.S.

Captain C. H. L. Meyer, I.M.S., called and examined.

20,136. (*The President.*) You are a Doctor of Medicine, I believe?—Yes.

20,137. I think you can give us some information with regard to the first outbreak of plague at Hubli?—Yes.

20,138. (*Mr. Hewett.*) I believe you were only employed during the first epidemic at Hubli?—Yes.

20,139. That was a very small one?—Yes.

20,140. Was the first case in a railway chawl?—Yes.

20,141. Did you trace how the infection came there? As far as we could trace it, it seems to have been brought from Poona by railway employés. Some railway employés had gone on leave to Poona, and they brought the infection with them on their return.

20,142. When we were there a short time ago it seemed to be possible that the first infection was due to the arrival of a family from Sholapur: do you think that that is likely?—I investigated that. I did not put any reliance upon it.

20,143. You think, then, that railway employés brought it from Poona?—Yes.

20,144. Was the railway chawl at that time in an insanitary condition?—Only, or chiefly, as regards ventilation. The ventilation was extremely bad. The chawls were placed together, and the windows were small. The weather was cold, and the houses were

shut in by verandahs. The ventilation, both external and internal, was very bad; it was very deficient indeed. The other sanitary conditions were fairly good.

20,145. How many cases occurred in the railway chawls?—35 altogether.

20,146. How long after the detection of the first case did you evacuate the chawl?—The first case occurred about October the 10th; I did not arrive there until the 27th, and we commenced to evacuate about October three or four days later.

20,147. How long did it take to complete the evacuation?—Four or five days.

20,148. At what time was the cordon placed around the railway chawl?—On the 23rd of October.

20,149. Did you find the cordon efficient?—We had a large force of police. They were native police, however, and we could not thoroughly trust them.

20,150. Do you remember how many police you had?—I think we had about 60 on duty constantly.

20,151. Notwithstanding that, people came into the chawls and got out of the chawls, did they not?—The way plague was once carried into Hubli, was this. A friend of a policeman came to see him. This policeman was on the cordon, and his visitor slept a night in the cordon—I really believe in one of the empty chawls. The police were in the habit of going into these empty buildings and sleeping in the verandahs of the chawls: it was very cold. This visitor got infection there, and carried it into the town.

20,152. Did any people escape from the chawls to the town and get infection?—No.

20,153. Did not a Goanese do so?—No; the only case we could trace was the one I have mentioned.

20,154. Did you find that the moving of the inhabitants outside their houses had the effect of stopping the epidemic in the chawls?—Yes.

20,155. How long did it take to do that?—The epidemic in the chawls was practically over by December the 13th.

20,156. That was six weeks after you had taken the people out?—About a month or five weeks after they had all been taken out.

20,157. What was the number you had taken out of the chawls?—About 1,200 people.

20,158. You think it is very important to prevent people from returning to infected houses after evacuation?—Yes; I was on plague duty for a short time at Lanauli, and then also at Hubli, and I found that if you disinfect houses ever so thoroughly, and let people go back again into the houses after a week or a fortnight, they frequently got plague again, as the houses are still dangerous.

20,159. Could you exclude all possibility of infection from outside in this case?—I could not, certainly.

20,160. How long would you keep the people out of their houses?—Until the epidemic was over.

20,161. Have you anything to say as to the various methods of disinfection?—I do not believe very much in the disinfection by solutions; I think it is quite impossible to reach every crevice in a house and thoroughly disinfect it in that way; I would rather trust to taking off the roofs of the houses and letting in the sun and the air. The burning out of the houses is better still.

20,162. What do you mean by burning out the houses?—In the railway chawls at Hubli, I unroofed the houses, and put into each separate chawl a quantity of combustible bamboo material and grass and set that alight. The interior of the chawls was so hot after this treatment that you could not go into them for two or three hours after the fire had burnt out. That method, I think, must be efficient.

20,163. When a disinfecting solution is used, do you think it is necessary that the whole of the roof should be disinfected?—I always thought so.

20,164. You think there is a possibility of infection from the roof?—Yes, rats run about in the roofs.

20,165. There was a scheme started in Hubli, I believe, for detecting plague cases?—Yes. The town was divided up into a number of wards. There were about 12,000 houses in Hubli and we divided the town into 120 parts. And over each lot of 100 houses we put a Supervisor. This man took the census of the houses and he visited them every day. He made reports of any new arrivals at these houses, and also in cases of death and of sickness.

20,166. During your time were many cases of sickness reported by the Supervisor?—Yes.

20,167. Lieut. Keelan who followed you, told us yesterday that the majority of cases in Hubli were reported after death; that would rather show that this system of supervision is not very effective?—It was effective in discovering the plague cases after death, not very effective in discovering them before death.

20,168. Do you know how the town of Hubli became infected in the second epidemic?—No; I could not tell you. I only know after I gave up charge at Hubli on January 9th, there were no further cases of plague until January the 30th. After that there were dropping cases all through February, March, and April, and then the cases gradually increased and the second epidemic was at its height in July.

20,169. Were you in Hubli at the time the Mahratta Gali was burnt?—No; before I left we evacuated the Mahratta Gali, and the houses all around the quarter which we supposed might be infected.

20,170. So far as we could ascertain when we were at Hubli the outbreak seemed to have stopped with the Mahratta Gali, and the second outbreak seemed to have been due to fresh infection; do you think it is likely to be possible?—Yes; fresh infection would be very probable, because infected Belgaum was not very far away, and there were other places not very far away where there was plague. The people were travelling about a great deal, and there was constant danger of infection being imported from these places, especially from the Belgaum district.

20,171. You know nothing about inoculation at Hubli?—No, it was after my time.

20,172. While you were there was the census of the inhabitants taken?—Yes.

20,173. In what month?—In December.

20,174. Was Capt. Leumann there at the time?—No.

20,175. Under whose direction was it taken?—Under the directions of the Collector.

20,176. Was any European employed in connection with the taking of it?—No, I do not think so.

20,177. During your time there, did anything come to your knowledge to make it appear that people had left the town?—No; they did not leave the town much in my time; there was no scare then.

20,178. Were people coming into the town from outside?—They were leaving the town and coming into the town in the usual way. There was no scare or panic—nothing to frighten them.

20,179. Do you think that at the time the census was taken the population would be normal?—Yes, I think it was found to be so by the figures. The normal population would be something like 48,000 or 52,000, I cannot remember which.

20,180. You had nothing to do with taking the census?—No.

20,181. The Supervisors took it themselves?—Yes.

20,182. How many houses was each Supervisor responsible for?—One hundred.

20,183. Do you know whether for the purpose of tabulating the census returns, the Supervisor visited each of these 100 houses on one particular day?—I cannot say that he took his census on one day; it was probably done within a week.

20,184. (*Dr. Ruffer.*) When you speak of efficient disinfection not being sufficient to stop plague in a house, you mean disinfection by heat?—I mean particularly disinfection by solutions. I had an experience of that at Lanauli; I carried it out under my own supervision very thoroughly; in fact, I worked the pumps myself. I found that when I let the people into the houses they got plague.

20,185. What solution did you use?—Perchloride of mercury.

20,186. Acid?—No; I dissolved it with sodium chloride; I used a strength of about 1 in 800.

20,187. Did you use it in pumps?—Yes.

20,188. Did you make the solution yourself?—Yes, so as to make sure it was correct.

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20,189. Did you find that the sublimate dissolved easily?—I made it over night, and left it to stand about 24 hours.

20,190. Did you find that it had dissolved by the next morning?—Yes.

20,191. What temperature did you get in the chawls by lighting the fire and burning the bamboo material?—It was a very high temperature. It was so high that you could not possibly go into the interior of the chawls

20,192. I want to know what you mean by a very high temperature. If the temperature was up to 60 or 70 you could not go into the room?—You could not face the heat. If you went to the door while the fire was still glowing, or even an hour after it had gone out, you could not enter the room.

20,193. Have you any idea what the temperature actually was?—No.

20,194. Do you know what degree of dry heat is necessary to kill the plague bacillus?—I do not know.

20,195. (*Prof. Wright.*) Was perchloride of mercury the only antiseptic you used?—Yes.

20,196. You did not try carbolic acid?—No.

20,197. Can you instance any particular cases which led you to doubt the efficacy of the disinfection?—I could not specify particular cases, but I know from my experience at Lanauli, as I told you just now, that when I had thoroughly disinfected houses there, and let the people return to those houses, after a week, they got plague in those disinfected houses.

20,198. Were all things turned out of the houses when they were disinfected?—Yes.

20,199. Were clothes disinfected?—Yes, everything.

20,200. Did you put the clothes in the solution of perchloride of mercury too?—Yes, and the furniture was all disinfected. Others have had similar experience in Bombay, and it was found that disinfection in that way was not efficient.

20,201. You took every precaution you could think of and disinfected clothes, floors, and roofs?—Yes, the the disinfection was very thorough; we tried to cover every spot. I attended to the disinfection of the houses and the clothing myself.

20,202. Have you any theory by which you can account for the want of success which attended your disinfecting operations?—Except that I think it is impossible when disinfecting the houses, to reach every crevice and corner in the woodwork and the stone work in the houses with disinfectants like that.

20,203. (*The President.*) When you disinfected by sunlight and fresh air and then allowed the people to re-enter, did you have any cases of plague in houses so disinfected?—The people never re-entered; the roofs were taken off and the houses made uninhabitable.

20,204. For what period of time?—In the case of chawls at Hubli they remained so until I left; I do not know whether those chawls have been re-entered or not.

20,205. You have no instances in which, after this method of disinfection, the houses were re-occupied? I do not know any instance.

20,206. You can hardly say whether it is efficacious; you cannot compare it with chemical disinfection?—I think it very important to keep the people out of the houses. After we had disinfected in that way, the doors were screwed up, so that the inhabitants could not possibly enter.

20,207. You have expressed an opinion that certain insanitary conditions were very favourable to the development and spread of plague?—What I meant was bad ventilation; especially want of sufficient air. The houses in Hubli and in the railway chawls were rather remarkable in that way. They were so very dark, even for an Indian city. The windows were very small and usually sealed up. There were a great many verandahs to the houses which prevented the circulation of air. The internal ventilation of the houses was extremely defective. In other respects the sanitary conditions of the houses in Hubli were excellent for an Indian city.

20,208. Were the dwelling-rooms clean?—Yes.

20,209. Was there any overcrowding?—Not greater than in other Indian cities; they are all overcrowded.

20,210. Overcrowding would tend to do what you say was so injurious—tend to vitiate the atmosphere, would it not?—Yes.

20,211. (*Prof. Wright.*) Did you lime-wash the houses after you disinfected them?—Yes, with solutions.

20,212. Do you not think that the application of lime is calculated to counteract the goods effects of the perchloride of mercury?—I did not think so at the time, but I do think so now.

20,213. Are the walls of the houses at Hubli made of mud, or of a mixture of mud and lime?—There is usually a mixture of lime in the walls, in these railway quarters, certainly.

20,214. Do you think that your ill-success in disinfecting might have been accounted for by that fact?—I had no bad success in disinfecting Hubli. At Lanauli I had to deal with mud walls. In Hubli itself, I carried out that disinfection, but I disinfected also by fire. In addition to that, I kept the people out of their houses.

20,215. Then it was your experience at Lanauli, which led you to doubt the efficacy of your disinfecting operations?—Yes.

20,216. What did your disinfecting operation consist of?—I used solution only, and did not disinfect by fire; I also limewashed the places, and I came to the conclusion from the experience I had of it in Bombay, that such a method was not reliable.

20,217. Do you think that the giving off of ammonia from cow-dung floors is a circumstance which might vitiate your disinfecting operations?—That would be an element.

20,218. Were any steps taken to neutralise the ammonia?—No.

20,219. (*The President.*) Were the floors cow-dung floors?—Yes, in almost all instances.

20,220. Supposing the materials of the floors consisted of a large quantity of albumenoid substances that would oppose the penetration, as it were, of the mercuric chloride, would it not?—Yes, it would.

20,221. (*Mr. Cumine.*) When you found that one application of perchloride of mercury did not appear to be sufficient to make the houses safe, did you try and repeat the application day after day?—No.

(Witness withdrew.)

Mr. W. T. Morison, I.C.S.

Mr. W. T. Morison, I.C.S., called and examined.

20,222. (*The President.*) You are in the Indian Civil Service?—Yes.

20,223. What is your position?—Collector of Land Revenue, Customs, and Opium, Bombay.

20,224. (*Mr. Cumine.*) You were Collector of Sholapur in 1897?—Yes, till January 1898, from January 1897, one year.

20,225. You can tell us then about the severe visitation of plague which took place in Sholapur city. What month did it begin in?—It began outside the town in September, and it spread to the town itself in October.

20,226. How long did it last?—It was practically extinct from the town of Sholapur by the end of January 1898, but in the meantime it had spread outside. I am merely speaking of the town itself.

20,227. Is it known where the infection came from?—It is believed to have been brought from Poona.

20,228. By sick persons, or by clothing, or what?—As far as we could ascertain it was brought by a Goanese servant, who came to an officer's house in the cantonment. How it was brought by him we do not know, but his was actually the first case that occurred. He had been to Poona.

20,229. Please give us a short description of the epidemic in Sholapur?—Plague first broke out towards the end of September 1897 in the Sadr Bazar, an isolated, compact suburb about half a mile from the town proper, but within Municipal limits. The suburb could have been easily evacuated at once, as the inhabitants were most of them of the lower and poorer classes, and subsequent experience showed that this would have been far the wisest step to take at first. But we were new to plague operations, and thought that wholesale evacuation was too drastic a measure, besides being likely to spread the disease elsewhere. So we decided to try and confine the plague to the Sadr Bazar by drawing a cordon of police round it. The place was well suited for this, it was compact, with open ground on all but one side, and we had what seemed a sufficient number of police.

20,230. Was the attempt to keep the people in by a cordon of police successful?—It was a complete failure. The people began to melt away like mist, and we found that we were only guarding a gradually emptying suburb. In the first place, we had to let most of the healthy adults go every morning to their work. We issued daily passes to them, but refused to let their women folk and children go, hoping that this would bring the men back every evening. But the attraction was not strong enough, and most of the men remained away and were soon followed by their families, who arranged somehow or other to get through the cordon, by procuring passes under false excuses, evading the posts by night, or bribing the sepoys. Meanwhile the plague was raging among those that remained, and the absolute necessity for complete evacuation then became apparent. This was carried out about 15th November, the people being collected into a camp known as the Moti Bagh Camp, a mile off. As soon as we got the people into the camp there was a sudden and marked decrease in the number of cases among them, and in about a fortnight they were almost free of plague. Before this, however, the plague had spread to the city, and by the beginning of November a number of cases were occurring daily. Sholapur is a place of 62,000 inhabitants, and the total evacuation of it was a work of such magnitude that it hardly occurred to us at first. We began by evacuating the worst infected quarters, and from the weekly reports I find that by the 27th November four quarters containing 400 houses in all were evacuated.

20,231. Did you find that partial evacuation, evacuation of the infected quarters, was sufficient to prevent the spread of plague in the town?—So far as we found, it prevented it spreading quickly, but it had not the effect of absolutely stopping its spread. We found that it spread it spite of evacuation. It was impossible, however, in spite of every precaution, to prevent some of the inhabitants of these quarters taking refuge with friends in other parts of the town, and the plague continued to spread until every part of the town was infected. Evacuation was then carried on on a larger scale, and early in December we determined to evacuate the whole town, and an order was issued that by the 31st December the town was to be empty. At the same time a large camp was laid out in a convenient open site between the town and the railway station, and another to the east of the town. In these sites, streets of shelters made of gunny bags were erected at Government expense (each shelter costing Rs. 5, I think) for the poorer people, and sites were pointed out to the well-to-do, where they could put up their own huts. Large numbers, too, went and lived on the fields outside the town. When once the people saw that total evacuation was decided on, little pressure was wanted to make them leave the town. There was naturally a rush for the best places in the new camp. The town, too, with its many empty and silent streets, often tenanted only by an occasional corpse, had an uncomfortable and haunted feeling about it at night, which was increased by exaggerated rumours that gangs of dacoits from adjacent Hyderabad territory were about to descend on it.

20,232. Did the town ever become completely empty, and if so, on what date?—It became completely empty by the end of 1897.

20,233. Can you show us by figures what the effect of this total evacuation was?—The effect of this total evacuation can be best realised from the study of the weekly returns of plague cases. The following table shows these returns from the end of September to the end of March:—

TABLE showing the PROGRESS of PLAGUE, Week by Week, within MUNICIPAL LIMITS of SHOLAPUR CITY, including all SUBURBAN HEALTH CAMPS.

Week ending	Imported.		Indigenous.	
	Cases.	Deaths.	Cases.	Deaths.
1st October 1897	—	—	13	9
8th "	—	—	6	4
15th "	—	—	26	8
22nd "	—	—	50	45
29th "	—	—	116	90
5th November 1897	—	—	146	117
12th "	—	—	143	118
19th "	—	—	221	182
26th "	—	—	363	297
3rd December 1897	—	—	502	377
10th "	—	—	501	436
17th "	—	—	300	246
24th "	—	—	134	149
31st "	—	—	53	36
7th January 1898	—	—	26	27
14th "	—	—	9	9
21st "	—	—	7	11
28th "	—	—	5	8
4th February 1898	—	—	1	2
11th "	9	1	—	7
18th "	1	—	1	4
25th "	1	1	5	3
4th March 1898	—	—	—	2
11th "	2	—	—	—
18th "	—	—	—	—
25th "	1	—	—	—

These figures, of course, include all cases among those living in the camps and fields outside the city. Little comment is necessary, as the figures speak for themselves. The sudden drop in the number of cases came in the week ending 24th December. The town was practically empty by the middle of December, only a few who had to make special arrangements for the removal of their valuable property remaining behind till the end of the month.

20,234. Did the camps enjoy immunity from plague?—Almost complete, and this was one of the most striking features of our operations. The largest camp, known as Pothepur, with over 10,000 inhabitants, was not more than 200 yards from the town walls. Yet with the exception of one or two cases, most probably of people with plague in them when they came to live in the camp, it was entirely free from plague.

20,235. Before people were admitted into the camp, and after they had been admitted into your camps, what precautions were taken?—Most careful precautions were taken in the camp under the supervision of Mr. Pratt, Assistant Collector, who was placed in charge of it; all new arrivals were made to bathe their bodies and clothes in disinfecting baths, and there was an inspection of each hut twice daily for plague cases. Our regular camps were almost entirely free from plague from the first, and that, too, at the coldest time of year, when the disease is generally found to be most vigorous in towns.

20,236. Did you find that bad results followed on the emptying of a plague-stricken town?—The plague, of course, spread outside.

20,237. I mean as regards the neighbouring villages?—It spread to them gradually, but to no very great extent. It went on outside for a long time after it was practically extinct in the town. I am of opinion, however, that in the case of Sholapur the plague was not spread so widely under our evacuation system as it would have been had we not gone in for evacuation, and that this will always be the case in similar circumstances. Before we decided on a general evacuation, it was found that a large number of plague-stricken people and their friends were leaving the town and hiding in the villages. In spite of guards placed on all roads, they managed to escape, generally at night: and every morning corpses were found lying on the roads leading from Sholapur and in the surrounding fields. What happened was that as soon as a case occurred in a house, all the inmates at once fled to escape infection and segregation. It was impossible, by merely putting

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guards on the roads and patrolling between them, to stop the people getting away; they would wait till night and creep through the fields and hedges, anywhere and in any way to get away. Nothing less than a fence round the whole town would have prevented them. Thus many of the sick and contacts, not detected by us, escaped to the villages, while the healthy, as a rule, remained behind. When only a partial evacuation of the worst quarters was being carried out, it was the interest of all the neighbours to help these fugitives to get away, so that their quarters might not be ordered to be evacuated. When complete evacuation is resorted to, there is not, in my opinion, the same danger to surrounding villages; a few more healthy people may take refuge in them, but there is not the same inducement for the infected or contacts to go; they are already undergoing the discomforts of *camp* life, and the segregation camp is not much worse, perhaps rather more comfortable; and it is no one's interest to help them to escape.

20,238. Would you tell us the chief conclusions you formed from your plague experience?—The conclusions which I would beg to submit to the Commission as the result of my experience in Sholapur are as follows:—

(*a.*) The immediate and complete evacuation of all villages, and of towns up to 40,000 or 50,000 inhabitants, when this is considered possible, is the best way of treating an outbreak of plague.

(*b.*) *Before* commencing the evacuation of a large town, complete arrangements should be made, and the necessary staff sent to their posts, for the thorough inspection of all surrounding villages, say, within 30 miles, and for immediately evacuating any village where plague is discovered.

(*c.*) Partial evacuation of towns and villages by clearing out the worst infected quarters only is not recommended where complete evacuation is within the bounds of possibility. It may reduce the mortality and make the disease move slower, but it will not stamp it out.

(*d.*) The system of keeping the inhabitants of an infected town or village to their houses by drawing a cordon round the place is not recommended, and will, it is believed, prove a failure, except in the case of small villages.

20,239. (*Mr. Hewett.*) Is it the case that before you evacuated any part of the town there had been a large number of cases of plague?—We started by slow evacuation. We began with houses only, and then we went on to quarters. We extended it gradually to small quarters of streets, and then to the whole town. I cannot exactly tell how many cases had occurred while these partial evacuations were going on.

20,240. Did you have contact camps and health camps?—We had.

20,241. Voluntary as well as compulsory?—They were all compulsory.

20,242. What was the largest number of cases, firstly, in the health camp, and, secondly, in the contact camp, at any one time?—I am afraid I cannot say that.

20,243. Can you give any estimate?—We had three or four health camps round the town.

20,244. Can you give us an approximate estimate of the proportion of the 62,000 inhabitants of the town who were in these camps?—I should think about 25,000 at least.

20,245. So that about 35,000 persons went elsewhere?—A great many went to their own lands in the vicinity of the towns. Sholapur is an agricultural town, and cultivation comes close up with it. Anyone with lands and fields would go with their friends and stay in their own fields. We had not any actual check over these small habitations in the fields, though they were always inspected for plague cases, but we could hardly say they were under our control; they were only inspected.

20,246. What proportion of the 62,000 inhabitants of Sholapur went, in your opinion, to other places?—There was a large exodus at the beginning of the plague. I should think about a quarter of the population probably had gone before the plague became bad at all.

20,247. And do you think that these people would have gone, whether you took any measures, or whether you did not?—I expect so. I do not think they could have been stopped. Sholapur is close to the Hyderabad frontier, and a large number fled across the border merely at the prospect of plague restrictions, as they thought in Hyderabad territory they would be outside our control entirely.

20,248. When you advocate complete evacuation of towns up to 40,000 or 50,000 inhabitants, you contemplate the possibility of a certain proportion of the population of such towns flying away into other localities?—Yes, I should think about a quarter. Of course, this is only a rough estimate; but I think that whenever plague appears at a place an exodus always begins at once, and goes on slowly and silently, so that, when it comes to checking the actual houses, you will always find that there are a great many empty.

20,249. Do you think that exodus is due to the fear of the plague, or to the fear of the measures taken to suppress it?—I think the people are afraid of both. I do not think it can be attributed solely to one or the other; I think equally to both.

20,250. Do you find that any particular classes left the town in the early stages of the epidemic?—Such as could travel easily, and had not to stay at home to earn their daily bread, could, of course, go; and those who had houses and friends elsewhere would always go at first.

20,251. Do you think that any measure could be devised which would keep all the people in a town of this sort?—I think it is quite impossible.

20,252. (*Dr. Ruffer.*) Was there a sudden running away when evacuation began?—No, I do not think so. We had arranged quarters for the people outside, and they thought they might as well stay. Those who remained behind had probably something to tie them to the place.

20,253. You said you began general evacuation in the beginning of December?—Yes.

20,254. Can you tell me, week by week, how many people had been evacuated in December?—I cannot tell you.

20,255. Can you tell me approximately whether half the number had been evacuated by the 10th of December?—I should think over half.

20,256. Could you tell me the mortality among the evacuated and the people in the town?—I could not tell you.

20,257. Do you think the mortality was larger in the town than in the evacuation camps?—Much larger in the town; it was extremely heavy; while outside there was hardly any.

20,258. (*Mr. Cumine.*) After all the people had been turned out of the town, did you find it possible to prevent them coming into the town during the day on business, or walking through the town?—We allowed anyone who had property of any value in the town to come in from time to time to open his house and see if everything was right, but they had all to be out in the evening. There was no one allowed in at night.

20,259. What did you do with all the shopkeepers?—They were all arranged in streets in the camp. We had a grain street and a cloth street, and so on. They had all their streets outside in the camps.

20,260. Were the people in the health camps free to go about their duties in the day?—Yes, quite free, there was no restriction.

20,261. Could they go into the town?—They could go into the town generally, but we tried to prevent them going in to any great extent, only there was no absolute prohibition. If anyone wanted to go in he was allowed to go to his house and open it.

20,262. There was a great exodus when the plague first appeared. If Sholapur was attacked again do you think there would be the same exodus, or anything approaching it?—I do not think it would be quite so bad.

20,263. When plague appeared in the town of Sholapur, and as it got towards the Nizam's frontier, did you on each occasion let the Resident of Hyderabad know that plague had appeared near his borders?—I do not think we did our duty exactly in that respect; we gave information afterwards. I think the charge against us on that point is just.

20,264. Though you evacuated this large town on that one occasion do you think it a practicable thing to do to go on evacuating it on every future occasion in which plague may get into the town—a large commercial town like that?—It is not a large commercial town, and I think it was that fact that helped us to evacuate

entirely. It is a great grain distributing centre, and this work can, of course, go on outside the town as well as in the town, but it cannot be called a commercial centre, I think.

20,265. Do you think it practicable to go on evacuating each time?—I think it could be done again; I do not know whether it is advisable, but it could be done. The people have got used to being out and they are quite content to be out. There were discomforts, of course, but there were no serious complaints.

20,266. (*The President.*) I judge that you did not regard partial evacuation as successful?—No.

20,267. This partial evacuation was not commenced, I think, until there was already a considerable amount of plague in the town?—No.

20,268. At the time you were partially evacuating, the people were running away?—Yes.

20,269. You had probably no provision made for those who wished to go away, in the neighbourhood?—We had not a large enough camp.

20,270. But supposing you had effected complete evacuation at the commencement, do you think you could have provided accommodation for this large multitude?—It could have been done. The people generally belong to the poorer classes, and can do with very small accommodation in camp. We gave them generally small triangular huts made of bamboo and cloth stretched over. The great majority of the people were housed in these huts.

Mr. W. T. Morison, I.C.S.

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20,271. Are your measures for detecting cases more perfect now than they were originally?—I have not had any experience of plague work since then.

20,272. In the latter part of your experience had it been improved?—My experience stopped at the end of my time in Sholapur, in January 1898.

20,273. Supposing there was an outbreak again, do you think that the early cases could be more successfully detected than they were before?—I cannot say, I do not think they would. I think that by the end of our campaign at Sholapur we had got everything arranged, and the inhabitants, too, were in more or less training.

20,274. Are you able to tell me whether the cases appeared to be equally severe, or not so severe at the latter part of the epidemic when the numbers were being reduced, compared with what they were at the earlier parts, or when the plague was most prevalent?—They seemed to me equally severe. The average mortality among the number of cases seemed to be as large at the end as at the beginning.

20,275. Therefore, possibly the falling off was not due to diminution in the virulence of the plague itself?—So far as I could say it was not.

(Witness withdrew.)

Mr. N. A. F. Moos called and examined.

20,276. (*The President.*) You are the Director of the Government Observatory at Bombay?—Yes.

Mr. N. A. F. Moos.

20,277. (*Mr. Hewett.*) You have prepared a chart,* have you not?—I have.

20,278. With what object?—In order to see if the mortality curve was in any way influenced by any seasonal factor or combination of factors. My attention was first drawn to it by a letter from Mr. Monteath, the Chief Secretary to Government, who asked me to submit the daily curves of temperature, expecting that these would present some resemblance to the mortality curve. By examination, I found that the vapour pressure curve ran more closely parallel (inversely) to the mortality curve than the heat curve; and it was on those suggestions that I thought that the two factors properly combined might give a resultant curve, fairly coincident with the mortality curve.

20,279. What are the data from which you have prepared the vapour curve?—From Regnault's formula. All the observations were taken at the Kolaba Observatory. From the readings of dry and wet bulb thermometers vapour tension was derived, and from this vapour tension the dew point is obtained from tables, and the dew point then is modified by the difference in the temperature of the air and the temperature of the ground. This would roughly and indirectly measure the ground evaporation, because according to a theory analogous to the theory of exchange in heat, the greater is the amount of vapour in the air, the less would be the evaporation from the ground, and the less the quantity of vapour in air the greater the exaporation from the ground. That which has greater humidity parts with its moisture to the other which has less, and it is on this theory that the tension of moisture in the air would measure indirectly the evaporation from the ground. Unfortunately, in the observatory, no direct observations of the atmometer or evaporometer are available. In the absence of these observations, the only possible approximate way to get the ground evaporation is by combining such factors as measure this evaporation indirectly; and these factors are the dew point, modified by the difference of the temperature of the air and that of the ground.

20,280. Where did you take that temperature?—At the Observatory.

20,281. At what depth below the surface?—Of course there are various depths at which temperatures are taken. For my purpose I have selected five feet below the ground. Temperatures are taken at various depths, from one inch to 12 feet; the maximum depth at which we take the temperature is 12 feet. The reason why I have selected five feet under the ground is this: the diurnal variation due to solar heat at Kolaba does not take effect in the ground beyond a depth of nine inches; the annual variation goes beyond this zone, but does not penetrate beyond a depth of 12 feet; I have therefore selected five feet as not being too shallow nor too deep; and, even taking the observations at other depths, the curve will be more or less analogous to the one which I have shown. Before I arrived at the data I made several trials with different depths and different factors, and the curves were found to be more or less similar. There is a slight difference in the runs, but the reasons of the discrepancies can be easily explained; the general run, however, is almost the same as that submitted in my chart.

20,282. (*Dr. Ruffer.*) Is your method liable to error; what is the percentage of error?—That I really cannot say, but I think that the general run of the curve would not be affected, although possibly the weekly fluctuations might.

20,283. How much do you think it could be affected? Would it be 10 per cent. or one per cent.?—I really cannot answer that question, as in cases of approximation it is difficult to judge exactly; but I do not think it would appreciably differ from the curve submitted.

20,284. Would it be such an error that it would affect your curve much?—No.

20,285. Visibly?—No, not visibly.

20,286. (*Prof. Wright.*) Do you call your curve a climatic curve, or a ground evaporation curve?—Yes, the so-called climatic curve represents approximately a ground evaporation.

20,287. Does your curve express the resultant of four different series of data: one, the temperature of the air; secondly, the temperature of the ground; thirdly, the amount of moisture in the air; and, fourthly, the amount of moisture in the ground? Are all those four factors taken into account?—Yes. The first three would indirectly represent the amount of variation in the ground evaporation.

20,288. I suppose the higher the temperature in the ground the more the moisture, and the greater would be the vapour-tension in the ground?—The greater the tension.

20,289. And in the case of the air I suppose that the less the amount of contained moisture and the higher the temperature, the greater would be the tendency to take up vapour from the ground?—Other factors remaining the same, the less the vapour in the air, and the higher the temperature of the air, the greater will be the tendency for the air to take up moisture from the ground. It is a question of relative tension. If the tension in the air is greater than that in the ground the air would part with its moisture to the ground, and *vice versâ*.

20,290. Your curve, then, represents the balance between the two tensions—the tension of moisture in the air and the tension of moisture in the ground?—

* *See* App. No. LVIII. in this Volume; the charts and notes put in by Mr. Moos are published with notes regarding them made by Dr. A. Buchan, LL.D., F.R.S., Secretary to the Meteorological Society for Scotland.

Mr N. A. F. Moos.

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Yes, it measures the tension of the vapour in the ground, as determined indirectly from the tension of vapour in air.

20,291. How is the vapour-tension of the ground determined?—I have not been able to determine, as already stated, the absolute amount of moisture in the ground.

20,292. Is there no method by which it can be directly determined?—There is; by observations with atmometers and evaporometers, but we have not got these instruments.

20,293. What do you substitute for them?—By a mathematical reasoning, knowing that the vapour-tension of the air varies inversely as the vapour-tension of the ground, the former, modified to the temperature of ground, would approximately and indirectly measure ground evaporation.

20,294. You mean to say that the tension of air would vary inversely with vapour-tension of the ground if it were freely exchanging vapour with the ground?—Quite so; the theory of exchange in heat may be analogously applied to the moisture too.

20,295. But the air is not freely exchanging vapour with the soil five feet below the surface?—The tension of the vapour in the two are relatively connected.

20,296. Why did you select this depth of five feet? Did you select it because you found the curve that you obtained in this way agreed with the curve of the mortality?—Yes. The average run here is much more close than when the other depths are taken, but the difference only lies in the weekly fluctuations, and not in the general run of the curve.

20,297. You said the seasonable variation ceased to make itself felt at 12 feet below the surface?—Yes.

20,298. You took as the basis of your curve the observation made at five feet below the surface, but I understand that your curve would have come out quite the same if you had employed thermometrical readings which had been taken at other depths below the surface?—Yes. The broad undulations correspond in each case, under certain limits.

20,299. But the small undulations?—They do not quite correspond.

20,300. Then did you choose the readings obtained at a depth of five feet, because the curve which was based upon these readings coincided most closely to the curve of the mortality?—Yes.

20,301. (*The President.*) How do you state the relationship between the surface evaporation, and the mortality rate from plague?—Whenever the ground evaporation was a maximum, the mortality was a maximum.

20,302. You have handed in a written statement here, which embodies all your views. I believe they are all contained in this paper?—Yes (*see* Question No. 20,309).

20,303. (*Prof. Wright.*) You think the mortality is greatest when the ground is giving off most vapour to the air?—Provided it is contaminated, because that is the basis on which I go. Under ordinary circumstances, in normal years, this seasonal increase in ground evaporation would be innocuous.

20,304. Does that mean that the mortality is greatest when the upper surface of the earth is most saturated with moisture?—It goes without saying, because if there is evaporation from the ground, there must be moisture.

20,305. But would the upper surface of the ground be most saturated with moisture whenever vapour was being given off from the ground; are there not two factors which determine the moisture of the surface of the ground: first, the deposition of dew from the air, and secondly, the moisture coming up from below?—I may say that the moisture of the air is mainly derived from the sea, but the moisture of the ground depends on the geological structure, and the environments of the place, as well as local causes: *e.g.*, the water supply, the proximity of lakes or tanks, and the free and profuse use of water, under which circumstances it is possible that the surface to a depth of five to ten feet may be thoroughly saturated with water.

20,306. Would not the moisture of the surface of the ground be more influenced by the deposition of dew than it would by the evaporation of vapour from the ground below?—Not so much; the inherent moisture of the ground would not be affected, specially in the deeper parts of the ground, by the deposition of dew.

20,307. But would not the moisture of the surface of the ground be most influenced by dew being deposited on it?—Just the very surface.

20,308. (*The President.*) As far as I understand, it appears that the more favourable the conditions for the virus to rise from the ground into the air, the greater is the mortality from plague?—Yes.

20,309. Will you put in all these papers, showing the data?—Yes. They are as follows:—

"The object with which the following inquiry was undertaken was to find whether any meteorological factor or combination of factors had any direct or indirect influence on the mortality of the city from week to week and month to month.

"At the outset it was noticed that no very abnormal feature could be detected in the various meteorological factors of the plague years, as compared with those of past years, except in one single factor, that of ground temperature.

"The yearly value of temperature fluctuated irregularly for about 40 years past about 83° Fahr. It, however, began to rise at all depths, feebly but steadily, from the year 1888, and the figures for 1896–97 and 1898 are exceptionally abnormal, *vide* table attached (Temperature of Air and Ground, 1846–98).*

"The first step, therefore, was to see whether any combination of this factor with other factors like air temperature, 'moisture,' &c. could give a curve which in its general run coincided with the mortality curve. After several trials it was seen that temperature of the air, temperature of evaporation, and temperature of ground five feet below the surface, if combined in a certain way, gave a curve which ran strikingly parallel to the mortality curve, and the principal, as well as the subordinate, maxima and minima points of which were coincident.

"The coincidence cannot be a mere accident for such a long period as three years; and it may be presumed, therefore, that some sort of relation does exist between the two as indicated.

"The nearest, though not *absolutely correct physical interpretation*, of the combination of factors adopted, is ground evaporation, and the curve so delineated may be accepted as representing ground evaporation.

"The question of importance in the above comparison is not about the arbitrary selection of a depth of five feet below ground surface, the selection of which will be presently explained; but whether the interpretation of the combination to mean ground evaporation could be justified. It was with a view to examine this point, that combinations were made in several ways by which the nearest approach to ground evaporation could be represented. The ground evaporation may, approximately, be represented by the formula:—

$$F = f - c\,(t_g - t_a) \times \frac{P - f}{3^0}$$

where t_g is the temperature of the ground at any given depth, t_a the temperature of the air, F and f being the tension of vapour at the two temperatures, and P the barometric pressure.

"The curve, however, has been drawn from figures given by $\left[D - (T_g - T_a)\right]$ where D represents the dew point obtained from a similar formula as given above. It may be noted that the curves given by either method are similar and identical. The latter is preferred because the calculations are less troublesome, and give no extra labour, as the factors D and $T_g - T_a$ are daily obtained in the observatory for other purposes.

"The selection of the arbitrary depth, 5 feet, is obvious now from the treatment adopted. In the expression $D - (T_g - T_a)$, if we take the temperature of the ground on the surface, which is almost equal to that of air above, $T_g - T_a = 0$, and the expression becomes D, that is, the curve would represent dew point only. If we go 12 feet below ground, where the temperature is constant throughout the year, and does not vary with solar heat, the fluctuations virtually represent ($D + T_a$ – constant). In both these extreme cases, therefore, the ground temperature will not be represented in the fluctuations of the curve. In order, therefore, to bring the ground factor effectively in the expression, the selection of a depth of 5 feet midway between the two extreme cases was deemed essential. Practically the temperature at 5 feet below ground, therefore, may

* *See* App. No. LVIII. in this Volume.

be taken to represent the integral effect of temperature at various depths from 0 to 12 feet, or the maximum effect.

"In conclusion, I beg expressly to say that I do not assert that the plague mortality, after inception of the germs on the soil, is wholly dependent upon seasonable causes. Obviously that cannot be, as other factors, known and unknown, must also affect the mortality of a great city like this. But I do wish to say that a *primâ facie* case has been made out, to show that the seasonable factor, as measuring ground evaporation, is one of the principal causes which influence the rate of mortality, and which, if conclusively proved, would have an important bearing on the whole question. It therefore justifies further investigation in that direction, and with that view I am strongly of opinion, that thermometric stations should be established in different parts of Bombay, specially in the affected districts like Byculla and Mandvi, &c., to enable ground and air temperatures and absolute evaporation results to be obtained directly, and I have no doubt that important side light would be thrown on the question of propogation of the disease. The indications of temperature, &c., at Kolaba, which is situated on high and rocky ground, cannot exactly measure the important factors under discussion which may be prevalent in the actual plague centres, and, therefore, the proposed stations are absolutely essential in the inquiry."

Mr. N. A. F. Moos.
21 Feb. 1899.

(Witness withdrew.)

Mr. A. G. MACKENZIE called and examined.

Mr. A. G. Mackenzie.

20,310. (*The President.*) You are Superintendent of one of the jails here, I believe?—Yes; I am Superintendent of H.M. Common Prison, Bombay. The information I shall give you will be with regard to the numbers in the prison, the number of those inoculated, and how long they remained afterwards.

20,311. (*Prof. Wright.*) Perhaps you will tell us how the prisoners were selected for inoculation?—I selected them myself. The first inoculation took place on the 1st January, 1898. There were 199 prisoners selected out of a population of 399, leaving 200 uninoculated, about one half.

20,312. On what principles were prisoners selected for inoculation?—On no principle whatever. I formed the prisoners up and selected a few here and there, and formed them into batches. It was an experiment, and half the prisoners were inoculated and the other half were not, and we wanted to see the result.

20,313. Do you think the prisoners who were inoculated differed in any respect from the prisoners who were not inoculated?—Beyond a doubt; I think the inoculated were protected.

20,314. I mean did the inoculated differ in age or physique from the uninoculated?—No, they were formed in a line and I simply took them out here and there. I made no particular choice.

20,315. Were the inoculated persons treated differently from the uninoculated?—No, except that they were sick for 24 or 36 hours, and had to be relieved from their labour.

20,316. During these 24 hours, were the uninoculated exposed to different conditions from the inoculated?—No, I made no difference whatever. They were all placed in their cells.

20,317. What were the results?—The inoculation took place on the 1st of January, 1898. Prior to that there were three cases of plague in the jail, and it was in consequence of that that the prisoners were inoculated. Two of those three cases were attacked on the 31st December, and the third on the day of inoculation, in the early morning, and those three died. After the inoculation there were 17 attacks up to the 29th July, 1898, among the uninoculated. There were only three attacked among the inoculated and they all recovered. Out of the 17 uninoculated, nine died and eight recovered.

20,318. How long were the inoculated and uninoculated under observation? I understand that some of the prisoners obtained their discharge soon after the inoculation had taken place?—My jail may be called a migratory prison; they are all short termers, in to-day and out to-morrow. At the end of July there were about five men left out of the 400; the others had all gone and an entirely new population was in the jail, short-term prisoners, some a few days, some one month, and some two months, and so on, and perhaps a few up to 12 months, say half a dozen.

20,319. Were the numbers of the uninoculated increased by fresh prisoners coming in?—Beyond a doubt, but I am only speaking with regard to the population which was in the jail on the day of inoculation.

20,320. You have confined your statistics to what happened amongst those?—Yes; the other men did not come under my observation; there was no need for it.

20,321. Among the 200 who were uninoculated 17 cases occurred?—Yes, up to July.

20,322. Have you any information about the subsequent fate of those who left the the prison?—None whatever.

20,323. We have had it in evidence that there were two suspicious cases of plague admitted to the Parel Hospital who had been inoculated in your jail. It was doubtful whether these particular patients had mild plague or whether they had mumps; do you know anything about these cases?—No, I do not. I know that three prisoners have been recently attacked, two during January, with plague.

20,324. (*Mr. Ruffer.*) Inoculated or uninoculated?—Uninoculated.

20,325. (*Prof. Wright.*) Have all the inoculated now left your prison?—They have all left except five.

20,326. Can you give us the dates on which the inoculated persons left?—Yes, I can give you the statement, and also a statement showing how long those who were not inoculated were in the prison and when they were discharged.*

20,327. How long did the epidemic of plague last?—Up to the 29th July.

20,328. You mean that there were dropping cases up to this time?—Yes, the 29th July was the last. The cases prior to that were on the 29th June, 18th March, 28th February, 10th February, 2nd February, and so. They were quite sporadic.

20,329. Were any of the prisoners seriously ill after inoculation?—No, they all recovered within 36 hours or so.

20,330. Do you think the amount of reaction which you got in the prisoners was fairly equal?—Some suffered more than others, but all about the same time, from about 24 to 36 hours. Some suffered very acutely, or, at all events, their complaints were greater. They may have been less able to bear the pain, but they appeared to suffer more.

20,331. (*Dr. Ruffer.*) Can you prepare a table giving the name of each prisoner, the age of each prisoner, the date of inoculation, the date of entrance into the jail, the date of exit from the jail, the number of the cell in which he got the attack, the date of the attack, and the result of the attack?—I put in a statement* which gives the particulars required; there is, however, no record of the particular spot where each attack took place. All the cases occurred in the quadrangle on the south side of the prison, the first four attacks being in a spot in the south-west corner of the corner of the barracks—a large barrack where the prisoners sleep in association.

20,332. (*Mr. Cumine.*) To what extent were the attacks among the uninoculated due to the mere accident of their having slept in a cell where there had been a previously infected person? I believe there was some plague in the jail before you performed the inoculations?—Yes, three cases in December.

20,333. Do you know in what cells those three cases lay ill?—Yes; they were all in what we call the lower barracks, not in cells—the barrack is a large building well ventilated.

20,334. With regard to the uninoculated persons who were attacked after the inoculations, could you tell us how many of them slept in that barrack in which the three people slept who were attacked and died before the inoculation?—Yes. To begin with, it is a large

* *See* Appendices Nos. LIX., A. and B., in this Volume.

Mr. A. G. Mackenzie, 21 Feb. 1899.

barrack. They were all three attacked, I think, on the lower floor and in a particular spot. It is a singular fact that attacks took place on the south western side of the barrack more than in any other part of the jail. I cannot account for it because the whole jail is clean and dry, but such is the fact, there were four cases here.

20,335. Had the three persons who had been attacked before the inoculation been sleeping in that spot?—I think I can find that out for you. (Note by witness on correcting proof of his evidence:—I find that they had been, and I then, on the 7th January, turned the whole of the convicts out of the buildings and made them sleep in the open; no case then occurred till the 15th January 1898.)

20,336. When uninoculated persons sleeping in that spot were attacked, were the inoculated persons sleeping there also?—Yes, there was never separation made at all.

20,337. Were they sleeping in that particular part of the barrack?—Yes. The men are shifted about indiscriminately. Prisoners are never allowed to sleep in one particular cell or one particular part of the barrack; that is our system of discipline. If a prisoner were kept in the same spot he could carry out his measures for escaping day after day, and so we shift them about.

20,338. As a fact, can you say that in this particular spot which appeared to be infected there were inoculated as well as uninoculated people sleeping after the inoculations?—Beyond a doubt, but they were all turned out and the building vacated on the 7th January.

20,339. Did you find that the inoculated people sleeping on the spot were attacked?—All the facts are to be obtained from my register. No inoculated man was attacked there; as a matter of fact the place was vacated as I have previously stated seven days after the prisoners were inoculated.

20,340. (*The President.*) When a case occurred, how did you dispose of it?—I sent cases to the Plague Hospital.

20,341. How did you treat the cells?—The cells were disinfected and the clothing destroyed, and no prisoners were allowed to be locked up in that locality for several days—10 or 15 days. I kept those cells as vacant as possible, but, unfortunately, at the time the jail was very much over crowded. The accommodation was for 160 prisoners and we had nearly 400, so it was terribly overcrowded. It is astonishing that the plague did not spread further in the jail, but I believe that the reason it did not was on account of the measures which I took. I burned a tremendous lot of quicklime, put it on boiling all over the place, roof, floors, the whole of the quadrangle and ground, and every corner, and I kept it up for several days. I am of opinion that I prevented it developing into an epidemic and that I destroyed the plague altogether in the jail ultimately.

20,342. I understood you to say that in a certain part of the jail cases did take place?—They were sporadic cases; it could not be called an epidemic.

20,343. Did you each time disinfect that part especially?—Each time.

20,344. How did you disinfect it?—With quicklime in a boiling state and carbolic dilute.

20,345. How was it diluted?—The Medical Officer prepared the prescription and it was mixed accordingly and thrown on.

20,346. Obviously, that part had not been properly disinfected?—I cannot account for it.

(Witness withdrew.)

Mr. C. N. Hall.

Mr. C. N. Hall called and examined.

20,347. (*The President.*) You are Superintendent of Her Majesty's House of Correction, Byculla?—Yes.

20,348. (*Prof. Wright.*) You superintended the inoculations in January 1897?—Yes.

20,349. How many cases of plague were there before inoculations were resorted to?—I cannot give you that information.

20,350. Were you present when prisoners were selected for inoculation?—There was no selection; it was voluntary. The prisoners were asked to come forward if they wished to be inoculated and those who came forward were inoculated.

20,351. How many were inoculated?—153.

20,352. How many were left uninoculated?—185.

20,353. Were these prisoners all in contact with each other after inoculation?—Yes.

20,354. Did the prisoners who were inoculated differ in any way from the uninoculated in age or health?—They were about the same.

20,355. Did they follow different occupations?—No, all one occupation, *i.e* intra-mural industries.

20,356. How do you know how many cases occurred among the inoculated and among the uninoculated?—I can give you that information, but I may mention that the Assistant Surgeon of that prison is here, and he will give you all information. The inoculated and uninoculated were all treated in the same way. There was one point of difference, viz.: that those who were inoculated did not work the next day. They said the fever was so great that they could not work, and they were excused. There was no difference in diet or anything else.

20,357. Were the uninoculated kept in a part of the jail where the plague had occurred before near the original plague cases?—No. After the plague occurred inoculated and uninoculated were all moved into work sheds; the whole body of prisoners was taken out of the cells.

20,358. (*Dr. Ruffer.*) Could you put in a table showing on which day the plague began, the names and ages of all prisoners, the date of entrance in the jail, the date of inoculation, the date of discharge, and the date of attack and the result?—This is with Mr. King's evidence.*

* *See* App. No. LX. in this Volume.

(Witness withdrew.)

Assist.-Surg. A. King.

Assistant Surgeon A. King called and examined.

20,359. (*The President.*) Are you the Assistant Surgeon of Her Majesty's House of Correction, Byculla?—Yes.

20,360. (*Prof. Wright*) Can you give us any facts about the inoculations which were done in the Byculla Jail in January 1897?—Yes.

20,361. Were you present when the prisoners were selected for inoculation?—Yes.

20,362. Is it your opinion there was no difference between those who were inoculated and those left uninoculated?—There was no difference at all.

20,363. And is it your opinion that the inoculated and the uninoculated were treated in exactly the same way afterwards?—Exactly, except that they did not do any work the next day or so.

20,364. Did any of the prisoners suffer severely from the inoculations?—Yes; a number of them suffered very severely.

20,365. Were there any among the inoculated who did not react to the inoculations?—Yes; they had no fever after the inoculation.

20,366. Can you trace whether the inoculated who subsequently got plague had or had not a severe reaction at the time of inoculation?—There were five men attacked with plague after inoculation out of which three died. Of those who recovered one had been inoculated once and the other had been inoculated twice.

20,367. Did all these five occur within a few days after the inoculation?—The inoculation was done at 3·30 in the evening of the 30th. Three of them got plague about six or seven o'clock the same evening. One case occurred on the 1st February and the other on the 6th February.

20,368. Then there was an interval in two cases between the inoculation fever and the attack of plague?—Yes.

20,369. Can you find out in those cases whether the inoculation fever was high or whether it was very insignificant?—In one case Dr. Haffkine thought it was the reaction fever, that is, the one which occurred on the 1st February, but the other case he thought was plague.

20,370. Had the inoculation fever been high or insignificant in the second case?—It was 102 or 103—the man had glandular swelling.

20,371. Do you mean he had glandular swellings when attacked with plague or after inoculation?—I think glandular swellings due to plague symptoms, not after the inoculation.

20,372. What interval was there between that man's inoculation and the attack of plague?—The first inoculation was done on the 30th January, and the second on the 2nd February; he got plague on the 6th February.

20,373. He had been twice inoculated?—Yes.

20,374. I suppose that the temperature which he had on the 3rd was due to his inoculation?—Yes.

20,375. Do you know how high the temperature was?—100·8.

20,376. Can you tell us what was his temperature after his first inoculation?—No, because I had been inoculated and was seedy myself at the time.

20,377. Will you look through the records and ascertain whether you have any data to enable us to decide whether the prisoners who reacted were absolutely protected, and whether there was a defect in reaction in those prisoners afterwards attacked with plague, stating also the dates on which the inoculated prisoners left the House of Correction?—I can give you a very accurate statement with regard to the second inoculation, done in 1898, but with regard to the first I am afraid I shall not be able to give you the data exactly, because I was inoculated and prevented from doing my work.*

20,378. (*Dr. Ruffer.*) Were the same prisoners present at the second inoculation as were present at the first?—A few of them.

20,379. Where were the buboes situated in the three cases inoculated by M. Haffkine, which were classed as having plague when inoculated?—Prisoner, No. 672, Keshav Umishankar, left inguinal and femoral glands. Prisoner No. 1356, Abdool Karim Goolam Hussain, left axillary glands, subsequently the whole lymphatic chain, as far as the wrist, was swollen and painful, including the left mammary. Prisoner, No. 1222, Govind Pandu, left axillary glands.

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20,380. Can you give us ages of all prisoners attacked?—Yes.

No.	Prisoner's No.	Name.	Years of Age
		1897.	
1	977	Bhiwa Bhagoo	24
2	1093	Rama Govind	22
3	1359	Narayan Dhondu	25
4	1349	Adamji Alibhai	28
5	1235	Khatow Lakhmidas	26
6	1094	Ramvalab Rampartab	22
7	1247	Antone Salvadore	25
8	1272	Balkhrishna Mahadu	27
9	1242	Gannoo Baloo	24
10	1362	Syed Jonas Ali	27
11	1276	Hossein Mahomed	25
12	1312	Walli Chand	30
13	1310	Vishram Jairam	50
14	1129	Raoji Jothu Powar	25
15	1144	Jiva Virchand	24
16	1224	Anna Vishnu	23
17	1229	Ganno Esso	32
18	672	Keshav Umishankar	24
19	1356	Abdul K. G. Hussain	38
20	832	Rama Lakshman	25
21	1261	Kallu Bhimha	35
22	1222	Govind Pandu	26
23	1336	Gunpat Babaji	26
24	1252	Jay Mohan Ganesh	25
25	988	Mahomed Hossein	30
26	1238	Babuji Sambhaji	28
27	1166	Mawji Bhowan	27
28	1297	Dhondu Sulkia	32
29	1332	Kalloo Luximon	26
30	746	Gangu Dongersey	50
31	1267	Baloo Krishna	30
32	993	Mamu Ibrahim	32
33	1366	Shamrao Sitaram	27
		1898.	
1	346	Sulleiman Allarakhia	25
2	230	Marotti Babuji	20
3	303	Gokul Dipchand	27

There were attacked in the Workhouse two inmates, viz., Robert Shepherd, 27 years of age, inoculated, and Michael Curwen, 32, not inoculated.

* *See* App. No. LX. in this Volume.

(Witness withdrew.)

Major H. P. Dimmock, I.M.S., called and examined.

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20,381. (*The President.*) You are a Doctor of Medicine of Durham University?—Yes.

20,382. And you are in the Indian Medical Service, and have had a large experience of plague in Bombay?—Yes.

20,383. I understand you were a member of the Plague Committee?—Yes; I was appointed in March 1897.

20,384. Will you give us your views as to how the bacillus produces plague?—By entry, primarily, I think, through the distal lymphatics.

20,385. What form does it generally produce then?—The bubonic is the most common, of course the development of buboes through the infection of the distal lymphatics.

20,386. The bacillus may infect through other channels, may it not?—Yes. I think it may effect entry through the mucous membrane, and probably through open sores of the skin, and also through the lungs.

20,387. What part of the gastro-intestinal canal do you think the bacillus enters?—I think it may be passed through the mouth and also, possibly, through the anus, because the natives use ablutions to a considerable extent, and it is possible that if there is any excoriation about the mucous membrance of the anus, infection of the lymphatics may take place in that region.

20,388. With regard to mouth infection, do you think the absorption takes place actually through the mouth surface?—I think so, in some cases. It is difficult to explain how it is that one gets infection of the glands in that region. Then there is the abdominal type of the disease, which must necessarily, I think, be the result of infection directly through the gastro-intestinal canal.

20,389. How is it introduced into the gastro-intestinal canal?—I suppose it is swallowed with food or liquid.

20,390. Have you any knowledge of the influence of the gastric juice upon the bacillus entering in that way?—Not from bacteriological knowledge.

20,391. From any source of knowledge?—I could not say that I have made any practical experiments with regard to the gastric juice.

20,392. What led you to say that it may be passed down as far as the stomach and be absorbed through the gastro-intestinal canal?—Simply from my clinical experience.

20,393. Of what kind?—Inference from the method of development of other diseases, such as typhoid fever and the general symptoms of the abdominal type, there being no evidence to show how the bacillus can have penetrated the system except through the intestinal canal.

20,394. I think you have seen some cases where you suspected that the nasal mucous membrane was the absorbing surface?—I had a very interesting experience before I was placed on the Plague Committee. A group of Parsees came in from Thana, and the children were

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suffering from nasal catarrh of a most severe type. The symptoms were, apparently, very acute—coryza, with a high temperature. There was no glandular enlargement in the early stages, but, subsequently, the mucous membrane of the nose and of the pharynx swelled up to a tremendous degree. Eventually, the lymphoid tissue became infected in the same way and the patient apparently died rather suddenly, of suffocation. Within three or four days—in the severe cases—I took three of the children to Professor Haffkine's laboratory, and he investigated the cases most carefully, and finally decided that he could find no evidence of their being plague. At first I thought they were some peculiar cases of diphtheria, but diphtheria is not very common. It does occur during the rainy season, but even then it is not very common. The cases then passed out of my sight—they came to the out-patient department of my hospital and passed out of my sight and went to reside in Mazagaon. From the Health Officer, Dr. Weir, I understand that they set up a very severe epidemic in that region, that they were a focus of a severe epidemic of plague in Mazagaon.

20,395. How many cases did you see?—I saw four.

20,396. In one family?—Yes.

20,397. Did you trace the source of infection?—They came from Thana.

20,398. Was there plague there at that time?—Yes.

20,399. Have you any evidence of any direct contact or relationship?—There were other similar cases occurring in the same house and also cases of plague.

20,400. The important point appears to be that these cases communicated plague to some others?—Yes, and they came from a plague infected centre.

20,401. I think you said that Professor Haffkine did not find the plague bacillus?—He found no plague bacillus. He investigated the blood and the discharge from the nose.

20,402. Have you observed any cases in which direct inoculation of plague could be proved?—We have had one case in which we could positively say there was inoculation, and that was one of the Roman Catholic Sisters at the Parel Hospital, Sister Elizabeth. Her's was evidently a case of auto-inoculation through a pimple or small boil; there was pain and swelling in the region of the boil. In other cases I have seen bullæ formed evidently at the site of inoculation.

20,403. Why evidently?—Because the general symptoms of plague developed afterwards, and because the glands in connection with the distal lymphatics of that region were inoculated.

20,404. From the cases which you have observed, will you give us the usual symptoms of plague?—The symtoms, of course, depend upon the region of inoculation. Where it is through the lymphatic system we get the bubonic form, which is the most common, of course. There are statistics in the two reports* of the Plague Committee, giving the proportions of the bubonic cases in the various hospitals. Some have a greater proportion than others. Then there is the infection direct into the blood-stream, the septicæmic form. These cases are not so frequent, but it is evident that they do occur, because cases have been met with, and I have seen them myself in the hospitals, in which the symptoms are those of general sapræmia. In fact, I think, all these classes of the disease very closely resemble septicæmia, as we understand it, both in the pathology and development of the symptoms. Then there is the other infection through the lungs which produces a distinct form of lobular pneumonia. The first case which came under my notice was the late Dr. Manser, whom I attended in association with Dr. Childe.

20,405. In what respect is it a distinct form of lobular pneumonia?—I mean it is unusual to find lobular pneumonia occurring primarily in a healthy adult. The lobules could be percussed out.

20,406. What is the character of the sputum?—It is sanguineous, of a peculiarly watery consistence, and contains a pure culture of bacillus. You simply have to stain it and put it under the microscope and you can see crowds of the bacillus quite plainly.

20,407. Now with regard to the symptoms?—I have mentioned the following in my précis of evidence:—(*a.*) Pyrexia, 103° to 107° by third day. Toxæmia more marked, with onset of pyrexia. Glands early enlarged, acutely tender, painful, (femoral, inguinal, cervical, axillary). Great nervous prostration in many cases. Pulse soft, quick, small, compressible. Delirium often violent. Vomiting, sometimes severe; silvery tongue characteristic; constipation. Dusky congestion of skin and conjunctivæ; appearance of face characteristic. Lethargy. Death usually sudden from syncope; cases vary in severity; fatal in about five days; recovery by long convalescence. Later cellulitis around glands and suppuration in a large proportion. (*b.*) Pyrexia high; rapid asthenia fatal in a few hours up to two days; always fatal. (*c.*) Lobular pneumonia; pyrexia not very high; toxæmia marked; rapid asthenia. (*d.*) Choleraic form.—No pyrexia; extreme collapse: diarrhœa; no cramps; death in a few hours. Peritonitic form.—Great and early distension of abdomen; dusky appearance; marked toxæmia and asthenia rapid and marked. Both forms always fatal. Direct infection of lymphatics in majority of cases; development of toxine in glands and discharge of same into blood; bacilli frequently not detected in blood. In septicæmic form organisms pass directly into blood; toxines and bacilli develop in great intensity. In pneumonic form toxines manufactured in air cells and absorbed by capillaries and lymphatics.

20,408. What do you say about hospital mortality?—The general mortality was 65 per cent. I take that from the general results of our report. In cases without buboes the mortality was about 75 per cent., and in treated cases 41 per cent.

20,409. I suppose that has come under your own observation?—As an administrative officer I simply observed the general results of the hospital. I was constantly going through the hospital.

20,410. This is one out of a great number of similar statements in that† report?—Yes; it is one large Plague Hospital.

20,411. Then it does not show generally what the hospital mortality is?—I have taken it—the figures of the hospital—as representative of the general mortality.

20,412. Will you give us your views as to the method of infection?—It is by direct infection of the lymphatics in the majority of cases, generally from a plague patient. In the large proportion of cases, I think, human beings are the carriers of the disease to human beings. In certain instances, there is no doubt that rats will communicate the disease, and diseased rats may possibly start an epidemic. I am almost inclined to think that rats were the original cause of the epidemic in Bombay. I have a letter from a large firm, Messrs. Ralli Brothers, dated 11th September, 1896. It is a report to the Health Officer of the Bombay Municipality. They state that they had 13 fatal cases of fever among their native employés and coolies who were working in the godowns at the Ryan Market, Frere Road, which is close to the docks. That was on the 11th September, 1896. There was evidence of plague previous to that.

20,413. How does that bear upon the introduction of plague into Bombay?—It is an inference from those cases. Before those coolies were attacked, large numbers of rats were found dead in the go downs.

20,414. That was in September?—Yes.

20,415. But plague began here in July; therefore, this does not bear upon the introduction of plague?—It is merely an inference.

20,416. What definite evidence have you that rats may convey the plague?—I have given a few examples in my précis. Frequently cases of plague developed in houses, and when the houses were searched dead rats were found. In another instance from those quoted in the précis, in a house in the Fort the residents reported that rats were dying in the house. We recommended them to evacuate that house and have it thoroughly overhauled. They were Europeans and they did not do so. A few days afterwards some of the servants were attacked with the plague, and a few days after that one or two of the Europeans were attacked with plague.

20,417. Can you exclude human conveyance in that case?—No.

* *See* Appendices Nos. XIII. and XIV. in Vol. I. of the Commission's Proceedings.

† "Report on the Bubonic Plague in Bombay, 1896–97:" by Brigadier-General W. F. Gatacre, C.B., D.S.O., Chairman of the Plague Committee.

20,418. Have you anything which absolutely shows that rats communicate or convey the disease?—The only definite case is that of the servant boy, a Goanese who removed a dead rat from the floor and burnt it; he was attacked with plague four days afterwards.

20,419. Might he not also have come in contact with a human subject?—Yes, he might.

20,420. Have you anything more definite?—No; one has nothing really definite.

20,421. So far as human conduction is concerned, what are the most favourable conditions for it?—Overcrowding in the small insanitary rooms in which the natives live. That is the primary cause, I think.

20,422. Did the people suffer most who lived on the ground floor or who lived on the upper floor?—On the ground floor.

20,423. Do you know whether any other animals besides rats may be infected with plague?—Cats have been found to be affected.

20,424. What was the evidence with regard to cats?—Dr. Sticker found the bacillus in cats. The cats were found suffering in the Phatak Wadi (Market Section). They were sent to Professor Sticker who found the plague bacilli.

20,425. What were the symptoms of plague in those cats?—The cervical glands are mostly affected; they are enormously swollen. There is œdema in the mucous membrane of the mouth, the throat, the pharynx, and they died of suffocation. They probably got it from killing infected rats.

20,426. Have you known other animals to be affected?—I have not met with instances of any other animals.

20,427. How long do you think a patient who has recovered from plague may remain a source of danger?—That has always been a very difficult point with me. When we were working on the Plague Committee it was a very important consideration so far as I was concerned, with regard to convalescent patients. I considered six weeks or two months ought to elapse before the plague patient was allowed to mix with people, but I have never been able to obtain evidence—although I have tried—as to whether convalescents would give rise to infection.

20,428. It is pure supposition?—I will not say it is supposition; I could not say it has not actually occurred.

20,429. Can you say it has?—I cannot say it has, but I should think it must be probable; they may be compared to contacts, and should, therefore, be segregated for a certain time. I am inclined to think that some epidemics at a distance, which have been hitherto unexplained, may have originated from convalescents.

20,430. What are your views as to disinfection?—I think it is effective if it is possible to carry it out entirely. If an infected room is thoroughly disinfected, I think that you may certainly be able to dispel the disease.

20,431. Have you any cases in which a room thoroughly disinfected has been reoccupied and plague occurred afterwards in that room or in that house?—There was an epidemic in Koli Wadi. The total number of attacks was 16 from February 21st to February 29th 1898. On the 17th March the whole of Koli Wadi was vacated.

20,432. Is that a locality?—Yes, a collection of houses where the fishermen live. A Koli is a fisherman. The evicts were sent to the Wari Bandar Camp for 10 days, and the whole of the district was then thoroughly disinfected and the roofs all opened out. There was no case afterwards for six months, although it was reoccupied.

20,433. How long after evacuation was it reoccupied?—It was reoccupied after 10 days.

20,434. There were several things done here; can you distinguish chemical disinfection and disinfection by the mere access of air and light? Have you any instances in which mere chemical disinfection was resorted to?—In houses with stone floors it has been noted. One Parsee chawl, Lalbag in Parel, was thoroughly disinfected by chemical disinfection. It was a good chawl with stone floors. The people reoccupied it under the same conditions that they had occupied it before and the disease did not occur.

20,435. What disinfectant was used?—Perchloride of mercury, one in 1,000.

20,436. How many cases of plague had there been in the chawl before?—I do not remember the exact figures.

20,437. Do you recollect whether it was a large or a small number?—I suppose about eight or 10 out of about 300 people.

20,438. Each chawl is a separate building, I suppose?—Yes.

20,439. Have you any instances in which houses or rooms have become reinfected after disinfection?—I am afraid a considerable number of the houses were reinfected.

20,440. Have you any evidence in which evacuation of houses appears to have been beneficial?—We have had many instances where evacuation has stopped plague.

20,441. Which of these measures which we have been talking about appears to be most effective in your experience?—I think there is nothing so effective as evacuation. Its effect is immediate, as I think I have shown by the statistics of the Wari Bandar contacts.

20,442. Perhaps you will put that statement in?—Yes, it is as follows:—

STATISTICS of the WARI BANDAR CONTACT SEGREGATION CAMP relating to the period from 1st January 1898 to 30th April 1898:

Total number of contacts admitted	5,790
Average daily admission	48·25
Average weekly population	2196·1
Average daily population	313·7
Maximum number of contacts in camp, on 5th March 1898	1,060
Total number of cases that developed plague	148
Percentage of plague cases over the number admitted	2·55

Out of these 148 cases of plague—

there developed symptoms on the day of admission			46
,,	,,	1st ,, after ,,	35
,,	,,	2nd ,, ,, ,,	19
,,	,,	3rd ,, ,, ,,	16
,,	,,	4th ,, ,, ,,	11
,,	,,	5th ,, ,, ,,	10
,,	,,	6th ,, ,, ,,	7
R.C. ,,	,,	7th ,, ,, ,,	1
R.C. ,,	,,	9th ,, ,, ,,	1
R.C. ,,	,,	12th ,, ,, ,,	2
		Total	148

The cases marked as "R. C." were amongst the re-contacts. They were in contact with a plague case occurring in the camp and were segregated for a fresh period of seven days. The period of segregation was at first 14 days, then ten days, and finally seven days, except in the cases of re-contacts.

20,443. Were you occupied in plague work here at the commencement of the epidemic?—Not altogether from the commencement, but I saw a good deal of it in the commencement.

20,444. Until what time?—I was on the Plague Committee from the 3rd March 1897 to the 7th May 1898.

20,445. Can you tell us what measures were adopted at the outset of the epidemic?—Only from hearsay. I was not associated with them.

20,446. What were the measures adopted during the time you were a member of the Plague Committee?—Segregation of the sick out of their dwelling-houses, removal of all persons from the room in which the patient was seized or had been lying sick, evacuation of the whole house if possible, destruction of the clothing, bedding, etc., which had been in use by the patient, thorough disinfection of the house, thorough cleansing and flushing of all privies and bathing places, and removal of all portions of the roof where such obstructed the sunlight and air entering the dwelling. The prevention of overcrowding was also considered of great importance. In the Second Committee, under Sir James Campbell, we modified those measures by establishing contact camps. The people who had been in absolute contact with the patients were segregated in a separate camp. These measures, I believe, are now adopted all over India; they were worked out originally by the Bombay Plague Committee.

20,447. They have been very successful?—Yes.

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20,448. What do you consider is the period of segregation?—I think, from the contact camp statistics, eight days at the outside. This seems a justifiable period from the figures. A comparison between the conditions of the people in camp and in their houses indicates the probably much greater infectiveness of the latter—confirms the opinion of the greater danger of plague-stricken human beings to others—illustrates how the epidemics spread—and helps to justify the measures adopted. I think the contact camp statistics are most reliable, because the contacts are persons who are most liable to infection, and they are the most likely people to give infection. I think that statistics from contact camps will cover all cases.

20,449. How do you account for the fact that plague stops when people are removed to segregation or contact camps or to hospitals as contrasted with the progress of plague when they are left in their own houses?—I think you have a barrier of fresh air between each person. The bacillus can only extend within a certain radius, and can only live under certain conditions such as darkness, dampness, and moist atmosphere. If you remove the people from the vicinity of such conditions you prevent them from becoming infected.

20,450. You have had a good deal of experience with regard to camps?—Yes.

20,451. You give the details of a considerable number in the supplement of the précis which you have handed in?—Yes. They are as follows:—

Camp.	Admissions.	Plague Cases.
Narel Wadi	16,385	40
Modikábna	24.330	64
Wári Bandar	23,542	90
Byculla	14,400	—
Dádar	1,017	2
Byculla Bridge	400	8
Narel Wádi Health Camp	924	6
Kennedy Sea Face	964	5
Goculdas Tejpal	1,560	12
Elphinstone	1,500	—
Cruickshank	192	—
Elphinstone Bridge	783	3
Ellapa Ballaram	1,447	16
Mahratta	1,839	31
Total	88,723	277

These figures further confirm the opinion—(1) That a very small proportion of plague cases occur amongst people in camp, even when these are from the most heavily infected parts of the city, as evicts certainly are. (2) That a very large population was passed through camp in a very short space of time and on the basis of (1) this alone must have had a great and inestimable effect upon the epidemic. (3) That even in Bombay, with the greatest difficulties in the whole of India to contend with in such a scheme, the camp system can be carried out for a great part of the year with advantage. (4) That the camp system is the basis of all plague operations and that it is the only procedure which has an immediate benefit, for plague disappears amongst new arrivals into camp within a week, *i.e.*, if primary cases in camp are properly dealt with. (5) That the incidence of plague at one time falls with greatest severity upon sections of the population, and that these sections vary in the extent to which they suffer according to the degree of overcrowding and the nature of the houses. The method of combatting plague in a large city must be by attacking sections of the greatest plague incidence. (6) That the small proportion of plague cases amongst detentions indicates that where no detention exists, the influx of persons from an infected to a non-infected city brings only a small proportionate number of plague cases, but that, from these lying *perdu* in the the slums, an overwhelming epidemic will arise within two months unless prompt and severe means are taken. This, consequently, establishes the absolute necessity of strict detention, and, further, confirms the opinion of the great danger of infected human beings as carriers of plague.

20,452. You have had some experience with regard to inoculation?—Yes.

20,453. What are the data on which your views with regard to inoculation are founded?—My views are based on actual experience of 149 cases. When M. Haffkine first came to Bombay I saw a good deal of his work in the laboratory and I assisted him occasionally in doing his inoculations, and I have had conversations with him and have a general conviction as to the scientific value of his inoculation.

20,454. Will you give me the data?—One hundred and forty-nine cases were inoculated. There had been 12 deaths from plague among various households before inoculation. After inoculation there were four attacks of plague: two died and two recovered.

20,455. Did you observe these cases which you inoculated for some period of time?—I have the evidence for the whole year.

20,456. Are these figures borne out by the after-history?—Yes; I have enquired about them myself.

20,457. Have you seen any evil effects following inoculation? Do you adhere to your statement with regard to that?—Yes.

20,458. Will you put that statement in?—Yes. The possible evil effects are (*a*) Initiates disease by putrescence, only through careless manipulation, *e.g.*, cellulitis, septicæmia. (*b*) Initiates disease by entrance of the minute particles of débris into the circulation, which would give rise to endocarditis embolic. I have met with one case which gave rise to this suspicion. A Pársee girl, aged about 14, became ill after inoculation, and died two months later. I saw her a few days before death and found her suffering from endocarditis. I have discussed the case freely with M. Haffkine and asked him if the fluid could be filtered in view of this danger. The history of the case throws doubt on the contention that the endocarditis actually resulted from inoculation. She had suffered from fever before, the family have a rheumatic tendency, and I have recently seen a brother of her's, aged nine, who has an irritable heart attributed to over-exertion. M. Haffkine's reply to my question was as follows:—"The solid part in the plague prophylactic is a very "essential ingredient and cannot be filtered off. You "will see that at once when I say that the anticholera "vaccine contains only the solid part and that the "chief modification in the plague prophylactic was the "addition of the liquid portion. The above informa-"tion will also allay, I trust, the apprehension with "regard to the danger of those solid bodies. For if, "in the inoculation against plague, we did, by now, "tens of thousands of operations, P. being the only "patient about whom the question was raised,—in the "inoculation against cholera the number of operations "counts by hundreds of thousands, without my "having heard of the complication now in question." (*c*) Excites latent disease to active manifestation, *e.g.*, rheumatism, phthisis, neuritis, hepatic congestion, albuminuria. I have no authentic data of this. I have met with cases of rheumatism and neuritis which have occurred shortly after inoculation and were attributed to it, but I could not say that the inoculation caused the complaints. There was one remarkable case of a dhobi in Byculla, found during a house visitation and apparently very ill. He had acute pain in the side, and the search party thought he was a case of plague. He had fever. He had been inoculated a few days before. I found it to be simply a case of acute neuritis of the intercostal thoracic nerves. (*d*) Produces varying degrees of impaired health subsequently and for varying periods. Many persons have complained of this, but as a rule it does not last longer than ten days. My own máli was ill for months afterwards with gastric irritability and great depression. Most of the persons I inoculated were able to go about their work in three days without inconvenience.

20,459. You have made frequent observations at five intervals in many of these cases which you inoculated?—Yes. All my own servants were inoculated.

20,460. What symptoms did you observe to follow inoculation?—Pain and swelling of varying degree at the site of inoculation. A preceding period of depression, nausea and sometimes diarrhœa, with pains in the muscles and in the joint if the subject has a tendency to rheumatism. A stage of reaction when the temperature rises to 102° or 103°. In some cases the depression continues and a sub-normal temperature is registered with no pyrexia. The depression lasts for 12 or 24 hours or in some cases longer, and appears in about 12 or 16 hours after inoculation. There may be acute gastric and intestinal irritation. A stage of

subsidence, marked by a fall of temperature, free action of the skin and somewhat depressed state of health, may last for a day or two longer.

20,461. What is your view about the duration of the immunity?—I believe the duration of the immunity lasts a considerable time, and if fully carried out I do not see why it should not be for a life-time. As in the case of other infectious diseases complete immunity is not obtained but general comparative immunity. A person may contract plague after inoculation, but this is in a much redued probability, and if contracted, the disease is not so likely to be fatal. I have no authentic data, but I consider the statistics published by M. Haffkine fully reliable, and they support this view. I do not see why it should not be for a lifetime provided the person is thoroughly inoculated.

20,462. Why do you say that?—I think it is possible it might be.

20,463. One might equally say it is possible it might not be. Have you any facts to lead you to any conclusion as to the duration of the immunity produced by inoculation?—In one or two households which I inoculated the servants have been free from plague up to the present time.

20,464. To what length of time do you refer?—In my own household, during the last epidemic, the wife of one of the servants contracted the disease. She had never been inoculated. Her husband and his brother, who had been inoculated the previous year, were in constant attendance on the girl and never contracted the disease. They went to the Parel Hospital with her and were attendants on her there.

20,465. The immunity lasted one year, you think?—Yes.

20,466. Is it usual for persons who go to hospital and attend on sick persons to contract the plague?—No; but before they went to the hospital the girl had been suffering for two days. It was not reported to me, so that they were in close association with her for two days.

20,467. Are the hygienic conditions bad in your servants' quarters?—No.

20,468. I presume they are good?—Yes.

20,469. Is it usual for plague to extend in good hygienic circumstances?—It is not usual, of course.

20,470. I assume that that house is in a good hygienic condition with regard to ventilation and light?—Yes, very good.

20,471. Have you seen any cases in which after inoculation with an ordinary dose of the fluid the temperature of reaction appeared to be very slight?—I saw a good many of those when I was inoculating for Professor Haffkine in the Grant Medical College. A good number of the cases came and said they had no fever, but felt very depressed, and their temperature had been sub-normal. They were intelligent people and had taken their temperature, and in some cases it was 97—sub-normal. They said the skin had been moist, and they had felt a good deal depressed, but there had been no febrile reaction.

20,472. Have you any evidence that there was an immunity conferred upon those persons?—No, I cannot say that.

20,473. Did the same sample of fluid produce a marked temperature reaction in other persons?—Yes.

20,474. (*Mr. Hewett.*) Have you found that in the quarters of Bombay which have good hygienic conditions the plague has not spread?—The Europeans and their native servants living on Malabar Hill lived under the very best hygienic surroundings, but yet the servants were attacked there and the epidemic spread all through the servants' houses, although very few Europeans were attacked.

20,475. Your servants were under the same conditions as the other servants on Malabar Hill?—I think so, practically.

20,476. Therefore, they were just as liable to attack?—Yes.

20,477. And the servants, generally, on Malabar Hill did not escape?—That is so; the disease spread from one end of Malabar Hill to the other.

20,478. Although the Europeans living in the same compound escaped?—Yes.

20,479. (*Dr. Ruffer.*) What do you mean by the abdominal type of the disease?—Evident symptoms of plague from the general clinical appearances, associated with great distension of the abdomen.

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20,480. What sign of plague was there in those patients? Had they buboes?—No.

20,481. How do you know they had plague?—From association with other plague cases.

20,482. Did you ever find the plague bacillus in any of those abdominal cases, or did you simply base your diagnosis of plague upon the fact that they were in contact with other cases?—I based my diagnosis principally on my familiarity with the general clinical aspects of the disease.

20,483. What were the clinical symptoms which led you to diagnose those cases as cases of plague?—The rapid onset of the disease, the high temperature, and the peculiar appearance of the patients, the congested eyes, and the facial aspect, the condition of the tongue and such like clinical symptoms. These were associated with very rapid development of abdominal distension.

20,484. Had they any buboes in the abdomen?—That I cannot say. We were not allowed to make a *post-mortem*.

20,485. Had they any intestinal catarrh or diarrhœa or anything of that sort?—No; there was no diarrhœa.

20,486. Did you examine the intestine after death?—No.

20,487. You made no bacteriological examination of any of those cases?—No.

20,488. You said that Sister Elizabeth had a pimple and got plague: where was that pimple?—It was near the groin.

20,489. How could she inoculate herself there? By scratching?—Yes; I think it was a mosquito bite, but I am not exactly sure. It was near the thigh.

20,490. What sort of pimple was it,—an acne pimple?—It was an insect bite, I think.

20,491. Were you able to trace any lymphangitis from that pimple to the next gland?—Yes.

20,492. Where was the next gland?—Above the groin.

20,493. The pimple was below the groin?—Yes.

20,494. Did she die?—Yes.

20,495. How long was the incubation period in her case?—I think four days.

20,496. You are not sure?—No.

20,497. Do you generally find the lymphatics leading from the point of inoculation to the next gland are enlarged? Supposing you get a bulla on the dorsum of the foot, do you find the lymphatics leading from that to the popliteal space or the groin are enlarged?—Yes.

20,498. Is that the rule?—It certainly was in the earlier cases I saw.

20,499. Are you in charge of any hospital now?—Yes, but not of any Plague Hospital.

20,500. In your précis you say "Tara Naikand's "Wadi in Dhobi Talao—house opened up; no disinfec- "tion, people suffered severely." Can you give us any details about that case?—I am afraid I cannot give you that.

20,501. You mean to say there was no disinfection, but the house was opened up and that people afterwards suffered severely from plague; is that it?—Yes. I remember the case, but I cannot give you the particulars. The house was opened up and there was no disinfection; I cannot remember exactly the reason why it was not done.

20,502. The people went back to their houses?—No; they were not moved out at all; the house was simply opened up waiting for disinfection, and then the disease still continued.

20,503. They were left in the house?—Yes.

20,504. (*The President.*) What does "opening up" mean?—The roof was taken off, the windows and partitions knocked down, and so on.

20,505. (*Dr. Ruffer.*) Can you state the conditions which influence the development of the bacillus outside the body, whether heat, moisture, and so on, will influence its vitality? How do you think the bacillus

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lives outside the body? What has it to live on?—In these little rooms, of course, the people spit on the floor, and they are very insanitary. Besides that, the floors are generally of cow-dung, which is rich in organic matter. The people themselves are, of course, also insanitary.

20,506. But how can it develop on the floor in saliva or in cow-dung? Supposing you take a tube of agar, which is a good nutritive substance, and plant the bacillus in one spot, does it spread from that spot? Does it not remain localised to that spot?—I suppose the bacillus must develop within the radius of a plague patient somehow or other.

20,507. What evidence have you to show that the bacillus develops or spreads from the excreta or in any other way?—One has not any actual evidence. I have never heard of anybody who had.

20,508. What excretions, do you think, are most infectious?—The saliva in pneumonic cases.

20,509. Do you think any other excretions are infectious?—I think the excretions of the mouth and nose are infectious. The excretions from the nose were most virulent in the case I have quoted. I am also inclined to think that fæces are infectious.

20,510. Why do you think so? Have you any facts bearing on it?—No, I have not.

20,511. When were the inoculations done at Malabar Hill?—In February 1897.

20,512. How long did you have those servants under observation afterwards?—My own have been with me all the time up to the present.

20,513. There were 149 of them; how long have you had the others who were not your own servants under your observations?—A good many of them are under observation. I always go and visit at the people's houses.

20,514. How many servants have you of your own who have been inoculated?—I think about 12.

29,515. How many of these 149 do you think you have seen since inoculation?—Eighty; that is, about one-half.

20,516. Have you seen any cases of *pestis minor*?—I am seeing them now.

20,517. We should very much like to see one; we have been trying to find them?—I saw them in the beginning of the epidemic of 1896 in the out-patient department of the Jamsetji Hospital.

20,518. What are their symptoms?—Principally mild fever, with slight enlargement of the glands, which passes away in three days.

20,519. Which glands are enlarged?—The cervical glands.

20,520. Why do you think they were cases of plague?—I thought *pestis minor* because I could not explain them in any other way. I have seen two or three cases lately.

20,521. (*The President.*) How long ago?—Within the last month.

20,522. Can you produce any?—I will do so if I can.

20,523. (*Dr. Ruffer.*) In one of the supplements to the précis of your evidence, you mention cases of re-occurrence of plague. Can you give us the facts relating to Dr. Daver's case?—I have not the facts.

20,524. Did you see the Customs clerk who was supposed to have suffered twice from plague?—No.

20,525. You mention a man who had the plague three times; Narayan Muljee: are you satisfied he did have it three times?—I am satisfied he had plague once. I do not see any reason to doubt that he had plague three times from the general description. I quoted them because I was convinced the history was sufficient.

20,526. You say there were two cases of recurrence of plague; what was the other?—They are quoted on page 151 of General Gatacre's report.*

20,527. Did that come under your own observation? They are reported to me.

20,528. Did you enquire into them yourself?—I saw one of them.

20,529. In the same supplement you mention the case of Mr. Lalla, who was inoculated twice and contracted plague and died. You also say:—"The "following evidence is in favour of the theory of "immunity; 15 All Saints Sisters, European and "Eurasian nurses, were employed on plague from the "J. J. Hospital, 11 were inoculated. None contracted "plague except one European nurse amongst the "inoculated. It was a mild attack and she recovered;" did you see these cases yourself?—They were under my continued observation the whole time.

20,530. Can you tell me how many of the non-inoculated got plague?—Only one; Miss MacDougal. She was infected through the mucous membrane of the eye and died. A pneumonic patient spat in her eye. That is a case of infection through the mucous membrane.

20,531. Then you have some evidence with regard to the Cama Hospital nurses?—Yes. Eight worked for several months on plague duty in the different hospitals. Of these there were four inoculated and four uninoculated. Amongst the inoculated there were no attacks of plague. Amongst the uninoculated there were two plague cases, one of which died and the other recovered.

20,532. (*Prof. Wright.*) You spoke of a series of coryza cases; had any of these patients buboes?—The glands of the neck subsequently enlarged. They had no buboes in the first instance.

20,533. Did any of these patients die?—Yes, several.

20,534. Did you succeed in tracing a connection between those coryza cases and other cases of plague?—No. They came from Thana, an outlying station.

20,535. You said that these cases were the starting point of an epidemic on Mazagaon?—Yes; that information was given to me by Dr. Weir.

20,536. You did not follow these cases up yourself?—No.

20,537. (*The President.*) In reference to the inoculations you performed, have you any facts as to the history of those who were not inoculated? Did they have much plague or no plague after the others had been inoculated?—I cannot tell you that.

20,538. There might have been none?—There might have been none, but I think there was plague among them. Plague was going on very severely all round.

20,539. With regard to the servants who had plague on Malabar Hill, how do you account for their having plague and for the friends living in the same compound, whether Europeans or natives, generally escaping the plague?—By the different conditions of housing.

20,540. Will you describe the difference?—The servants have small rooms, and if they are left to themselves they close the windows at night and shut the doors and live in a close, confined atmosphere.

20,541. You attribute the infection in their cases to the inefficient ventilation and light?—Yes.

20,542. I thought you told us that in the case of your own servants the hygienic conditions were good?—They were very much better than the general conditions of the ordinary natives.

20,543. Better than those you now refer to as the average conditions on Malabar Hill?—No; I think they are about the same all over Malabar Hill.

20,544. I thought you told us that the average conditions were bad. I suppose you admit that they are as bad as you have described them?—Yes.

20,545. But you previously told us that, in reference to your own servants, the conditions were good. How do you reconcile these two statements?—I mean that the conditions were good provided that the people themselves maintained those conditions.

20,546. Is it within your own knowledge that the servants in your compound did maintain good hygienic conditions in their houses in regard to air and light?—Yes, they did.

20,547. And they contrasted favourably with the general standard?—Yes.

(Witness withdrew.)

(Adjourned till to-morrow.)

* Report on the Bubonic Plague in Bombay" by Brigadier-General W. F. Gatacre, D.B., C.S.O., Chairman of the Plague Committee, 1896-7.

At The Secretariat, Bombay.

FIFTY-THIRD DAY.

Wednesday, 22nd February 1899.

PRESENT:

PROF. T. R. FRASER, M.D., LL.D., F.R.S. (*President*).

Mr. J. P. HEWETT.
Prof A. E. WRIGHT, M.D.
Mr. A. CUMINE.
Dr. M. A. RUFFER.

Mr. C. J. HALLIFAX, (*Secretary*).

Khan Bahadur N. H. CHOKSY called and examined.

Khan Bahadur N. H. Choksy.

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20,548. (*The President.*) What are your medical qualifications?—I am a Graduate of Medicine of the Bombay University.

20,549. Are you in the employment of Government?—No, I am in Municipal employment.

20,550. You have been in charge of Arthur Road Hospital ever since the plague began?—Yes.

20,551. (*Dr. Ruffer.*) How many cases of plague, altogether, have you seen?—Altogether nearly 4,000 cases have been under me; most of them have been seen by me personally.

20,552. You are of opinion that there are several types of plague?—Yes.

20,553. *Pestis minor, pestis ambulans, pestis bubonica, pestis septica, pestis pulmonaria* and non-typical forms of plague?—Yes.

20,554. Do you think these types merge into one another, or are they distinct?—I think they are quite distinct except in the case of the septic type where the simple bubonic form merges into the septic.

20,555. Do you think there is a special form of pneumonic plague?—Yes, there is.

20,556. Could you give us the symptoms of *pestis minor*?—The symptoms commence with a little febrile re-action lasting for a day or two accompanied by malaise, pain and tenderness and one gland may be enlarged. After a little rest in bed and ordinary treatment, or practically without any treatment, the patients recover.

20,557. How do you diagnose these cases from other bubonic enlargements?—Because they occurred at the time when the epidemic was amongst us, and they have not been observed in other periods.

20,558. Supposing you had no plague in the town, would you still be able to recognise this form of the disease?—From the experience we have now had, we should certainly look upon these cases with suspicion.

20,559. Suppose there had been no plague in the city of Bombay for two years, would you be able to diagnose a case of *pestis minor* without bacteriological examination?—I am afraid not.

20,560. What is the difference between such cases and *pestis ambulans*?—It is the difference of degree. In *pestis ambulans* you will generally find a patient with one or more enlarged buboes going on for a long time, and the buboes either remain as indolent swellings, or they sometimes suppurate after about two, three, or four weeks after the initial symptoms.

20,561. How do you diagnose a case of *pestis ambulans* from any other form of bubonic enlargement?—We exclude all other chances of error, such as venereal contamination or septic poisoning, or wounds or abrasions, and thus we arrive at the diagnosis by a process of exclusion.

20,562. I will put it to you in this way; there were soldiers, in the Shropshire Regiment for instance, with enlarged glands going on for five, six, or seven weeks, or even longer; how could you diagnose a case of *pestis ambulans* from one of these cases?—We take into consideration whether they have been in localities which have been infected, or whether they have come in contact with plague patients, or whether they have remained in areas which are infected, in which case they will certainly be looked upon as suffering from *pestis ambulans*.

20,563. The history of the case or the bacteriological examination are the only data on which to form a diagnosis?—I am afraid bacteriological examination would not be of very much use in these cases except, perhaps, in early stages—perhaps for the first day.

20,564. Then your means of diagnosis are limited?—We diagnose by the clinical features and the surrounding history of the patient.

20,565. Do cases of *pestis ambulans* have initial fever?—Yes, they have.

20,566. Have you ever made bacteriological examination of such cases?—No.

20,567. How do you know that they are cases of plague?—From the enlargement coming on suddenly with a little feverish reaction during the epidemic of plague, and when there is no other cause to account for it, no local injury or venereal taint.

20,568. What is the end of these cases? Does the bubo suppurate, or does it resolve without suppurating?—Sometimes they suppurate and at other times they remain like indolent swellings for a long period.

20,569. How long?—I should say sometimes for months together.

20,570. Without suppuration?—Yes.

20,571. What do you consider to be the simple bubonic form and the simple septic form of plague?—There is the difference of degree only with regard to the symptoms of the simple bubonic and the septic type. The symptoms which are described in the Report* all through apply generally to the ordinary bubonic plague. The septic form, of course, is more aggravated with extensive infiltrations and is more fatal.

20,572. In the pulmonary form of plague the lung symptoms are predominant?—Yes.

20,573. Then you have non-typical forms of plague in which you have slight fever, diffused swellings with more or less infiltration generally over the parotid and cervical glands, absence of glandular enlargements, and pain, and gradual glandular subsidence from the application of cold and the internal administration of calcium chloride. How would you diagnose such cases as plague?—Just in the same way as the first, *minor* and *ambulans pestis;* they occur during the time of the epidemic or during its wane.

20,574. Could you diagnose them unless there was plague about, or without a bacteriological examination?—The diagnosis, I must say, is inferential only: we have made no actual bacteriological examination of these cases.

20,575. Have you ever seen persons contract plague from such cases?—No.

20,576. How do you know for certain that these cases are plague?—They occur during the epidemic, they have a history of exposure to plague infection, they

* Report on Bubonic Plague, upon Observations of 939 Cases treated at the Arthur Road Hospital, Bombay, from 24th September 1896 to 28th February 1897, by K. B. N. H. Choksy

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have lived in plague-infected areas or houses and they also have fever.

20,577. Have you seen cases of mixed relapsing fever and plague in the same patient?—Yes.

20,578. Have you found in these cases the micro-organisms of relapsing fever and of plague?—Yes, in the same patient.

20,579. Both in the blood?—Yes.

20,580. Are these cases usually fatal?—No, they seem to run a more or less milder course, and there are more recoveries, when there is a mixture of these two diseases, than otherwise.

20,581. Have you ever seen cases of relapsing fever and diarrhœa?—Yes.

20,582. In your opinion are these the cases which have been described as abdominal plague?—I think so.

20,583. Why?—Because I must say I have not up to now seen a single case which could be called a typical case of abdominal plague, a case in which there are no external buboes, and no other symptoms except purely abdominal symptoms. I have not seen a single case of that typical form up to now.

20,584. Have you ever seen cases of cholera simulating plague?—No.

20,585. Have you ever seen cases of relapsing fever together with diarrhœa which looked like cholera?—Yes, very often relapsing fever patients have been sent to the hospital as cases of cholera, and on examination they have been found to be cases of relapsing fever and not of cholera at all.

20,586. What do you think is the ordinary mode of infection in plague?—The most frequent is infection from the skin

20,587. What evidence have you to show that?—The evidence is, of course, seen in the preponderance of the buboes in the inguinal and femoral regions in those people who generally go about barefooted.

20,588. Have you examined the feet of all the patients who had inguinal buboes?—Yes, I have examined them very frequently with scarcely satisfactory results as regards the local re-action except in a few cases.

20,589. In how many cases do you think you have found local reaction?—Not more than five or seven per cent.

20,590. What do you mean by local re-action?—I mean something to show the point of entrance of the virus into the system—an abrasion or any channels of infection in the skin itself.

20,591. Would you consider an ordinary scratch a local lesion?—In this sense, that there is a channel for absorption from that.

20,592. Do not you find in a great many of these cases scratches round the outer malleolus? Do you not find scratches there from the irritation of mosquito bites and so on?—Yes we do.

20,593. Is it not possible that plague might enter through a scratch and leave no local lesion?—Yes.

20,594. I understand that many patients have cracks and fissures on the soles of their feet and elsewhere?—Yes.

20,595. Do you not think plague might come through them?—Yes, it might.

20,596. Without giving rise to any local inflammation?—Yes.

20,597. In a certain number of cases you have found small pimples on the dorsum of the foot?—Yes.

20,598. What did you find inside these pimples?—Bacteriological examination was made on two or three occasions and almost pure cultures of plague bacilli were taken out from them. We made an experiment in some cases by injecting pure carbolic acid into the pimples; the result was there was a sudden cessation of all inflammatory signs in the bubo; the fever subsided, and the patients seemed to show a rapid rate of recovery.

20,599. When you found a pimple on the dorsum of the foot, was the bubo already formed?—Yes, the bubo was already formed, and the history always was, that the pimple preceded the bubo by two or three days.

20,600. When you treated the pimple the bubo disappeared?—It gradually got smaller and all the symptoms abated.

20,601. Do you think the bubo is the sign that the plague-bacilli have invaded the gland already, or do you think that the bubo is due to the absorption of toxines?—That would be difficult to say, except by actual examination of the bubo itself.

20,602. Does not the fact that the antiseptic treatment of the pimple cuts short the disease, point to the possibility that the bacilli have not yet reached the gland?—I should think so.

20,603. You have no facts bearing upon that?—No.

20,604. Have you ever found blisters or primary sores at the point of inoculation?—Yes.

20,605. What are these blisters like?—They were blisters, either with a spherical cuticle over it, or with distinct umbilication on the top. In those cases the appearance was quite peculiar. It seems just as if a circle of glass had been put over the skin and a membrane tied over it, and the umbilication resembled something like that of magnified smallpox eruption.

20,606. Did you find plague bacilli in them?—Yes, I think they were found; I did not make any investigation personally.

20,607. Did you find pus?—No.

20,608. What was the fluid like?—It was serous fluid, more or less stained with blood.

20,609. How long do you find bacilli in the bubo?—Up to the period of suppuration and a little after.

20,610. I suppose at first there are a great many bacilli?—Yes.

20,611. Are they found not only in the bubo, but round the bubo also?—I have no experience of that.

20,612. You say the bacilli disappear when suppuration sets in?—After a time.

20,613. How long after?—Two or three days after the buboes have been incised we found the bacilli almost disappear.

20,614. How did you ascertain that—by cultivation?—By cultivation.

20,615. Do you find that other micro-organisms take their place?—Certainly. The ordinary microbes of pus must be there.

20,616. I want to know whether you found the pus microbes in the bubo before or only after suppuration?—I have not made any investigation previous to the incision of buboes.

20,617. In your opinion the sputum is infectious in cases of pneumonia?—Yes.

20,618. Is it infectious in any other cases as well?—I have seen a case only lately where there was nothing more than ordinary bronchitis, when the sputum was bronchitic, and still the plague bacilli were found in almost pure cultures.

20,619. Do you find it, in septicæmic cases, as a rule in the sputum?—Yes.

20,620. Do you find it in all cases in which you get secondary affection of the lungs?—If there is any infectious sputum it is found; but I have made no distinct investigation with regard to that.

20,621. Supposing a patient does not spit but has a slight cough, do you find the bacilli in his sputum then?—I have made no investigation with regard to that. I may be allowed to add that most of my practice has been clinical; I have had very little time, and have done very little bacteriological work myself.

20,622. Do you think the disease is contracted by inspiration?—Yes; it must come through inspiration in cases of pulmonary plague.

20,623. Why?—Because the lungs are the primary seat of infection, and we find the absence of plague-bacilli in the blood.

20,624. Supposing you get a cut on the foot and get a bubo in the groin, you find no bacilli between the foot and the groin; supposing on the other hand, instead of having a bubo in the groin, the secondary lesion was in the lungs, *i.e.*, plague-pneumonia, would you find bacilli either in the blood or in any other organ?—I have made many examinations with regard to the blood, and have found that in some of the most typical cases of plague which have proved fatal there was a total absence of plague-bacilli in the blood from cultures.

20,625. How do you think the plague-bacillus is inspired into the lungs? I believe that you agree that it is not to be taken by the digestive canal?—Yes.

20,626. How does the bacillus get from the mouth into the lungs?—It must go with the inspired air. Besides, there is a certain amount of desiccation when the sputum is thrown about and gets dried up.

20,627. You think it is the dry sputum which is inspired with the air?—Yes.

20,628. Have you any evidence to show that the bacilli of plague will resist dryness?—I have no personal knowledge. There is this to be said, that the class of people from which we draw our patients are in the habit of spitting about anywhere and everywhere, in any part of the house. You will find that the sputum must lodge in the different parts of the room, and you will find it in various stages of dryness or moisture; it is quite likely that some of that may be absorbed, and produce the symptoms.

20,629. You have no evidence to show that plague-bacilli resist dryness?—I have made no experiments with regard to that.

20,630. Have you any evidence to show that either the fæces or urine are infectious?—No. Examinations were made, but we did not succeed in getting any plague cultures out of them.

20,631. You do not believe in the possibility of infection through the alimentary canal?—No.

20,632. Why not?—I have seen no evidence to support that view.

20,633. You have seen nothing like an abdominal case of plague?—That is so.

20,634. Have you ever seen primary lesions which you think were primary in the intestines of plague patients?—No.

20,635. Do you think that the vomited matter is infectious?—I do not think so.

20,636. Has not that a bearing upon the possibility of infection by grain?—I think it has indirectly; if the grain were the channel through which the plague germs entered, the primary symptoms would be in the intestinal tract or abdominal cavity.

20,637. Do you think the grain infectious?—No, I do not think it is infectious.

20,638. Some people say it is. For the sake of argument, we will assume the grain is infectious; how can it be infectious?—In the first place, most of the grain which is used is cooked; therefore, seeing that heat is destructive to the plague bacilli, there can be no infection, the bacilli would be destroyed.

20,639. Then the only way in which grain could be infectious is by handling it, the bacilli entering through cuts in the fingers?—In that case there would be no possibility of its entering directly into the alimentary canal.

20,640. Have you any evidence to show that flies, fleas, ants, bugs, and mosquitoes are infectious?—I have no evidence.

20,641. Have you ever noticed that the primary lesion in plague was just in places which would be likely to be bitten by fleas or bugs?—The exposed parts are always likely to be bitten by fleas, bugs, or ants.

20,642. How can the dorsum of the foot get infected from the earth? You say in your précis that, in a great many cases, the primary lesion was on the dorsum of the foot?—It may be through an existing abrasion. There have been cases in which infection has gone into the system through shoe-bites.

20,643. Would it be produced there by scratching, or flea bite or mosquito bite?—It is possible.

20,644. The natives sit a great deal with their hands on the earth?—Very often.

20,645. Do you think that fleas, ants, and bugs, might predispose to plague by producing local lesions which may be scratched?—The poorer classes of people are always full of scratches and lesions, without any bugs or fleas or any other extraneous agents.

20,646. What is the incubation period of plague in your opinion?—I think it varies from about three days to a fortnight.

20,647. Why do you think so?—In cases of mild form, like *pestis ambulans*, we have found from investigation that the period of incubation has been over the normal limit. We have put down ten days; it has gone up to a fortnight.

20,648. Could you give us a typical instance?—I can cite the case of a patient who came from Calcutta on a ship called the *Cleobaria*. The ship left Calcutta, touched at Colombo, and then came to Bombay. Thirteen days after the ship had left Calcutta, the man developed fever and bubo in the groin. He had no venereal taint and nothing to account for it, no injury. He was brought to Bombay and sent to the hospital. We made a close investigation of the case, and we found that the bubo had already softened, and in two days it suppurated. An incision was made, and we found that the contents were the ordinary slough found in cases of plague.

20,649. Did you find plague bacilli?—Unfortunately the investigation for plague bacilli was made too late—about 36 hours after the bubo had been incised and dressed with antiseptics.

20,650. Was there plague in Calcutta at the time?—It was not reported to be existing at Calcutta at the time. There was a great deal of controversy with regard to that particular case.

20,651. Where would he have got plague if there were no plague cases in Calcutta?—I am inclined to believe there were plague cases in Calcutta amongst the poorer classes.

20,652. What was the exact date?—It was the year before last. The patient was admitted into the Arthur Road Hospital on the 6th of October 1897.

20,653. Why do you think that case was plague? Was it simply from the appearance of the bubo?—Yes, and from the history of the case, the man having been in an infected locality in Calcutta.

20,654. But you have no evidence to show that Calcutta was infected?—They had cases of plague in Calcutta at the time. I do not know whether they were officially declared or not.

20,655. Do you think that the power of resistance or the general state of health of the patient may extend the incubation period?—Yes.

20,656. Have you any evidence to show that?—There is no direct evidence bearing upon the point, because it is impossible to fix the day upon which the poison could have been so absorbed.

20,657. It is simply an impression on your part?—Yes.

20,658. Have you any typical cases showing that the incubation period was over three days, or is it simply your impression?—There was a very typical case which I came across four months ago, the case of a Parsee lady. I was called to see her, and I found that she had ascending lymphangitis extending from the inner malleolus right up to the femoral region, with enlargement and pain of the inguinal and femoral glands. It seemed to me there was also an iliac (pelvic) bubo. I examined her very closely and I found nothing. When I came to the heel I found that there was a regular sore. I inquired as to when that sore appeared. She said that ten days before she had been to the Fort and had put on a new pair of shoes; the same day she developed that sore. Ten days after the development of that sore she had the bubonic symptoms. This indicates a period of incubation of ten days, the formation of the sore, and probably the inoculation of the poison, occurring at about the same time. She was living in a non-infected locality.

20,659. Did you find plague bacilli in that case?—It was not bacteriologically examined; it was treated with curative serum.

20,660. Did she recover?—She recovered. She had a bubo on the opposite side as well; she had a left pelvic bubo afterwards which developed in the course of the disease.

20,661. Did the buboes suppurate?—No, they resolved.

20,662. Do you generally find lymphangitis extending from the point of inoculation to the buboes?—Not always.

20,663. Do you find it frequently?—I have seen a great many cases.

20,664. Coming now to the clinical symptoms; can you give us any information as to the attitude of the patient?—I have described the attitude of the patient

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in my report*; the attitude is very greatly influenced by the condition of the patient as regards delirium or meningeal symptoms.

20,665. Is not the position of the bubo an important point with regard to the attitude of the patient?—Not particularly.

20,666. Does he not generally draw up his leg on the side of the bubo?—No; the general position is the extension of the legs, one leg crossed over the other; it is almost invariable. In children, drawing up of the leg on the affected side is more often noticed.

20,667. Does he not generally flex his legs—one crossed over the other?—Only when the infiltration is very great; otherwise, if the tension is not so very great, you will find the legs extended; that is what I say in my report,* that the attitude of patients depended upon the stage in which they were admitted. Generally, they were too far advanced to assume any characteristic position; but if recumbent they generally lay flat on the back, with the limbs extended and almost invariably one leg crossed over the other. This was so often observed and so persistent, that every time a thorough examination had to be made, the legs had to be separated apart. Otherwise, the position of the body depended upon the nervous condition of the patient. In the apathetic cases, the patient would generally lie on the one or the other side, with the arms and legs flexed, the former against the chest and the latter against the abdomen, in fact literally doubled up. If the patient could assume a sitting posture, the attitude indicated thorough prostration and helplessness, the head bent upon the chest, the arms and legs hanging listlessly.

20,668. Have you ever noticed anything peculiar about the gait of the patients?—They have a tottering gait.

20,669. Do you attribute that to the weakness of the limbs?—Yes; want of co-ordination.

20,670. But does not that peculiar staggering gait come on long before the patient is weak?—It comes on on the second or third day.

20,671. Does it not come on at once?—No, not at once, I may mention that I have had very few opportunities of seeing fresh cases; most of the hospital cases are in an advanced stage.

20,672. There is something characteristic about the aspect of the patients?—Yes.

20,673. Would you describe that?—The aspect of the patient was almost always characteristic, although it was considerably obscured during the height of delirium. Ordinarily, the plague seemed to be stamped upon the patient's features, giving it a characteristic, and not to be easily forgotten, appearance. The features depicted fear and anxiety, or sadness and resignation, as if from an intuition of the impending fate. In fact, what is called *facies pestica*, so characteristic of the affection, is typified in this mixture of dread anxiety and resignation.

20,674. I suppose the aspect of the patients afterwards varies with the condition of his lungs or his mental condition—whether he is delirous or not; there is nothing peculiar about it afterwards?—That is so.

20,675. Have you ever noticed anything special with regard to the speech of the patient?—The speech is always characteristic. It resembles that of a person under the influence of drink. At first the syllables are all spread out, the voice is thick and husky; sometimes it is very sharp, and the patient replies in a very angry tone.

20,676. Some patients are very irritable?—Yes.

20,677. Most of them are resigned?—Yes; of course then they are in a semi-comatose condition, having huskiness and thickness of speech.

20,678. Have you ever noticed aphasia going on during the course of plague?—Yes; it comes on very suddenly.

20,679. It is complete aphasia?—Yes.

20,680. The patient cannot articulate a single word?—No.

20,681. It is not only aphonia, but complete aphasia?—Yes; absolute aphasia; when the patient recovers he seems to speak like an infant first learning a word; although he is able to articulate, there is a complete loss of memory of words which sometimes lasts for a very long time, sometimes extending over a year or a year and a half before he completely recovers his speech.

20,682. He understands everything that is said to him?—Yes.

20,683. You say in your précis that there is a period of general malaise preceding the appearance of the bubo; is that usually the case?—No, not as a rule; of course it is impossible to get any information from these patients. Some of the more intelligent said that they felt out of sorts for some time.

20,684. Do not the bubo and the rise of temperature come on suddenly and very frequently without any warning?—Yes, rigor and rise of temperature, followed by the appearance of the bubo.

20,685. Have you seen any cases where there was no warning at all where the man has suddenly become very ill?—Yes; while at his work.

20,686. Does the temperature go up very suddenly, or slowly?—There is a difference as regards that in different cases; because we know that in some cases that have virulent infection, the temperature goes to the maximum within the first 24 hours, and rises up to 107 or 108.

20,687. How long does it take as a rule?—It takes three days to attain the maximum.

20,688. Does it go up more quickly in children than in adults?—Yes.

20,689. Do you think the rise in temperature in children is frequently greater than in adults?—Yes, frequently greater.

20,690. Is the temperature a criterion with regard to the gravity of the case?—No, the temperature alone is not a criterion as to the gravity of the case.

20,691. Do you mean to say that you have had very fatal cases with a high temperature?—And also with a low temperature.

20,692. Is the temperature a remittent one?—Yes, it is remittent.

20,693. When do you get remissions?—Practically, the remissions commence on the second or third day.

20,694. It goes down in the morning?—Yes, and rises in the evening. There is a difference of about a degree or a degree and a half between the morning and the evening variations.

20,695. What is the highest temperature you have ever observed?—110°.

20,696. When the patient gets well does the temperature fall down suddenly or gradually?—Generally gradually.

20,697. How long does it take to reach the minimum?—About four or five days.

20,698. And it reaches the normal about the tenth day?—Yes.

20,699. Did you ever get sudden falls of temperature?—Sometimes; they are generally associated with collapse and death.

20,700. Is that sudden fall of temperature sometimes preceded by a rise of temperature?—No, not generally.

20,701. When the bubo begins to suppurate do you get suppuration fever afterwards?—Generally when the temperature goes to about normal on the tenth day we find that the bubo has already suppurated.

20,702. When the bubo is opened do you get further fever?—Further fever goes on only if the slough is underneath the skin or if there is some deep-seated suppuration which we are not able to get at; otherwise the course is only the difference of a degree—between 98 and 99.

20,703. Have you ever taken the temperature after death?—No; but in some cases I have found hyperpyrexia in examining the bodies soon after death; I have examined the patients after death, and sometimes examined them when there was some doubt whether they were dead or not.

20,704. And you have found very high temperature?—Yes.

20,705. But you have never taken the temperature some hours after death?—No.

* Report on Bubonic Plague, by K. B. N. H. Choksy. Bombay, 1897.

20,706. When does the bubo generally appear?—Generally after fever or coincident with it.

20,707. How many hours after the rise of the fever?—Eight or ten hours at the utmost.

20,708. Do you find the appearance of the bubo is delayed sometimes?—Yes; it comes on sometimes as late as six or ten days after the initial fever.

20,709. Do you find that in mild cases or severe cases?—In some severe cases I have seen that—in cases that have terminated fatally.

20,710. Is the pain in the bubo severe?—It is very severe as a rule.

20,711. Is it severe when you leave the patient alone, or only severe when you touch it?—That severity depends upon the tension of the bubo and the infiltration round about it. Sometimes you find a very small bubo extremely tender. In other cases you can move about large masses without pain.

20,712. I am afraid I did not put my question clearly; suppose you leave a patient alone and do not touch the bubo, is there any pain in the bubo?—Yes.

20,713. What sort of pain?—Dull aching pain; of course, the patients are not able to describe it exactly, they say it is an acute pain.

20,714. When you touch it, is it tender?—Yes.

20,715. Do you find that the swelling extends to some distance around the gland?—The gland gradually gets larger and larger until it has attained a certain size; then there is cessation of further swelling.

20,716. Does an axillary bubo spread down the sides of the chest and arms?—Yes, with a few exceptions, and almost always accompanied with extensive infiltration.

20,717. And the buboes in the neck?—Yes, it is the same.

20,718. Does it sometimes extend to the larynx?—Yes, and presses upon the larynx; it presses on the œsophagus and prevents deglutition.

20,719. What is the exudation like, is it serous?—It is either serous or sero-sanguineous.

20,720. And contains a large number of plague bacilli?—Yes.

20,721. Are there hæmorrhagic symptoms?—Yes, especially in the neck.

20,722. What is the swelling like to the touch?—The bubo feels like a dense mass; the infiltration feels more or less doughy, softish; in fact, you might almost call it fluctuating.

20,723. Do you sometimes get erysipelas from the bubo?—Yes, especially in the cervical bubo.

20,724. Have you found the streptococcus of erysipelas in these cases?—I have not made any examination.

20,725. Do you think the rapidity of exudation is a prognostic sign?—Yes, a prognostic sign, and almost always meaning fatal results.

20,726. You mean when it spreads quickly?—Yes.

20,727. Do you think the size of the bubo is any prognostic sign?—None at all, except that in some cases where there are very small buboes, the cases have terminated fatally.

20,728. Will you put in a table giving the total number of femoral and other buboes, brought up to date and work out the percentages?—(Witness subsequently submitted a statement* showing the position of buboes and mortality in 2,723 cases. He explained that, on account of pressure of work, it was not possible to supply the figures for 4,000 cases.)

20,729. Does inflammation of the lymphatics spread from the point of inoculation to the bubo?—Yes.

20,730. Does it spread from the bubo to other buboes?—Yes.

20,731. You have seen that?—Yes.

20,732. I suppose the bubo may slough, may it not?—Yes.

20,733. What is the character of the slough?—The slough is very peculiar; there is generally a mixture of slough and pus; I mean you find the buboes softish, and when you make an incision all you find is that it contains a central cavity with a drop or two of pus; and the whole mass is sloughy; the slough is very hard and sometimes almost cheesy, at other times it looks like a thick tissue, something between cartilage and muscular tissue.

20,734. Does it separate easily?—No, it takes a long time; in only a few cases is it possible to take out the whole gland *en masse*.

20,735. Do granulations form easily, or is the healing slow?—The healing is not very rapid, although granulations seem to form soon.

20,736. I think you said in your report* that it sometimes takes a very long time to heal?—That depends upon the state of the infiltration. In ordinary suppuration with very little or no slough, you find the incision heals very quickly. When the pus and sloughs are extensive or buried between the sheaths of muscles they take a much longer time.

20,737. Do you find the pus has the tendency to extend between the muscles and in the muscular tissue?—Yes; sometimes it forms a regular abscess.

20,738. When the bubo is in the iliac or femoral regions?—Yes.

20,739. When the bubo is in the femoral region, does the pus burrow as far as the popliteal cavity?—Yes.

20,740. Of course, that takes a long time?—Yes; the patient dies from exhaustion by the suppuration.

20,741. Have you seen sloughing of the muscles of the abdominal wall?—Yes, but I think that is generally due to the caustics applied by the patients themselves, except in cases of local necrosis. There is a regular dissection of the abdominal wall.

20,742. Have you ever seen sloughing of the genital organs?—Yes; the whole scrotum sloughs, and the testes have been exposed; in females you will find the labia in an almost gangrenous condition.

20,743. What is the pus like?—Sometimes it looks a thick creamy healthy pus, as we find in ordinary abscesses; at other times it is thin and watery; sometimes it is stained with blood.

20,744. You say in your précis it is an acrid pus?—I mean the irritant pus.

20,745. You mean, it irritates the parts around it?—Yes.

20,746. Do you find that the blood vessels suffer in a bubo?—I think they are easily cut, and their walls become soft.

20,747. Have you ever had secondary hæmorrhages?—Yes, from ulceration.

20,748. How long did the hæmorrhage last?—We stopped it promptly by tampon, and by means of compression we were able to control it altogether.

20,749. Are you sure that it was the femoral vein?—I think so.

20,750. Hæmorrhage from the femoral vein is usually fatal, is it not?—This case did terminate fatally afterwards.

20,751. Not from hæmorrhage?—No; but it took much time to control the hæmorrhage.

20,752. Do you find the sinuses take long to heal?—Yes.

20,753. What is the cicatrix like?—It is generally puckered in cases with extensive infiltration, and it has sometimes a keloidal appearance.

20,754. With regard to the symptoms in the nervous system, what are the reflexes like?—They are dulled.

20,755. Have you tried the patellar reflex?—Yes we generally have the patellar as well as reflex of the ankle-clonus.

20,756. What is the character of the delirium?—It varies; it may be either a muttering delirium or a most violent and maniacal delirium with homicidal tendencies. The patients try to get out of bed, and run out of the hospital. They try to crawl beneath their beds; they go and fall upon a contiguous bed.

20,757. Is the delirium nocturnal or diurnal?—It is more nocturnal.

* *See* App. No. LXI. in this Volume.

* Report on Bubonic Plague, by K. B. N. H. Choksy. Bombay, 1897.

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20,758. Is it anxious delirium—do not they show any symptoms of fear?—Not when they are in delirium; it is impossible to say what their exact mental state is at the time.

20,759. Is it more marked in young than in old people?—I think in young and old—but, perhaps, more in infants and children.

20,760. How long does it last?—It lasts from about four to six days; of course there are patients that take a longer course.

20,761. Have you found in patients a tendency to get out of their own beds and crawl into somebody else's bed?—Yes, they do that very often, and we have to tie them down.

20,762. Do they try to assume a certain position?—Yes.

20,763. Do they try to escape?—Yes.

20,764. What is the state of the sphincters?—There is almost always retention of the urine; in fact, every other patient used to have a catheter used.

20,765. When does that come on?—Immediately after the second or third day.

20,766. Do you get involuntary evacuation of the urine in some cases?—Occasionally; except it may be, perhaps, in a patient who is in a comatose condition from over-distention of the bladder.

20,767. Have you ever noticed that patients have a tendency to assume a professional attitude, such as praying?—Yes, such as cart-driving and cab-driving; some were in a devout mood, falling on their knees with their hands clasped, as if in prayer.

20,768. Shoemakers will sit down as though they were making shoes?—Yes.

20,769. Some are extremely merry, are they not?—Yes.

20,770. They burst into peals of laughter?—Yes, and that is associated with tetanic symptoms; almost all those cases of loud laughter are cases in which the tetanic symptoms are developing, or are in full strength.

20,771. Do you mean they get tetanus?—Yes.

20,772. You have seen cases of tetanus occur?—Yes, tetanus with marked spasms and opisthotonos and arching of the neck as well as of the back.

20,773. In patients whose buboes are opened?—No, early cases, on the second or third day.

20,774. Are these cases of mixed infection of plague and tetanus?—It is impossible to say.

20,775. You have not made bacteriological examination of that?—No, the cases all proved fatal.

20,776. They were typical symptoms of tetanus?—Yes; in fact, sometimes the spasm was so strong that all the respiratory muscles were fixed like a board; the use of nitrate of amyl or nitro-glycerine averted death in some cases.

20,777. I think you said that they were all fatal?—I mean averted temporarily.

20,778. What is the sensibility—are they hyper-sensitive?—Sometimes they are hyper-sensitive, and wince before the finger touches the bubo.

20,779. They are afraid of being hurt?—Yes.

20,780. Did you find when you actually touched them that they felt the pain more than the ordinary patients?—The state of pain depended upon their delirious condition; sometimes they are roused out of deep coma by pressure.

20,781. Some are talkative, are they not?—Yes.

20,782. They talk the whole time?—Yes.

20,783. Have you seen instances of hallucination?—Yes.

20,784. Have you seen any hallucination of the sense of smell?—No.

20,785. Where the patient thinks he smells all sorts of things?—No, I have not noticed that.

20,786. Have you seen convulsions?—Yes, especially in children; of course there are something like epileptic convulsions, in some cases before death.

20,787. Have you found as an after-effect of plague a sort of apathetic condition lasting for some time?—Yes.

20,788. How long does it last?—I have seen it last three or four months.

20,789. What are its symptoms?—The patient lies in bed, doubled up, generally with his eyes half-closed; he is not cognisant of his surroundings or of his own personal state; he might pass urine and fæces in bed; he lives a sort of mechanical existence, everything which is forced down his throat he takes, and his mind is a perfect blank during the period.

20,790. He gets into a state of imbecility?—Yes.

20,791. Have you seen mania following plague?—Yes, but only in two cases. If the case takes an unfavourable turn, the patient gets feebler and feebler, is bundled up in bed, extremely peevish, whines or cries when disturbed; gradually it becomes extremely difficult to feed him or administer medicines, and he sinks into a marasmatic condition until death supervenes, the patient dying a complete physical and mental wreck. The condition of the patient in this stage is extremely pitiable, he becomes wasted to a mere skeleton and looks more like a corpse than a living being. The extremities are cold, pulse extremely feeble, eyes half or wholly closed, respiration shallow; there are sordes on the gums and teeth, the saliva lodges between the lips, and at each expiratory effort a little trickles down the angles of the mouth, mucus and bronchial secretion are visible at the orifices of the nose, and may be similarly discharged at each expiration. All the normal reflexes being dulled, the patient is speechless, and takes very little nourishment, or has to be supported on nutrient enemas. Even from this condition recovery is occasionally possible, and has occurred in about half-a dozen cases.

20,792. Have you ever seen hydrophobic symptoms in a patient?—No.

20,793. Have you ever seen deafness?—Yes, deafness is complained of.

20,794. Is that deafness due to some local condition of the ear, or is it central?—It is central.

20,795. Have you examined the ears of the patient?—Yes, very often.

20,796. You have not found local lesion?—No.

20,797. Have you ever found any hallucination of taste?—No.

20,798. Have you ever found permanent loss of speech after plague?—No, not permanent.

20,799. They have always recovered?—Yes.

20,800. Have you anything to say with regard to the circulatory system?—In no other acute infectious disease does the pulse—an index of circulation—present so many variations in force, frequency, volume and tone as in bubonic plague, and in no other disease is the disproportion in the normal ratio between the pulse, temperature, and respiration so divergent. The character of the pulse, apart from its frequency, varies with the course of the disease. The full and bounding pulse which is so often associated with high temperatures is conspicuous by its absence, and in the majority of cases the pulse is easily compressible, extremely feeble and very frequent. The feebleness of the circulation is well demonstrated by the feebleness of the pulse, so much so that in some cases with temperatures of 103, 104 or 105 the extremities may be not only cold, but almost icy-cold, and bathed in cold clammy perspiration. This was, indeed, very characteristic and was often noticed. Another index of the weakened heart's action was the pulse itself which could not be felt or counted at the wrist, and heart beats had to be counted instead. As the case advances, the pulse becomes intermittent, regularly or irregularly, more often the latter, and distinctly dicrotic. The dicrotism in some cases is extremely well marked, and in advanced cases may really be considered a reliable diagnostic sign. There is always a change for the better in this stage of circulation if the patient is going to recover, but, if otherwise, the state of the circulation becomes still graver, as shown yet by another variation in the condition of the pulse. It becomes of a beaded character—in fact, a regular water hammer pulse of aortic insufficiency—and it is not difficult to conceive what is thereby indicated. A step further and the inevitable must happen—failure of the heart's action. And this may be gradual or sudden. In a large number of cases the failure is gradual, the beaded pulse becomes quite imperceptible, the body temperature is scarcely

maintained, the circulation getting more and more feeble, and eventually collapse with fall of temperature and death result. In these cases much could be done to support the circulation and stimulate the heart's action, and it may even be possible in a few cases to avert failure and death. But no such hope can be entertained where the failure is so sudden—so surprisingly sudden,—as even to raise a doubt as to whether the patient is actually dead or not. A patient tries to sit up in bed, or gets out of it, or makes some sudden exertion, and falls down either on the bed or on the floor, and is found beyond all hope. Very often the heart fails suddenly without any exertion on the part of the patient. He may be sleeping quietly and dies suddenly. In sudden heart failure very often there are no indications of cardiac failure, the pulse may be of good volume, and tension, frequently easily compressible, but yet indicating no immediate danger.

20,801. Have you ever found murmurs in cases of plague?—Not distinct.

20,802. Have you ever found leucocytosis in the blood?—Yes.

20,803. What is your experience with regard to that?—In a number of cases examined by myself and Dr. Pöch, of the Austrian Mission, we found the number of leucocytes varying from 12,000 to 28,000. This was the largest number found in any case.

20,804. In fatal cases?—I cannot say that exactly.

20,805. You consider that the presence of bacilli in the blood is a grave prognostic sign?—Yes.

20,806. In how many cases have you found bacilli in the blood?—Generally, I found bacilli in cases which were very near death, or which had assumed a grave complexion.

20,807. Have you found bacilli disappear from the blood, and the patients recover?—Yes; but in a very few cases indeed.

20,808. In how many cases, do you think?—We had examined about 240 cases all told; but I do not think that in more than four or five cases was the thing observed.

20,809. Have you ever noticed epistaxis?—That is one complication; but it is not common unless the case is mixed with relapsing fever.

20,810. Have you ever seen cases in which the larynx is affected in plague?—Yes; the tonsils are affected first, and the larynx secondly.

20,811. Have you ever seen it secondly affected from the cervical glands?—Yes; and I think primary also, affecting the cervical gland, the poison passing from the tonsils and affecting the neck buboes.

20,812. We have been told that there was a form of primary laryngeal plague; have you noticed that?—I think the laryngeal plague is more a case of cervical buboes, with pressure upon the larynx from a mechanical cause.

20,813. Have you ever seen a pseudo-diphtheritic membrane on the tonsil?—Yes.

20,814. In cases where the bubo was in the neck?—Generally.

20,815. Do you, then, consider the tonsils are the point of entrance?—I believe so.

20,816. But it may be, possibly, brought in by food?—It may be from ordinary inspiration.

20,817. Or by food?—If the food is the channel, you ought to see some abdominal symptoms as well as infection of the tonsils.

20,818. Supposing the virus has been stopped at the tonsils?—That would show a selective action on the part of the tonsils to take up the whole virus, and not allow any to go into the stomach.

20,819. Supposing it went into the stomach, would it not be killed by the gastric juice?—You mean to say the secretion of the mouth would not interfere with it also?

20,820. You have no facts with regard to that?—No.

20,821. Is the hypostatic congestion of the lungs common?—Yes.

20,822. Did you find plague bacillus in the sputum in cases of hypostatic congestion of the lungs?—There is hardly any sputum in some of these cases.

20,823. Is the œdema of the lungs a common symptom?—Yes.

20,824. When does that supervene?—Generally, on the third day.

20,825. Do you think the œdema of the lungs is generally due to recumbent position, or to something more?—I think it is due to the recumbent position and flagging circulation.

20,826. You have already told us about primary pneumonia; did you get secondary pneumonia in plague?—Yes.

20,827. What organisms did you find then? Did you find the pneumo-coccus or bacillus of plague?—Both have been found in the same patient.

20,828. Is it lobular pneumonia?—Yes, in small isolated patches.

20,829. We have been told, in certain cases, that you get the whole of the lung, or a large part of it, involved; have you seen any such cases?—No; we have made several *post-mortems*.

20,830. It has always been lobular?—Yes.

20,831. What are the characteristics of the tongue?—The tongue is dry, and covered with a thick white fur in the centre; it is red at the tip and edges. The character of the tongue varies with the disease. In advanced, and neglected, cases it is of a brownish colour, and of regular rusty appearance; the lining chips off in flakes, leaving ulcers.

20,832. Have you ever seen a tongue of which the centre is perfectly clean and the two sides are furred?—I do not recollect such an instance. (Note by witness on correcting proof of his evidence—I have seen one case since the above evidence was recorded.)

20,833. You do not think it is a characteristic of plague?—No, it would not be common.

20,834. Have you ever seen acute glossitis in plague?—Yes.

20,835. What are the characteristics of that?—It is generally associated with cervical buboes and infiltration; the tongue is almost round over one-half and the other half is enormously swollen: the patient is unable to open his mouth, and has difficulty of respiration and deglutition.

20,836. Have you seen it in septicæmic plague?—I cannot say whether these cases are septicæmic.

20,837. Have you seen hæmorrhages from the tongue?—No primary or direct hæmorrhage except through ulceration.

20,838. Have you ever seen fissured tongues which bled?—Yes.

20,839. In septicæmic cases?—Yes, I think so.

20,840. What is the appetite like?—The appetite is almost nil in some cases; occasionally, in the acute stage, the patient asks for food or says he is hungry.

20,841. We have been told that the appetite is preserved in a good many cases; that is not your experience?—I do not think so, the patients take whatever is given to them by the nurses, and they have very little choice in the matter, they might like it or they might not like it, but they usually do not ask for food or say that they feel hungry.

20,842. Is thirst very great?—Yes.

20,843. Is it a marked symptom?—Yes, they always call for ice water.

20,844. Is the vomiting bilious?—Yes, generally; I think it is due partly to the calomel and soda which they get as treatment when they are admitted; but, apart from that, vomiting is at times very persistent and troublesome.

20,845. Lasting several days?—Yes.

20,846. Have you noticed any blood in the vomiting?—Occasionally.

20,847. Have you noticed hiccough?—Yes, it is very persistent either in acute or convalescent cases.

20,848. How long does it last?—It may last for a fortnight, practically defying all treatment.

20,849. Have you found that in septicæmic cases and bubonic cases only?—Yes.

20,850. What is the state of the bowels?—Generally costive; diarrhœa comes on just before the critical period, just before death, in some cases.

20,851. There is nothing special about the motions?—No, except a peculiar smell, which I found very characteristic and very offensive.

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20,852. Have you found that melæna is a common symptom?—It is observed in a certain number of cases.

20,853. Is that observed just before death?—No, as an ordinary complication.

20,854. Do you think a patient has frequent hæmorrhages in the bowels?—Yes, and in the stomach also in some cases. There is coffee-ground vomit, partly-digested blood, and sometimes almost pure blood, is brought up.

20,855. What are the characteristics of the urine?—It is always highly-coloured and albumen is present, the chlorides are diminished, and there are always hyaline casts.

20,856. Have you ever seen uræmic symptoms?—No.

20,857. Have you ever found bile pigment in the urine?—Yes.

20,858. Frequently?—Yes.

20,859. How do you test for it?—By nitric acid and sugar, I think.

20,860. In another report, I find that the free excretion of urea and uric acid and absence of albumen blood and renal casts in the urine are favourable signs, and the return to the normal quantity secreted and the presence of chlorides one of the first signs of amendment; is that your experience?—Yes, the chlorides do gradually commence to increase as the condition of the patient ameliorates.

20,861. Do you think that the catamenial discharge has any influence on plague in women?—It is brought on prematurely by plague.

20,862. Do you find it has a bad influence on the course of the disease?—It might have, indirectly, by causing too profuse hæmorrhage, and thereby further weakening the patient.

20,863. Is it your experience that it is so?—I think so.

20,864. I believe pregnant women usually abort and die?—Yes, recoveries are very rare.

20,865. There is an idea among people that syphilis protects against plague; have you ever seen a syphilitic patient with plague?—I have seen patients with fresh venereal infection and having all the symptoms of plague in them. I do not know about constitutional syphilis of long standing—I have had no experience of that.

20,866. But you have seen primary syphilis and plague in the same patient?—Yes.

20,867. Do you ever see plague in the fœtus?—No, I have not made any investigations with regard to that.

20,868. What have you to say with regard to cutaneous and cellulo-cutaneous symptoms?—The cutaneous manifestations of plague are many and various, and may be noted as:—(*a.*) Ordinary blebs or blisters. (*b.*) Blebs or blisters with hæmorrhage or acrid serous pus. (*c.*) Dark depressed blisters, ordinarily called black boils. (*d.*) Localised cutaneo-cellular necrosis. (*e.*) Gangrene. (*f.*) Abscesses.

(*a.*) Ordinary blebs or blisters.—These with serous contents were observed in a few cases at an early stage. The blister comes on generally without any appreciable cause or after, perhaps, slight friction, the limbs becoming somewhat hot. If punctured, the contents are perfectly serous like those of any ordinary blister, but the cuticle appears of a dark brownish colour. The surrounding skin may have a threatening darkish look and if not properly attended to, the limb may become gangrenous.

20,869. Are these primary lesions you speak of on the dorsum of the foot, for instance?—Not necessarily.

20,870. You say that the limbs become gangrenous; how far does the gangrene extend?—I have seen it extend almost to the half of the lower extremity.

20,871. Does the patient lose the limb?—Generally, a line of demarcation forms; I have not waited for that, I generally injected the sublimate at the periphery just where it was spreading; that has checked further spread of the gangrene.

20,872. Did you ever have to amputate?—No.

20,873. Will you proceed, please?—(*b.*) Blisters with hæmorrhagic acrid serous pus.—These were observed in some advanced cases, the blisters covering almost the whole body, but more especially the limbs, and running together into large shapeless bags. The contents were generally made up of unhealthy pus or a mixture of it with blood.

(*c.*) Dark depressed blisters.—The so-called "black boils" were observed in a large number of cases, and they seem to bear a distinct causative relation to the buboes in their neighbourhood. They differed from ordinary boils in that no inflammatory aureola surrounded them. They consisted essentially of a blister, in which the cuticle had become raised sharply and evenly from the skin, without any redness or signs of inflammation. In fact, it appeared as if a glass circle had been fixed over the skin and its top tied with transparent tissue paper. The top was centrally depressed or umbilicated, and from this there descended to the bottom a central slough, reaching down to the skin. The space around the slough was filled with pus or sero-sanguineous fluid. When the cuticle was removed and the central core or slough separated from the base, there remained a punched-out unhealthy looking ulcer which was difficult to heal, and took a long time to granulate. If seen early, the cuticle of these blebs used to appear normal.

20,874. Did you ever see localised cellulo-cutaneous necrosis?—Frequently.

20,875. What does it look like?—In the advanced stage you will find a large dark greenish looking surface, surrounded by inflammatory aureola, which is depressed in the centre, with a line of demarcation all round.

20,876. It looks like wash-leather?—Yes.

20,877. Can you check that in any way?—I have succeeded in checking it by injecting sublimate at the circumference.

20,878. Have you ever seen desquamation after plague?—No.

20,879. Have you seen any eruption of the skin?—Yes; like herpes and sometimes like eczema.

20,880. Where were the eruptions?—They were generally spread over a part of the body; and the herpes were generally confined to the face.

20,881. What part of the face?—Generally along the lips.

20,882. The same as you get in pneumonia?—Yes.

20,883. Is there such a thing as plague-rash?—I have seen in some cases, just before death, small petechiæ spread over the body, or sometimes in contiguity with exudation, but none can be said to be a characteristic sign of plague.

20,884. Have you ever seen gangrene of the limb after plague?—Yes.

20,885. How far did the gangrene extend?—In one case right up to the forearm.

20,886. From where?—I think from the thumb.

20,887. Did the man get a local lesion of the thumb?—I think there was a local lesion.

20,888. Did you have to amputate?—No, the gangrene was also stopped by sublimate. In both the cases in which it was very well marked the sublimate stopped it.

20,889. You had only seen two or three cases of that?—Yes.

20,890. Have you ever seen abscesses?—Yes; they form in all parts of the body.

20,891. Have you seen petechiæ form?—Yes, before death.

20,892. It is said that there is a peculiar smell in plague; have you noticed that?—Yes, I have noticed it very often; very marked, especially when the plague epidemic is at its height.

20,893. Do you think it is more the smell of the dirt of the patient?—No.

20,894. Do you think it is peculiar to the plague?—It is peculiar to the plague; I have often been attracted by the smell, and have asked the nurses to examine the bodies, which they have found to be thoroughly cleaned and disinfected. The smell sticks to the patient even after disinfection.

20,895. Captain Thompson describes it as an earthy, cellar-like smell?—I do not think I can describe it in any particular way.

20,896. Have you ever seen complications of the eyes?—Yes.

20,897. Can you tell us about them?—There is generally ulceration either of the cornea leading to

erforation or hypopyon, or a general destruction of ıe eyesight in a short time.

20,898. What is the cause of that? Is it due to in-ɔction of the eye from the bubo?—No; I do not think ıere can be any direct infection of that kind. There hardly any time for it; it comes on in the acute ;age.

20,899. Do you get conjunctivitis?—Yes.

20,900. Do you think it is due to inability to close the yelids?—There is such an amount of injection at first ·hen the patient gets plague that there is very little ifference between it and acute inflammation; and in-ammation comes on long before there can be any ·ritation from inability to close the eyelids.

20,901. Have you seen any photo-phobia?—Yes. In ome cases there is lachrymation also.

20,902. Do you sometimes get arthritis coming on in lague?—Yes.

20,903. Is that in cases treated with serum?—Yes; nd often in cases not treated with serum.

20,904. How long does the arthritis last?—It may last hree or four weeks. There is no actual arthritis lways; sometimes you will find arthritic pain without rthritis; the patient complains of pain.

20,905. Do you get effusion into the joints?—Yes, and welling.

20,906. Do you ever get pus in the joints?—No.

20,907. In how many cases does it occur?—In about or 5 per cent. of cases.

20,908. Can you tell what are the factors which would ffect the prognosis?—There are very many different auses which lead to it.

20,909. Do you find that stout people suffer more than hin people, and so on?—Stout people die much sooner han thin people.

20,910. You think obesity has a distinct influence?—Yes, I have found some of the Bombay workmen—abourers in the docks—who have got very good hysique—succumb to the infection in a short time, nd die within 24 or 36 hours after infection.

20,911. Can you put in a table of mortality up to the resent time, including all your experience, and giving articulars of race, sex, age, and occupation?—I have ot two tables* here—one, beginning in September 1896, nd ending in February 1899, showing the number of ases treated, and deaths within 24 hours, and deaths ithin 48 hours, the total mortality, the recoveries, nd the percentage of mortality. The other tables equired I will prepare and submit later.

20,912. Do you find that children die more easily from lague than adults?—During the first epidemic, I found hat the incidence of mortality was very great among hildren; but, subsequently, they seemed to make more ecoveries in proportion to the adults. They had con-ulsions and other intercurrent diseases, which also arried them off in large numbers.

20,913. Do you find that many or few children come nto the hospital?—I think a good proportion are hildren.

20,914. Do you find a good proportion of children nder five or six years old coming into the hospital?—Yes.

20,915. What does a patient usually die of?—I think ailure of the heart's action is the primary cause.

20,916. Do you find œdema of the glottis?—In some ases.

20,917. Hæmorrhage also?—Yes.

20,918. Have you ever found nervous sequelæ in plague asting for a long time?—Yes, such as paraplegia, eripheral neuritis, paralysis of the extensor muscles, nd wrist-drop as in lead palsy.

20,919. Have you seen them in the feet, or chiefly in he hands?—Chiefly in the hands.

20,920. Have you ever seen spastic-paraplegia?—No.

20,921. Mental depression?—Yes.

20,922. Have you ever seen dysentery following lague?—It might be there, but nothing marked.

20,923. Have you ever seen relapses in plague?—Yes, have seen they are very frequent, in fact, nearly 0 per cent. or 60 per cent. develop an iliac or pelvic ubo.

20,924. What becomes of the bubo?—It resolves generally, and if it suppurates it means long illness and probably death.

20,925. (*Prof. Wright.*) Do you get another rise in temperature?—Yes, and in fact the patient gets through a regular secondary, but milder, attack of plague.

20,926. (*Dr. Ruffer.*) Have you ever seen a man who had plague twice?—No.

20,927. Do you think that plague brings out certain latent diseases?—Yes, especially malaria and phthisis.

20,928. Do you think it has a good effect on certain infectious diseases?—It might modify them in this way; as relapsing fever and plague modify each other, it is possible that other diseases might be favourably or unfavourably modified. I have seen small-pox and plague together in the same patient.

20,929. How long do you think a patient remains infectious after plague?—After the buboes have suppurated, I suppose all the infection disappears in about three or four days after the opening of the buboes.

20,930. Suppose the patient has lung symptoms as well?—Then it depends upon the duration of the lung symptoms.

20,931. Have you any experiments to show that the plague bacillus remains in the lungs of a plague patient for some length of time?—No.

20,932. The late Doctor Davda, who got plague in the hospital, had 10 c.c. of Doctor Yersin's serum injected as a preventitive about three weeks previous to his illness?—That is so.

20,933. And he had plague and died?—Yes.

20,934. Can you tell me anything about Dr. Galeotti's serum?—The first time that that serum was used in Bombay was in June 1897. We had only six cases in Bombay, and they gave very satisfactory results. That was the first experience, just when the first epidemic was declining.

20,935. How did you choose those six cases?—They were not selected; they were injected as they came in. At that time the admissions were few, and there was not much chance of making any selection.

20,936. Have you the details of these cases?—Yes.

20,937. Have you notes?—Yes.

20,938. Can you put them in, with the temperature curves?—Yes. (Witness subsequently stated that no detailed clinical notes of the cases treated were kept on account of pressure of work and want of proper assistance. The charts of all the patients were, however, available, with such remarks as were made on the same at the time of treatment and after death. These he forwarded together with a report on all cases treated with serum in the hospital.†)

20,939. Do you find that the injection of Dr. Galeotti's serum produces any marked effect on the temperature?—Yes, it does, if one or two injections have been made.

20,940. What happens?—The temperature falls by about three or four degrees, and there is a general improvement in the condition of the patient.

20,941. Improvement shown in what way?—As regards his mental condition and circulation and buboes.

20,942. Anything else?—The buboes cease enlarging, and some altogether disappear. Of course, we go on injecting the serum by greatly diminishing doses until the temperature comes to the normal and stops there.

20,943. Have you had any experience of it since those first six cases?—We had 257 cases done last year, which gave us 145 deaths and 112 recoveries, a percentage of about 56 mortality.

20,944. What is the normal mortality in your hospital?—The normal mortality is 71 per cent.

20,945. How did you choose these cases?—At first we took all cases. We found that there were about 30 moribund in the list of 257 treated; we found that the serum was simply wasted upon them, and, subsequently, we eliminated all the moribund and all the convalescents and semi-convalescents.

20,946. When do you begin to do that?—In April.

* *See* App. No. LXI. in this Volume.

† *See* Report by K. B. H. N. Choksy on cases in the Arthur Road Hospital, Bombay, treated with serum, with six charts of cases treated in June and July 1897, and 47 charts of cases treated in March 1899, printed in this Volume as Appendices Nos. LXII. and LXIII. In App. No. LXII. is also included a report on Lustig's serum by Captain L. F. Childe, I.M.S.

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20,947. After doing how many cases?—About 50 or 60 cases in the early days.

20,948. Then you eliminated all the moribund and convalescents and semi-convalescents?—Yes.

20,949. In spite of that, your mortality remains 56 per cent.?—Yes, the mortality of the moribund is included in the 56 per cent. Thirty cases are included; if they are eliminated, the percentage is about 49 or 50.

20,950. Did you eliminate all the cases that had been ill for some time, or did you choose your cases in any way?—We did not select them; we took them as they came in.

20,951. Did you inoculate every case that came in, or every other that came in; did you get controls?—With regard to controls, there is a great deal of difficulty. There is great difficulty in getting proper controls.

20,952. Will you continue your explanation with regard to controls?—I find there is a great deal of difference with regard to the races from which the patients are selected, one race having a higher mortality than another; then, again, the position of the buboes has a great deal of influence with regard to the mortality, axillary buboes, for instance, giving a mortality of 82 per cent., whilst the inguinal buboes have a mortality of only 66 per cent. Controls, in order that they may be proper for comparison, must be more or less similar as regards age, sex, race, duration of disease, position of buboes, general condition, and the state of the circulation. If they do not answer to these requirements, any conclusions arrived at must be fallacious. And it is not always possible to get such controls. The following tables bring together the statistics I have compiled of cases in 1898 treated with the serum and those not so treated:—

TABLE showing the NUMBER of CASES treated with Prof. LUSTIG'S SERUM during 1898, and comparing the MORTALITY between SERUM and NON-SERUM CASES.

1898. Months.	No. of Cases treated with Serum.	Died.	Recovered.	Percentage of Mortality.	Percentage of Mortality in Non-serum Cases.
March - -	35	23	12	65·71	75·89
April - -	95	59	36	62·11	69·37
May - -	43	20	23	46·51	73·68
June - -	—	—	—	—	57·14
July - -	9	3	6	33·33	90·90
August -	13	6	7	46·15	83·33
September -	38	20	18	52·63	96·72
October - -	24	14	10	58·33	95·12
Total -	257	145	112	56·42	79·12

	No.	Died.	Recovered.	Percentage of Mortality.
Non-serum cases during the same period.	752	595	157	79·12

The above 257 cases include over 30 moribund cases. Subsequently all moribund cases were excluded, and so also all semi-convalescent and convalescent cases. The cases thus treated represented the average type of ordinary acute cases. In the deaths from serum-treated cases have been included several cases that had died during convalescence, at periods varying from four to ten weeks after the injection of the serum. In fact, they had died from intercurrent diseases, and not strictly from plague.

20,953. Have you excluded from these tables all the pneumonic cases?—We had no primary pneumonic cases.

20,954. What are the other good effects you noticed from Dr. Galeotti's serum?—The convalescence was rapid and there was less suppuration of the buboes.

20,955. Had it any influence on the cough of the patient?—Not that I noticed particularly.

20,956. Did you make any observations of micro-organisms in the blood?—They were made in my presence by Dr. Galeotti in two or three cases only; and it was found that the micro-organisms disappeared after the injection of the serum. Cultivations were made.

20,957. Did you notice anything else?—No.

20,958. I believe you had some experience with regard to Prof. Haffkine's serum?—Yes.

20,959. How many cases?—That was in December 1896 and January 1897. There was a mortality of 40 per cent.; 14 of those were acute cases, of whom six died and eight recovered, giving a mortality rate of 42·85 per cent. The number, however, was very limited. Then we had another trial with Prof. Haffkine's serum, side by side with Prof. Lustig's serum. There was no difference in the mortality between the serum cases and the non-serum cases.

20,960. Was there any difference in the mortality between Prof. Haffkine's series and Prof. Lustig's series?—Yes, in favour of Prof. Lustig's series.

20,961. You say that Prof. Haffkine's inoculation gave a percentage of mortality of 42·85, whereas Prof. Lustig's gave 56 per cent.?—49 per cent., excluding the moribund.

20,962. How long did you try Prof. Galeotti's serum?—It was tried from March to October.

20,963. What was the mortality in the preceding year from March to October?—Our epidemic of 1897–98 became practically over in June 1898. Then we had a rather severe recrudescence in July, August, September, and October.

20,964. Can you give us the mortality in the same months in the preceding year when no serum was tried?—The mortality during the epidemic:—

In March	1897	was	67	per cent.
In ,,	1898	,,	75	,,
In April	1897	,,	56	,,
In ,,	1898	,,	58	,,
In May	1897	,,	40	,,
In ,,	1898	,,	54	,,
In June	1897	,,	22	,,
In ,,	1898	,,	57	,,
In July	1897	,,	53	,,
In ,,	1898	,,	78	,,
In August	1897	,,	63	,,
In ,,	1898	,,	76	,,
In Sept.	1897	,,	66	,,
In ,,	1898	,,	82	,,
In October	1897	,,	51	,,
In ,,	1898	,,	84	,,

20,965. It was in 1898 that the serum was tried, was it not?—Yes.

20,966. In 1897 the average mortality was lower than 56 per cent., when there was no serum tried?—In 1897 we had no recrudescence during these months.

20,967. In 1897 the mortality was 67, 56, 40, 22, 53, 63, 66, 51 per cent., and in 1898 it was 75, 58, 54, 57, 78, 76, 82, 84 per cent.?—That is so.

20,968. Therefore the percentage was considerably higher when the serum was used than when it was not used?—The percentage was higher, not because serum was used, but because of the fact that from June to October 1897 there was no epidemic of plague; there were sporadic cases only: whereas in 1898 we had another severe epidemic during those months.

20,969. But, comparing the two, the proportion is not in favour of the serum?—The comparison would be fallacious, as the conditions were not identical. If we compare the mortality of serum cases and non-serum cases, there is a difference of nearly 23 per cent. in the same period. From March to October 1898 we treated 257 cases with serum, and 752 without serum. The mortality in the serum cases was 56 per cent., and amongst the non-serum cases 79 per cent.

20,970. Do you exclude moribund cases among the uninoculated people?—If we exclude the moribund among the uninoculated the percentage would stand at about 69. It has to be remembered that, among the uninoculated are included convalescent and semi-convalescent cases, otherwise the mortality rate in them would be higher.

20,971. That is only a difference of 9 or 10 per cent.?—Nearly 13 to 14 per cent.

20,972. What have you to say with regard to the *post mortem* appearances? — If it were possible to convey in one word the principal *post-mortem* signs of the changes that are found in the system, that would be hæmorrhage. Hæmorrhages in every conceivable and inconceivable part of the body, hæmorrhages in the dura-mater, in the larynx, in the mediastina, in the epi- and peri-pleura, in the epi- peri- and endocardium, and in the myo-cardium, in the lungs, in the stomach, in the intestines, kidneys, liver and spleen, in the bladder, in the intima of the vena cava and jugular veins, hæmorrhages in and around the buboes and in the subcutaneous cellular tissue of the neck, chest, arm, forearm, thigh, and legs, and even in the retro-peritoneal connective tissue. These hæmorrhages in one form or another were observed in all the 54 autopsies that were made, varying in extent in the organs affected, according to the nature of the case. It may be as well, however, to give a brief summary of the *post-mortem* appearances observed in the various parts of the body.

Brain and Membranes.—The meninges were injected, but hardly any traces of acute inflammation were found, except in one case. Hæmatoma of dura-mater was once observed; excess of fluid in the arachnoid cavity was well marked in a few cases, the ventricles being full, and the choroid plexuses œdematous and swollen. The brain substance showed the gross lesions, but it appeared softer than usual, the convolutions being swollen and œdematous. In fact, stasis had produced changes in the brain similar to those in the lungs, namely, œdema, but in a less degree on account of its peculiar circulation. No hæmorrhage was found in the brain substance.

Spinal Cord.—The spinal cord with its coverings was examined in a few cases, but beyond congestion nothing else was noticable.

Pharynx.—The pharynx was generally found congested, especially so in several cases with infiltrations. A pseudo-diphtheritic membrane of a dirty greyish brown colour was seen in a few cases covering it, as well as the tonsils, and extending towards the larynx. The tonsils showed occasionally abscesses within their substance, with acute inflammation and some hæmorrhage.

Larynx.—The larynx was injected and inflamed, and in some cases hæmorrhage was noticed beneath its lining membrane. The structures surrounding it, as well as the soft parts that line it, were œdematous, and the aryteno-epiglottidean fold was often found to be greatly swollen and œdematous, blocking the entrance and exit of air. The pseudo-diphtheritic membrane, above referred to, was noticed in two or three cases to extend to the larynx with deep cervical buboes and infiltration. The laryngeal symptoms were, as a rule, well marked. The thyroid was generally found to be normal. The trachea showed nothing beyond injection.

Lungs and Pleura.—The lungs were practically "blood-full" and in most cases, when there was not much extensive pneumonia, greatly œdematous. On section, a large amount of sero-sanguineous fluid flowed out of them, in fact they seemed to be thoroughly soaked in it like a sponge. The mucus in the bronchioles was frothy and blood-tinged. If pneumonia existed, it was of the characteristic lobular type, in isolated patches, having a mottled grey appearance, generally well raised above the surrounding lung surface, and surrounded by a ring of conjested lung, gradually merging into healthy lung tissue. Hæmorrhagic infarcts, abscesses, and occasionally a stray tubercle was noticed in some cases. Pleurisy was sometimes met with, and was of a fibrinous character. Hæmorrhages more or less extensive were noticed on the parietal, visceral, and diaphragmatic pleura, the latter being the less frequent, and of the two former, parietal pleura being the most often noticed with large hæmorrhagic patches. Hæmorrhages were also observed in the connective tissue of the anterior and posterior mediastina. The bronchial glands were observed in a few cases to show the characteristic tumefaction and swelling of plague.

Heart.—Pericardial fluid was found in some cases to be larger in quantity than normal. Hæmorrhages were found on the pericardium, epicardium, and also endocardium. They were also met with in the substance of myocardium, which was generally soft and in a state of acute degeneration, occasionally fatty. In some cases it was so soft that it could be rendered pulpy between the finger and thumb. The cavities were generally dilated, the aorta normal, and occasionally fresh vegetations and hæmorrhages were observed on the valves.

Œsophagus.—It was generally normal but in such cases as had extensive hæmorrhage in the gastro-intestinal mucous membrane, it also shared in the same, and showed occasional hæmorrhages.

Stomach.—The stomach was hardly ever found to be in a normal state, its mucous membrane was generally bile-stained and showed hæmorrhages which were extremely characteristic, inasmuch as they were always punctate, the same character being also noticeable throughout the intestinal tract. In the absence of hæmorrhages there was occasionally intense injection and fresh erosion of the mucous membrane.

Liver.—The liver appeared generally slightly swollen "blood-full," and in a state of acute infective degeneration, occasionally fatty. Its substance was soft and friable, and the capsule at times adherent. In one case only emboli of plague bacilli were noticed. The gall bladder was almost always full, containing thick, viscid, dark-greenish bile.

Spleen.—The spleen showed an acute tumor or enlargement, but not large enough to become prominent. It was generally in a state of acute degeneration, the capsule was adherent, and its substance soft, pulpy, and, in some cases almost diffluent, the spleen appearing like a bag containing a homogeneous jelly. Hæmorrhagic infarcts were noticeable on its surface beneath the capsule, and occasionally small abscesses and cysts were present. On section, the colour was of a deep chocolate brown, sometimes almost hæmorrhagic, the Malpighian bodies were swollen, and could, occasionally be distinctly seen, and so also the trabeculæ and the fibrous structure. In cases that had suffered from malaria, the peculiar dark pigmentation due to it was also noticed.

Kidneys.—The kidneys were in a state of acute parenchymatous degeneration, the capsule adherent, and stellate, and other hæmorrhages and hæmorrhagic infarcts were noticed in its substance. The cortex was thin, and the pyramids swollen, and so also were the Malpighian capsules which were at times traceable; small cysts and abscesses were also noticed in its substance. The suprarenal capsule appeared to be normal.

Mesentery. — The mesentery also showed hæmorrhages, and they were, besides, noticed in the retro-peritoneal connective tissue and in the loose connective tissue in the abdomen, surrounding the various organs. In some cases hæmorrhages were noticed on the inner aspect of the abdominal wall and also between the recti muscles. The mesenteric glands were not often seen to be much enlarged or infected. Whenever there was plague infection, they showed slight tumefaction and swelling, and could be at once made out from their dark purplish colour and from the hæmorrhages surrounding them. Hæmorrhages were also noticed in the ovaries as well as around the iliac glands Whenever the glands along the course of the vena cava showed infection, hæmorrhages were noticed in the connective tissue and around them as well as in the intima of the vein extending several inches both ways. The same was also noticeable in the case of jugular and femoral veins.

Intestines.—The intestines, small and large, showed punctate hæmorrhages, and in one case these hæmorrhages extended from the œsophagus to the rectum. The mucous membrane was generally found slightly inflamed, and sometimes slight ulceration was noticed.

Buboes.—The appearance presented by the buboes was quite characteristic, and their section, whenever infected, always showed characteristic appearances such as are not met with in any other affection. They were swollen, enlarged and surrounded by sero-sanguineous or hæmorrhagic exudation. On section, they appeared of a dark-purplish colour and "blood-full," and the appearance, as stated above, was always characteristic. The exudations around the glands have been already described, and so also the cutaneous manifestations found during life. Whenever the parotid was enlarged it appeared like a large lobulated mass, varying in size from four to six inches, and on section all the small component lobuli stood out distinct.

Bones and joints.—The marrow of long bones like the femur was often noticed in a state of acute red degeneration, as in other infective diseases, and the synovial fringes in the knee-joint distinctly infected.

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The *post mortem* appearances above described are from 54 autopsies that were made in my hospital, 47 by the members of the Austrian Plague Commission, and the rest by the hospital staff. The thoroughness, the extreme care and the intelligence which characterised these *post mortems*, each of them being a complete demonstration, reflect great credit on Dr. Heinrich Albrecht, the head of the Commission, and his assistant, Dr. A. Ghon, who conducted them, and I must own my deep sense of obligation to them for the way in which they utilized the material I had provided, and which forms the basis of these notes, and whatever credit attaches to them belongs to Dr. H. Albrecht.

20,973. How many of the attendants suffered from plague?—Very few; altogether, I think, there were only six of them; all those who contracted the disease were living outside in infected houses and not on the premises, and it is impossible to say whether they had infection from the hospital or from outside.

20,974. Did not one of the doctors get plague?—Yes, that doctor was working in the hospital and his house was not situated in a very sanitary locality.

20,975. Do you disinfect floors in your hospital?—We have patent stone flooring now and we wash it with disinfectants frequently.

20,976. Do you disinfect the clothes of patients?—Yes.

20,977. How do you disinfect them?—They are dipped in sublimate, carbolic acid, or phenyle.

20,978. For how long?—For five or six hours in a solution of 1 in 1,000 of sublimate, and in 5 per cent. solution in the others. They are steamed according to the native fashion, and washed.

20,979. What is the temperature you get in your steaming apparatus?—It is over 200° Fahr., and the steaming is continued at this temperature for over an hour. The steam penetrates through all the clothing, &c.

20,980. Will you describe the steaming apparatus?—It consists of fire, with a shallow copper basin containing water, and there is a sort of plinth round about the vessel, which is fixed, and wet clothes are piled on the top to the height of two or three feet, and the whole is tied up with a big thick blanket so as to make it as airtight as possible. The fire is ignited underneath, and the steam passes through the wet clothes.

20,981. How far do you think the steam penetrates?—I do not think it penetrates very far. It is the native system which they have been using for ages past, and they have made no improvement in it.

20,982. Did you notice plague in inoculated people during that time?—I had only a few cases, and in them the disease seemed to take a very favourable turn.

20,983. How many cases did you see?—About 12, with one death.

20,984. Had these people certificates of inoculation?—They had; they generally came from institutions where all these records are kept. Some came from Sir David Sassoon's Reformatory, and some from other schools. In every case particulars were supplied with regard to previous inoculation.

20,985. Can you put in a table showing how long after the inoculation they got plague?—(The witness subsequently wrote as follows:—I regret that I am not able to furnish a table as desired, as it has not been possible to sort out all the charts. No case, however, was inoculated within six months of getting plague; in all cases the inoculation had been performed from 7 to 12 months before).

20,986. Have you had any experience of inoculation yourself?—No; I have done no inoculations.

20,987. I believe you admit the friends of patients to the hospital?—Yes

20,988. How many friends come in generally?—If the friends wish to attend on the patients we allow two, one for the night and one for the day. Visitors are allowed at all hours except during the visits of medical officers.

20,989. Do the friends of the patients get plague?—There was only case. The husband of a woman who was attending upon her got plague while in attendance on her. But that is the only case.

20,990. How long do the attendants stay in hospital?—Throughout the convalescence.

20,991. And they do not get plague?—No.

20,992. (*Mr. Cumine.*) Are the attendants allowed to sit and sleep on the floor?—They sit on the floor, and sometimes sleep there too.

20,993. (*Dr. Ruffer.*) You disinfect the floor, I understand?—Yes; formerly we had earthen floors, and new flooring has been put down lately.

20,994. (*The President.*) Had you had any cases among the attendants?—Only the one I have just mentioned.

20,995. (*Mr. Cumine.*) You used to pour down disinfectants pretty frequently?—Yes; phenyle or some powder: still, there was a lot of plague-infected material going into the ground and sinking into the floor.

20,996. Before the floor of the hospital was paved it was plain mud, was it not?—Ordinary earth.

20,997. Was there any cowdung upon it?—During the plague epidemic we had hardly time to cowdung the floor; it was cowdunged during the interval; but during the actual plague visitation it was not done, because that would have necessitated the patients being turned out, and that we could not do.

20,998. I wish to know whether the difference between the mud floor in your hospital and the mud floor in an ordinary native house is that the latter is cowdunged and yours is not cowdunged?—That is so, and also we use disinfectants in the hospital, which is not done in the case of ordinary houses.

20,999. (*Prof. Wright.*) Have you had any opportunity of testing Simond's theory, that plague is communicated by the bites of insects; have insects ever been very common in your hospital?—They are very plentiful, especially flies and mosquitoes, at this season.

21,000. Are the nurses and medical attendants bitten by fleas and bugs?—Certainly by fleas and mosquitoes.

21,001. Do you think that occurs to any great extent?—To a very great extent indeed.

21,002. You do not attach much importance to Simond's theory in view of that fact?—I do not.

21,003. You do not think that insects are very likely to carry infection?—The conditions are different in the hospital from those in the closed rooms of the houses; but so far as the hospital is concerned, the evidence points the other way.

21,004. How do you think the fleas in the hospital differ from those in private houses? Are fleas not continually passing from the patients to the medical officers?—The patients are more cleanly and we get rid of all their clothing, and I suppose most of the fleas and bugs would go too. But certainly the mosquitoes are there in very large numbers.

21,005. Have you formed an opinion that plague is less common in children than in adults?—No.

21,006. Have you seen any cases of coryza associated with plague?—No, nothing very marked. (Note added by witness on correcting proof of his evidence:—Some have been noticed since this evidence was recorded.)

21,007. Have you had any epidemic of pneumonia in the hospital; pneumonic cases which have infected other people there?—No.

21,008. Have you any rats in your hospital?—There are plenty of rats in the compound.

21,009. Have you noticed that these rats become infected with plague?—I have never seen a dead rat in the compound.

21,010. You have seen numbers of living rats?—Yes.

21,011. Do you know whether this is the experience in other hospitals?—I do not know that.

21,012. Do you think these hallucinations which you have described are all hallucinations of sight?—Of sight and sound, both.

21,013. Have you noticed that they are associated with a particular kind of headache?—Headache is always complained of, but nothing very marked to associate it with one form of hallucination or other.

21,014. Have you noticed that œdema occurs on the front chest immediately before death?—We find in axillary bubo there is sudden exudation, and the patient dies.

21,015. Have you seen any œdema in the chest when there was no axillary bubo?—No.

21,016. Have you ever seen what are called choleraic cases of plague?—No.

21,017. Do you think plague patients are made much worse when they are moved to the hospital during the course of their attack?—It all depends upon the state in which they are. I would not advocate the removal of a moribund patient, or one who is likely to die in a few hours; it would be of no use to the patient. I should say that the earlier the patient is brought to the hospital, the better chance he has of recovery.

21,018. You have not seen that treatment, independent of serum treatment, has any effect on the course of plague?—The only treatment which seems to have any effect is to keep up the state of circulation. Patients have been kept up for two, three, or four days simply by hypodermic injections of strychnine, strophanthus, and digitalis, where otherwise life would have ebbed away in a few hours.

21,019. (*The President.*) Did they ultimately recover?—No, most of them ultimately died; it is simply a question of doing all you can for the patient.

21,020. (*Prof. Wright.*) You have said that blisters occur in the course of the plague; do they occur in several regions of the body at once?—I have seen only two or three cases in which there were multiple blisters all over the body; they are generally single.

21,021. Those are secondary blisters; they are not associated with the source of infection?—The multiple blisters are secondary.

21,022. What about the single ones?—I think they are primary.

21,023. (*The President.*) With regard to these modified cases of plague, *pestis ambulans*, and *pestis minor*, do you know if they are infectious?—I do not think they are.

21,024. Have you any evidence one way or the other?—I have seen a number of cases—not in the hospital, but outside—in private practice, where they have occurred; and there have been no further cases in the same families.

21,025. Have you generally found pneumo-coccus in pneumonic cases?—In secondary pneumonia the pneumo-coccus was found along with plague bacilli.

21,026. In all the pneumonic cases of plague?—I do not think so; I have seen in primary pneumonic cases of plague only pure cultures of plague bacilli.

21,027. Pneumo-coccus does not occur?—That is so.

21,028. I understand that the impairment of articulation, to which you have referred, is very frequent?—It occurs in about 10 or 15 per cent. of cases.

21,029. It appears to be aphasia?—Yes.

21,030. Is there actual power of articulation or not?—I do not think there is any loss of actual power of articulation, because the patient understands fully what you say, and makes an effort to bring out what he ought to reply, but cannot say it; he might make a certain sound.

21,031. But not an articulate sound?—I do not think he can make any.

21,032. He understands what you say, and tries to say something in reply; but that something which he says is not intelligible?—Sometimes he cannot say anything at all.

21,033. With regard to the temperature which you describe, have you any temperature-curves which will illustrate your statement?—Yes, I can put in a chart.* There are a number of charts attached to the report† here which give the characteristics of the first temperature curves of the epidemic.

21,034. Have you any typical pulse-tracings?—Yes, I have a number of sphygmographic tracings‡ taken by Dr. Galeotti and Dr. Polverini.

21,035. How do you explain the pulse which you have described as being exactly the same as the pulse of aortic regurgitation?—The contractile power of the heart seems to be feeble, and the contraction is not able to expel the full quantity of blood which the ventrical contains; there is insufficient filling of the arteries.

21,036. I understand you to say that this pulse is like aortic regurgitation; do you know if there is actual regurgitation into the left ventrical?—I did not say actual regurgitation, but I saw it resemble it in this way, that the total quantity from the ventricals is not expelled and the arteries are insufficiently full.

21,037. In that case you would not have a large expansion of the artery?—That is so.

21,038. It ought to be a large expansion, ought it not—a large volume of blood projected into the artery?—The volume is much less than one would expect.

21,039. Then it is not a very typical aortic regurgitation pulse. Perhaps you will explain what you learn from the tracings?—Yes.‡

21,040. With regard to the occurrence of blood in the urine, you are quite satisfied that all these cases which you speak of as being cases of hæmaturia actually have blood corpuscles?—Yes; we have examined them and found it was not hæmoglobinuria, but hæmaturia.

21,041. (*Dr. Buffer.*) Could you tell us whether you have noticed any difference in the type of the diseases during these three epidemics?—I find the type of each succeeding epidemic becomes more severe; certainly the type at the present time is more severe than we have ever had.

21,042. Have you ever noticed a difference in the frefrequency of the lung symptoms?—They varied in different periods; at one period the lung symptoms are predominant, at others they are absent.

21,043. Is it so at present?—In the present epidemic the primary pneumonic cases were very frequent, but now we find they are very few.

21,044. Referring again to the cases of aphonia and aphasia, do you find that certain of those cases which, when spoken to, did not try to answer, did absolutely lose the power of even emitting sound?—Yes, they did.

21,045. (*Mr. Cumine.*) Are the cases of *pestis ambulans* confined to a particular season of the year?—No, they occur all through the epidemic, from its rise to its decline.

21,046. (*Prof. Wright.*) Do you find that the severity of the cases goes down towards the end of the epidemic?—Yes, it does.

21,047. You get a larger percentage of recoveries?—Yes.

21,048. (*The President.*) How is relapsing fever modified when it occurs in plague?—I should say that plague is modified by relapsing fever, and the way in which this occurs is very curious. The patient is brought in with all the symptoms of plague, and with bubo; the temperature runs a regular plague course for three or four days, and perhaps remains persistent up to the sixth day, and then we find a sudden drop exactly as in relapsing fever; after the bubo is incised the patient proceeds into convalescence. Then there is an interval between the fall of temperature and relapse. I have at present two or three cases in the hospital which are very typical of that.

21,049. Perhaps you will include these among your charts?—Yes*; *vice versâ* a patient comes in who has relapsing fever with no bubo, and during the apyrexial stage he develops femoral or inguinal bubo and seems to go through an attack of plague. Plague seems to be considerably modified by relapsing fever.

21,050. Then you say pyrexia occurs at a stage when it should have occurred in relapsing fever and not when it usually occurs in plague?—That is so. I should like to put in a statement with regard to the details of the various series of cases treated with Prof. Lustig's serum, as follows:—

TABLE showing the DETAILS of the various SERIES of CASES treated with Professor LUSTIG'S SERUM in 1898.

Months, 1898.	Series.	Horse No.	No. of Patients treated.	Died.	Recorded.	Percentage of Recoveries.
		I.				
March - - -	1	1st bleeding	24	18	6	
April - -	4	2nd „	41	30	11	
May - - -	8	3rd „	6	5	1	
August - -	12	4th „	3	1	2	
September - -	15	5th „	16	11	5	
Total - -	—	—	90	65	25	27·77

* *See* App. No. LXIV. in this Volume.

† Report on Bubonic Plague, by K. B. N. H. Choksy, Bombay, 1897.

‡ *See* App. No. LXV. in this Volume. No further explanation (*see* question No 21,039) has been received with these tracings.

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Months, 1898.	Series.	Horse No.	No. of Patients treated.	Died.	Recorded.	Percentage of Recoveries.
		II.				
April	5	2nd bleeding	13	8	5	
May	9	3rd „	17	6	11	
September	13	4th „	12	6	6	
Total	—	—	42	20	22	52·29
		III.				
March and April	2	1st bleeding	20	11	9	
April	6	2nd „	24	12	12	
May and July	10	3rd „	16	6	10	
September	14	5th „	10	3	7	
Total	—	—	70	32	38	54·28
		IV.				
April	3	1st bleeding	8	3	5	
May	7	2nd „	11	5	6	
July and August	11	3rd „	12	6	6	
October	17	5th „	12	6	6	
Total	—	—	43	20	23	53·48
		V.				
October	18	1st bleeding	12	8	4	
Total	—	—	12	8	4	33·33
Grand total	—	—	257	145	112	56·42

There is one point I should like to mention. Three years' experiences at the Hospital gives us an average mortality of 71·44 per cent. The following figures may be of interest to the Commission:—

TOTAL PATIENTS TREATED at the ARTHUR ROAD HOSPITAL up to end of January 1899.

3,919—died—2,800	71·44 per cent.
Died within 24 hours	29·36 „
Died within 48 hours	18·01 „
	47·37 „
Recovered under ordinary treatment	28·56 „
	75·93 „
Remain which could be influenced by serum	24·07 „
	100·00 „

From these 24·07 per cent. eliminate some that must die after 48 hours, others succumbing either through sudden heart failure, complications, pyæmia, intercurrent diseases, &c., say, about 10·07 per cent., thus leaving only 14·00 per cent. who could be pulled through by the serum.

21,051. (*Mr. Hewett.*) How many die in the first 24 hours?—About 30 per cent., and about 19 or 20 per cent. in the following 24 hours.

21,052. (*Dr. Ruffer.*) Unless you got a serum which acted immediately?—I should say that all these cases were past all hope; they could not possibly be saved except by a miracle.

(Witness withdrew.)

(Adjourned till to-morrow at Poona.)

At The Council Hall, Poona.

FIFTY-FOURTH DAY.

Thursday, 23rd February 1899.

PRESENT:

PROF. T. R. FRASER, M.D., LL.D., F.R.S. (*President*).

Mr. J. P. HEWETT.
Prof. A. E. WRIGHT, M.D.
Mr. A. CUMINE.
Dr. M. A. RUFFER.

Mr. C. J. HALLIFAX (*Secretary*).

Capt J. B. Smith, I.M.S.

23 Feb. 1899.

Captain J. B. SMITH, I.M.S., called and examined.

21,053. (*The President.*) You are in the Indian Medical Service?—Yes.

21,054. What are your medical qualifications?—M.B., M.C.H., and I am also Bachelor of Arts.

21,055. You are also the Civil Surgeon here?—No, I am the Assistant Civil Surgeon.

21,056. You are prepared to give evidence in regard to Karad, I understand?—Yes.

21,057. In the first place, would you tell us how plague originated at Karad?—As far as it is known it was an epidemic that was concealed.

21,058. Can you give us the history of its origin?—In the end of January and the beginning of February 1897, Mr. Young, one of the Survey Department, reported that there were about 13 or 14 cases in the Karad taluka. From that onwards nothing particular happened till the month of March, when the death-rate began to go up. The average death-rate for five years for the town of Karad was 21. The death-rate rose in March to 30, in April it rose to 40, in May it rose to 108, and in June it apparently fell to 52. Major Baker, I.M.S., who was the Plague Supervising Officer, thinks it was due to concealment. In July it rose to 230, in August there were 730 deaths, in September 313, and then it fell in October to 62, in November to 29, and in December to 26.

21,059. What is the normal population of this town?—About 12,000.

21,060. Did you ascertain how plague originated in the town?—No. It was concealed for two months, and it was not notified as plague till the 7th of July. On the 1st of May cholera was notified in the town, and most Medical Officers who have had anything to do with Karad have come to the conclusion that it was not cholera but really plague.

21,061. There was no special means of ascertaining what caused the people to die at that time?—No, except the ordinary Municipal register.

21,062. There was no inspection of corpses?—No, not at that time.

21,063. Having ascertained that there was plague, what did you do?—Before I went there, a Plague Flying Column was sent there on the 13th of July. I did not go there until the 2nd of September, when the epidemic was at its height.

21,064. What had been done before you went?—Search parties were instituted of the ordinary character, and corpse inspection was started. Patients were removed to hospital and their friends were segregated, but no evacuation was carried out.

21,065. By friends, do you mean contacts in general?—Yes.

21,066. There was a special difficulty, you think in the diagnosis of the earlier cases. What was that due to?—It seems to me that very possibly the earlier cases were not bubonic.

21,067. On what grounds?—I have no grounds; it is a hypothesis. I will give you the reason for it afterwards. In the taluka, in the villages round Karad, where plague was in an earlier stage than in Karad, the cases were very largely non-bubonic, so that one would assume it was possibly the same in Karad itself. In several villages I afterwards heard that the earlier cases were non-bubonic. In one village, called Kesa, the earlier cases were always non-bubonic,

21,068. How were these cases known to be plague cases at all?—From their general resemblance to plague—the plague countenance, and the eyes.

21,069. You have seen such cases?—Yes.

21,070. Will you state the grounds on which you came to the conclusion that they were plague cases?—They have absolutely the same appearance as a plague patient, except that there were no buboes—the suffused eyes, peculiar tongue, the stupid expression of the face, the high temperature, and the hot burning skin, and their rapidly fatal termination.

21,071. What is this peculiarity about the tongue?—It is a tongue whitish in the centre, with red edges and tip.

21,072. You are arguing by analogy?—Yes, because probably nobody knows what actually did take place in Karad at first.

21,073. After you came on duty, were the measures extended in any direction?—The measures were very largely extended. The staff of the Plague Column was considerably increased. A large number of coolies were imported from Bombay, some European Inspectors were sent, and also another doctor.

21,074. That is in regard to the staff, but as to the measures themselves, what was the extension?—The only extension was that they were made more perfect. No extra measures were taken; the measures previously in existence were carried out more perfectly.

21,075. Did you ever adopt evacuation on a larger scale than you have described?—No, not in Karad town.

21,076. It was only partial evacuation?—We had no evacuation.

21,077. Only the actually affected people and the contracts were segregated?—Yes. It was in the middle of the rains, and it was extremely difficult. The whole trade of the place was paralysed, and it was practically impossible to get accommodation for the people. We had the greatest trouble in getting accommodation for the ordinary segregation and observation camps.

21,078. In those houses where the inmates had been removed as you have described, what measures did you adopt?—We disinfected them with perchloride of mercury, we opened the roofs, and made large holes in the roofs all along the ridge of the pole, two feet on either side; I took off all the tiles and all the matting underneath, and exposed them to the air and then disinfected them with perchloride spray.

21,079. How long were they left unoccupied?—They were certainly left unoccupied for ten days, the period during which the people were segregated; in the majority of cases they were left longer.

21,080. When re-occupied, did any plague cases occur in these houses?—Not to my knowledge.

21,081. Do you mean that you have no knowledge?—I mean that I should probably have heard of any case. I should have known of any case if it had occurred. There was a case afterwards, in the general disinfection of the town.

21,082. There was a further measure then; what was that measure?—Yes. We did not know what houses were affected in the first instance, and so we commenced at one end of the town, and disinfected right on to the other end.

21,083. About how many houses would that represent?—We disinfected altogether about 2,200 houses before I left, and they were still at it.

21,084. What happened to the inmates of these houses while they were being disinfected?—The inmates had to live in their own verandahs; there was no accommodation for them elsewhere. They refused to go to the segregation camp.

21,085. Were the clothes, and everything in the houses, disinfected?—Everything was disinfected, except things that would be ruined by the perchloride.

21,086. Clothes also?—Yes.

21,087. How was that done?—By perchloride; we steeped everything in perchloride, except woollen and leather goods, which were put out in the sun.

21,088. What was the general result of these measures in so far as the progress of plague was concerned?—The plague went down very rapidly. Here is a chart showing the course of the epidemic. (The witness handed in two charts).*

21,089. In what particular does it show it?—When the measures became effective, the plague mortality and the plague attacks went down. About August the 23th, Major Baker visited the place and got an extra staff, and things were put on a better basis, and from that onwards the death-rate went down, and the attacks went down.

21,090. You had commenced these measures in August?—They were commenced at the end of July first, and they were put on a better basis in the beginning of September. The plague broke out in July, and the plague measures really only began about the end of July. The Plague Column arrived there in July.

21,091. The plague continued to increase?—Yes, until the very end of August.

21,092. While these measures were being carried out?—They were being carried out in an ineffective way.

21,093. Will you describe the difference between ineffective and effective in this case? What do you mean by effective in one instance and ineffective in the other? What was the difference?—Everything was done more thoroughly after the beginning of September. In disinfection, at first, they had too small a number of coolies to overtake the houses infected; after that we got enough coolies to keep pace with the epidemic as it went along. The houses were disinfected within one or two days after they were infected.

21,094. When that was done thoroughly, you think the epidemic began to fall?—It began to fall, but whether it was a coincidence or not, I cannot say.

21,095. Did you find much difficulty in carrying out disinfection thoroughly?—I had the greatest difficulty, extreme difficulty.

21,096. What kind of difficulties?—Unless you are constantly present with your coolies, they have a tendency to shirk their work, partly through laziness and partly through corruption.

21,097. Is there any special difficulty in villages; you refer to that in your précis?—The difficulty of providing European supervision. Unless disinfection is done with European supervision, I do not think it is much good, because these coolies, if you do not watch them continuously, get very careless.

21,098. What kinds of floors had the houses you were dealing with?—Earthen and cow-dung floors.

21,099. Did you do anything more than apply perchloride or other disinfectant solutions to these floors?—No; the floors were not touched, except to soak them with perchloride solution.

21,100. The walls and the ceilings also?—The walls, and the ceilings, the floors, the furniture, and everything was taken out of the house that could be removed, and before returning, they are put through solution.

21,101. What did you regard as the chief channel of infection?—I cannot give any opinion on that.

21,102. Have you any idea as to any channels of infection; how is the plague communicated from one person to another?—It seems to be a local infection of the house, but I had no leisure at the time to follow up the cases one by one.

* *See* App. No. LXVI. A, and B, in this Volume.

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21,103. You have no instances in which it seemed to be conveyed, for example, by one person to another?—No. I had one instance where it seemed to be conveyed by furniture, but as the people were related to one another, of course there is a flaw in the argument The family got plague in the house before disinfection. After the people had been taken to the hospital they sent some of their furniture to a brother-in-law's house and in about four or five days some people in this house got plague. But then that may not have been by the furniture, as they were nearly related.

21,104. They may have come into personal contact with one another?—Yes. That is the only instance I know of communication by furniture apparently.

21,105. Have you any instance as to the conveyance by animals of the infecting virus?—No, all the rats were dead at the time I got there.

21,106. You had some experience with Yersin's curative serum?—Yes.

21,107. Would you detail that experience?—There are 32 cases treated. I propose to hand in a statement showing the details.

21,108. Perhaps you will summarise them?—There are 32 cases treated, and of those 21 died. There were two cases which were doubtful as to whether they were plague or not, and two were very mild cases.

21,109. These recovered?—Yes. The statement is as follows:—

STATEMENT of CURATIVE INOCULATIONS performed by DR. SIMOND, at KARAD, with M. YERSIN'S SERUM.

Serial No.	Register No.	Names.	Age.	Sex.	Caste.	Street.	Date of Admission.	Date of Death.	Date of Discharge.	Dr. Simond's Notes.
1	639	Chandra Subhana	9	Female child.	Mahratta	Mangalwar Pet.	5.9.97	7.9.97	—	Bubon inguinal droit et pneumonie 2e jour de la maladie, cas très grave, 40cc. sérum en 2 fois.
2	638	Datto Ram Krishna	7	Male child.	„	Somwar Pet.	„	5.9.97	—	Bubon axillaire et pneumonie à droit 2e jour de la maladie, cas très grave, 15cc. de sérum injecté 6h. avant la mort.
3	639	Krishna Mahadéo	32	Male	„	„	„	6.9.97	—	Bubon inguinal droit parotide embarrassée, 2e jour, cas très grave, 60cc. de sérum en 2 fois.
4	643	Shita Kom Pandoo	25	Female	„	Karad	6.9.97	8.9.97	—	Bubon axillaire et bubon cervical 3e jour de la maladie, cas très grave, 60cc. sérum en 2 fois.
5	649	Dhond Bhat Manikhat	50	Male	Brahman	Guruwar Pet.	„	9.9.97	—	Pneumonie double, pas de bubons, 2e jour de la maladie, cas très grave, 90cc. sérum en 4 fois.
6	647	Bhima Kom Natha	30	Female	Mahratta	Karad	„	7.9.97	—	Pneumonie à droit, et petit bubon inguinal aphasie 2e jour, cas très grave, 40cc. de sérum en 2 fois.
7	650	Shindoo Rama	39	Male	„	„	„	—	5.10.97	Bubon inguinal droit 4e jour de la maladie, cas léger, 20cc. de sérum injecté à sa demande sans nécessité.
8	648	Kasai Kosh	60	Female	Weaver	„	„	13.9.97	—	Bubons cervicaux, bubons axillaires et inguinaux, 3e jour, cas très grave, 50cc. de sérum en 3 fois.
9	657	Yesoo Ramchandra	14	„	Mahratta	„	7.9.97	„	—	Bubon inguinal droit, coma, 2e jour de la maladie, cas très grave, 30cc. de sérum en 2 fois.
10	658	Duyanoo Jivaba	50	„	„	„	„	7.9.97	—	Bubons cervicaux et pneumonie 3e jour de la maladie, cas très grave, 30cc. de sérum en 1 fois injecté 7h. avant la mort.
11	652	Bahiru Narayen Kosti	21	„	Weaver	„	„	—	15.9.97	Jeune homme auquel j'ai pris du virus dans le bubon. Bubon inguinal à gauche 2e jour, cas très grave, avec stupeur et mutisme, 60cc. de sérum en 3 fois.
12	654	Chandra Sakharam	20	Female	Mahratta	„	„	10.9.97	—	Bubon cervical et bubon inguinal 2e jour, cas très grave avec coma, 80cc. de sérum en 3 fois.
13	681	Govind Gopala	38	Male	„	„	8.9.97	„	—	Peste intestinale, pas de bubons ni de pneumonie 3e jour, cas très grave, stupeur et aphasie, 20cc. de sérum.
14	680	Keshoo Rajoram	30	„	„	„	„	—	6.10.97	Pas de bubon ni de pneumonie, fièvre forte 3e jour, cas douteux, 40cc. sérum en 2 fois.
15	684	Radha Maruti	24	Female	„	„	9.9.97	11.9.97	—	Bubon axilliare et stupeur profonde, 2e jour, cas très grave, 40cc. de sérum.
16	688	Kashi Kom Tukoram	18	„	Bhoijee	Bhoijeelane	„	9.9.97	—	Bubon inguinal et stupeur absolue 2e jour, cas très grave, 30cc. serum.
17	691	Ganoo bin Vithoba	20	Male	Mahratta	Karad	10.9.97	15.9.97	—	Bubon inguinal, délire, 4e jour, cas très grave.
18	693	Manjula Yessoo	25	Female	„	„	„	—	5.10.97	Bubon inguinal 2e jour, cas de gravité moyenne, 40cc de sérum en 2 fois.
19	704	Yema Kom Rama	20	„	„	Mangalwar Pet.	11.9.97	15.9.97	—	Bubon inguinal 2e jour, cas très grave, 30cc. sérum.
20	711	Walli Mohamed	60	Male	Musalman	„	12.9.97	—	7.10.97	Bubon inguinal droit 2e jour, cas de gravité moyenne, 4cc. sérum en 2 fois.
21	717	Chandra Sakhoram	20	Female	Mahratta	Karad	14.9.97	—	17.11.97	Bubons inguinaux à gauche et à droit 2e jour de la maladie, cas très grave avec stupeur et mutisme, 80cc. sérum en 3 fois.
22	708	Bhagoo Kom Hari	18	„	Sali	„	„	15.9.97	—	Bubon inguinal gauche, coma, 4e jour, cas très grave. Peste contractée dans l'hopital, 30cc. sérum.
23	724	Kashinah Mahadeo	30	Male	Brahman	Mangalwar	„	—	12.10.97	Bubon inguinal 2e jour, cas grave, 90cc. de sérum en 4 fois.
24	735	Appa Sakhoram	50	„	„	Somwar	15.9.97	—	17.10.97	Ni bubon ni pneumonie, fièvre forte, 105°, cas douteux, peut-être peste intestinale légère, 40cc. sérum en 2 fois.
25	743	Mahadeo Babajee	23	„	Mahratta	„	16.9.97	22.9.97	—	Peste intestinale sans bubons ni pneumonie 2e jour, cas très grave avec stupeur et délire, 70cc. de sérum en 3 fois.
26	753	Radha Kom Rama	25	Female	„	Karad	17.9.97	—	6.11.97	Bubon inguinal gauche 1e jour, cas très grave avec stupeur et convulsions des fièvres mutisme, 80cc. de sérum en 3 fois, parait evolver vers la guérison.
27	757	Moruti bin Nathajee	28	Male	„	„	18.9.97	21.9.97	—	Bubons cervicaux à droit et à gauche 1e jour, cas très grave, 60cc. sérum en 3 fois.
28	763	Haribai Narayen	55	Female	Hindu	Somwar	19.9.97	19.9.97	—	Bubon axillaire 4e ou 5e jour cas très grave avec faiblesse extrême du cœur, 30cc, sérum quelques heures avant la mort.
29	772	Sukhina Kom Shaku	44	„	Musalman	Karad	21.9.97	—	22.10.97	Bubon inguinal gauche petit et indolent 3e jour, cas de gravité médiocre, 40cc. de sérum en 2 fois.
30	778	Kondai Ganoo	40	„	Hindu	„	22.9.97	30.9.97	—	Bubon inguinal droit 2e jour, cas de gravité moyenne, 40cc. de sérum.
31	781	Bhagirathi Kom Krishna.	32	„	„	„	„	—	24.9.97	Bubon axillaire gauche 2e jour, cas léger, 10cc. de sérum injecté sur sa demande sans nécessité.
32	785	Tukoram Babajee	14	Male	„	„	24.9.97	24.9.97	—	Bubon inguinal droit, coma, 3e jour, cas très grave, 30cc. sérum quelques heures avant le mort.

STATEMENT showing the PERSONS who got PLAGUE after being INOCULATED by Dr. SIMOND at MASUR, &c.

No.	Name.	Age.	Sex.	Caste.	Occupation.	Date of 1st Inoculation.	Date of 2nd Inoculation.	Date of Attack with Plague.	Date of Death.	Remarks.
1	Vishnoo Ganesh Bapat	22	Male	Hindu	Hospital Assist.	10.9.97	—	1 .10.97	16.10.97	
2	Parashram Balwant Fadke	25	„	Brahman	P. Candidate	„	—	14.10.97	„	
3	Rama Raju Bhosle	45	„	Mahratta	Labourer	„	—	—	—	Was attacked, but recovered.
4	Ganoo Babajee	18	„	Hindu	Watchman	„	—	16.10.97	19.10.97	
5	Bala Wald Hasan Faral	26	„	Musalman	Taral	„	—	10.10.97	15.10.97	
6	Gangadher Jaganath	38	„	Hindu	Postmaster	25.9.97	—	15.10.97	20.10.97	
7	Wasudeo Sidoram Khandeker	32	„	„	Schoolmaster	„	—	24.10.97	27.10.97	
8	Dhondi Rama	25	„	Wani	Trader	„	—	—	—	Was attacked with plague.
9	Ramchandra Narayen	23	„	Hindu	„	„	—	—	—	Out cured.
10	Gundoo Bhiyanga	20	„	„	„	„	—	11.10.97	19.10.97	
11	Bali Rawjee Patil	39	„	„	Farmer	„	—	20.10.97	25.10.97	
12	Parshotam Narayen	45	„	„	Trader	„	—	—	—	Died at Kalgoon.
13	Krishna Keshao	16	„	„	„	„	—	7.10.97	14.10.97	
14	Duyanoo Mathari	10	Child	„	Schoolboy	„	—	2.11.97	7.11.97	
15	Shauker Narayen Tate	7	„	„	„	„	—	26.10.97	1.11.97	
16	Narayen Vasoodeo	7	„	„	„	„	—	18.10.97	—	Recovered.
17	Waman Chuitamau	45	Male	„	Trader	„	—	—	—	Attacked with plague and recovered.
18	Krishna Ganpata Teli	35	„	„	„	„	—	27.10.97	2.11.97	
19	Rama Gundappa	30	„	„	„	„	—	—	—	Had plague, but recovered.
20	Heri Kom Lawla	35	Female	„	Nil.	„	—	21.10.97	26.10.97	
21	Rama Krishna	38	Male	„	Cultivator	„	—	23.10.97	27.10.97	
22	Damodher Govind	12	„	„	Kulkarni	„	—	23.10.97	1.11.97	
23	Bagoo Antajee Aphale	7	Child	„	„	„	—	18.11.97	22.11.97	
24	Aki Tukaram Gujar	7	„	„	Nil.	„	—	6.9.97	10.11.97	
25	Walli Pandoo	6	„	„	„	27.9.97	—	3.10.97	9.10.97	
26	Dhowbhai Wald Mohidin	45	Male	Musalman	Cultivator	„	—	21.10.97	26.10.97	
27	Pandoo Joti Matane	25	„	Mahar	„	„	—	6.12.97	10.12.97	
28	Hasan Wald Hajarat	7	Child	Musalman	Taral	„	—	6 11.97	7.11.97	
29	Rajoram Sakhoram Sutar	12	Male	Mahratta	Carpenter	„	—	13.10.97	16.10.97	

21,110. (*Dr. Ruffer.*) Where was the serum used?—In Karad town.

21,111. (*The President.*) Was it used in the hospital?—Yes.

21,112. Did you apply it?—No. Dr. Simond applied it.

21,113. Do you know the details?—More or less.

21,114. Do you know what dose was given?—The dose is all given in the statement, which also gives Dr. Simond's own notes on the subject.

21,115. What was the general result in your opinion?—That it was ineffective, and of no use.

21,116. What was the mortality?—The mortality was as bad (65·6 per cent.) as the average hospital mortality.

21,117. The mortality was greater than the average hospital mortality?—Dr. Simond's cases were, perhaps, better diagnosed than our general Plague Hospital statistics. I should say the mortality rates were about the same.

21,118. Have you had some experience of prophylactic treatment?—Yes. I have very little experience about Haffkine's. In Karad town and the districts we did 136 cases altogether, and so far as I know none of those people got plague.

21,119. You guard yourself; have you not been able to follow the history of these persons?—The ones in Karad up to the time I left had not got plague. I know about 101 up to the time I left Karad that had not got plague.

21,120. How long after inoculation would that be?—A varying period; some were inoculated a fortnight before I went away, and some were inoculated very much earlier than that.

21,121. How much? What was the longest interval and what was the shortest?—I should say about a month and a half or two months, probably. Of course we commenced inoculating at the time when plague was practically over in Karad.

21,122. This was the period of the fall of the plague?—Yes, at the end of the plague.

21,123. About when was that?—About the middle of November the death-rate in the town was down to normal. The reason these people were inoculated was that they wanted to go out into the infected villages for trading purposes. Sixty-one people got inoculated on that ground.

21,124. What is your opinion of the value of that evidence as showing whether there was any protection produced or not?—I should think, as there were 61 people who went backwards and forwards to infected villages, they stood a very good chance of getting plague if they had not been inoculated.

21,125. Are you aware of any number of people who were not inoculated, who also went backwards and forwards between the villages?—You will see them marked in the upper chart.* Those cases that occurred, the imported cases, were amongst the uninoculated people who went backwards and forwards.

21,126. Do they represent a larger proportion than those actually inoculated?—I should say they represented about the same.

21,127. (*Mr. Hewett.*) I do not know whether you stated the population of Karad?—It was about 12,000 in 1886.

21,128. Was there an Assistant Surgeon there when plague broke out?—There was a Hospital Assistant.

21,129. I suppose he had not had any previous opportunity of seeing a case of plague?—No.

21,130. Apparently plague went on till the end of January 1898?—Dropping cases, yes. Most of them were imported as you will see by the upper chart.*

21,131. I see that the curve goes up and down almost evenly. The epidemic went up to a certain point, and then down again. The descending line is very like

* *See* App. No. LXVI. A in this Volume.

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the ascending line. Do you think that this indicates that the suppression of the epidemic was clearly due to the measures taken?—Only this, that when measures began to be very thorough the epidemic began to go down; it is a coincidence.

21,132. When do you think that the measures began to be done very thoroughly?—I should say about a week after I went there.

21,133. When was that?—I went there on September the 2nd.

21,134. The plague had already begun to decline?—It had, but Major Baker had been there before me, and he had the searching done more thoroughly.

21,135. When did he go there?—On the 26th of August. A slight decline took place at once.

21,136. But starting from the week ending the 28th of August which was the week of the maximum number of attacks there were 518 attacks in the four preceding weeks, and 479 in the four succeeding weeks; do you think those facts show that it was the measures that caused the epidemic to decline?—All I can say is that it is a coincidence, if they have nothing to do with it.

21,137. Would you expect that the decline would be more rapid if it was the result of the measures taken?—No, I do not think so.

21,138. What is the particular measure to which you attribute the decline of the epidemic at Karad?—I consider disinfection had the great effect, and taking the contacts out of their houses.

21,139. Have any of the houses which were disinfected in Karad become re-infected?—They have within the last month or two.

21,140. Have you been able to trace this re-infection to people coming from other places?—I have nothing to do with Karad now, but I have been told by people on the spot that they have been infected by people in the Pathan taluka.

21,141. Does your experience at Karad show, in your opinion, that in order to suppress an epidemic of plague, it is not necessary to insist upon evacuation of the place?—If I have to go back to Karad, and at a suitable time of the year, I should prefer evacuation.

21,142. On what ground?—On the ground of the effect it had in villages. People were turned out of the villages by executive order, as soon as the rains had subsided, and the death rate went down very rapidly.

21,143. (*The President.*) I understood you to say you had no experience of complete evacuation?—That was in charge of the Medical Officer who actually did the work, and I only dealt with the subject as his superior officer. He is at home now, so he cannot be called.

21,144. (*Mr. Hewett.*) Were these villages evacuated during the rains?—They were evacuated immediately after the rains. They were evacuated partly by the will of the people themselves. As soon as the rains ceased they went out into their fields.

21,145. Do not you think that evacuation on black cotton soil is very difficult to effect during the rains?—That was the difficulty in dealing with the people in Karad and the villages during the rains; it was impossible to do it.

21,146. When you evacuated these villages after the rains were the people whom you turned out allowed to go where they liked?—They were in most villages.

21,147. Do you think that was a good plan?—I think there is a great risk of their going to other villages.

21,148. But you think it is a good thing to get them out of the infected site?—Yes. There are two kinds of villages that one must take into account; agricultural villages, and commercial villages. The agricultural villagers are not likely to leave their village, they are accustomed to go out and live in their fields in the cold weather, and in the dry season, and they have no particular object in going off to other villages; whereas in commercial villages weavers, for instance, cannot carry on their trade in the fields, and they must go to some place where they can get a house.

21,149. Does that chart* show the results of the evacuation of these villages?—I think so. One shows the rainfall, and the other the course of the epidemic. You must make allowance in these charts, however, for the fact that the returns are made by the village officers, who are very slow in sending in their information with regard to plague. But as soon as the rain ceased the epidemic went down, in about three weeks I think.

21,150. Did you observe any instance in which a village having been evacuated, because a particular portion of it was infected, the inhabitants, on returning to the village, got plague in other portions of it?—I think, as a matter of fact, the whole of the village was evacuated. The Second Assistant Collector, who was the Chief Plague Authority, issued an order that they were to clear out, and they did, into the fields. There was no partial evacuation at all.

21,151. I was asking about what happened when the people were allowed to go back to their villages?—I was not there then.

21,152. Were the villages entirely disinfected?—They were supposed to be.

21,153. Did not you find that a difficult process in a village?—Yes, it is almost impossible in the villages.

21,154. Why?—Because you have to trust to people of a very low social scale who are very open to bribes, and who in many cases are careless about the process, and think that things are of no importance which might really be of great importance.

21,155. Do you think that it is essential to disinfect the roof of a house?—That I am not in a position to say. I am not a bacteriologist.

21,156. Do you think that it is possible to disinfect the roof of an ordinary house in a village?—I do not think it is possible to do it thoroughly; I am almost certain it is not. What I should propose to do in a village, as soon as a plague case is reported, is to have the house in which it occurred immediately unroofed. In a village you have not any police, as a rule, except the ordinary village police, and it is impossible to make certain that the people do clear out of the infected houses. But if you take the roof off, I do not think it matters very much whether the people live in the house or not; they would have lots of fresh air and sunlight.

21,157. Do you know whether the villages which were infected in Satara in 1897, have become re-infected now?—As a matter of fact I do not think they have ever been absolutely free since.

21,158. (*The President.*) Were those villages evacuated?—Yes.

21,159. (*Dr. Ruffer.*) Can you point out on this chart the day when the evacuation took place?—I am sorry I cannot give you the order, but as soon as the rains were over the people began to clear out at once, partly of themselves to attend to the cultivation of their fields.

21,160. Looking at this chart it would appear that 10 weeks elapsed before the disease disappeared?—Yes, but it went down with a run. The fair weather began in the week ending November the 6th.

21,161. When did the disease almost disappear?—The lowest figures are for January the 15th.

21,162. How many weeks is that?—Ten weeks.

21,163. How long did it take in the village where no evacuation took place?—Seven weeks.

21,164. That is seven weeks as contrasted with 10?—Yes.

21,165. What then is the benefit of evacuation?—It is possible to carry it out effectively, while it is not possible in villages to carry out disinfection.

21,166. You said you stopped the epidemic quicker by evacuation, but, in this case, the epidemic seems to have stopped quicker by disinfection?—I believe you can improve upon that by adding evacuation to disinfection.

21,167. Why do you think so?—Because you have the people absolutely away from the infected site.

21,168. But in the infected villages it took a longer time?—As I told you, it took some time for the people to clear out, to make certain that the rains had stopped. Those are the weeks that are reported at the Mamlutdar's office, and it takes the village officers a certain amount of time to send them in. They are very slow in sending in a document of that kind, so you must allow those returns to represent, I should think, two weeks earlier.

21,169. You saw the cases treated by Dr. Simond?—Yes.

* *See* App. No. LXVI. B in this Volume.

21,170. I see he mentions two cases of intestinal plague. Have you any experience of that form of plague?—No, not except what he has there. I should have called that the ordinary non-bubonic form.

21,171. Septicæmic plague?—Yes, septicæmic plague.

21,172. Have you ever seen a case of what you might call intestinal plague, where the intestinal lesions were very clearly marked?—No, I cannot say that I have.

21,173. Have you any experience of plague *post mortem*?—No.

21,174. (*Prof. Wright.*) What inducement has a villager to offer bribes to a disinfecting coolie?—To save his kit being put into perchloride solution.

21,175. Does that spoil the clothes?—It does not spoil cotton clothes, but it spoils woollen clothes, and it spoils leather.

21,176. (*The President.*) In those villages that you have referred to, what was the time occupied in effecting evacuation?—That I could not tell you. It was left to the village officers to turn the people out, and in some villages they would be quick, and in others slow. It was done simply by an order of the Second Assistant Collector, and issued to village officers.

21,177. You have no personal knowledge?—No.

21,178. How do you know they were ever entirely evacuated?—I heard it from the Medical Officer who was under me, and from the Assistant Collector.

21,179. So far as you know, how long did it take?—I would rather not answer that question; I do not know.

21,180. It may have been a very long time or a very short time?—I should think it was within a fortnight in all cases, perhaps less.

21,181. What means were taken to prevent evacuated persons from returning to the houses in the village or villages?—The village officers were supposed to prevent that, and that is the weakness of the system; you have to trust so much to village officers in these matters.

21,182. You do not think that is an effective method of preventing the return; you think they probably did come back?—It is not an effective measure, and I think that people probably did come back every now and then. They would give a rupee or so to the village officer and go back for kit, or to stay in the house.

21,183. You cannot present these as a model example of evacuations; there were a great many flaws in the process?—No, I do not present them as a model example. There were a great many flaws. There were no means to prevent the people going to other villages, which is a very great flaw, in my opinion. There was one village which was taken in hand by Dr. Robertson. He passed all the people out of the village through an observation camp, and allowed them to go anywhere. He then got all the village disinfected, and he brought them in again in the same way through the observation camp, and that village remained uninfected and I believe it is still uninfected.

21,184. You think that plague was stopped in the first case?—Yes, and so far as I know, it has not occurred again in that village.

21,185. Within that time, was it stopped among the evacuated?—I think there was only one or two cases among the evacuated altogether.

21,186. Have you got any figures?—I cannot give any figures. This was a year and a half ago, and I am now away from my office, and I have no means of procuring those figures.

21,187. You have no access to the records?—No.

(Witness withdrew.)

Lieut.-Col. W. J. Fawcett, R.A.M.C., called and examined.

21,188. (*The President.*) You are in the Royal Army Medical Corps?—Yes.

21,189. And you can give us evidence about Poona and Kirki?—About Poona and Kirki cantonments, and the suburban Municipality from the commencement of work by the Committee formed by Government under the Presidency of General Duncan, on the 17th November 1897 up to August last, when I left the Committee. But plague had ceased in the portions of the country under our control about the 11th of March, which was the date of the last case.

21,190. (*Dr. Ruffer.*) Could you tell us what the population of the Poona cantonment was at the beginning of the plague epidemic?—It was believed to be between 25,000 and 30,000 people.

21,191. When was the last census taken?—That I could not say. The above was the information I received at the time.

21,192. What was the population in Kirki cantonment?—The cantonment bazar about 2,000, and the followers of the Royal Artillery 1,700; but that is merely a rough guess.

21,193. Plague appeared first in the Poona cantonment on the 27th of June, did it not?—Yes.

21,194. There are two cases among the followers of the 2nd Bombay Lancers?—Those were the first recorded cases.

21,195. Have you any reason to believe that there was plague before that?—I was not present at the time. I fancy it is very probable that there were cases of plague before that.

21,196. Why?—From what I have seen of the difficulty of diagnosing first cases of plague.

21,197. You say in your précis of evidence, "On the 2nd of July a sawar was attacked." What is a sawar?—A sawar is a trooper of a Native Cavalry regiment.

21,198. On the 4th there were three cases, on the 5th, one child, and on the 6th, two sawars and two children, all in the 2nd Bombay Lancers?—Yes.

21,199. You have been told that the disease "had been imported with grain purchased by the regiment in Bombay." Could you give us any evidence as to that?—Not the slightest evidence. It is contained in a Report* made by the late General Duncan, and when writing the report on plague in Poona I did not feel myself justified in altering it in any particular. I do not hold myself responsible for that statement. At the time it met with general approval.

21,200. (*The President.*) Does that apply to the whole of this Report*?—Not to the whole of it, only to the first five pages. The foregoing is a statement with which I do not agree. I see no grounds for agreeing with it.

21,201. (*Dr. Ruffer.*) Why not?—My experience of plague leads me to believe that it is not at all probable that grain will carry the germ of plague for any considerable length of time.

21,202. In this particular instance?—In this particular instance I know that plague existed in the villages around the cantonments, although I cannot prove it, before June. I know it from my experience in talking to the villagers, and in the bazars afterwards, that there was plague in the surrounding country before that.

21,203. Then the plague spread to other squadrons of Cavalry in the cantonments, did it not?—In the Cavalry lines in the cantonment, yes.

21,204. Can you tell us when the first cases occurred in the chawls in the borders of the lines?—I could not. I was not present.

21,205. Have you any evidence to show that there were cases in the chawls before the regiment was attacked?—None whatsoever.

21,206. Could you tell us whether there had been plague in the Kirki bazar during that time?—There were no cases in June. In July there were cases in the bazar in Kirki. There had been three cases in May, but none reported in June.

21,207. Do you think that these were really the first cases, or only the first cases that were noticed?—I think they were the first cases that were noticed, but that must be a matter of opinion.

* "Report on Plague in Poona and Kirki cantonments and suburban limits from 17th March 1897 to 19th March 1898," by Lt.-Col. W. J. Fawcett, not reprinted with the Proceedings of the Commission. The opening portion of this report had been written by Lt.-Gen. J. Duncan.

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21,208. On the 7th August disease broke out in the Royal Artillery followers' lines?—Yes.

21,209. And these followers were moved block by block into the health camps?—Yes.

21,210. Were the lines disinfected?—They were disinfected; at least, it is reported so.

21,211. How?—By the use of perchloride of mercury, I understand.

21,212. You did not do it yourself?—I was not there at the time.

21,213. Did any cases of plague re-occur when the regiment went back to those lines?—In Kirki. I think it would be better for me not to answer that definitely because I was not there, and the records kept at the time were extremely confused, and any information I could give you that I derived from records would be practically worthless with regard to that portion of the epidemic in Kirki about the re-infection.

21,214. Why do you think it is worthless?—I have seen the records, and I have conversed with men who were working at the time before the appointment of Major Temple. They were overworked; it was raining night and day, and there was a scene of confusion; there were not sufficient hands and there were not sufficient heads.

21,215. Major Temple assumed charge on the 20th of August?—Yes.

21,216. Can you tell us what measures he enforced?—He commenced by instituting search parties, which were very carefully carried out indeed, that is to say, I think from the time he commenced work it would have been impossible to conceal a case within the limits of the cantonment. The bazar at that time was almost deserted; instead of containing 2,000 there was not probably more than 600 people in it; the lines were being evacuated at the time.

21,217. What had become of the 4,000 people?—The other 1,600 people. There were 2,000 inhabitants of the bazar alone who fled here and there throughout the country—melted away. It would be almost impossible for a case of plague to exist for 24 hours without being discovered under Major Temple's arrangements. On discovery of a case it was at once sent to the hospital, the whole of the inhabitants of the house were at once placed in segregation camp, and the house immediately disinfected with very elaborate disinfecting arrangements—quite a toilet—and I think that constitutes practically the whole of his arrangements.

21,218. The houses were disinfected with perchloride of mercury?—Yes.

21,219. How long did the people remain in segregation camp?—10 days.

21,220. They were then allowed to go back to their houses?—Yes.

21,221. When were the last cases of plague under this system in Poona cantonment?—The last case of plague occurred on the 11th of March, but practically the last indigenous case occurred in Poona cantonment about the 28th or 29th of December.

21,222. The epidemic in Kirki was practically at an end in September, was it not?—Yes.

21,223. Did the neighbouring villages get infected?—All the villages in the immediate neighbourhood were infected.

21,224. How many?—There were practically only two near Kirki, and they were both infected.

21,225. What were the names of the villages?—They are two small villages near the cantonments; their names are not given on the map I have.

21,226. There was some plague among the Royal Artillery followers in Kirki, was there not?—Yes, there was very severe plague indeed amongst them.

21,227. How many followers are there, and how many died?—There are about 1,700 followers, but the number that died is uncertain. The families are continually varying in numbers.

21,228. In August there was a total of 181 cases and 80 deaths among the followers?—Yes.

21,229. Did you perform any inoculations among the followers?—Inoculations were performed.

21,230. When?—That I cannot tell you. I have seen the records, and I had hoped to lay the particulars before the Commission, but I regret to say that the records are valueless. The only Medical Officer there was was overworked at the time. He inoculated the people it is true, and he endeavoured to follow the course of the life-history of those people afterwards, but the results he arrived at are so widely different from those arrived at by other people that I decline to produce them.

21,231. How many did he inoculate? Does he know that?—No.

21,232. Does he know how many deaths occurred amongst those who were known to be inoculated?—I have not got the records with me, and I can only say that it was impossible to arrive at any exact conclusion. There were numbers of pieces of paper with the same names repeated.

21,233. So that you think the whole of the figures are valueless?—Yes. I think the figures are valueless. The conclusions are different from those arrived at by Prof. Haffkine. They are valueless in my opinion.

21,234. Could you give us the figures as they are?—I can ask for the records, and bring them if you wish. They are not in my possession at present.

21,235. In whose possession are they?—They are in the possession of the Deputy Assistant Adjutant-General of the Poona District.

21,236. Can you give us any information about the Bombay Sappers and Miners in Kirki?—The Sappers and Miners were not infected until much later than the followers of the Royal Artillery. Their lines are situated on the other side of the river. They had dropping cases amongst them.

21,237. What is the number of the Bombay Sappers and Miners?—I could not actually say; I should think about 700.

21,238. They had three men, two women, and six children attacked, had they not?—Yes.

21,239. Eleven cases and nine deaths?—Yes.

21,240. Were any inoculations performed among the Bombay Sappers and Miners?—No.

21,241. When were the Bombay Pioneers attacked?—The 28th Bombay Pioneers were also attacked in October, and the last case occurred on the 26th of December. Seven men, nine women, and seven children suffered. There were 20 deaths.

21,242. Were the Royal Artillery followers or the native troops sent out of their lines, or did they remain in their lines?—The Sappers and Miners and the 28th Bombay Pioneers were camped out. They had a few cases among them. The result was eminently beneficial. Three cases occurred among the 28th Bombay Pioneers after they were camped out.

21,243. How many cases had they had before?—Seven men, nine women, and seven children; and deduct three patients from the total.

21,244. How many cases were there among the Bombay Sappers and Miners?—Only two cases occurred after they went out. One of those was a man who was engaged in whitewashing in the infected lines.

21,245. How soon after evacuation did these three cases of plague occur?—As regards the depôt of the 28th Pioneers—the camp was formed on the 28th October 1897. Cases after 1st November 1897 occurred as follows: one soldier on the 12th November; one Banniah on the 23rd November, and one soldier on the 25th December. As regards the Bombay Sappers and Miners—the camp was formed on the 7th January 1898. One case occurred on the 7th January and one on the 22nd January.

21,246. You had a Plague Hospital in Kirki, had you not?—Yes.

21,247. We will now return to the Poona Cantonment. Poona Cantonment was re-infected on the 9th of August, was it not?—Yes, it was said to be re-infected then.

21,248. It was supposed to have been re-infected from Kirki, was it not?—Supposed to have been.

21,249. Do you think it was actually re-infected, or do you think plague has been going on?—I have no doubt it was re-infected. I fancy that there were dropping cases of plague in the supposed interval between May and August.

21,250. In the interval between May and August was there anything like corpse inspection or search parties?—I was not here at the time, and I am not quite certain whether there was corpse inspection or not. My personal experience only began late in October.

21,251. Can you tell us anything about plague among the troops, Native or otherwise, in the cantonment?—The disease first attacked the troops in cantonment on 28th September; on that date two cases occurred in the 19th Bombay Infantry; on 29th another case occurred in the same regiment, and six in the 2nd Grenadiers; prior to this, however, there had been some cases in the bazar of the 2nd Grenadiers, quite a village, being very much overgrown and overcrowded. On this date, 29th September, both regiments were moved into camp on the Poona Racecourse. As cases continued to occur after these regiments moved into camp, the camp of the 19th Bombay Infantry was moved on to the top of Gibbet Hill on 15th October, and that of 2nd Grenadiers to the foot of Gibbet Hill on 23rd October; three cases having occurred in the lines of the depôt of 14th Bombay Infantry, on 27th October they likewise were moved into camp on Gibbet Hill.

21,252. How many cases occurred after these regiments were removed into camp, and what were the dates of the recrudescence of each case?—The particulars are as follows:—

2nd Bombay Grenadiers. Encamped on Racecourse, 29th September, 1897.

1st October	- -	One case in a child.
3rd ,,	- -	,, ,, woman.
3rd ,,	- -	,, ,, child.
10th ,,	- - -	,, ,, boy.
13th ,,	- -	,, ,, follower.
18th ,,	- - -	,, ,, ,,
21st ,,	- -	Four cases in soldiers.
22nd ,,	- - -	One case in a woman.

Camp moved 23rd October.

14th November	- -	One case in a soldier.
15th ,,	- -	,, ,, woman.

Depôt 14th Bombay Native Infantry. Encamped on the 28th October, 1897.

5th November	- -	One case in a child.
5th ,,	- - -	,, ,, follower.
6th ,,	- -	,, ,, child.
10th ,,	- - -	,, ,, ,,
18th ,,	- -	,, ,, woman.

Camp moved further from cantonments.

19th Bombay Native Infantry. Encamped on Racecourse, 29th September, 1897.

30th September	- - - 2 soldiers.
1st October	- - - 3 ,,
2nd ,,	- - - - 1 woman.
3rd ,,	- - - - 1 soldier.
3rd ,,	- - - - 1 woman.
7th ,,	- - - - 1 child.
11th ,,	- - - - 1 soldier.
12th ,,	- - - - 1 ,,
12th ,,	- - - 1 woman.
13th ,,	- - - - 1 soldier.
13th ,,	- - - 2 women.
13th ,,	- - - 2 children.
14th ,,	- - - - 1 soldier.
14th ,,	- - - - 1 woman.

Camp moved 15th October, 1897.

15th October	- - - 1 follower.
17th ,,	- - - 1 soldier.
21st ,,	- - - - 1 ,,
22nd ,,	- - - 1 ,,
24th ,,	- - - - 1 ,,
28th ,,	- - - 1 ,,
10th November	- - - 1 ,,

These regiments were sent into camps without any disinfection of clothing or kit, as they were moved into tents.

21,253. Had there been any disinfection among any of these regiments?—No; they were marched out without disinfection of any kind. The following additional precautions were taken after moving these camps: each corps had a Banniah with it in camp who was required to purchase weekly the grain required for the men, and not to issue it until it had been exposed to the sun and air for 24 hours; the guards over the lines were on duty for a week, and during that time were not allowed to enter the Quarter Guard or any of the buildings, but were accommodated under canvas, and on being relieved were segregated for another week before being allowed to rejoin their regiments; as cases still continued to occur it was thought that this was due to the families visiting the bazar, and therefore the next precaution was to move the families away from the men and further from the bazar, and also to place guards over them, to prevent them leaving their camp. No further cases occurred amongst these Native Infantry regiments after this last precaution.

21,254. I believe plague broke out in the Transport lines among the followers?—Yes. Plague broke out in the Transport lines among the followers and their families, about 400 strong, on 5th October, and up to 4th November 24 cases had occurred; on that date the whole of the followers and their families, numbering altogether about 400, were moved into camp near the rifle range, but as 14 more cases occurred, arrangements were made on 13th November to move the camp a few yards to the front across the main road running parallel thereto. Each tent was sprayed with perchloride of mercury solution, then carried across the road and put up; the followers and their families including even their cats and dogs, their cocks and hens were carefully disinfected before being allowed to cross the road and re-occupy their tents; the experiment was eminently successful, for after that date not a single case of plague occurred among those who had crossed the road, proving convincingly the efficacy of disinfection.

21,255. How did you disinfect the cocks and hens?—I think I may say that that is a joke of the late General Duncan. I believe it is a fact that they were dipped into the perchloride of mercury solution before crossing the road, but I am not sure that I directed that extreme measure.

21,256. Did you disinfect the clothing?—Every single article that these people possessed was dipped.

21,257. The result was that you had no plague?—The result was that there was not a single case of plague across the road, except two children I found ill in a family on the right hand side of the road. We left them there, and they developed into plague in a day or two. There was also one case of a man who was left on watch in the Transport lines. He was the only man left in the lines. A few days after this health camp had been carried out, this man died of plague in the lines. I think that shows that infection was still rife in that ground, and did not travel across the road with the people who had been disinfected.

21,258. Plague went on in the cantonment?—Yes.

21,259. What measures did you take in the cantonment?—On the 17th of November the Plague Committee, as established by the Government of Bombay, commenced actual work. On the 27th of November, a Plague Committee was formed by a Government order, but we commenced work on the 17th November. At that time there was a great deal of confusion. People on plague duty were overworked. According to our views, and according to General Duncan's opinion, all was not being done that could be done, chiefly owing to the men on plague duty being overworked. On the 17th of November, arrangements were made for a more efficient manner of dealing with it, or, at all events, a more complete method of dealing with it. Major Ross was placed in chief executive charge. Plague at that time was so diffused throughout the cantonment that it was exceeding difficult to grapple with it, and, after careful consideration, it was decided to completely surround the enemy, and attack him simultaneously from all quarters. With this in view, the cantonment was divided into three divisions, each under a British officer, and each division was divided into a number of sub-divisions, in all 17, each under a supervisor, who was either a Native gentleman or a pensioned Native officer. Each supervisor had to prepare a list of all the inhabitants in his sub-division, with the object of keeping a check on arrivals and departures, and by this means to facilitate the segregation of the persons arriving from infected areas. Each subdivision had a search party, varying in numbers according to the density of the population in the particular area they had to search. Each division had a depôt or head-quarters, where the divisional establishments were located during work hours, and the depôts were as follows:—

No. I. Division—Nusserwanjee Petit Hall, Bhawani Pet Road.

No. II. Division—Charitable Dispensary, Connaught Market.

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No. III. Division—Camp, Brigade Parade Ground.

The divisional establishment was common to all the sub-divisions of each division, and consisted of:—

Medical official.
Ambulance party.
Disinfecting party.
Whitewashing party.
Segregation party.
Four ambulance carts.
Two disinfecting carts.
Two limewashing carts.
Two coolies carrying basins and bottles of disinfecting lotion, for the use of the search parties to disinfect their hands and clothing at once after handling a case.

No. I. and II. Divisions each had two lady searchers for visiting Muhammadans' houses where there were pardah ladies, and each lady was accompanied by a small escort of two soldiers. With this sub-divisional and divisional establishment nearly every house in Cantonments was searched daily. As cases were found by the different search parties, intimation was sent to the depôt. The Medical Officer went to diagnose the case; if pronounced plague, ambulance, disinfecting and segregation parties followed. The patient was removed to hospital, with one member of the family as an attendant, the remainder of the family or household were taken to the depôt for segregation. The effects were removed from the house, either left with the neighbours, sent to a store kept for that purpose, or taken with the family at their own discretion, and the house was disinfected and closed. Whitewashing was carried out the following day. All persons collected for segregation during work hours were taken in batches at the completion of the searching of each Division to the segregation camp. Where dead bodies were found two soldiers were left in charge of the funeral rites. As far as possible, members of the household only were allowed to handle the body, the remainder being kept at a distance. The soldiers accompanied the party, and as soon as the body was disposed of, the members of the household were taken straight to the segregation camp by one soldier, and the remainder of the party who assisted at the funeral rites were taken to the nearest disinfecting depôt by the other soldier, they and their clothing were disinfected and they were released.

21,260. Did you find much difficulty with regard to corpse inspection?—None whatever.

21,261. Corpse inspection was done before the body was moved?—Yes.

21,262. Did you find much difficulty among Muhammadan women?—None whatever. I have never heard of a complaint of any kind. Had there been any complaints I should have been sure to hear of them; but, as a matter of fact, I never heard of a single complaint. There are not very many Muhammadans living in the cantonment, but there is one lane which is certainly full of them. In any case there was a lady doctor available to visit Muhammadan women, if necessary.

21,263. What have you to say with regard to hospitals?—From motives of economy, the General Plague Hospital, and all private hospitals, consisting of four, Parsee, Muhammadan, Hindu, and Marwari, were all common to Poona City, Suburban Municipality, and cantonment. With the exception of the Parsee Hospital, they were outside Cantonment limits, and were entirely managed by the City Plague Committee, the cantonment bearing a share of the General Plague Hospital expenses.

21,264. I believe you issue death certificates?—Yes. There was reason to believe that the existing practice of accepting death certificates from other than Commissioned Medical Officers led to the concealment of plague in some instances, consequently orders were issued by the Plague Committee that death certificates from all causes other than plague would only be accepted from a Commissioned Medical Officer, and arrangements were made for a Medical Officer to be available at fixed hours, morning and evening, to view all dead bodies and to grant the certificate free of charge.

21,265. Did you evacuate the houses in which there was a plague case?—Every house in which a plague case occurred was evacuated completely.

21,266. For how long?—For 10 days.

21,267. Were the people placed in health camps?—When a case occurred people were placed in the segregation camp; the whole inhabitants of the house.

21,268. Were they allowed to go back to their work?—No, they were confined to the camp.

21,269. Do you think you got all the people to go into the segregation camp?—I should not like to answer positively with regard to that. I know we took an immense amount of trouble to get them all. I think we got very nearly all the people in Poona cantonment; but then you must remember that we had an enormous staff, both of Natives and Europeans, and European soldiers and Native soldiers.

21,270. What percentage of people do you think escaped?—I could not give the numbers.

21,271. In your opinion, would it be 5 per cent., or 20 per cent., or what?—Nearer 5 than 20 per cent.

21,272. I believe the people were made to take a bath in a solution of carbolic acid, 1 in 40, on entering camp?—Yes.

21,273. Do you think the people found that very painful?—That was modified, and phenyle was used afterwards on some occasions. I do not think that a bath in a solution of 1 in 40, made up with soft soap, is painful. I have often had my hands in a solution of 1 in 20 for a considerable number of hours a day.

21,274. I should think that to the scrotum, for instance, it might be?—There was never any objection made. Hundreds passed through the tubs.

21,275. Did the people go voluntarily into the health camps?—Not quite voluntarily. Major Ross and myself decided, on one occasion, to evacuate a certain block, it being riddled with plague cases. We went down in the morning and said to the people, "Now you have to go out." We got carts for them, and any number of soldiers to help them. They went out very quietly and happily, especially after they had seen some blocks evacuated. Towards the end of the epidemic they had their things ready, and were out in two or three hours.

21,276. You never evacuated a whole town?—Not at the same time.

21,277. Could you tell us how many deaths you had in the segregation camp?—No.

21,278. Could you tell us the number of cases?—No; I do not think they have been recorded. I have tried to obtain the information.

21,279. Can you give us the number of deaths in the health camps?—No. I can absolutely state, however, the number of cases of plague.

21,280. There was only one?—One absolutely; nominally there were one or two others.

21,281. Did you have a roll-call in the health camps?—Yes, at nine o'clock at night.

21,282. Did you find that all the people turned up, or did a certain number of them escape?—A certain number of them escaped.

21,283. How many?—A small proportion. The people were punished; it was not worth while to be absent.

21,284. How do you know that people actually answered to their names; might not someone else have answered for them?—We had a Native officer and two corporals—sepoys,—and three or four men. The Jamadar called the roll. Each corporal had his division and each man had his sub-division. They soon knew the number of people in each hut.

21,285. I believe when the Wanauri bazar people were sent to the health camp, a small row of huts was overlooked?—Yes. When the bazar people were sent to the health camp a small row of huts was overlooked, and the inmates left undisturbed. Amongst these people five cases occurred and three terminated fatally within 24 hours. The remainder were then segregated without delay. This object lesson proved of great value, and was not forgotten by the people.

21,286. Were you able to turn out the people in Wanauri village?—We had not room in the health camp, and about 1,500 Wanauri villagers practically remained unattended to until January.

21,287. And plague went on?—Yes; plague went on and on in that village until the people themselves fled to their fields. They brought plague with them into the fields, and cases continued. Our search parties used to visit the huts in the fields, and all the cases which are shown as cantonment cases in January are

practically from the fields of Wanauri and Wanauri village.

21,288. I suppose there was no disinfection?—No. The people left their houses, flying into the fields. It was with considerable difficulty that we could find the cases. They were scattered over miles.

21,289. How did you disinfect the houses in the cantonment?—Every house in which a case occurred had the roof or a portion of it removed. First of all the interior of the house, the floors, the walls, and dusty corners were lightly sprayed with a solution of perchloride of mercury. Every moveable thing inside the house was removed. All the people's clothing which had been left behind (the people having gone to the segregation camp) was disinfected with perchloride of mercury solution. All the collections of rags and rubbish, and all odds and ends which were valueless (and which collect to an enormous extent in some of these native houses) were brought into the street and burnt. During the earlier part of the epidemic we took up the floors; but we very soon abandoned that as unnecessary. Then the whole of the interior of the house was sopped with perchloride of mercury solution, until the floor was a bog. This was all done under the supervision of a British soldier. Practically every house in the bazar was done under the supervision of a British officer. Every house was seen wet by a British officer. As soon as it had dried it was whitewashed. The latrines were most carefully attended to; in fact, not a detail that one could think of was left undone.

21,290. Did you demolish any house?—Yes, we demolished a considerable number of houses.

21,291. How many?—The details are given in Appendix "B" of my "Report on Plague in Poona" and Kirkee Cantonments and Suburban Limits, from "17th March 1897 to 19th March 1898."* They may be summarised as follows:—70 houses were demolished or pulled down to be rebuilt, and one house was improved by the provision of windows for ventilation. This cost Rs. 5537 in payments of compensation money.

21,292. Have you anything to say with regard to the manner in which insanitary houses are formed?—Yes. It may be of interest to describe the process by which these houses get into this insanitary state, a procedure that can be seen in full operation in any regimental or Sadr Bazar, but which has now received a severe check in this cantonment. The operations are as follows: A row of houses consisting of single or double rooms is sanctioned with a broad street between the rows. The inhabitants first proceed to make a small enclosure in front of their houses, marked by a row of stones; the stones are succeeded by a low mud wall; then a few sheets of tin forming a verandah are put up partly resting on the mud wall and partly on a few sticks; the wall rises in height and the sheets of tin increase in number from year to year; then a few tiles and rafters are put up and before many years go by there is a room or, if the owner is more enterprising than his neighbours, even rooms both front and back; though it is true the door-way is not more than 4 feet high and the inner rooms are quite dark, but these are not considered drawbacks by the inmates.

21,293. I believe you had some plague among European troops?—Yes. Two cases occurred in soldiers of the Royal Irish Rifles employed on plague duty in the city, one on the 20th and one on the 29th of January, both patients were in hospital at the time plague was diagnosed, suffering from venereal disease, admittedly contracted while on their rounds in the city. On the 4th November 1897 a woman, the wife of a sergeant living in the Wanauri Barracks, was attacked. She had recently been shopping in the Sadr Bazar. All three cases ran a very similar course ending in recovery after a prolonged convalescence. In the cases of the two men the bacillus was searched for and it was recognised in one. All three patients had buboes in the groin, which suppurated. One of these men had been inoculated in April; in fact, he had been inoculated twice with Haffkine's serum. He got plague in January. The other had not been inoculated. About 50 men had been inoculated and 1,900 men had not been inoculated. One of the 50 inoculated men got plague, and one of the 1,900 inoculated got plague.

21,294. (*Prof. Wright.*) How long after inoculation?—The inoculation was performed in April, and he got plague in January—10 months.

21,295. (*Dr. Ruffer.*) I believe a certain number of squirrels died of plague, did they not?—Yes. In December 1897, when plague was very bad, I noticed squirrels crawling along the road, and not able to go up the trees. Then I heard some people saying that they saw three squirrels drop off the trees dead. After that I could at any time find the corpse of a squirrel about here if I looked for one.

21,296. Have you any evidence to show that these squirrels died of plague?—No.

21,297. I believe a grey squirrel in a cage died in the house of a European woman?—The wife of a sergeant who was attacked in November had a pet grey squirrel in a cage in her room, and it died suddenly the second day of her illness, when she was going to the hospital.

21,298. You have no evidence to show that that squirrel died of plague?—None whatever. I was too busy at that time to make any experiments.

21,299. What measures did you take to prevent re-infection?—By the middle of December the epidemic in cantonments was considered checked, and the following measures were adopted to prevent re-infection:—

1. A corpse inspection by a Medical Officer in every case of death.
2. Every case of sickness in the native population to be at once reported by the head of the house to the Supervisor of the district, by whom it was at once brought to the notice of the Medical Officer on plague duty, who, with the least possible delay, saw the case and determined the nature of the complaint. Both these measures worked wonderfully well and smoothly, the people knew and trusted their medical man.
3. Every arrival from outside the station had to spend 10 days in the health camp before being permitted to take up his residence in the bazar; but this rule was modified to suit many emergencies.
4. The normal population of the Sadr Bazar is probably 25,000, but by the end of November there were not more than 12,000 people in it at the utmost. All unoccupied houses were securely closed and sealed under the orders of the Cantonment Magistrate. No house was re-occupied until it had been well cleaned in the fullest sense of the word, disinfected and limewashed.
5. Surprise night visits were made by the troops on plague duty under their officers, and it was remarkable how few breaches of the regulations numbered 2, 3, and 4 were brought to light, and when a few delinquents had been punished the necessity for the night visits ceased. The people had learnt to trust in the plague measures, and to a great extent co-operated in them.
6. Under the Poona City Plague Committee a strict surveillance of all arrivals by rail was arranged for, those coming to cantonments were sent to the health camp, and if they came from an infected locality obliged to sleep there for 10 nights. Many modifications and relaxations of this rule were from time to time introduced but detailed description does not come within the scope of the present statement.
7. On the 16th January a sanitary cordon of cavalry was established around the city, cantonments, and suburban limits to deflect the traffic on to the main roads, where observation posts were established, supplied with disinfectants, and small camps were erected in which the people wishing to enter were detained till they were seen by a Medical Officer.

It may be noted that, as far as can be gathered, but six cases of sickness were sent during the whole time from the posts to the Plague Hospital.

21,300. Will you give us the orders for the cordon?—They were as follows:—

1. From 6.30 a.m. till 6.30 p.m. no carts, pack animals, or coolies carrying other than articles of merchandise will be allowed to pass through the cordon *except* by one of the authorised roads where there is a post.
2. Agricultural labourers, coolies carrying articles of produce for sale, persons in charge of cattle, and all persons without other effects than what they are actually wearing will be allowed to pass at all points.
3. The object of the cordon is to insure that all persons and families who have been living outside Poona will, with their effects, be properly

* Not reprinted with the Proceedings of the Commission.

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disinfected before being allowed to return to reside in their houses in Poona. The question of their disinfection and retention at the several posts does not concern the cordon, it will lie with those in charge of the posts, and the men employed on cordon duty are simply to insure that all such persons and families pass by the posts and not by other roads.

4. From 6.30 p.m. till 6.30 a.m. all carts, pack animals, or coolies carrying articles such as household goods and wearing apparel will be stopped outside the cordon and not allowed to pass. But all empty carts, unladen animals, night-soil carts, rubbish carts, and carts or animals carrying grain, produce, and articles of merchandise only, will be allowed to pass freely.

21,301. What were the orders for posts?—They were as follows:—

1. A cordon has been placed round Poona for the purpose of stopping the importation of plague by persons returning to reside there from infected areas.
2. Posts have been established at all the main approaches to Poona, and the duties of the cordon are simply to make all such persons pass by the Posts and not by other roads, and to stop their passing at all during the night when the work at the Posts must of necessity cease.
3. The N.C.O. in charge of each Post will arrange for the inspection of all traffic that passes and will work as follows:—

(I.) All carts, animals, or persons carrying only articles of merchandise will be allowed to pass without detention.

(II.) All empty carts, night-soil and rubbish carts, unladen animals, and persons carrying nothing but their wearing apparel, will be allowed to pass without detention.

(III.) All carts, animals, and persons conveying articles of household use, bedding, clothing, &c., will be stopped either passing in or out.

(*a.*) All those passing out will have their persons and effects disinfected, and they will be allowed to proceed.

(*b.*) Those passing in will be disinfected and detained till an Officer on plague duty decides what is to become of them.

(IV.) Europeans in Government employ will, with their servants and belongings, be allowed to pass at all times.

(V.) All persons showing signs of sickness will be sent or detained for medical inspection.

(VI.) Persons arriving at a Cantonment Post, who propose to reside in the city, will not be disinfected in cantonments, but will be passed under escort to the City Segregation Camp, Parvatti Road, with a note of their numbers. Persons arriving at a City Post, who propose to reside in cantonments or Suburban Municipality, will be similarly passed under escort to Cantonment Health Camp, Shunkerset Road.

21,302. What are the Sanitary Orders for Troops on Plague Duty?—They were as follows:—

1. They are to live in camp.
2. Sober men only are to be chosen.
3. The working parties are to be inspected by a Medical Officer daily before they move off; those with any sign of fever or debility to be struck off duty.
4. Abrasions on the hands or face are to be specially looked for; the men may grow beards.
5. A good breakfast is to be supplied before work begins.
6. Arrangements should be made to supply a midday meal in cases where the men cannot return to camp in time for their dinner.
7. Putties should be worn to prevent the dust from infected houses coming in contact with the legs.
8. It should be impressed on the men that a fruitful mode of infection is through abrasions on the skin, and all such should be reported at once to the Medical Officer.
9. A supply of carbolic lotion, 1 in 40, is to be sent with each party, and in the interval of work the hands should be swilled with it.
10. The parties should be warned not to commence thorough cleaning until some preliminary disinfection of an infected house has been carried out.
11. Disinfectants should be freely used in camps.
12. Immediately on return to camp from plague duty the whole of each man's clothes will be changed, and the suit taken off and hung in the sun, then each man will take a bath with phenyle in it; carbolic soap will be liberally supplied.

21,303. I believe there was also some plague in the suburban part of the town?—Yes. That was in a tongue of land between the city and Poona cantonment and Kirki. It also contains the Poona railway station. For a long time every case found in the railway station was shown as an imported case of plague in the Suburban Municipality.

21,304. Did you take the same measures?—Practically the same measures.

21,305. In your report* on plague in Poona and Kirki cantonments and suburban limits, you give some medical notes on the late epidemic, and you say, "The "perchloride of mercury used must be the English "salt. A native-made salt, supposed to be perchloride, "has, I know, been used, but it is worse than useless, "as it consists of more than 50 per cent. of calomel, "as well as some sulphate of mercury, and clay." Have you had this native salt analysed?—Treacher and Co. told me that they had analysed it, and had found that it consisted chiefly of calomel, and something which they believed to be sulphate of mercury and clay.

21,306. In your experience has English salt been used, as a rule, or native salt?—When I came here native salt was the only thing used.

21,307. Native salt is very much cheaper, is it not?—Yes. It was impossible to make the solution with it. It was issued in packets, and each packet is supposed to be sufficient to form a bucketful of solution of 1 in 1,000. I examined some of the packets. The weight, I think, should have been 184 grains; but the weight of these packets varied from rather less than $\frac{1}{2}x$ to $2x$. It was impossible to make more than from 30 to 50 per cent. of any single packet dissolve in water.

21,308. Can you tell us how much clay the perchloride of mercury contains?—I do not know. There were impurities.

21,309. There must have been a considerable quantity of impurity if only 50 per cent. dissolved?—You could not make 50 per cent. of native salt dissolve with any amount of sodium chloride.

21,310. So that your 1 in 1,000 only contained ·2 per cent.?—It is something very small. Even by the addition of acid you could not make more than 50 per cent. of the native salt dissolve.

21,311. Do you think that the native habit of sleeping on the floor has anything to do with their getting plague?—I think so. It is, however, merely a matter of opinion, and not of fact.

21,312. (*Mr. Hewett.*) The habit of sleeping on the floor is more prevalent in this part of India, than in Northern India, is it not?—Yes. Charpoys are very generally used in the north of India, and I think that may have an influence in causing a plague epidemic to be more easily controlled in Northern India than in Southern India.

21,313. In your Report* you say that the total number of cases in Kirki was 690. What was the population among which these cases occurred?—3,700.

21,314. In Poona cantonment there were 422 cases among a population of 12,000?—At that time the population had probably sunk to 12,000.

21,315. The number of deaths at Kirki was 561?—Yes.

21,316. And in Poona cantonment 298?—Yes.

21,317. Was there anything particular in the character of the population of these places which made your task easier than it might have been in other parts?—I think so. The people were well acquainted with the habits of Europeans. They were people whose livelihood depended upon the Europeans living in Poona. They were not afraid of Europeans; they had been accustomed to them all their lives. This, undoubtedly, made our task much easier.

* *See above.* Not reprinted with the Proceedings of the Commission.

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21,318. Do you consider that even passive opposition on the part of the people would have made your task much harder?—No one could hope to control an epidemic unless the population were on his side.

21,319. (*Prof. Wright.*) Did you find any objection to disinfection, did the people try and conceal their best clothes or bribe the disinfectors?—I think it is hardly possible. I have known a bribe offered to British soldiers, but it was quite useless. The people had much better submit; and they did submit. I do not think there was any concealment. At the first the lady of the house would do her utmost to conceal her best belongings, and carry them away through the back door, as soon as she knew a search party was coming. Very little could be done with success, however, in that direction. We had such an enormous body of people helping us that the back door was as carefully guarded as the front door. There was no escape.

21,320. (*The President.*) Do you endorse the opinion of Major Ross, which you have quoted in your Report* ?—Yes. Major Ross writes:—"The effect of health-"camping was almost magical: of 4,000 people taken "from the worst infected areas, only one undoubted "plague case occurred after they had returned to "their houses: in that instance the woman who was "attacked was only six days in camp, and was taken "ill the evening of the day she left." I believe that firmly; in fact, I can go further and say that the total numbers were 6,100, and that was the only undoubted plague case that occurred after the return of those 6,100 people from the health camps.

21,321. You endorse it from your own observation?—Entirely.

21,322. What do you mean when you state, in your Report,* "On the other hand, no disease is so easily "combatted by the resources of the modern sanitary "science"?—In my experience, the measures which were successful in removing plague from Poona cantonment were those which, if applied to an epidemic of scarlet fever or diphtheria at home, would not produce the same results. Plague, in my idea, is a purely local, narrow disease, and is easily dealt with. Its means of spreading are so local and few that it is perfectly possible, by following out the ordinary sanitary rules, which would be applied in every epidemic, to obtain a much more successful result than when dealing with an epidemic of any other infectious disease. Nothing but these facts would justify the amount of money we have spent on plague; a similar expense in an epidemic like scarlet fever in England, or here, would be waste of money, because the results would not be commensurate. I hold that the results have justified the expenditure. I may also say that I have heard it said that the measures were taken on a falling epidemic, and were therefore successful. But I must say that these measures in our instance, neither in Poona cantonment, nor in Poona city, were adopted on a falling epidemic. The epidemic increased, or certainly held its own for the first weeks afterwards. It rose for two or three weeks before we commenced our measures. We commenced our measures in Poona cantonment on the 17th of November, but it was not until the beginning of December that efficient measures were adopted in Poona city; yet the epidemic continued as virulent in Poona city as it had been, while it was declining in Poona cantonment.

21,323. Do you still adhere to the opinion you have expressed in your précis that "It is unfortunately an "axiom that plague in its epidemic form is only to be "found where dirt, darkness, and overcrowding pre-"vail; in a word, in those places where sanitation is "unknown, impossible, or neglected. Most fortu-"nately, it cannot be carried to any appreciable "extent by water or air, and the bacillus which causes "it, though capable of multiplication with extra-"ordinary rapidity in a suitable nidus, has neither "power of resistance, locomotion, or spore formation, "and dies at once in the light of day"? Does that entirely represent your present views?—Yes; I hold by every word of that.

21,324. On the last page of your précis you express certain views with regard to inoculation. I suppose those views are based partly on your own observation?—Yes. Observation and reflection, I think, enable me to draw the conclusion that it would need to be universal in order to be successful.

21,325. Will you read what you say?—That is what I say:—"I feel that I have not sufficient experience "to add anything to the knowledge of inoculation and "its uses; but I may be permitted to say that it "appears quite clear that if inoculation is generally "adopted, in supersession of the ordinary sanitary "regulations, as a measure for the suppression of "plague, it should be universal, which is obviously "impossible; because, if any minority in an infected "city remains uninoculated their doom is sealed, they "must be practically exterminated. A little reflection "will prove this, while the inoculated are, in a great "measure, protected for a limited time, and are "thereby given a feeling of security, their houses, "clothing, bedding, etc., continue to hold and carry "the seeds of the disease, and will undoubtedly dis-"seminate it far and wide, to the utter destruction of "the uninoculated minority."

21,326. From as full a consideration of the subject as you have been able to give, you still endorse those opinions?—Those are the conclusions I have arrived at. It is true that they are not absolutely mathematically correct, but they are conclusions one arrives at from experience and reflection upon the subject.

21,327. (*Dr. Ruffer.*) You say in your précis, "The "sanitary cordon as adopted round the city and can-"tonment in January 1898 appeared necessary at the "time, but it can now be shown that it was not really "required." Kindly tell us upon what you base that statement?—I am afraid it would be very difficult to show that absolutely. I am prepared now to think that it was not really required at the time.

21,328. Why?—Because, after we had put it on, we came to examine carefully into the working of the cordon, and we found, especially at night, that it was perfectly easy to get through. Internal quarantine, as I said at the time it was put on, can never be effective. Anyone who chose to get through it, especially at night, could do so with greatest facility. One could take a horse, or a carriage and four, through it if one wished. There is not the least difficulty.

* *See above*; not reprinted with the Proceedings of the Commission.

(Witness withdrew.)

Mr. H. C. Venis.

Mr. H. C. Venis called and examined.

21,329. (*The President.*) You have had experience of plague duty in Poona?—Yes.

21,330. Have you an official position here?—I was sent out by the Secretary of State for India.

21,331. Will you mention your medical qualifications?—L.R.C.P., L.R.C.S. Edin.

21,332. (*Prof. Wright.*) What duties do you perform at present in Poona?—My duties include a variety of things—my chief duties are corpse inspection and examination of the sick.

21,333. Is every case of sickness notified to you?—Yes. Cases of illness in the city are reported by the friends of the patient, or by the volunteers either to the Medical Officer on duty at the Central Plague Office, or to the Cheif Volunteer at the Ward Office of the division in which the patient lives. The Chief Volunteer then forwards the report of the sick to the Medical Officer.

21,334. Do you look after one ward or one division of the town?—No, we (*i.e.*, my colleague and I) are the Medical Officers of the whole city.

21,335. Do these notifications of sickness come in?—Yes, either by the friends of the patients directly to the Medical Officer at the Central Office, or through the agency of the volunteers.

21,336. Do the Medical Officers attend all through the 24 hours?—They attend for 12 hours from seven in the morning till seven in the evening.

21,337. No Medical Officer attends during the night?—No.

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21,338. Notifications of the sick all come in during the day?—Yes.

21,339. When they come in do you go immediately to see the sick person?—Yes, but if there are some corpses to be examined, the sick are taken after the examination of the corpses, or on the way there; it all depends upon circumstances.

21,340. Is there opposition to your examining the sick?—During my 15 months' experience I have had only about two or three cases in which the friends of the sick person objected to medical examination.

21,341. Is every case of sickness reported to you, such as a case of diarrhœa?—Yes, all cases of sickness.

21,342. Even mild attacks of fever?—Yes.

21,343. And headaches? I want to know whether the cases of sickness which are reported are only grave cases?—No, not only the grave cases. I had a case of deafness reported to me yesterday morning.

21,344. There are comparatively slight cases among these returns?—Yes.

21,345. Then you go round and see them?—Yes.

21,346. What examinations do you make of them? Do you content yourself with determining whether they have plague, or do you examine them to see what is the matter?—Up to last February, when plague was rife, we did not diagnose beyond seeing whether a case was plague or not, but after that time, when the epidemic died away, we arrived at an approximate diagnosis in each case.

21,347. And that is your practice at present?—Yes.

21,348. Did you find many of these patients had been sick for a number of days before you were called in?—Some of them.

21,349. What are the regulations—that they shall report one day's illness to you?—No. Parties of volunteers go round in each division of the district. They notice the sick people in their homes and report.

21,350. Do the volunteers go into the houses?—Yes, native gentlemen, who have volunteered to report cases of illness.

21,351. What is their work? Do they go into houses and force an entrance, or are they allowed to go in voluntarily?—They are allowed to go in voluntarily. They ask at the doors if anyone is sick.

21,352. Does most of the information come to you by means of the volunteers, or is it notified by the relatives?—About half and half. The people voluntarily report cases of sickness now.

21,353. When these deaths are notified to you, do you find that a large percentage of them had been notified to you previously as sick cases?—Yes.

21,354. What is the percentage?—From the 25th January to the 31st December 1898 I examined 1,809 sick people, and of those 601 died.

21,355. Every corpse had been previously seen by you during life?—Yes, *i.e.*, every corpse included in the number (601) mentioned above, which is the number of those people who, previous to death, had been examined and certified by the Medical Officers. This is the form of certificate:—

No.

MEDICAL CERTIFICATE.

Name _____

Age _____

House No. _____ Peth.

Certified that I have examined this person and that _____ is not suffering from Plague but from _____

Special Plague Medical Officer,
Hd. Qrs. Segregation Office,
315, Shukrawar Peth.

Date ____ 189

No.

MEDICAL CERTIFICATE.

Name _____

Age _____

House No. _____ Peth.

Certified that I have examined this person and that _____ is not suffering from Plague but from _____

Special Plague Medical Officer,
Hd. Qrs. Segregation Office,
315, Shukrawar Peth.

Date _____ 189

The left-hand side is kept by us, and the right-hand half is given to the patient.

21,356. When you examine corpses do you issue another form of certificate?—Yes, I should rather call it a pass, a cremation or burial pass.

21,357. What examination of the corpses do you make to determine whether the person has had plague or not? Have you got that certificate with you when you go to see the corpse?—In many cases.

21,358. You examine the certificate which the patient had during life?—Yes, because we find it (*i.e.*, the left-hand duplicate of it) in the Medical Officer's Office.

21,359. If that says that the patient died of plague or was ill of plague you take no further steps?—That is so.

21,360. You make a fresh examination?—Yes, many of those certificates have been issued six or seven months before.

21,361. The patient had been sick six or seven months?—Yes.

21,362. Then you make a fresh examination of the corpse; what do you examine for?—Specially for buboes and glandular swellings of any sort.

21,363. Are there any other signs of plague which you can detect in a corpse?—The general appearance and the previous clinical history.

21,364. What is there in the general appearance which would put you on the track?—The last few cases which we have had in Poona City have been of the septicæmic type of plague.

21,365. How do you determine those to be cases of plague?—There is a peculiar facial expression about the corpse; the features are contracted, and there is a sort of anxious expression which the deceased had before death, and the eyes are fixed and glistening. The whole countenance has a worn expression, as it were; the cheeks are sunk in, and the tongue, in the majority of cases, exhibits a thick white fur, and in a few of the septicæmic cases, a black fur. By the time the Medical Officers arrive, if many hours have not elapsed, the body is distinctly warm. There are no buboes in those cases.

21,366. Do you find a plague corpse keeps warm longer than that of a person who has not died of plague?—Yes, it is a continuation of the increased temperature.

21,367. How soon after death is the corpse examined?—In some cases within half-an-hour or an hour, but if I am out on my rounds and the death is reported, it may be two hours before the corpse is examined.

21,368. A patient who dies after seven in the evening is not examined till the next morning?—That is so.

21,369. Is there trouble with the relatives?—We have had two or three complaints.

21,370. What are the complaints, that the laying out of the patient has been delayed?—Yes.

21,371. Have you any other complaints but that?—None that have been brought to my notice.

21,372. Do they object to your examining the bodies of females?—Not of females particularly. I have had two cases of objection which I have called case A and case B in my précis of evidence.

21,373. Perhaps you will tell us what the objections were founded on?—The first case was that of an ascetic, a holy man, a Fakir, I had seen this patient myself, and he was suffering from dysentery. He had also been under treatment at the Sassoon General Hospital, from which institution he got the certificate. When I saw his corpse about five days after my first visit, I had no need to examine the corpse because I myself had seen. His friends produced a certificate which I had given him (the right hand duplicate), and they also produced a certificate from the Surgeon of the hospital. There was no swelling, and the cause of death was quite plain. In another case an objection was raised on the ground of the patient's caste. He was a Brahman, and they asked through the Chief Plague Authority if a Brahman medical man might examine the corpse for buboes in the presence of a Medical Officer. This condition was readily granted, and the native medical man examined the body in my presence. I was as close as possible, and satisfied myself there were no buboes.

21,374. Those are the only cases?—Those are the only two cases in my experience lasting 15 months.

21,375. You have examined all the female corpses just the same?—Yes. My colleague, Dr. Botenson, had an objection raised when he was examining a female corpse, but that has not occurred in my experience.

21,376. How many cases of plague have you detected by corpse inspection?—106 cases of plague out of a total of 3,035 corpses examined.

21,377. How many of those cases were detected as plague cases during the corpse inspection which had been recognised to be plague when they were previously visited?—I cannot answer that question because when a case is diagnosed as plague it is sent off to the Plague Hospital or the observation ward, and they die there.

21,378. These are all cases which were not recognised as plague in the sick visitation?—Yes.

21,379. You think the visitation of the sick by itself would not suffice, because these 106 people slipped through your hands during the sick visitation?—Yes. (Witness, in correcting proof of his evidence, added a note correcting his statement as follows:—None of these 106 cases of plague were seen during life, and they were seen for the first time as corpses.)

21,380. How many plague cases did you discover when you visited the sick?—The total number of cases of illness in Poona City examined during life and certified by the Special Plague Medical Officers from the 25th January 1898, to the 31st of December 1898 was 1,891, and of these cases 601 resulted fatally. From March up to about the end of December, by means of our sick examination system, we discovered 25 or 26 cases of plague which were sent to the Plague Hospital or to the observation ward. Before that I had no figures.

21,381. Out of every five cases of plague discovered by means of sick visitation and corpse inspection, was only one discovered by sick visitation and four by corpse inspection?—Yes.

21,382. You think that of these two measures corpse inspection is by far the most useful?—It is very valuable.

21,383. Do you think the sick visitation might be omitted?—No, I do not think so, because that is a very useful mode of procedure too in helping us to detect cases of plague in the early stages.

21,384. What advantage do you think you get from that:—the lessening of the spread of inspection?—Yes, certainly, because as soon as a case is seen and diagnosed to be plague, arrangements are made for the removal or treatment of the patient, and thereby the further spread of the disease is prevented.

21,385. In your whole year's work you only found out 25 cases by your sick visitation?—Yes; they were seen and removed within a very short time.

21,386. Were these cases which you took out moribund or in the early stages of plague?—In the early stages, many of them.

21,387. If they had been moribund, I presume you would have got them a few hours afterwards as corpses?—Yes.

21,388. Do you know how long they had been sick, or were the facts you got not sufficiently trustworthy?—We have notes of all these cases which will be produced by the Chief Plague Authority.

21,389. Have you any other duties in connexion with plague besides sick visitation and corpse inspection?—Yes; I had medical charge of three camps—the railway detention camp, the segregation camp, and the Bhamburda health camp.

21,390. Are there any statistics available to tell us what the percentage of attacks was in the segregation camp?—Yes, I will supply those.

21,391. Can you compare those with the incidence of plague in the town? Can you tell us whether the percentage of attacks among the contacts was greater than the percentage of attacks in the town?—Yes. (The following statement was supplied later by the witness:—

Week ending.	Segregation Camp.			City.	
	Cases.	Admissions.	Percentage.	Cases.	Percentage.
8th October -	3	420	·710	114	·190
15th „ -	3	169	1·775	180	·300
22nd „ -	3	218	1·376	242	·403
29th „ -	6	324	1·851	347	·578
5th November	6	556	1·079	396	·660
12th „ -	5	986	·507	316	·526
19th „ -	3	850	·352	413	·688
26th „ -	9	971	·926	382	·636
3rd December	4	527	·759	376	·626
10th „ -	3	502	·597	424	·740
17th „ -	2	539	·371	334	·556
24th „ -	2	717	·278	211	·351
31st „ -	2	391	·511	155	·259

The percentages of attacks are taken week by week, from 2nd of October to the 31st December 1897, this being the period during which the epidemic was at its height. The comparison with the percentage in the City is not easy to obtain owing to the great exodus of people during the epidemic, and is taken for the same three months for 60,000 people. The total percentage is given below, the mean between the epidemic population and the normal being taken as a fair average of the population during the whole outbreak. The percentage for the City for the whole epidemic, taking the population at 90,000, is 5·16.)

21,392. Have you done any disinfection work?—Lately I have been placed in charge of disinfection work.

21,393. What houses have you disinfected?—The Medical Officer, in the course of his rounds, makes a note of all the houses in which plague deaths have occurred, or plague sicknesses, and he reports those to the Central Office. There are two Superintendents for the city of Poona, and they take out disinfecting parties to disinfect those houses.

21,394. Who are those Superintendents?—Captain Lockhart Muro and Mr. Holland.

21,395. Are these officers directly in charge of the disinfecting parties?—Yes.

21,396. How many disinfecting parties go out in the day? Are there only two disinfecting gangs, each with with an officer?—There are so many men available at the present time for this disinfecting work, and those men are divided up. If the houses all happen to be in one direction in the division of one officer, he takes the whole party.

21,397. Is there a European officer sent out with every gang?—A Superintendent goes out with a party.

21,398. Does a European officer superintend the disinfection of every house?—Yes; the European officer being the Superintendent as well.

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21,399. Where are the disinfectants made up?—At the Municipal Technical School, where the disinfecting apparatus and materials are stored. The disinfectant used is an acid solution of perchloride of mercury.

21,400. Are those concentrated disinfectants?—Yes.

21,401. Do you know what strength they are?—1 in 725, that is what we are using at the present time.

21,402. That is not the solution sent out?—No; it is sent out in a strength of 1 in 100.

21,403. A 1 per cent. solution is sent out, and that is diluted?—Yes. A wooden corn measure is taken filled with the original solution; that is placed in a wooden bucket, and water is added from a pail to about four inches from the top. That makes a solution of 1 in 725. For the disinfection of clothes we use a strength of 1 in 1,000.

21,404. You dilute the solution 7·2 times?—Yes.

21,405. Why was this inconvenient measure taken?—It is convenient for us, because it is very easily obtained. Each disinfecting party takes the solution with them in small casks. We can easily get water in Poona, and the solution is made to this strength on the spot.

21,406. How is the disinfectant applied? Is it sprayed on?—We have pumps. Before the coolies are allowed to enter a room the floor is wetted with the solution. To protect the coolies, I may add, I was supplied with wooden sandals to protect their feet. Then the floor is scraped up, and the floor and walls are thoroughly disinfected.

21,407. Has everything been removed from the room before disinfection was undertaken?—Yes.

21,408. How are the articles which are removed disinfected?—With the solution.

21,409. Are they dipped into the solution?—Yes, at the discretion of the officer in charge of the party.

21,410. How is this discretion exercised?—The clothes are disinfected in the solution of 1 in 1,000.

21,411. Supposing they are clothes which would spoil with the solution, what is done? Is there an alternative measure?—We have compensation, and so on.

21,412. What is done with the metal vessels?—They would not be disinfected.

21,413. Are there a good number of articles which may escape disinfection?—Certainly.

21,414. Do you know whether any cases of plague have occurred in your disinfecting gangs?—No cases have been brought under my observation since I have been in charge.

21,415. Have disinfecting gangs been inoculated with Professor Haffkine's prophylactic?—No.

21,416. You have sent in a statement showing the statistics of corpse inspection, with a variety of diagnoses; do you put that statement in?—Yes. It gives the total number of corpses examined, together with the probable or approximate cause of death in each case. No *post-mortem* examinations are made, therefore the exact cause of death is not ascertainable. The statement is as follows:—

STATISTICS of CORPSE INSPECTION, POONA CITY, 1st January to 31st December 1898.

Probable Cause of Death.	January.	February.	March.	April.	May.	June.	July.	August.	September.	October.	November.	December.	Total Deaths from each probable Disease.
Accidents	3	5	5	6	—	2	2	5	5	7	3	3	46
Alcohol	—	—	1	—	—	—	—	—	—	2	—	—	3
Asthma	1	9	10	—	—	—	—	—	—	—	—	—	20
Blood Diseases	—	1	1	2	2	—	3	2	2	3	5	5	26
Bronchitis	1	17	21	26	20	23	31	35	42	45	34	37	332
Broncho-Pneumonia	—	2	8	9	5	9	15	17	14	18	26	23	146
Brain and Nervous	1	—	3	1	1	5	2	5	1	8	9	3	39
Cholera	—	—	—	—	—	—	—	2	—	—	—	—	2
Colic	—	—	8	3	2	2	1	—	2	—	—	1	19
Convulsions	2	4	6	11	8	6	11	20	4	11	23	11	117
Croup	—	—	—	—	—	—	—	—	—	1	1	—	2
Debility	—	—	—	—	—	—	3	3	—	4	3	2	15
Dentition	—	—	—	2	—	—	—	—	—	—	—	2	4
Diarrhœa	6	14	17	37	46	47	80	127	85	74	42	55	630
Dysentery	1	4	6	2	3	3	2	2	5	8	13	6	55
Dropsy	—	4	6	1	2	6	5	5	6	2	4	10	51
Fevers	—	2	5	6	8	12	11	12	7	6	5	19	93
Goitre	—	—	—	—	1	—	—	—	—	—	—	—	1
Heart Disease	2	9	2	7	4	3	6	1	3	5	5	5	52
Hernia	—	—	1	—	—	—	—	—	1	—	—	1	3
Hydrophobia	—	—	—	1	—	1	—	—	1	3	1	—	7
Inanition	9	12	18	23	25	24	59	73	59	42	39	36	419
Leprosy	3	1	4	1	1	2	1	4	1	1	4	2	25
Liver	—	2	1	2	2	5	2	1	1	2	5	4	27
Malignant (cancer)	—	2	1	—	—	3	5	—	1	2	2	2	18
Measles	—	—	—	—	—	—	—	—	—	—	2	5	7
Opium-poisoning	—	1	—	—	1	2	—	—	—	—	—	—	4
Peritonitis	—	1	1	—	2	4	—	—	—	—	4	—	12
Plague	90	10	2	1	1	—	—	1	—	1	—	—	106
Pneumonia	—	1	3	4	6	1	—	6	5	4	7	4	41
Puerperal	1	1	—	2	3	1	4	1	5	4	3	6	31
Rheumatism	—	—	—	1	—	—	—	—	—	—	1	—	2
Rickets	—	—	2	3	2	4	—	—	—	—	2	—	13
Senile decay	23	9	8	14	9	9	11	6	14	10	22	20	155
Small-pox	—	1	1	—	—	1	—	1	—	—	—	—	4
Snake-bite	—	—	—	—	—	—	1	—	—	—	—	—	1
Spleen	—	—	—	—	—	—	—	—	—	1	—	—	1
Still-born	4	8	5	9	6	13	8	18	13	14	14	15	127
Sudden death	—	—	—	3	2	2	—	2	4	—	—	2	15
Syphilis	1	—	—	1	—	—	—	1	3	1	1	3	11
Tuberculosis and Phthisis	4	10	11	15	15	16	26	10	15	23	30	20	195
Unknown	63	3	13	—	—	—	—	—	—	—	—	13	92
Urinary	—	3	—	1	2	1	3	1	3	2	4	7	27
Whooping cough	—	—	—	—	1	1	3	1	—	3	13	10	32
Zenana Hospital	—	—	—	—	—	2	4	1	—	—	—	—	7
Total number of deaths per mensem	215	136	170	194	180	210	299	363	302	307	327	332	3,035

21,417. Are these diagnoses arrived at by corpse inspection?—Yes.

21,418. You have diagnoses, for instance, of snake bite and spleen; were those diagnoses made at the corpse inspection?—Yes, from the clinical history and from the mark of the snakebite, and so on.

21,419. Would you be able to make any of these diagnoses independently of the medical history which has come to you?—In many cases we can; we could see for ourselves.

21,420. Let us take these things one by one; could you see alcohol?—We had the history of the three cases of alcoholism mentioned in the table. They were examined and certified during life.

21,421. I am asking you, could you diagnose it independently of the history, because the history depends on the patient who may give you a wrong history?—We go by the history and the general aspect.

21,422. You do not profess that these diagnoses, such as alcohol, blood diseases, and bronchitis, are all derived from corpse inspection?—No, they are the approximate causes of death, arrived at by a general consideration of the facts of the case. All these diagnoses are found by corpse inspection *plus* previous examination of the deceased during life, because many of them were certified. The clinical history was gone into as fully as possible, and the approximate cause of death arrived at.

21,423. (*Mr. Cumine.*) What is the population of that part of the town in which you had the inspection of sick persons and corpses?—Not less than 130,000. We inspect the whole city.

21,424. Do you, personally?—I and my colleague personally..

21,425. What is the penalty for not reporting sickness?—There is no penalty—at the present time, that is to say.

21,426. What is the penalty for not reporting a death?—There is no penalty now.

21,427. Supposing a person is found to be dead at 7 in the morning, what means have you of knowing whether he was ill before 7 o'clock the previous evening? When plague is very virulent people are taken ill and die within 12 hours, do not they?—Yes.

21,428. When they are reported to be dead at 7 o'clock in the morning, what is the means that you have for knowing that they were or were not ill before 7 o'clock the previous evening?—The clinical history supplied by relatives and friends of the deceased.

21,429. Supposing they declared the man was not ill before death, or that he was taken ill after 7 o'clock in the evening, what means have you of knowing it is not true?—We have no direct means, but we reconcile the statement with the general appearance of the body.

21,430. What is the largest number of corpses you have ever had to inspect of people who have died during the 12 hours of night?—I have examined as many as 27 corpses in one day, and 18 in the morning, but I cannot tell you how many died in the previous 12 hours. Some of them I know died an hour before I arrived.

21,431. 18 is the largest number you have had to examine in one morning?—Yes.

21,432. How long would it take you to examine 18 corpses?—These were not reported at the same time. They were in different parts of the city, and reports came to me up to 11·30 or 12 in the day.

21,433. So that the inspection of these corpses took you to 11.30 or 12 in the day?—Quite that.

21,434. If somebody died at 5 minutes past 7 on Friday evening, his corpse might not be examined until 12 o'clock mid-day on Saturday?—That is not so, because that particular case would be reported the first thing in the morning by the friends of the deceased and that corpse would be seen first.

21,435. What is the maximum number of deaths which you have had reported to you at 7 o'clock in the morning as having occurred within the previous 12 hours?—I cannot tell you that. I do not think we have a means of knowing, because we do not keep the hours at which reports are made by the friends.

21,436. How long does it generally take you to get through the corpses of people who have died in the preceding 12 hours at night?—On the occasion I have mentioned, when I had 18 corpses, they were finished at about a quarter to 12.

21,437. It is possible, is it not, that a person might die at 7 o'clock in one evening and the corpse not be examined till 12 o'clock the next day?—If the cases were reported at 9 or 10 o'clock I would be on my rounds which would fully occupy 1½ hours, because the sick have to be examined, especially urgent cases of illness. When I come back, if there are four or five deaths reported, I start out again.

21,438. What is your answer then?—I say it is impossible to tell, because we do not notify the time at which death reports are received at the Medical Officer's office, but I should not think it is possible, because if a person died at 7 o'clock at night the friends would be anxious to give information the first thing in the morning, and then that corpse would be seen forthwith.

21,439. When you go to breakfast in the middle of the day, who is there present to do corpse inspection of any one who may die?—There is an interval of three hours.

21,440. Then you are not on duty ready to do corpse inspection from 7 in the morning till 7 in the evening?—The work is divided between the two Medical Officers from 7 a.m. to 7 p.m., with the exception of an interval at noon.

21,441. What is the interval in the middle of the day during which there is nobody on duty?—From 12 to 3.

21,442. Supposing that by 12 o'clock you had not finished an inspection of all the corpses of people who had died at night and in the morning, what would you do?—We would finish them.

21,443. Is it not the case that sometimes in bubonic plague the buboes subside before the man dies, or just after?—I have not met with an example.

21,444. In some cases, I think, the disease was not diagnosed as plague in the lifetime of the person, but was diagnosed as plague after death. In how many cases did that occur? Was it 106?—These 106 were cases diagnosed which had not been previously reported—they were the corpses seen.

21,445. They had not been seen in life at all?—That is so.

21,446. Were these people living in Poona or did they come from outside anywhere?—I take it they were residents of Poona.

21,447. How had they got the plague?—The month of January (when plague was very bad) was the second bad month. You will remember that in the last outbreak of plague for the week eneing 11th December there were 470 odd cases of plague. In January the cases were 90, in February 10, and in March two, and in April one. Then we had one imported case in June and one again in October, making a total of 106. Then, of the imported cases which occurred sporadically, there was one in August, one in October, and one discovered by the surveillance system. Of course, these 90 cases were not seen during life; we were at the height of the epidemic, which was of a virulent type.

21,448. Since July last has there been any case where the patient has been seen in life but the illness has not been diagnosed as plague, but has been diagnosed as plague after death on corpse inspection?—No, there has not been any such case, because they were diagnosed during life. They were found out during life through our system of reporting sickness.

21,449. (*Mr. Hewett.*) How many of the 106 cases of plague which were detected by corpse inspection were bubonic plague?—The great majority were during the months of January and February.

21,450. Were any of them cases of pneumonic plague?—Not at that time.

21,451. What proportion of the people who are sick do actually report their sickness and obtain certificates, in your opinion?—We could easily calculate that. The present population has largely increased, and is now about 130,000. It has increased from last June, when the city got very full indeed. Out of that number, 1,891 have been certified up to the end of the year.

21,452. Do you think that half of the people who are sick report themselves as sick?—I should not think as many as half.

21,453. Do you think anything like half do?—No.

21,454. I see that you certified 45 persons in the month of June, and examined 210 bodies in that month What proportion of the cases of sickness do you think

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were reported then?—That would be about one-fifth or nearly one-sixth.

21,455. Why would it be nearly one-sixth? It would be about one-fifth, assuming that almost everybody who fell sick and was not reported to be sick died of his sickness, would it not?—Yes; but, as I said before, it is almost impossible to tell, unless one went into all the districts of the city and found out which districts were reporting most cases of sickness and in which districts most deaths occurred during the corresponding period.

21,456. If all the mild cases of sickness were reported to you, would not you expect to find the number of cases of sickness far in excess of the number of deaths?—Yes.

21,457. Then in the months of November and December, when you certified 192 and 153 sick respectively, and when you examined 327 and 332 bodies respectively, a good deal of the sickness could not have been reported to you?—Yes.

21,458. Have the people here any objection to treatment by means of European medicines?—No. I cannot say that they have any great objection.

21,459. Do they prefer to be treated by their own medical attendants?—Yes.

21,460. Is the pardah system in force to any considerable extent here?—In certain parts of the city.

21,461. Among whom?—The Muhammadans.

21,462. (*Dr. Ruffer.*) Will you look at your statistics of corpse inspection of Poona city? (*See* Question No. 21,416.) In the month of January you had 90 cases of plague. I see that you had 63 cases unknown. Do you think some of them might have been plague?—These 63 cases were marked "Not plague" because no approximate cause of death was then required. This was during the height of the epidemic.

21,463. I do not understand why?—We either marked these cases as "plague" or "not plague." The diagnosis in these cases was that it was not plague. The approximate cause of death was not arrived at.

21,464. How do you know it was not plague?—The Medical Officer decided that.

21,465. How could you diagnose a case of plague pneumonia after death?—From the history and appearance of the body.

21,466. What appearance of the body or what history would enable you to diagnose a case of plague pneumonia from a case of ordinary pneumonia?—It would have the appearance of plague.

21,467. Supposing the man had no bubo?—In that case the clinical history would have to decide the point.

21,468. Suppose you had a case of acute pneumonia, how would that differ from a case of plague pneumonia?—It would not necessarily differ.

21,469. To what do you attribute the large number of cases of deaths from inanition in July, August, September, and October? I see the deaths from inanition jumped up from 24 to 59?—We include deaths from marasmus, lack of suckling, and deaths occurring among sickly and rickety children. At this time we had the rainy season, which is always a fatal time for weakly children.

21,470. To what do you attribute the great rise of death from bronchitis during September and October?—To extremes of temperature, cold chilly nights, followed by warm sunny days.

21,471. Does the Poona Municipality provide you with a microscope?—Yes, we have one at the Plague Hospital.

21,472. Have you the means of making a bacteriological diagnosis of a case of plague pneumonia?—No, but we are taking bacteriological specimens now, and we send them to Bombay for examination.

21,473. Have you an incubator and the necessary machinery for making cultivating media?—No.

21,474. Have you the necessary material for making cultivations?—Yes.

21,475. Then you can make a bacteriological diagnosis here in a doubtful case?—No; but we send to Professor Haffkine's laboratory.

21,476. Supposing you want to make a bacteriological diagnosis, would you make it here or would you send it to Bombay?—It would be sent to Bombay.

21,477. (*Mr. Cumine.*) Of these 3,035 people you have mentioned in your statistics of corpse inspection, was not the illness reported before death in any case?—That number includes all reported and not reported. This is a record of all the corpses seen from the 1st January to the 31st December, and includes cases reported during life.

21,478. With regard to these 106 deaths put down to plague, I understand you to say that you had received no previous notice of illness?—Yes. In this record we did not take into account the fact whether they were seen or not seen. This is only a statement of corpse inspection.

21,479. Do those 106 include cases in which the illness was reported before death?—No.

21,480. And cases in which the illness was not reported before death?—This only means *corpses seen in the city of Poona.* During the months of August and October two cases, which are marked "1" and "1" respectively, were seen by the Medical Officer, having been reported by volunteer gentlemen, and they were sent off to the General Plague Hospital, and died there. This does not include any which were seen by the Medical Officer and died in hospital.

21,481. Among those 106 corpses, were there some cases in which the illness had not been reported before death?—Yes, all of them, indeed, were cases "*not reported*" before death.

21,482. Are there some in which the illness had been reported before death?—None.

21,483. Can you tell us in how many cases the illness had been reported to you before death?—One case was seen by my brother (who was then my colleague) when *in extremis*, and death ensued in half-an-hour. Word was brought to him and he went back again. I think he had another case like that. These are all corpses actually seen after death. You cannot call them being reported in life, because the patient was in a moribund condition.

21,484. (*The President.*) Had you known of these 106 cases previous to the corpse inspection?—No.

21,485. I understand you to say that when you had information of a plague case in life you did not again examine the corpse after the patient died, but gave a pass for burial?—No, we saw the corpse.

21,486. Do you look at the corpse of every man who dies here?—Yes.

21,487. (*Prof. Wright.*) I understand that every case which is diagnosed as plague during life is sent to the Plague Hospital?—Yes.

21,488. Then his corpse is not re-inspected there, is it?—I do not know. We have nothing to do with the Plague Hospital's internal administration.

21,489. This does not include people who died at the Plague Hospital?—No; but yesterday morning a case was sent by my colleague suffering from plague, and the same day word was brought to me that that man had died from plague before removal to hospital, and that corpse was seen by me in the city of Poona.

(Witness withdrew.)

Prof. G. K. Gokhale.

Professor G. K. GOKHALE called and examined.

21,490. (*The President.*) You hold an office in the Fergusson College, Poona?—Yes.

21,491. What is your position?—I am Professor of History and Political Economy.

21,492. (*Mr. Hewett.*) You have only been employed during the second epidemic in Poona, I think?—Yes; I was doing work here as a plague volunteer during the time.

21,493. Between what dates?—From the middle of September to about the end of February.

21,494. What were your duties?—Up to the middle of November I had merely to accompany search parties

composed of soldiers, both British and Native. Afterwards, in addition to this duty of search I had to visit the detention and segregation camps on certain days, and advise the Superintendents of those camps upon matters on which they wanted to consult me.

21,495. Were you enabled to trace the source of the second infection at Poona?—No. The general impression here was that the poison was first imported from Kirki, but I am not sure that it was so, seeing that after the first epidemic we had stray cases occurring, say, one or two a week, in the town all through the intermediate months.

21,496. Have you any opportunity of discovering how a place gets infected with plague?—Yes. In my search work I often came across instances which appeared to me to indicate that the soil must be getting infected first, and that the infection must be spread chiefly through the soil.

21,497. What led you to form that opinion?—I found, for instance, that men who were sleeping on the ground floor were most liable to be attacked, while men who were sleeping on the first or second floor had very few cases among them. I also found that in the hospitals and segregation camps, where good care was taken to disinfect the soil, there were very few cases.

21,498. Do the majority of the people of the lower classes sleep on the floor without any beds?—They just put a mat or something like that on the ground.

21,499. How do you think that plague proceeds from an infected house to the neighbouring houses?—The process of dissemination seems to me to be this:—The poison in the system of a plague patient or of an affected rat or some such animal acts on the soil, if the soil is in a fit condition to be affected. Certain poisonous matter is then produced in the soil. The virulence of its infection seems to depend on the nature of the soil, and also, of course, upon atmospheric conditions. Persons who come into contact with such soil, and whose constitutions are not of a character to resist its power, come to be attacked. The infection seems to creep from the soil of one house to the soil of another, where the conditions are favourable. Affected rats, in many instances, carry the poison from the infected house to localities close by. Affected human beings carry it to longer distances, and thus form new centres of infection in congenial soils.

21,500. Have you had an opportunity of observing whether the infection proceeds by contact between human beings?—No, I do not think it does.

21,501. Or by clothes?—There were a few instances in which men who had come from Bombay were found to have brought the infection with them, though they themselves were not attacked.

21,502. Did you notice any affected rats in Poona?—Yes, several.

21,503. At what particular time did you observe them?—I specially saw them at the commencement of the epidemic.

21,504. In what months?—In September and October, particularly, I think.

21,505. Did you notice that plague was more virulent in any particular class of houses in the town than in others?—Yes. Generally, I found that houses which were damp, dark, and ill-ventilated were most exposed to the attack.

21,506. You are of opinion that the best means of dealing with it is to keep the patient away from the infected house?—Yes, the patient, as also the healthy inmates of the house. *Prof. G. K. Gokhale.* 23 Feb. 1899.

21,507. Have you had any opportunity of noticing the effects of disinfection?—I think so. Wherever these cases were discovered houses were disinfected, and it was then found that the infection did not spread to the adjoining houses; at any rate, not in the same rapid manner in which it had done so before.

21,508. Have the people here shown any objection to disinfection?—Yes, there is a natural disinclination to it.

21,509. Is this due to the feeling that it may destroy their property?—Partly. But there is personal disinfection as also disinfection of clothes.

21,510. Which do they object to?—They object to both, but personal disinfection is more disagreeable to them.

21,511. Are the people of a large town less inclined to move out into the neighbouring fields than the inhabitants of an agricultural village?—I should think so.

21,512. What are the reasons which would deter them from going out?—Their habits of life are such that they are not used to living in the open fields. Moreover, in large cities it is a costly thing to have huts put up, and many of the people cannot afford them. They are also afraid that their property, if left behind, would be stolen, and if they take it with them they are afraid of robbers.

21,513. Do you think that the people report the great majority of cases of sickness which occur in the town?—I think they do.

21,514. Before death takes place?—Yes. There may be a few cases where it is not reported, but, as a rule, they are now ready to report, because they know the advantages which they now secure by reporting.

21,515. How do you account for the great excess of deaths over reports of sickness?—Do you mean at present?

21,516. In the month of December last?—I was told that there was exceptionally heavy mortality among children, and that these cases were not reported.

21,517. Is your opinion based on facts?—I give you what I have heard.

21,518. (*Dr. Ruffer.*) Do you have many houses of two and three storeys in Poona?—Yes.

21,519. On what facts do you base your opinion that people in the upper storeys did not get plague so much as people on the lower floor?—For four or five months I was going about with the search parties. We found that most of the cases which occurred were on the ground floor.

21,520. Do the poorer people live on the ground floor?—Yes, as a rule. In fact, they live in houses which are only one storey.

21,521. Are the ground floors more crowded than the upper floors?—I think so.

21,522. Is there more ventilation on the upper floors?—Yes.

(Witness withdrew.)

Mr. Vishnu Anant Patwardhan called and examined.

21,523. (*Mr. Cumine.*) I believe you are a Municipal Commissioner?—Yes, at present. I was not during the epidemics. I received a letter from Sir Andrew Wingate stating that Government desired me to give evidence. *Mr. V. A. Patwardhan.*

21,524. You have come to tell us about the city of Poona during the second outbreak of plague?—Yes.

21,525. Have you lived long in Poona City?—Yes, for more than 30 years.

21,526. Which were the plague operations with which you were connected?—(1.) The house-to-house visitation by soldiers and volunteers in search of plague cases. (2.) The segregation and quarantine camp at the Sawar's Gate. (3.) The disinfection and detention camp at the Poona Railway Station. And latterly I have been serving as a Chief Volunteer in charge of one of the wards into which the city has been divided for plague administration.

21,527. Will you tell us what lesson the experience of Poona especially impressed upon you?—When the plague broke out for the first time in Poona, no one knew exactly what measures to adopt, and every step that was suggested as possible to help the suppression

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of plague was adopted. The people were new to these measures. They were panic-stricken and they considered these measures to be more horrible than plague itself. The result was the concealment of plague cases *which helped the spread of the disease.* No plague measures should, therefore, be so severe that people will try to evade them. The maximum of efficiency should be sought for with the minimum of inconvenience to the people. The experience of Poona has fully taught us that it is with the co-operation of the people themselves that this end can best be secured.

21,528. Will you tell us what has been done in Poona during the last few months to prevent the re-infection of the town?—The whole city is divided into 16 wards. A native gentleman called a Chief Volunteer is entrusted with the sole business in connection with plague in that ward, and a European officer supervises his work. The Chief Volunteer finds out native gentlemen to co-operate with him voluntarily, and one to be in charge of each sub-division. A complete census of each house in the ward is taken and is kept corrected up to date. The householders are required to report all new arrivals, all departures, every illness, and every death. They have also been required to report the appearance of dead rats in any house. The volunteers in charge of each sub-division visit each house once a week to check the census. All new arrivals by rail or road are required to report themselves at the Ward Office, and are kept under surveillance for 10 days, that is, they are required to present themselves at the Ward Office every alternate day during this period, and their names are entered in the census if they stay longer in the city; but persons leaving Poona during this period are freely allowed to go. No one on the census list can go by rail or road without a pass from the Ward Office, and no one can arrive by rail and enter the city unless he is vouched for by some volunteer, and information as to the locality he comes from, and the place where he will put up on arrival, is furnished. If any passenger, released on surveillance, fails to appear at the Ward Office, the gentleman (volunteer) who vouched for him is called upon to produce him or to explain for his absence. Every illness is to be reported, and a Plague Medical Officer is to examine the sick person and certify to his illness. If any illness which is not previously reported turns fatal, the inmates of the house are kept under surveillance for 10 days. No corpse can also be removed from the house until examined by a Plague Medical Officer. In this way a watch is kept over every individual inhabitant, and any case of plague, indigenous or imported, is brought to notice within a short time. The sick person is then at once removed to hospital, the inmates of the house are kept under surveillance, and they are asked to vacate the house for disinfection. The first cases of sickness are thus detected, isolated, and effectively dealt with, and are not allowed to grow into so many different centres for dissemination of plague. Had this very thing been done in December and January 1897, when we had imported plague cases from Bombay, we would have probably had no epidemic at all, or we would have at least considerably arrested its growth.

21,529. Have you any figures to put in to show us how this organisation protected Poona?—From July 1898 to the end of January 1899 there were in all 28 cases of plague, 4 of which were indigenous and 24 imported. In each of these cases the measures I have described were taken, and we believe that those measures prevented each of those cases becoming a plague centre.

21,530. Do the inhabitants of Poona city themselves appreciate the importance of this organisation?—All plague restrictions are, of course unpleasant, but the people realise the necessity that they must choose the less of the two evils. I believe that people have learned the value of these measures.

21,531. Has the late infection of Poona been due to a Poona person or an outsider?—It is due to an outsider, a Bombay man, residing here for a short time, and having frequent communication with Bombay.

21,532. Was not the person in whose house he lived to blame?—The house was vacant and this man hired it, but we do not know whether the owner of the house looked after it and knew of this, and the consequent illness, &c.

21,533. Have you noticed during the first outbreak that one particular part of the city was affected and during the second, another part?—Yes. During the first outbreak the eastern part, and during the second the western part, were particularly affected. It was a remarkable feature in these outbreaks, that while the disease was seriously raging in the eastern part, only stray cases occurred in the western part, which did not form centres of infection; and that while it raged in the western part only stray cases occurred in the eastern part. In some instances, houses which were affected at the first outbreak were also affected at the second.

21,534. Have you any figures with regard to that?—Yes, I have figures only for three Pets which formed the western part. In Narayan Pet, from January to May 1897, there were 27 cases and 21 deaths; in Sadashiv Pet there were 96 cases and 79 deaths, and in Somwar Pet there were 82 cases and 60 deaths. That was during the first epidemic. During the second epidemic, that is, October, November, and December 1897 to January and February 1898, in Narayan Pet we had 195 attacks and 169 deaths, in Sadashiv Pet we had 544 attacks and 429 deaths, and in Somwar Pet 317 attacks and 260 deaths.

21,535. Where do you think the poison lies in an infected town?—My impression is that there is something wrong in the soil of the infected house. I also observed, looking to the situation of the houses, that the poison appears to pass or be carried from house to house.

21,536. Have you noticed on which floor people are affected most?—On the ground floor. I have prepared a statement for houses in the Narayan Pet, and there you will find a number of small blocks of houses affected. I have myself gone over these—I know the locality.

21,537. Will you tell us your deductions from it?—That a block of houses becomes infected, and when there is a patient in one house my impression is the poison passes along or is carried to and infects the soil of the next house. I say the soil, because I find that in hospitals and segregation camps and so on the soil is not affected, and no amount of contact affects the other persons, but it is the residence in an affected area itself which communicates the contagion. If there is one case in one house and the people continue to reside in that house, it is my experience that other cases follow in the same house.

21,538. How do you think the infection spreads from house to house?—In Poona the poison imported from Bombay grew in the soil of the affected houses and spread from house to house, *how* it is very difficult to say! Rats may be one of ths agencies which spread the plague. They are also the first to be affected, and they serve as a warning to human beings to quit the locality. I know of houses where dead rats were discovered and removed, but where no cases of plague occurred. These houses are, however, well built and well ventilated, and were occupied by fewer inmates after the appearance of dead rats. I consider that poison is not carried from person to person by mere contact, however close, but is imbibed by a person by residence in an affected locality, which is not sufficiently exposed to the sun and breeze. The experience at the several hospitals supports me in this belief. I know of cases in hospital as well as outside hospital, where sick wives and other relations were attended upon night and day, and not even the vomit of the patient on the body of the attendant affected the latter. Some of these patients recovered and some died. The very small number of fresh attacks compared with the total number of admissions in the detention and segregation camps also supports this view. Native volunteers who accompanied search parties and entered affected houses barefooted or even touched sick persons were in no way affected. I have a statement showing the number of fresh cases in the several camps and hospitals.

21,539. Will you tell us what those figures are?—In the railway station detention camp from the 13th February to the 13th May, 13,606 people were admitted, and there were only 23 attacks. In the Bhamburda health camp from December 1897 to May 1898 there were 8,282 admissions, out of which 4,807 were from

infected parts, 2,935 were from arrivals by road and 540 arrivals by rail; that was when the railway detention camp was not ready. There was only one plague case. Then in the Sawar's Gate segregation camp from the 12th March to the 12th June 1897, 3,461 people were admitted and there were only 41 attacks. At the Hindu Hospital the rule was that each patient was allowed to have one attendant. There were 104 patients and an equal number of attendants, and among those attendants there were only two cases during the first outbreak, that is from the 15th March to the 31st May 1887. During the second outbreak from the 1st October 1897 to the 15th March 1898, 533 patients were admitted with an equal number of attendants, and among the attendants there were two cases, and two amongst the establishment, which numbered 57. In the General Plague Hospital from the 1st June 1897 to the 31st March 1898 there were 4,138 patients, and I take, as before, an equal number of attendants. No case of illness is reported among the attendants, but only among members of the establishment. There were 376 persons employed, out of which 26 were attacked.

21,540. Do you consider isolation of the sick to be necessary?—I consider the isolation of the sick to be necessary in the interests of the patient himself. If the locality of the house is affected, the patient has less chance of recovery by continuing to reside in the same house. Few people who continued in the same house recovered; while the non-removal of the first case led to several other cases in the same house. Instances of this can be given in numbers. Also about 20 to 30 per cent. of the cases which were removed to hospitals recovered.

21,541. What is your opinion of the importance of the evacuation of infected houses?—Evacuation of the affected house is also necessary in the interests of other inmates. Wholesale evacuation is highly objectionable especially in the monsoon and cold weather. If the persons who came in close contact with the patient leave the affected house that is quite sufficient. Where they should go is a question. I do not think all such persons, high and low, should be segregated at one place, as was done in the Poona segregation camp. Well-to-do, middle and upper class people, though they were willing to make their own arrangements to segregate themselves effectively, were not allowed to do so. If rich people go and occupy their garden houses, and if others go to reside in other vacant houses in an unaffected locality, I think they should be allowed to do so, provided they agree to remain under daily surveillance and medical examination at any of the Ward Offices. As to the lower classes, unless you provide accommodation for them, evacuation is a misery to them. They suffer by exposure to heat and cold, to which people living in crowded houses in a city are not accustomed. Inmates of neighbouring houses should be asked to vacate only in those cases where it is absolutely necessary to do so, but they need not be carried to health camps outside the city. They should be permitted to reside elsewhere in the city. If they are kept under daily surveillance for 10 days, that will be sufficient.

21,542. What have you to say about suspicious cases?—In suspicious cases the inmates were segregated even before the case was declared to be one of plague, and they had to undergo the pangs of segregation for nothing. This should be avoided. I have noticed in my own experience that a person who was subsequently found to be suffering from syphilis was removed as a suspicious case, and the inmates were then and there segregated and had to remain in the segregation camp for ten days.

21,543. You do not say, do you, that all the suspicious cases turned out not to be cases of plague?—No, what I mean is unless a case is declared to be plague the inmates should not be segregated. If we have a complete census of the locality we know who the inmates are and we can get hold of them, and when a case is declared to be plague they can be segregated.

21,544. Do you think that one central Plague Hospital is enough?—No. The ward hospitals, as in Bombay, are more suitable. We are naturally averse to remove our sick to hospital, but if a hospital could be provided for in the ward itself the *opposition of the people would be reduced and concealment of the plague cases would be discouraged.* If a separate house could be secured close by for segregating the contact people, so that they could frequently visit their sick and attend upon them, it would be all that could be desired. People in general have no faith in hospital treatment, and especially a central hospital as in Poona. The extreme desire of the people to go to their caste hospitals supports me to some extent in this view. In Poona the hospital and the segregation camp were at the two extremities of the town, one to the north and one to the south, which caused extreme inconvenience and irritation. The patient died in hospital and his relations did not even know of it in the segregation camp. Such a state of things is most repugnant to native feeling, and the remedy is to have the hospital and the segregation camp close to each other.

21,545. You attach great importance to this point?—Yes.

21,546. What have you to say with regard to disinfection?—On this point I am not in a position to give any decided opinion, because disinfection of houses was carried out, but I have something to say on the other side. Several dead bodies were found on the roads, and several deaths were reported which could not be traced as having occurred in any particular house, in which cases no disinfection took place either of clothing or anything else. These very clothes were, of course, used again, and my general impression is that the people continued to occupy those houses with safety and even used the clothes.

21,547. I thought you said it could not be found who the people were?—It could not be found to what houses they belonged, but they must have been residents of Poona. They must have belonged to some house, and my impression is that the people removed the dead body in order to free themselves of the consequences which would follow reporting the case. In those cases the houses could not be disinfected. Disinfection of affected houses was carried out with particular attention. Well-ventilated, spacious houses, exposed to heat and sun, which were surrounded by affected houses, could not have escaped the contagion. They were in several cases vacated and locked, or in some cases occupied by a few servants and others. They were not disinfected, and yet no cases occurred in them; and they were re-occupied with safety. What seems necessary is merely evacuation and exposure to the sun and air for some period. If the inmates desire to re-occupy the house early, it is safer to disinfect it and allow it to dry before it is re-occupied.

21,548. What sort of houses, in your experience, were the ones most affected?—My experience at the time of the house-to-house visitation showed that most of the houses affected were either old buildings with moist floors, or ill-ventilated and with insanitary surroundings; and in such houses more cases than one occurred, while the number of houses which were marked affected, but which were spacious and well-ventilated, was very small, and it was very doubtful whether they were at all affected. Out of 486 houses in Narayan Pet 84 were were affected, and out of them only about eight or 10 were good buildings. I have prepared a statement of these 84 houses which gives particulars, as follows:—

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STATEMENT of affected HOUSES and their CONDITION with DATES of ATTACKS from September 1897 to March 1898 in NARAYAN PET WARD, DIVISION Number XIV., POONA CITY.

Serial No. of House.	Dates of Plague Attacks from September 1897 to March 1898.				Total No. of Attacks.	Remarks as to Condition of House and other Special Particulars.
4	14.10.97	1.12.97	—	—	2	No plinth, has ground floor only, old rotten house, house occupied by Dhobis.
5	23.10.97	4.11.97	—	—	2	Has plinth in front but outside rotten, has ground floor, inhabited by Mahrattas, who make tin pots.
7	10.11.97	—	—	—	—	Rickety shed, as used for cattle.
10	2.11.97	—	—	—	—	Has no plinth, occupied by goldsmith, not ventilated.
14	11.11.97	—	—	—	—	One storeyed, is a good building.
22	29.10.97	—	—	—	—	Damp soil, no ventilation, old building, front part temple, back part used as residence.
23	11.12.97	—	—	—	—	Is gymnasium.
26	12.12.97	—	—	—	—	Rotten building used as a cattleshed, recently rebuilt.
27	7.12.97	—	—	—	—	Rotten building, extremely dirty, no plinth.
30	27.12.97	—	—	—	—	Milkman's house, used as cattlesheds.
31	27.12.97	—	—	—	—	
35	1.12.97	2.12.97	17.12.97	—	3	Narrow space, a building of two Khans adjoining a garden. Old building ill-ventilated.
36	21.12.97	—	—	—	—	Residences of milkmen, no plinth. Extremely dirty sheds.
37	18.11.97	—	—	—	—	
54	8.12.97	—	—	—	—	No ventilation, moist floor.
69	30.11.97	15.12.97	—	—	2	There was no ventilation, windows made by Municipality recently.
90	4.11.97	—	—	—	—	Old ricketty building, moist, adjoining river.
93	7.12.97	—	—	—	—	No ventilation, no plinth, old building.
98	31.10.97	—	—	—	—	Stables, one-storeyed.
111	7.12.97	—	—	—	—	Old building, ill-ventilated, moist floor, dirty surroundings.
112	28.10.97	—	—	—	—	Temple, good building.
121	5.12.97	—	—	—	—	Overcrowded. A small wretched and close building without light and plinth.
135	30.11.97	—	—	—	—	Overcrowded chawl, and occupied by shoemakers, surroundings dirty and unhealthy.
142	5.10.97	—	—	—	—	Very dirty house adjoining river, moist floor.
154	13.11.97	—	—	—	—	Extremely wretched building, moist floor.
184	10.11.97	—	—	—	—	Ill-ventilated, no plinth.
197	14.11.97	—	—	—	—	Shoemakers' building, ground floor only.
206	22.11.97	—	—	—	—	No plinth, moist floor.
234	12.11 97	—	—	—	—	The old house destroyed and new one built.
235	10.11.97	—	—	—	—	No plinth, ill-ventilated.
250	27.12.97	13. 1.98	—	—	2	Adjoining garden, ill-ventilated.
253	7.12.97	—	—	—	—	No ventilation, no plinth.
262	12.10.97	19.10.97	4. 1.98	—	3	Overcrowded. Row of shops in front, old rotten building, moist floor.
265	12.12.97	—	—	—	—	Moist floor.
300	10.12.97	—	—	—	—	Old ricketty building, extremely dirty.
302	19.12.97	—	—	—	—	Sweeper's house.
321	4. 1.98	—	—	—	—	Windows opened by Municipality recently.
323	22.12.97	—	—	—	—	Old rotten house.
333	10.11.97	—	—	—	—	A row of four rooms on the ground floor, adjoining a garden, no ventilation and no plinth.
340	10.11.97	—	—	—	—	There was no case in this house. A patient in the Plague Hospital wrongly gave his address in this house. It was marked as affected, and the segregating party broke open inner doors and boxes and cupboards to search contacts. The owner had left for Berars and a servant was kept to watch. It is a newly-built and substantial building.
347	14.12.97	—	—	—	—	A close house without ventilation or light, and inside dirty.
348	12.12.97	—	—	—	—	A good building, proper ventilation.
349	5.12.97	—	—	—	—	A temple, a good building, clean, newly-built house; contagion caught in No. 362, opposite, the house on the other side of the road.
353	11. 1.98	—	—	—	—	A washerman's house, a wretched row of rooms on the ground floor, moist and dirty.
357	9.12.97	—	—	—	—	An old ricketty building without plinth, not well ventilated.
362	6.12.97	7.12.97	9.12.97	10.12.97	4	An old building on the ground floor, moist, not well ventilated.
363	21.10.97	—	—	—	—	Not a good house; vegetation in the rear.
365	23.10.97	23.10.97	—	—	2	Overcrowded. An old house, rooms on ground floor well ventilated, part one-storeyed, ricketty.
367	30. 7.97	—	—	—	—	An old ricketty house, moist and dirty.
368	10.11.97	—	—	—	—	Moist floor, rooms on ground floor, ill-ventilated, recently holes made by Municipality in the house.
369	18.12.97	—	—	—	—	A good building, well ventilated, but is connected with 367.
371	31.10.97	5.11.97	—	—	2	Barber's house, surrounding is dirty.
372	6.11.97	—	—	—	—	A narrow house, dirty, ill-ventilated, moist floor.
374	29.11.97	—	—	—	—	An old house without any ventilation.

Serial No. of House.	Dates of Plague Attacks from September 1897 to March 1898.					Total No. of Attacks.	Remarks as to Condition of House and other Special Particulars.
376	18.10.97	—	—	—	—	—	An old ricketty house, rooms on ground floor moist floor inside below the level of the road.
383	20.10.97	21.10.97	25.10.97	27.10.97	30.10.97	16	An extensive ground, let out on ground rent, where poor people have built rows of ricketty small houses on the ground floor, without any plinth, without any drainage; very dirty. After a few cases the inmates dispersed, but, when affected outside, came to die in their own houses. Hence the large number of attacks At the first outbreak no cases occurred in these houses. Was overcrowded.
„	30.10.97	31.10.97	2.11.97	5.11.97	6.11.97		
„	6.11.97	8.11.97	8.11.97	10.11.97	15.11.97		
„	25.11.97	—	—	—	—		
390	2.11.97	6. 1.98	—	—	—	2	Dirty surrounding.
391	29.11.97	—	—	—	—	—	Old ricketty rooms on ground floor, ill-ventilated moist floor and surroundings dirty.
392	22.10.97	—	—	—	—	—	
398	16.11.97	—	—	—	—	—	Ill-ventilated, moist floor, narrow, one-storied building without open space, waste water drained through floor of house to the front road.
400	29.10.97	—	—	—	—	—	
402	21.10.97	22.10.97	—	—	—	2	An old building.
404	12.10.97	—	—	—	—	—	Milkman's house, used for cattle; inside dirty and ill-ventilated.
407	20.10.97	20.10.97	—	—	—	2	An old house, close, moist floor, one-storeyed.
409	14.10.97	—	—	—	—	—	Ground floor, extremely moist, ill-ventilated inside, no plinth, one-storeyed, old house.
412	5.11.97	—	—	—	—	—	Inside extremely wretched, moist floor, without light or ventilation; outside, one-storeyed ground building.
418	24.10.97	—	—	—	—	—	
420	9.11.97	—	—	—	—	—	An old one-storeyed house where casees occurred on the upper floor, ground floor moist.
421	13.11.97	—	—	—	—	—	An old overcrowded house, ill-ventilated, moist floor.
430	24.11.97	—	—	—	—	—	A part of the house old, and part newly built and well ventilated.
434	4.11.97	—	—	—	—	—	Ill-ventilated, surrounded by houses on both sides.
443	12.11.97	14.11.97	—	—	—	2	Ill-ventilated; inside, dirty and moist, rooms on ground floor in the rear, waste water drained through the ground of the house; outside, open and one-storeyed house.
448	26.11.97	3.12.97	5.12.97	12.12.97	13.12.97	5	House of a milkman, old rooms on ground floor moist, close to a garden. Cattle sheds.
449	31.10.97	31.12.97	—	—	—	2	Overcrowded. An old ricketty building on ground floor, moist and dirty.
454	30.10.97	—	—	—	—	—	Old house, no proper ventilation.
461	12.12.97	—	—	—	—	—	Old house, no plinth.
465	20.10.97	—	—	—	—	—	On the same level with road, moist floor.
466	7.12.97	—	—	—	—	—	No plinth.
472	9.11.97	—	—	—	—	—	A close house surrounded on all sides by houses, extremely moist floor, one-storeyed, without plinth.
473	4.11.97	25.11.97	—	—	—	2	A ricketty chawl, moist floor. Overcrowded.
477	30.11.97	—	—	—	—	—	New building, proper ventilation.
480	12.11.97	—	—	—	—	—	Old one-storeyed house, moist floor.
481	9.11.97	—	—	—	—	—	Good ventilated house, no plinth.
484	1.11.97	—	—	—	—	—	Very good building, ventilation proper, inmates say that there was no case in the house, occupied by a native doctor.

The floors are without any plinth, and moist during the monsoon and so on. Generally the houses had insanitary surroundings. In this very Pet 74 deaths could not be traced to any particular house, but my impression is that they must have been residents of the locality.

21,549. What have you to say with regard to corpse inspection?—Examination of corpses was strictly carried out. It ensured correct registration of deaths from plague, and enabled the authorities to trace out every affected locality where a plague death had occurred. In some cases the examination was delayed for several hours. With us, the dead body is to be removed from the house to the cremation ground as early as possible. But if a death took place at 10 p.m. in the night, the dead body had to be kept in the house till 10 a.m. next morning, to allow of its inspection by the Plague Medical Officer. This delay in the examination of corpses caused great annoyance. Corpse inspection is a necessary measure in times of a plague epidemic, but arrangements must be made for examination at all hours of the day and night. It is a necessary measure, but it should be carried out with as little inconvenience to the people as possible.

21,550. What do you think is the necessity for the measure?—If people conceal plague cases or do not report illness, that is a test by which we can ascertain even after death if there are signs of plague. I believe if every corpse is inspected there is a great guarantee of every plague case being detected.

21,551. Who do you think should have the duty of disinfecting and limewashing the houses?—I think that might be left to the owners themselves. If a rule were framed in Poona to that effect, we could very effectively carry it out by providing simply somebody to supervise that the work is done properly. If it were left to the people themselves, they would do it more carefully and willingly in their own houses.

21,552. Is there any other point you wish to lay before us?—Yes. I think that the plague measures should not be enforced equally in all cases. Captain Lockhart Mure, under whom I am at present working, made it a point to make certain exceptions in particular cases, modifying the rules. At the railway station he exempted high-class people from the disinfection of clothing and person, and even in the case of surveillance he allowed some, I believe, to be exempted altogether, and others were only asked to appear on the first and last days of the 10 days. They were, of course, educated people. I think that discretion should be used in enforcing the rules.

21,553. (*Mr. Howett.*) When the plague broke out at Poona, was there a byelaw of the Municipality requiring deaths to be reported?—Yes, there was.

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21,554. Was that byelaw allowed to fall into disuse?—Even after the enactment of the byelaw the procedure was that the Municipal Inspectors of the different wards and the sweepers made inquiries of deaths and reported, or the constable at the nearest police station, who got information, reported it.

21,555. Did the byelaw impose the obligation on the householder?—Yes.

21,556. Was it enforced against the householder?—I think not.

21,557. (*Mr. Cumine.*) Is there any penalty for not reporting cases of sickness, at present?—Of course, the notification issued provides that people who fail to report will be prosecuted, but as a matter of wisdom this rule is not enforced, because, in plague matters, the officers do not like to go to the extreme with regard to prosecution. These measures are against the ordinary habits of the people, and although the people have to submit to them, the officers do not go to great lengths in the matter.

21,558. Is there any advantage held out to people to report cases of sickness?—Yes.

21,559. What is the advantage?—From June 1898, it was a rule that, if illness was previously reported and death ensued, the people were not put to any inconvenience, they were not segregated, nor even kept under surveillance, but if death occurred, and the people had not previously reported the case, they were kept under surveillance for 10 days.

21,560. Was that carried out?—Yes, for a time, until we had the present epidemic.

21,561. (*The President.*) You say in your précis, "It "is said that the inmates of the affected localities "should not be allowed to run over the whole place. "There is much truth in this." What is the truth?—It is likely that some of them might carry the germs of contagion with them, and though a small percentage, still some of them might develop plague.

21,562. Then that may altogether destroy some other measures which you may adopt by their spreading the infection all through the town?—The percentage of such infections, as shown by the results of the segregation camp, is so small that I propose that instead of taking them to the segregation camp they might be kept under surveillance, and if you could watch them for 10 days you could pick out any case which developed plague.

21,563. But the information you get with regard to sickness appears to be very imperfect?—But we should have the people under surveillance, which means they would have to report themselves every day to the Ward Office for medical inspection by the Medical Officer, or, in the absence of the Medical Officer, by the Ward Officer. That would be a guarantee that they would appear for inspection every day. In case they did not, we could find out what the cause was, and if there were any cases of plague we could take them to the hospital.

21,564. In those houses of the worst character which you refer to, where the plague was most prevalent, was there most overcrowding?—In some of the houses, but not all.

21,565. Are they the localities where overcrowding is most prevalent?—Yes, they are. There is a chance of more cases occurring in those houses, and I have noticed one or two houses where more cases occurred owing to overcrowding.

21,566. With regard to corpse bearers, do you find that they have been infected by bearing corpses?—I am not personally aware of that.

21,567. You have heard of no cases?—No, not where they were affected. But even if they were, there is one fact which we must bear in mind, and that is that usually the relations and neighbours of the deceased come to carry the corpse. If there are relations amongst, them it is very probable that their death might be due to the original contagion they carried, and not necessarily from contact with the corpse.

21,568. You mean they might have become infected by coming in contact with the patient during life?—Yes.

21,569. You do not know of any instances in which direct infection appeared to be conveyed by carrying the corpse?—No. Mere contact is not sufficient to carry the contagion in my opinion.

21,570. (*Prof. Wright.*) Do you think any bodies are surreptitiously buried?—There was one case in the Sadashiv Pet where a dead body was reported to have been buried privately. It was found out and the man was prosecuted and punished.

21,571. Is it difficult to escape corpse inspection?—Yes; this was very early in the first outbreak.

21,572. Do you think it is difficult now?—I do not think any corpse could be surreptitiously buried now.

(Witness withdrew.)

Rai Bahadur B. C. Sathe.

RAI BAHADUR B.C. SATHE called and examined.

21,573. (*Mr. Hewett.*) You are a retired Deputy Collector living here?—Yes.

21,574. Were you employed in any plague duty in any of these epidemics?—Yes; I was employed as a volunteer in the ward in which I reside, that is the Sadashiv Pet.

21,575. During both epidemics?—In the first epidemic I served simply as a volunteer, but during the second epidemic I did a good deal under the Superintendent of the ward.

21,576. What were your duties during the second epidemic?—I was there to collect other volunteers to go round the Pet with the military to find out sick cases, to superintend disinfection of the houses, to report cases of sickness, also to report when corpses were found, and generally to do any work which was told to me by the Superintendent.

21,577. Are you at present employed on that duty?—Yes.

21,578. In reporting cases of sickness?—Yes, searching houses for sick persons, reporting them, and also, if necessary, looking after conservancy on the premises.

21,579. Had you any opportunity of observing how Poona got infected the second time?—During the second epidemic, the Kirki people came to Poona without any restriction and brought the infection.

21,580. Did you find the same places get infected in the second epidemic as in the first?—At first it was rather peculiar. The places which were secure from the first epidemic got the second epidemic at first, in that part of the town which was considered to be fairly clean

21,581. Did you notice that any houses which were well ventilated, and of good structural condition, had plague cases in them?—That is my humble opinion, especially with reference to a particular part of the town, namely, the south-western portion of the town somewhat near to the river.

21,582. Do you mean that in the south-western portion of the town some good houses were attacked?—Yes.

21,583. Is that the part of the town in which the best houses are?—I would not call them the best houses, but they are in the best locality, I should say. The houses are of an ordinary nature, but they are on a raised platform and have sufficient air and light, having back and front verandahs, and there the infection for a time raged very high.

21,584. Were they private houses or lodging houses?—In some of them the owners lived with lodgers also. We have very rarely different lodging houses and private houses; they are all jumbled up together.

21,585. Did the people show a dislike to leaving the town and going to camp?—They did not. As far as they could, they left *en masse*. At least, three-quarters of the Sadashiv Pet people ran away to different places. I have locked and sealed very many houses in Sadashiv Pet where they were kept vacant by the owners.

21,586. You did not find them averse from leaving their houses?—When they found it convenient to go away voluntarily they went away.

21,587. Do you mean that they did not like to go away if the plague authorities suggested it to them?—When people of their own free will could make proper accommodation for themselves they went outside, but when

they found their means not sufficient to do what was necessary outside they were reluctant to go.

21,588. Were the great majority able to make arrangements or not?—The great majority were able to do it.

21,589. Did you disinfect the houses in which plague cases did not occur as well as those in which plague cases did occur?—In Sadashiv Pet all the houses were disinfected.

21,590. Do you think that is necessary?—As it was carried out during the second epidemic the disinfection consisted of two portions. The first was disinfection with some mercurial liquid, and no one will object to that. But secondly there was disinfection by the spreading of quicklime on the floor, and most people objected to that very much. Although I cannot pronounce any scientific opinion about it, yet I say from experience that our people in their own houses, and all children, go about without any covering on the feet, and the spreading of lime over the floor keeps the feet burning for a number of days. The people very much objected to that, but they did not care about the liquid.

21,591. Do you think that it is unnecessary to disinfect a house adjoining a house in which plague has occurred?—I think the solution is all that is necessary. I think that the quicklime is not necessary unless the house is very bad.

21,592. I understand you to mean that houses where cases of plague have not actually occurred, but which adjoin such houses, should be disinfected, but that it is sufficient to disinfect them with perchloride of mercury?—Yes.

21,593. Did any instance come to your notice in which after a house had been thoroughly disinfected the people re-occupied it and cases of plague recurred in it?—I am sorry to say that in Sadashiv Pet such cases may have occurred, but I cannot remember that at this moment, because as soon as there was a case so many people left the place and did not turn up for a couple of months.

21,594. Do the great majority of the people here sleep on the floor or on beds?—Many of them sleep on the floor with hardly a mat.

21,595. Do you find the people are now ready to report all cases of sickness?—My own impression is that they have got thoroughly used to the system which has been prevailing for the last six months, and that they always do their utmost to report cases to the office.

21,596. How many cases of sickness have been reported in your own ward during the past week?—I think about 8 or 10.

21,597. How many corpses have been examined during the same week?—About 10 or 12 were inspected.

21,598. Then all the cases of sickness were not reported?—I will tell you how it was. Some of those sick cases were considered to be suspicious, or thought to be plague cases, and some of them were removed to the hospital, and when the fatality took place in the hospital we had nothing to do with it.

21,599. Do you only report severe cases of sickness?—No; all cases of sickness that come to our notice are reported and we leave it to the Medical Officers to decide whether it is or is not a plague case.

21,600. What is the population of your ward?—I believe about 5,000 or 6,000.

21,601. It is very healthy if there are only 8 or 10 cases of sickness in the course of a week, is it not?—I believe we have enjoyed a pretty good time during the last five or six months.

21,602. (*Prof. Wright.*) Were any of those 8 or 10 cases plague cases?—In the last week there were some plague cases.

21,603. Then you ought to add to the corpses which have been inspected also the corpses of those who died in the hospital?—We have nothing to do with the corpses of people who died in the hospital.

21,604. But if you added those to those which you examined in your ward you would have more dead bodies than cases of sickness, would not you?—Yes.

21,605. (*The President.*) Were there many cases of plague in the second epidemic?—There were very many cases in the second epidemic in the ward to which I belong.

21,606. How many?—I think about three or four hundred or even more.

21,607. Between what dates was the mortality very high?—From the 12th September 1897 to the 7th February 1898.

21,608. What ward was is it you refer to where the houses are in good condition?—I refer to my own ward, the Sadashiv Pet ward.

21,609. Are all the houses in good condition there?—I think most of them are in fair condition.

21,610. There are no bad houses?—Very few.

21,611. They are a better class of people?—Yes.

21,612. Did many slight cases of plague occur in any one individual house?—There have been some such houses where three, four, five, and six cases occurred.

21,613. One after the other?—Yes, and simultaneously sometimes.

21,614. In these cases where they occurred one after the other were the cases which first occurred removed? Were they removed one after the other from the house?—In some cases the first case was removed and the house disinfected. Then between that disinfection and the first attack another case cropped up which proved fatal. Then that case was removed. Sometimes when these cases were going on unfortunately these was a third case fatal at once, perhaps through fright. The people in most cases got it because they lived in a close atmosphere and in very small rooms.

21,615. Could you give us a statement of the houses in which plague occurred, showing the nature of each house, the total number of people living in each house and the number of rooms, and including the kind of floor and the means of ventilation?—Yes. I have prepared two statements of all the houses affected, giving the above particulars, and these may be abstracted as follows:—

——	Number of Houses.	Average Number of Persons in each House.	Average Number of Persons attacked.	Average Number of Deaths.	Nature of Ventilation.	Kind of Floor.
Class I. -	70	16	2	1·4	1 fair, remainder good.	Mud.
Class II. -	113	14	1·8	1·3	4 fair, remainder good.	Mud.
Class III. -	81	12	1·7	1·4	1 fair, remainder good.	Mud.

21,616. Have you anything else to say?—I would like to bring to the notice of the Commission one thing with regard to locomotion by railway. In the matter of locomotion there was a great cry all over the place that there was no uniformity of procedure adopted. For instance, things which were considered sufficient by the Poona authorities were not considered sufficient by the neighbouring district authorities or Native States where the Political Agent's will was supreme. In fact, the rules with reference to locomotion varied in each district, and consequently people who were obliged to travel from one place to another were placed in an awkward position, not knowing whether it was safe for them to travel or not. In many instances I have found people who wanted to go to a place and come back within two or three days, who did not know whether it was possible for them to do the journey within the specified time because at the place they were going to there were certain rules against which they could not guard themselves beforehand. What we all wished was that anything which might pass with one authority, might also part with another authority, unless in the meantime the circumstances of the traveller undergo a change.

21,617. You have found the differences very inconvenient to travellers?—Yes.

(Witness withdrew.)

Assist. Surg. E. S. Bharucha.

24 Feb. 1899.

Assistant-Surgeon E. S. Bharucha called and examined.

21,618. (*The President.*) You have had hospital experience, I think?—Yes, partly hospital, and I was also in charge of the Flying Column No. 4 at Kaledhon in the Satara district.

21,619. (*Prof. Wright.*) Where were these flying columns equipped?—There were about eight columns and they were organised in Bombay. My column was No. 4.

21,620. Was the column organised when you took charge of it?—No.

21,621. Then you had to do the organization yourself?—Yes.

21,622. Who were the men in your column? How was the gang recruited?—I was the head of the gang, and under me there were three Hospital Assistants and three medical students from the Byramji Jeejeebhoy Medical School, Poona.

21,623. How many men were there in the gang?—Besides the above we had six sweepers and a dozen coolies, and, in addition to that, more than 50 men locally obtained direct from the place.

21,624. Were you supplied with pumps?—Yes.

21,625. What sort of pumps?—At first I had a few brass pumps, but afterwards I had a large supply of wooden pumps sent by the Surgeon-General.

21,626. How did you get the disinfectants?—I got them direct from Bombay by making requisitions from time to time on the Surgeon-General.

21,627. What disinfectants did you use?—We used in the early beginning phenyle and carbolic acid, because I found it locally there.

21,628. You had nothing with you when you began?—No. I was sent out from Poona without any notice. What I found there was a few brass pumps, phenyle and carbolic acid. These were supplied to me by the Collector at Kaledhon.

21,629. Do you know where they got that?—They must have got them direct from Bombay. On my going there I represented my requirements to the Surgeon-General and within a fortnight I received a full stock of instruments and disinfectants from Bombay.

21,630. How did you make up the disinfectant used by your gang?—I used to make it up with four ounces of corrosive sublimate and eight ounces of common salt which I obtained from the bazar, and added 30 gallons of water. I had wooden casks supplied to me which held about 30 gallons of water.

21,631. What strength does that make it?—Approximately 1 in 1,000.

21,632. Did the whole of the perchloride of mercury dissolve?—Yes, very nearly.

21,633. Did you take it out with you for your disinfection operations in that solution?—I used to have packets made up and sent round to all the different members of the staff. Each packet contained four ounces of the corrosive sublimate and eight ounces of salt which was dissolved in water in those casks. These casks were supplied from Bombay and they held 30 gallons of water.

21,634. Was it hard or soft water?—It differed, but in most places it was hard water.

21,635. Before you went down to Kaledhon I think there was cholera reported?—It was reported in Kaledhon and the district round about.

21,636. Do you think this cholera has any connection with plague?—I do not think so. My own belief is that most of those cases of cholera which were reported were, properly speaking, cases of plague. Nobody had an opportunity of seeing them; there were no medical men on the spot. It was an out-of-the-way spot altogether.

21,637. Is there any symptom of plague which might have been mistaken for cholera?—Not so far as medical men are concerned, but I think ignorant people could mistake it; or possibly it was returned as cholera in order to prevent the difficulties which would arise if plague were reported.

21,638. You think that any epidemic disease would be reported as cholera by the villages because they know that best?—I believe all these villages were reported as having cholera before plague, but no village which was affected after my taking over the charge was shown to have had cholera before plague.

21,639. You say in your précis that the evacuation of infected villages is very effective when it is carried out early; have you any instance in which evacuation was very effective?—Yes, I have instances where evacuation was carried on by the people voluntarily. One such village was Valuj.

21,640. Were you on the spot?—Yes.

21,641. When did you first get notice?—On the 16th October 1897, and I visited the place on the 17th or 18th October.

21,642. How many plague cases had there been?—Ten cases of plague had taken place before I went there. I believe plague was probably there two or three days earlier than that, but they were all discovered on the 16th. When I arrived there I was surprised to see the people had of their own accord gone out to live in the fields—almost the whole village.

21,643. How many inhabitants has this village?—Approximately about 800 people. I found it entirely empty with the exception of two or three village officers in charge.

21,644. Did the people go back into the village?—Not for a month-and-a-half.

21,645. Did not they go back from day to day to fetch things?—They were almost all agricultural people, and had very few wants. They took everything from their houses and lived in camp.

21,646. How many plague cases occurred after evacuation?—On the 16th and 17th there were 10 cases, and then there were no cases on the 18th, 19th, 20th, 21st, 22nd, and 23rd, not even in the fields.

21,647. How did you know there were no cases? Did you go round to the huts?—Not personally, but my assistants went round the huts. There was a regular searching carried out. On the 24th two cases were reported. These were in the same family which had been affected before. Then on the 25th another case was reported, on the 26th another; on the 27th there was no case, on the 28th there were three cases, and then practically the disease disappeared.

21,648. Did you stay on the spot until the 28th?—I was not in the same village, but I visited it every second or third day. I had a number of other villages to look after.

21,649. After the 28th you say there were no other cases?—There were cases, but I know how those cases occurred. A man had been living in the village who was one of the village officers and a Brahman. He was not an agriculturist, and did not know where to go, and he lived in the outskirts of the village. I believe he got his infection from the village. The second case which occurred after that was the man who attended on him.

21,650. And then did the plague epidemic stop?—Yes, there was no more of it. There were altogether 19 cases there. That is an example of a village where voluntary evacuation was carried out with good results.

21,651. Do you often find that buboes are small at the beginning of an epidemic?—Yes, even smaller than a betel nut.

21,652. Were these cases very severe?—Yes. Comparatively speaking they were more severe than those where the buboes were more marked. I have often observed that. They are almost invariably fatal.

21,653. Did you see any mild cases of plague, so-called *pestis ambulans*?—I have said in my précis that in one village an outbreak of mumps preceded an attack of plague, and this was also noticed by me in Poona. I was in charge of the observation wards of the Sassoon General Hospital there. From a particular house from 20 to 25 famine children were brought in—it was famine time then—all suffering from mumps. One of them afterwards developed characteristic symptoms of plague. She had mumps, and afterwards developed a bubo in the groin, and all the other characteristic signs of plague.

21,654. You have seen this also in the plague-infected villages, I understand?—In one village, Mahuli.

21,655. Was that at the beginning of the epidemic?—Yes; in fact, I diagnosed these cases as mumps at

the time, but I now believe they were cases of *pestis ambulans*.

21,656. Did these symptoms only occur in children?—In Poona only in children, but in my village they were in men, women, and children.

21,657. Then I believe you have seen an association of small-pox with plague?—Yes. This has come to my notice in three villages. In one village very near my headquarter station, named Chitale, about 14 cases of small-pox had taken place before plague occurred there. The early cases of small-pox were not seen by me, but by the vaccinator of the district, and he proposed vaccinating the remaining part of the population, but the people refused. Then in the same family where the last two or three cases occurred, I was told there were some cases of fever, but there were no signs of small-pox. I went to see them. I could not come to any conclusion, because these cases of fever which occurred were in the same family where the cases of small-pox occurred, and I thought they might be cases of small-pox without any eruption, although I had never seen such cases. I deferred the diagnosis until the next day, when one of the two suspicious cases died and the other developed a bubo. From that time onwards regular cases of plague were seen.

21,658. You say that sometimes the incubation period has extended to 14 days?—Yes, I have noticed it in a case in Poona. I believe this case was probably reported to you by Col. McConaghy in Karachi. It was a Parsee child who arrived in Poona on the 3rd December 1896. She came from the district of the Fort in Bombay where plague was then raging very fiercely. Thirteen days after that the child was doing well. On the 14th day I was called in to see the child about two o'clock in the afternoon, and she had a temperature of 104, which in the evening went up to 106.

21,659. When did the child get this temperature?—On the 17th December, the 14th day after arrival from Bombay.

21,660. Do you think the child may have had clothes on her which came from the infected district?—I do not know.

21,661. (*Dr Ruffer.*) Was there any plague in Poona at that time?—In the cantonment where the child was living there was no plague then. *Assist.-Surg. E. S. Bharucha.* 23 Feb. 1899.

21,662. Was there any plague in Kirki?—Long after that.

21,663. In the town?—There were suspicious cases beginning to come, but there was no reason to believe that this child had access to the city.

21,664. (*The President.*) With regard to those cases of mild plague which you mistook for mumps, was the swelling on one side or on two sides?—Sometimes on one and sometimes on both, in the parotid region.

21,665. Was it infective?—I considered them as infective, because there were so many other children brought from the same place. They all recovered, with the exception of the one who got the bubo in the groin. That case was transferred to the Plague Hospital and died there.

21,666. You think the duration of an epidemic may be greatly modified by the measures which are adopted?—Yes.

21,667. Especially by what measure?—I believe two measures are of the greatest importance, viz., early and complete evacuation if you can get early notice, and a careful searching of the fields and the removal of any plague case occurring in the fields to hospitals, and separating those people from the healthy. In the village of Lengre where this searching was not done very well, plague continued some time, so I had the fields searched under my own supervision, and succeeded in bringing out 10 cases in one day, whereas formerly only one or two cases where reported. For only two or three days after this cases came to be reported, and subsequent to this no more cases occurred, and the place up to date remains uninfected.

21,668. Can you give us an example in which the duration lasted for only a fortnight?—It lasted 16 days in one village named Chitale, and 21 days in the village called Valuj.

21,669. Were the measures there completely carried out?—Yes. In the village of Valuj there was voluntary evacuation.

(Witness withdrew.)

(Adjourned till to-morrow.)

At The Council Hall, Poona.

FIFTY-FIFTH DAY.

Friday, 24th February, 1899.

PRESENT:

PROF. T. R. FRASER, M.D., LL.D., F.R.S. (*President*).

Mr. J. P. HEWETT.
Prof. A. E. WRIGHT, M.D.
Mr. A. CUMINE.
Dr. M. A. RUFFER.

MR. C. J. HALLIFAX (*Secretary*).

Major W. L. READE, R.A.M.C., called and examined. *Major W. L. Reade, R.A.M.C.* 24 Feb. 1899.

21,670. (*The President.*) You are on Special Plague Duty here?—Yes.

21,671. As the Chief Plague Authority in Poona city?—Yes.

21,672. When did you assume this office, towards the end or at what time, relatively, to the first epidemic?—I came out originally in February 1897.

21,673. So that you can speak of the first epidemic?—I was here for several days at intervals during the first epidemic.

21,674. When did the first epidemic commence here?—It commenced here in December 1896.

21,675. Was it discovered how it originated?—I think there was very little doubt it was communicated from Bombay by train. I do not think that has ever been traced accurately, but I think there is little doubt it was by railway intercourse.

21,676. Would you give us an account of the first epidemic in Poona and the amount of the plague which occurred?—It was about the middle of February that measures were first taken for search parties and compulsory isolation of the sick in hospital.

Major W. L. Reade, R.A.M.C

24 Feb. 1899.

21,677. You are aware of the account* which has been written of the epidemic?—Yes. Dr. Lowson and myself were in an advisory capacity to the Committee. We used to advise the then Chairman of the Committee with regard to measures we thought necessary.

21,678. You adopt this account of Mr. Rand's as being an accurate account of the epidemic?—Yes, I have read it, and I take it as an accurate account.

21,679. Do you know when the first epidemic terminated?—It was over at the end of May 1897. There were a few dropping cases in May. Then during June and July occasional cases occurred. The total mortality, however, was heavy; it was accounted for by cholera, there was a cholera epidemic at the time.

21,680. When was Poona entirely free from plague after this epidemic?—There was an interval during June and July. With the exception of, I think, one case in July, it was practically free in June and July 1897.

21,681. When did plague again appear in Poona?—There were imported cases from Kirki in August 1897.

21,682. Did that constitute the commencement of a second epidemic?—Of course it is difficult to say whether it might have been what is called an indigenous outbreak. I have no evidence of that, but there was direct evidence that Kirki was infected, and we have direct returns of imported cases from Kirki.

21,683. Could you describe the course of this second epidemic, starting in August 1897?—There were four imported cases on the 30th July from Kirki. That was practically the start of it. Then there was one on the 1st of August, one on the 7th, and one on the 9th of August, and so on. Then it went up to an average of about one a day. Then the indigenous cases commenced, I mean the indigenous cases were above the imported cases, but during all that month of August we had continuous records of imported cases from Kirki.

21,684. What was the course subsequently?—I was not here during this time from August to September, I came here in October. On the 11th September there were 14 cases, then it went continuously upwards, and on the 20th November there were 400 cases a week. It reached its acme on the 11th December, and then from that day there was a continuous fall down to the end of February 1898.

21,685. Could you give us the total number of cases in this epidemic?—The total mortality from plague was 3,633, and the cases were 4,644.

21,686. What has been the history of plague in Poona since February 1898, when the epidemic you have just spoken of terminated?—Since February 1898 to February 1899 we have only had imported cases. The last week we have had a little outbreak in Sadashiv Pet from an imported case from Bombay, in which we have had the so-called indigenous cases, but which have resulted from the imported ones.

21,687. You remained practically free until this month?—Yes, until this month. We have had 30 cases in the last nine months, since the epidemic of 1898.

21,688. These were all imported cases?—Yes, all of them.

21,689. Are you in a position to state briefly what were the measures employed during the first epidemic?—Rigorous isolation of all the sick in hospital was carried out, compulsory detention of the contacts in segregation camps, and disinfection of houses and clothes, and there was an organisation of search parties by British soldiers and sepoys accompanied by native workers.

21,690. And the segregation was a partial one, as far as I understand?—It was compulsory detention in a segregation camp in the 1897 epidemic. Evacuation was not carried out in this epidemic. There were no health camps. From February 1898 to the present date observation of contacts has been carried out.

21,691. Is the rapid decline in the first epidemic, from December 1896 to the end of March or April 1897, coincident with any plague measures that were adopted?—Undoubtedly.

21,692. Which plague measures?—The measures which were brought into force, namely, the isolation of sick into hospital, and the segregation of the contacts.

21,693. There was a decline, and I understand you to say that took place after these measures had been adopted in the first epidemic?—There was, I think I might say, a continuous fall in the first epidemic.

21,694. What measures were adopted in the second epidemic?—In the second epidemic we relied more on the help of the native people, and we gradually lessened the strictness of the measures, and after a time we modified them more. We have done away with the compulsory detention of the contacts in a segregation camp, and we have lessened our measures quite recently with regard to hospital treatment, but I think the chief measure was the lessening of the strictness of our segregation system. We created a system of open segregation by which people were allowed out during the day, and they had to report themselves at night. We had a roll-call every morning.

21,695. Previously, they were kept in the camp?—Yes, in a guarded camp.

21,696. In the second epidemic how long were they segregated?—Ten days.

21,697. What was the treatment of the evacuated houses?—Thorough disinfection with perchloride of mercury.

21,698. To what extent was evacuation carried out? What was included in the evacuation in the second epidemic?—In the second epidemic we rarely disinfected more than one house unless we had some evidence to show that the adjoining houses had been contaminated. We judged each case on its merits, and if there was likely to have been freedom of intercourse between the adjoining houses, the Plague Officer had power to disinfect this house also; but it was not a rule.

21,699. Were those measures adopted at any particular stage in the second epidemic, or throughout the epidemic?—We depended more on gaining the confidence of the people, and we elaborated the volunteer system more, and formed Chief Volunteers. Colonel Creagh and myself were appointed to the then existing Committee, and I think the principal change was the appointment of Chief Volunteers, who went round in a spirit of conciliation; and more power was given to them, and now they are appointed the Chief Executive Officers of the Pet.

21,700. Were the measures adopted in their full development at the commencement of the second epidemic, or were they elaborated during the course of that epidemic?—We changed the measures to this extent, with regard to the searching more particularly; before, the searching was done by one centralised party, and then the other system came into force—what one might call the decentralised system—by which the Superintendent was appointed the plague official for each Pet, and he was the responsible officer for that Pet.

21,701. What was the effect of all this modification?—These measures came into force about a week before the fall, the 27th November is put down as the date of the revised measures, and from the 11th of December there was a continuous fall.

21,702. What essential difference had existed in the treatment of your plague measures anterior to the fall, as contrasted with the measures which took place at the time of it or subsequent to the fall?—I think the most important one was undoubtedly one of organisation, a decentralisation by which each Plague Officer was made responsible, and he had his own equipment complete. He was the Plague Officer of the division to whom all the inhabitants of that particular Pet could apply. There was a plague office established in each of the plague divisions.

21,703. Putting aside the fall in the mortality, what do you think was the valuable result of this change of organisation that you speak of?—I think it gave more confidence to the people.

21,704. In what way?—That they had someone to apply to.

21,705. Did it result in your getting better information as to the cases?—Undoubtedly.

21,706. You got better and more reliable information?—Certainly.

21,707. Your information before was not so good, and, therefore, you could not carry out segregation so thoroughly?—I think one would argue that was so.

* "Report on the Poona Plague Operations," by the late Mr. W. C. Rand, I.C.S., Chairman, Poona Plague Committee, printed on pp. 301 to 357, Vol. II., of "The Plague in India, 1896-97," by R. Nathan, I.C.S.

21,708. The more perfect your means of obtaining early information, the more perfect were your measures?—Undoubtedly.

21,709. The plague continued to increase until this improvement of measures had been attained, I believe?—Yes.

21,710. And then it fell rapidly?—Yes.

21,711. What are your views with regard to evacuation?—I think, from the experience I have gained of evacuation, that it is a most valuable measure. On the other hand, I am not at all sure whether a lesser alternative might not be as efficient. If we get early information of cases I am inclined to think, from our experience here, that one can limit the zone of infection; that being so, evacuation would be unnecessary.

21,712. With that condition, that you have a limited zone of which you have information?—When I said a limited zone, I meant more particularly that each particular case could be limited to the house in which it occurred. In our experience here up to now, January 1899, we have been able to limit the spread in this way, and even if several cases arise, these can in a large town be prevented from spreading by prompt action. Of course I am speaking now of one's experience of limiting plague in a big city, which experience is naturally extremely small.

21,713. Your experience has hitherto been of cases where you have had early information of isolated cases, not more than one or two in number; in that circumstance what action do you consider sufficient?—We have been able to limit the plague to the particular spot in which we get information of it, by the patient going to hospital, or if a corpse was found, the corpse being removed, and the people in the house being placed under surveillance, disinfection of the house, and disinfection of the clothes of the people who inhabited the house being carried out.

21,714. If the area affected be a larger area, you may require to modify those measures, may not you?—Just now one might say that this system is on its trial. This particular outbreak we have now is more or less confined to a block of buildings about 100 yards square, and I was inclined to think that it was due to some infective agency that we had not before in Poona, rats or something like that; but I still think that our operations will be effective to treat this block in the same way as we treated the cases before, and without evacuation.

21,715. Not paying any attention to the immediately contiguous houses?—No; unless we have evidence that there has been some direct contact. A house leading into another house, perhaps without any wall, and where neighbours are known to have had intercourse with each other, would be disinfected.

21,716. You would treat contacts in the same way as actual patients?—We would treat that house in the same way as we treated the other house.

21,717. Why do you think removal from the house to a health camp, or isolated place, stops the extension of the plague?—You get away from the infected floor, and you disinfect the infected clothes. I think both the floor, and particularly the clothes, are very infective—as far as I know they are the only factors, with the exception of pneumonic sputa, that have proved to be infective.

21,718. Is there any other factor which may explain the good results of removing people into a health camp?—They are placed under better hygienic conditions.

21,719. What evidence is there, to your knowledge, that that is an operative condition?—It is a presumption that if you take people from ill-ventilated houses, and place them in good airy houses, that the hygienic conditions are better, and one can say that under better hygienic conditions the general health will improve.

21,720. As a general principle?—Yes.

21,721. Have you any experience with regard to segregation camps which would lend weight to your supposition? In these segregation camps was the general hygienic condition the same as in the localities from which the inmates had been removed?—No, much better.

21,722. You have stated that you consider that infection is due to the infection from floors and clothes, and from pneumonic sputa. What influence do you consider rats or other animals to have upon the spread of plague?—I have had no direct experience of the spread of plague by rats, although from what one reads one is inclined to think there must be something in it.

21,723. Do you know if any large number of rats did die here during any of the epidemics you have been concerned with?—No, we have never had a large mortality in rats.

21,724. There was no warning given in any house where plague occurred by the death of rats, of the possibility of plague occurring in the house?—No; we have received reports of rats having died, but I do not know that I can remember having seen a dead rat in Poona. I certainly have not seen any in this last outbreak.

21,725. Why do you think that the condition of floors is a great means of propagation of plague? On what data do you make that assertion?—Because if you move people from those floors they do not get the plague.

21,726. Have you ever removed them without also disinfecting?—Their clothes were disinfected on arrival in camp. I have said in my précis of evidence: "In "the infection caused by floors I also include infected "clothing."

21,727. How do you think floors would retain the plague virus and thus become a special means of propagating plague?—I think that any floor that contains material for the propagation of germs will retain them longer, and be a more favourable bed for their spread.

21,728. What kind of floors were they?—Mud floors.

21,729. Such as would be favourable for the retention of organic matter?—Yes.

21,730. Would that extend to stone, or wooden, or concrete floors?—That I have very little experience of. There are practically no floors of that sort in Poona.

21,731. You wish to speak of mud floors, which contain greater organic matter?—Yes.

21,732. I think that, generally speaking, you are of opinion that pneumonic cases can be detected by medical examination?—Yes.

21,733. Will you tell us how you detect such cases?—One arrives at it by a process of exclusion mostly. There is a history of cough, and there is sometimes a very frothy condition round the mouth of the corpse—a congestion round the mouth and nostrils.

21,734. Supposing you found a person who was ill, how would you form the diagnosis of pneumonic plague? On what grounds would you arrive at a conclusion, before death?—One would naturally go by a bacteriological test, had one facilities for doing so.

21,735. I ask you, because you have made the statement that a pneumonic case would be detected by medical examination before it develops into the contagious stage?—That is, during life in a health camp.

21,736. Assume it is in a health camp?—Because these people in the health camp who are under medical observation would, on the first sign of fever, be put into an observation ward before they develop into the pneumonic stage of plague.

21,737. You mean, then, not so much the discovery of the pneumonic plague as the prevention of harm from any possible pneumonic cases?—Quite so.

21,738. You had no certain means of detecting pneumonic plague, had you?—No. I mean they would be detected before they developed into the pneumonic condition.

21,739. Have you any experience with regard to inoculation as a plague measure?—No. I have been in villages in the district with Professor Haffkine to assist him, but I have had no experience.

21,740. Have you formed any opinion as to its value as a plague measure?—I think it may be of value in segregated communities, such as people in jail, and in regiments. Universal inoculation seems to me to be a difficult thing to carry out.

21,741. Assuming that it is really preventive, you mean that it is limited in its applicability?—Yes.

21,742. Have you had any experience of the reinfection of any houses which have been disinfected?—No, none at all.

21,743. Have you had any experience of cases which suggest recrudescence of the disease, that is to say, apart from a fresh importation?—No.

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21,744. You do not believe in recrudescence in the soil?—I must say I do not now. I was quite prepared to believe in it, but with the experience that one has now had, I do not think that belief was justified. I can only argue from an extremely limited amount of experience from a plague point of view, but, judging more particularly from Poona, a city which has been badly infected for two years, if there is such a thing as recrudescence in the soil, I think one would have had an outbreak before now.

21,745. In the meantime, experience shows you that plague does not occur without any direct infection from either clothing or persons?—Undoubtedly, it does not occur without direct infection.

21,746. With regard to disinfection could you describe what disinfectants you have employed here?—We have employed always the perchloride of mercury solution, 1 in 1,000, which we have recently strengthened to 1 in 725, with hydrochloric acid.

21,747. An acid solution of perchloride of mercury?—An acid solution, yes.

21,748. How do you apply it?—It is syringed on to the floor and walls of an infected house through the medium of wooden syringes. This is very freely done.

21,749. Is the syringing done on one or more occasions in any given house, or do you repeat it before the house is re-occupied?—Only once.

21,750. Do you think it necessary to include in disinfection the ceiling and every part of a chamber, or do you limit yourself to the floor and to a part of the wall?—As a matter of fact, when we are about it, we do the whole room, but I should be inclined to say that the floor and a very few feet of the walls was sufficient. At the same time, I think, from our present knowledge of plague that it would be a rather dangerous experiment to limit it to this. When one is about it, it is very easy to squirt everywhere.

21,751. Did you disinfect the articles in the chamber with the same chemical disinfectant?—That was always done by a steam-boiler—when the people were brought to the segregation camp. This was done away with in the segregation camp, and latterly we have been employing the railway disinfector—sending the people's clothing there.

21,752. Have you any distinct case in which clothing itself, apart from a living being, has communicated plague to a person?—The best example in my experience was the Karachi health camps in 1897. They had enormous health camps there. I think at one time there were something like 20,000 people in camp, and plague at that time in those camps was as bad as it was in some of the worst infected parts of a big town; in fact in one of the Karachi reports it was stated that there was hardly a hut that had not a case in it. After the disinfection of clothing was carried out, they came under the ordinary conditions of a health camp, that is to say cases did not develop in the camp after the incubation stage had elapsed.

21,753. The conditions in all other respects were the same?—Exactly the same.

21,754. How do you think the virus gets admission into the body?—I think it must be by inoculation.

21,755. That is so far as the skin surface is concerned?—I think, possibly, the tonsils may get inoculated also.

21,756. You think it may be introduced through a mucous surface as well as the skin surface?—Yes.

21,757. How do you think the virus penetrates to the skin surface; how does it reach the blood ultimately?—I could not give you an opinion on that. I have not been able to trace any local seat of inflammation, in the majority of cases.

21,758. Any lesion, or what?—Cracks or cuts in the feet of the natives. They are generally cracked, but I have rarely been able to trace any lesion or point of inoculation.

21,759. Have you any opinion as to whether the virus can pass through the unbroken skin or not?—Only purely negative ones—that one looks for these abrasions and one cannot find them.

21,760. You also think that it is absorbed by the tonsils?—I think it is physiologically probable.

21,761. Have you any case to justify that statement?—The infection of the cervical glands would possibly show infection through the mucous membrane of the tonsil.

21,762. The jaw glands, the parotid, and so on?—Yes.

21,763. Have you seen cases of that sort?—Yes, a great many.

21,764. Have you any evidence to show that it may be absorbed through the alimentary canal?—No

21,765. You have seen no such cases?—I may say I have not done much hospital work; my experience has been chiefly administrative.

21,766. Would you give us some account of what you consider a sufficient organisation for stamping out plague in a town?—We have in Poona a system of 12 or 13 Chief Volunteers, and we have had a surveillance system, and we have been able to treat each case of plague as it arises. Now that we have infection in a large block, we have had to increase our disinfecting parties. We had 10 soldiers employed as disinfectors, and six coolies from the Municipal authorities, for each party, and we formed 10 disinfecting parties, each party under a soldier. In that way one can work with a skeleton organisation, which can be readily increased if the disease was progressing.

21,767. What do you trust to in order to prevent the introduction of plague into a town, and to discover it when it has been introduced?—The first line of defence is by the rail—inspection at the railway station. An inspection of all passengers who arrive at Poona takes place there. Of course there are some who get out at some of the wayside stations and come in by road.

21,768. What is done with them if they are detected at the railway station?—If there is a rise of temperature they are sent to hospital.

21,769. For how long are they detained?—Until definite plague symptoms have developed. At the railway station the people who arrive in the town are placed under surveillance, which is done in this way, a certain number of volunteers, native gentlemen, vouch for people they know, respectable inhabitants of the town. A certain number of the arrivals have what we call "ward passes," which are passes given by the Superintendents of the divisions stating that the person is known, and resides in a certain house, and is going away for three or four days; and when he comes back again he produces this pass. He is then allowed out. There are a certain number of people who can give no fixed address, people coming to the city to work, and so on, and they deposit their baggage and bundles in a camp close by the station; these bundles are entered in a register, and then the people are allowed to go into the town. They are given the address of the Chief Volunteer of the division in which they are going to live, and as soon as they have intimated to him the house they wish to live in, they get a release ticket, and this is presented to the officer in charge of the railway camp, they are then released and placed under ten days' surveillance in the ward office of the Pet they are going to live in.

21,770. They have to report themselves?—Yes, every alternate day.

21,771. That is in order to get as early information of plague as possible, I suppose?—Yes.

21,772. Have you any other means of getting early information of plague?—Then we have sick certification, people have to report, or are supposed to report, all illnesses, and reports are recorded on a list.

21,773. When they report any sickness, what step is taken?—The medical officer on duty proceeds to the house to diagnose the case.

21,774. Are there any other steps that you take for getting early information regarding plague?—Finally, we have the system of corpse inspection. We have the volunteers, in conjunction with the census clerks, who keep up the census of the houses.

21,775. Is that census checked now?—Yes.

21,776. At what intervals?—Once a week.

21,777. What do you mean by corpse inspection; what does it cover?—It is really a death registration.

21,778. Every dead body is examined?—No dead body can be moved from a house until a certificate has been given by an inspecting medical officer.

21,779. All these measures are intended to give you early information?—Yes.

21,780. You consider that the most important information of all that you can obtain, I suppose?—Certainly.

21,781. And those measures which you have adopted, you have already told us, have been so far successful in preventing indigenous plague?—Yes.

21,782. (*Mr. Hewett.*) What population had you to deal with at Poona during the second epidemic?—From 60,000 to 80,000. I remember going into this question with Mr. Plunkett. He put it at 80,000. We thought it was less, but he, probably, had better information. That was his estimate at the time.

21,783. What is the population at the present moment?—It was taken about a month ago, and it was 130,000.

21,784. That is a good deal above the normal?—Yes, it is 12,000 above the normal.

21,785. Has the mortality been above the normal during the last three months?—Yes.

21,786. Higher than you would expect from the increase of population?—Yes.

21,787. How do you account for that?—We have an influenzal epidemic and also measles, and whooping cough, which cause a large mortality, particularly amongst infant life, and there is no doubt that the influenzal influence is causing mortality among aged people.

21,788. Dr. Venis put before us yesterday a statement of the system of corpse inspection up to the 31st December last. (*See* Question No. 21,416.) Have you got it for a later date?—Yes. (The following statements for the whole of January 1899 were supplied by the Witness subsequently:—

Corpse Inspection, January 1899.

1st	5		
2nd	8		
3rd	18		
4th	11	Total for 1st week	65
5th	6		
6th	8		
7th	9		
8th	9		
9th	11		
10th	19		
11th	11	Total for 2nd week	99
12th	16		
13th	18		
14th	15		
15th	14		
16th	9		
17th	9		
18th	13	Total for 3rd week	85
19th	16		
20th	14		
21st	10		
22nd	13		
23rd	10		
24th	22		
25th	15	Total for 4th week	92
26th	10		
27th	8		
28th	14		
29th	9		
30th	17	Total for last three days	44
31st	18		
Total	385		385

Mortality according to Age.

Under 1 Year.	From 1 to 5 Years.	From 5 to 15 Years.	From 15 to 30 Years.	From 30 to 60 Years.	Above 60 Years.
118	90	12	48	53	64

N.B.—The mortality in children under five years of age is striking, viz., 208, and may in all probability be due to the extremes of temperature that prevailed during this month. In January 1896, the year taken for comparison, the total under this head was 59.

Causes of Death.

I.—Puerperal diseases	10
II.—Senile decay	13
III.—Stillborn children	24
IV.—Inanition and marasmus	39
V.—Whooping cough	11
VI.—Measles	8
VII.—Tuberculosis (including phthisis)	28
VIII.—Convulsions	12
IX.—Diarrhœa (including enteritis)	46
X.—Dysentery	5
XI.—Accidents (including burns, drowning, &c.)	7
XII.—Hydrophobia	2
XIII.—Syphilis	3
XIV.—Leprosy	1
XV.—Disease of heart	6
XVI.—Malaria and other fevers	12
XVII.—Brain diseases	3
XVIII.—Ill-defined causes (including jaundice, abscess, &c.)	14
XIX.—Malignant disease (cancer)	2
XX.—Broncho-pneumonia	40
XXI.—Bronchitis	78
XXII.—Lobar pneumonia	15
XXIII.—Diphtheria	1
XXIV.—Kidney disease	5
Grand Total	385

Mortality for the Month of January.

1896	259
1897	250
1898	497
1899	385

N.B.—In the years previous to 1899, the still births are not included.)

21,789. Can you tell us how many deaths there were during January?—385.

21,790. How many of those were put down to influenza?—There are 40 bronchial pneumonia and 78 bronchitis; and there are 15 lobar pneumonia—all influences of the influenzal prevalence.

21,791. Were there any cases in which the cause of death was not determined during the month of January?—They are all under certain headings.

21,792. There is a heading "Unknown" in the list by Dr. Venis. Was there any similar heading in January?—They are "Ill-defined causes, 14."

21,793. That is a new heading?—Yes.

21,794. You have not got this item "Unknown" in January?—No; they are "Ill-defined causes."

21,795. Was there plague in the villages in the neighbourhood of Poona at this time?—Yes, in January.

21,796. I understood you to say that you did not believe in a local recrudescence?—I have had no experience of it, and I am inclined not to believe in it.

21,797. You made a reference in your précis of evidence to the second outbreak at Karachi having been possibly due to local recrudescence. Do you withdraw that now?—I must say from my experience now, that, if we were going to have a recrudescence here, we would have had it by this time.

21,798. Do you find that the cases which have occurred in the last few days in Poona have occurred either in the houses which were previously infected, or in the neighbourhood of houses which were infected in the previous epidemic?—This I have not ascertained.

21,799. Do you consider that the system which you have now of surveillance of passengers by rail and road is the maximum of interference which can be legitimately taken with the movements of the people? Is it effective in preventing people whom you wish to control from getting into the town without observation?—Up to now I can say it has been efficient.

21,800. You would not advocate any further restriction with the movements of the people?—No, I should not.

21,801. You have not had a general evacuation of Poona?—No, only in isolated blocks.

21,802. Do you think that it would be feasible as a compulsory measure to have a general evacuation of a town of the size of Poona?—Anything, of course, is possible.

21,803. I mean with an efficient control over the inhabitants of the place?—It is entirely a matter

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of money. A town can be moved if the money is forthcoming.

21,804. Would you not require an enormous force to control the evacuated inhabitants?—I should think it would be practically impossible.

21,805. If you attempt to completely evacuate a large town, would the probable result be that you would scatter plague cases all over the country?—I think it is very likely.

21,806. When you have plague in a village, do you think there is the same objection to evacuate the whole of it as there is in the case of a large town?—No, I do not.

21,807. Are the villagers accustomed to camp out in their fields?—Yes.

21,808. Do you think that the villagers of an agricultural village are likely to go moving about the country because they were camped out owing to plague?—I do not think so. I think the agriculturists will still stay by their villages.

21,809. The majority of villagers who are agriculturists are wrapped up in their village site, and having no business elsewhere, would not be likely to go away?—Yes; but that is not applicable to all villages.

21,810. You have villages in which the population are more like the population in a town in this respect, —that they are more accustomed to moving about over a larger area?—Yes.

21,811. Do you think that voluntary evacuation in such villages as those is likely to lead to plague spreading about from place to place?—I think so, undoubtedly.

21,812. Would you ever allow home segregation—in Poona, for instance?—It has never been allowed.

21,813. Would you allow it?—I should be sorry to say what I would not be prepared to allow in plague. One gets new ideas every week in plague.

21,814. Do you think that it would be impossible to allow a patient to remain in his own house, provided that is suitable, and that he can be isolated, and properly attended in it?—This is supposition. I think it is possible that it might be done. I am only presuming that certain conditions were followed.

21,815. Would you please specify the conditions?—The strictest disinfection of floors. You would have to evacuate the house while the disinfection was going on. In double-storeyed houses, I should be very sorry to say one way or the other.

21,816. You have formed no definite opinion against house segregation under such conditions?—I have not.

21,817. What arrangements do you now make as regards segregation of the other inhabitants of a house when you take a patient to hospital?—They are sent to the segregation camp; it is really a surveillance camp; it is not close segregation.

21,818. How long are they kept there?—10 days.

21,819. During those 10 days are they permitted to re-visit the house in which a case of plague has occurred?—The house is immediately disinfected and thrown open. As soon as it is dry it can be re-occupied.

21,820. They may be there during the day, but not during the night?—That is so. They sleep in the camp for the first four days.

21,821. Do you disinfect all the houses in the neighbourhood of a house in which plague now occurs?—No.

21,822. You do not consider that to be necessary?—I may say, with regard to this recent outbreak, that I was under the impression that it might be due to some rat agency. I do not think so now, in fact, I am sure that it is not. I thought that there might have been some universal floor-infection by rats. We commenced a modified sort of evacuation; but now each house is being treated in the ordinary way. In the case of a death from plague the house is evacuated and disinfected.

21,823. Does the system now in force in Poona, as regards the treatment of the patient, the treatment of the other inmates of the house, and the disinfection of the house, represent your mature experience as to the best way of dealing with a large outbreak in a town?—Yes.

21,824. Do you find any objection on the part of the people here to corpse inspection?—No, none.

21,825. If you have any specific instance, perhaps you will tell us?—The only specific instance brought to my notice was with regard to the inspection of the corpse of a fakir. It was brought to our notice by one of the Chief Volunteers of a Pet who said that the man was of holy habits and an ascetic, and that the people did not wish his corpse to be handled by an European doctor. We assented, and said that they might have a Brahman doctor, the European doctor being present. There was no difficulty about it. There was also one objection from a Muhammadan, but that was easily got over. The corpse was allowed to be inspected.

21,826. Have you any system of pardah among the Hindus here?—No, none to my knowledge—not in Poona city.

21,827. Is there a Muhammadan population among whom the pardah system prevails largely?—I should say not.

21,828. Is it an influential one compared with the Hindu population, and compared with the Muhammadan population of other places?—The Muhammadan population is about 15,000.

21,829. That is, relatively, very small, is it not?—Relatively small to the other inhabitants.

21,830. Is the Muhammadan population, as compared with the Muhammadans in other places, a highly important population?—There is a large community of Borahs, numbering about 1,000. I should not say that there is the same number of educated people amongst the Muhammadan population here as there are amongst the Hindus, in fact, I am sure there is not.

21,831. Would you consider that the Muhammadans have the same influence and importance in the city of Poona as they have in some of the more important cities of the Punjab and North-Western Provinces?—I have no data upon which I can give an opinion.

21,832. A statement appears in the first few lines of your précis with regard to the number of cases of plague which occurred in the health camp during the second epidemic?—Some 4,800 persons were sent to the health camp at Bhamburda from infected parts of the city. I say in my précis that out of this number only three cases of plague occurred. I ought to have said that out of this number only one case of plague occurred. I have that from the records of the General Plague Hospital.

21,833. Have you any figures as regards railway and segregation camps since the late Mr. Rand's report* ?—There is the report on the railway detention camp by the Superintendent who was there.

21,834. Could you give us similar figures to show how many persons went into the railway detention camp, and how many cases of plague developed there?—The total number of contacts segregated in Sawar's Gate Camp was 13,213, and there were 146 cases of plague. In the Station Camp there were 13,670 persons detained and 23 cases of plague occurred. Nine deaths, not from plague, occurred in camp.

(The following facts appear from the report of the superintendent of the railway detention camp referred to in the previous question, which is not reprinted with the Proceedings of the Commission. The camp, to which the report relates, was opened on the 13th of February 1898, and the report gives figures regarding it up to the 20th May 1898, when only 47 inmates remained. These figures are as follows:—

For the Week ending Friday	No. of Persons detained.	No. of Persons discharged.	Balance.	No. of Cases of Plague detected in the Camp.
1898.				
February 18th	192	8	184	0
„ 25th	270	155	299	0
March 4th	588	244	643	0
„ 11th	795	505	933	5
„ 18th	941	708	1,166	2
„ 25th	644	942	868	1
April 1st	1,173	721	1,320	4
„ 8th	1,900	1,055	2,165	3
„ 15th	1,534	2,698	1,001	4
„ 22nd	1,302	881	1,422	1
„ 29th	1,525	1,351	1,596	1
May 6th	2,153	1,388	2,361	1
„ 13th	589	2,801	149	1
„ 20th	64	166	47	0
Total -	13,670	13,623	47	23

* Printed on pp. 301 to 357, Vol. II. of "The Plague in India, 1896–97," by R. Nathan, I.C.S.

Of the 23 cases of plague, 2 were discovered on the 5th day after the patients entered the camp, 1 each on the 6th and 8th day, and 2 on the 9th day. It is probable that most of these patients were ill before their sickness was discovered. Of the *détenus* 8,450 came from Bombay, and 5,220 from other places; they were grouped in the camp by castes, but there was no special grouping of persons according to the place they came from; and though cases from elsewhere were taken out of the trains, only persons from Bombay became sick of plague while in the camp. It is therefore probable that the persons who developed plague after a stay in camp of 5 days or more, did not acquire their infection in the camp.)

21,835. Can you give us any information which would bear upon the period of incubation? Can you tell us, day by day, how many cases developed, and, if there was a prolonged period of incubation in any case, state whether it was due to re-contact or not?—The following statement shows the period of incubation:—

Plague occurred within	24 hours	in	21	cases.		
,,	,,	after	1 day	,,	20	,,
,,	,,	,,	2 days	,,	17	,,
,,	,,	,,	3	,,	22	,,
,,	,,	,,	4	,,	22	,,
,,	,,	,,	5	,,	15	,,
,,	,,	,,	6	,,	7	,,
,,	,,	,,	7	,,	6	,,
,,	,,	,,	8	,,	8	,,
,,	,,	,,	9	,,	2	,,
,,	,,	,,	10	,,	2	,,
,,	,,	,,	11	,,	2	,,
,,	,,	,,	12	,,	1	,,
,,	,,	,,	13	,,	1	,,

The last two cases were due to the patients being kept under observation for a few days before a definite diagnosis of plague was arrived at.

21,836. Have you had any instance where plague has moved to a house adjoining an infected house without any means of communication between them, and without any apparent communication between the residents of the two houses?—I cannot say that, unless one could eliminate all human means of intercourse. I have never been able to do that.

21,837. Although you have an open mind as regards the communication by rats, you do not think that other animals communicate the disease?—I cannot answer that.

21,838. Do you know whether there was any Health Officer here when plague broke out in the first epidemic?—Yes.

21,839. Do you know what qualifications he had?—He was a Parsee gentleman, an L.R.C.P., of Edinburgh.

21,840. Is there a Health Officer now?—Yes.

21,841. Who is he?—Dr. Dantes, a Goanese.

21,842. Do you know anything of the ordinary powers of the Health Officer?—The powers of the Health Officer have practically been absorbed by the Plague Committee.

21,843. Have you as the Executive Officer in charge of plague operations here rather larger powers than the Health Officer?—Very much larger.

21,844. Do you think that it is necessary under ordinary circumstances that the Health Officer should have as large powers as you have now?—No, I do not.

21,845. Do you think that it is necessary in ordinary times that the Health Officer should have more enlarged powers than he already has?—Yes. I think the Health Officer would find it rather hard to define his own powers here.

21,846. Would it not be desirable that his powers should be strictly defined?—I think so.

21,847. Do you know whether there was any byelaw as to the registration of deaths in Poona at the time when plague originally broke out?—I could not say.

21,848. Do you think that in a Municipality such as Poona there should be a byelaw requiring the registration of deaths under ordinary circumstances?—Undoubtedly.

21,849. Do you think that there would be any difficulty in rigidly enforcing such a byelaw, provided that the Health Officer's authority was properly maintained?—I do not think that there would be.

21,850. Do you think that it would be easy to enforce such a byelaw provided the Health Officer had no more authority than he had under ordinary circumstances—than he had at present?—I think it might be enforced.

21,851. Is it your opinion that there is any measure which is more urgently required in connection with the suppression of the plague than some system for securing the accurate and early notification of cases of death of all kinds in the larger cities?—Early notification is the most important measure to enforce from a plague point of view.

21,852. Have you had any experience in rural areas?—I have been in charge of this district for the last seven months.

21,853. Do you regard it as desirable to endeavour to secure early and accurate notification in rural areas as well?—I think it is, but less important than in towns.

21,854. Do you think it would be very difficult to secure such notification in rural areas under ordinary conditions, at all events during the next few years?—As a routine system I think it would be extremely hard to get a reliable return.

21,855. Do you think that it would be possible in a large Municipality like Poona to have a byelaw which would require that cases of epidemic disease should be reported under ordinary circumstances?—I think it is a most advisable law; but it would be impossible to carry it out in India. You mean the same notifications of contagious diseases as in England?

21,856. Quite so. First of all notification of sickness, then notification of death. You agree that notification of death could be insisted on. I want to know whether you think that a similar byelaw requiring notification of epidemic sickness could be carried out?—It is not possible here, not in the present state of things, where 90 per cent. of the people die without being seen by a doctor.

21,857. (*Mr. Cumine.*) Do you consider that the measures in the second epidemic were materially different from the measures in the first epidemic?—Yes.

21,858. It seems that the first epidemic lasted from five to six months, and had a climax of 453 attacks; and that the second epidemic lasted six or seven months, and had a climax of 420 attacks. Can you point to any material difference between the course of the two epidemics? Can you tell us to what extent the modification of the measures in the second epidemic made the second epidemic run a different course from the first epidemic, made it cease sooner, or made it produce fewer attacks? I want to see to what extent the difference in measures produced a difference in the course of the epidemic. You say the measures differed materially in the second epidemic from those in the first. Did the course of the second epidemic differ from the course of the first? Did the change of the measures in the second epidemic produce a difference in the course of the epidemic?—I cannot say. I do not think I have compared the charts.

21,859. Dr. Venis gave a table of statistics on corpse inspection. Does the number in his table mean the number of corpses that were inspected, or does it mean the number of corpses the inspection of which revealed the cause of death—the number of cases in which the cause of death was not known until the actual inspection of the dead bodies? Which does it mean?—His table shows cases that have been inspected by a medical officer during his corpse inspection of the city.

21,860. It merely means the number of dead bodies that have been looked at?—Quite so.

21,861. It does not mean the number of dead bodies whose cause of death has been ascertained purely and solely by looking at the corpse itself?—It means every case that died out of hospital.

21,862. Would it be possible to tell us to what extent in the case of the corpses in this table the cause of death was ascertained by the inspection of the corpse

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and the inspection of the corpse alone, without having seen the patient before death, and without having had any history of the patient's illness? For instance, I find heart disease put down there. Could you tell, by inspecting the body of a person who died from heart disease, that heart disease was the cause of death?—No, certainly not.

21,863. That would be one case in which corpse inspection would not in any way assist in discovering the cause of death?—That is so.

21,864. Could you tell us whether, amongst these corpses, there are any corpses in which the cause of death (including plague) was discovered by corpse inspection alone?—Yes.

21,865. I want to see what the value of corpse inspection is when you have corpse inspection alone. Supposing you found a body lying on the road with nobody near, and nobody to tell you the history of the case, and you had nothing to look at but the body alone, that would be an instance where corpse inspection was the only thing you had to enable you to find out the cause of death. After eliminating all cases where you had other things to help you, how many cases will remain in this table where you had nothing but the view of the body itself to enable you to tell what the cause of death was?—We know that 85 per cent. of the cases of plague died of bubonic plague, which is an objective feature in a corpse.

21,866. Are not buboes sometimes absorbed immediately before, or immediately after death?—I have never heard of a case. It is not my experience.

21,867. In the case of a certain number of people who die of plague, it is possible to tell from the circumstances and surroundings that they died of plague, is it not?—Yes.

21,868. For instance, if two people died in this room yesterday, and a third person dies to-day, would not that fact of itself, without examining for buboes, be enough to make you assume that the person died of plague?—One would be extremely suspicious that it was so, there being two cases in one house.

21,869. You have only had two or three objections made to the examination of corpses, but have you had any complaints from people saying that corpses have lain for a very long time in their houses because the medical officer has not come to examine them?—Yes.

21,870. How many such cases, roughly, have you had?—I have certainly had two or three complaints. In the volunteer meetings that have been held that point has come up for discussion. There have undoubtedly been two or three complaints made in that way, but very few written ones.

21,871. There have been undoubtedly complaints of what?—That a delay has occurred.

21,872. (*Prof. Wright.*) You receive information of sickness in the town by reports, do you not?—Yes.

21,873. These reports come to you from doctors practising in the town?—Yes.

21,874. And from volunteers?—Yes.

21,875. And, further, from the relatives of the patient?—Yes.

21,876. Would you tell us what percentage of reports come from the relatives of patients?—I am afraid I do not know that.

21,877. Could you prepare a table and give your sources of information?—There were 2,863 cases reported altogether.

21,878. During what period?—12 months.

21,879. For 12 months, 1898, the number of cases given in the table prepared by Dr. Venis as certificated is 1,891?—Those are by volunteers, doctors, and relatives.

21,880. Was this table prepared under your supervision?—Yes; but I have not looked over the figures recently.

21,881. Am I right in saying that a very few cases were reported by the relatives of the patients?—I should not like to say what the percentage was.

21,882. Have you an impression as to whether few or many were reported by the relatives of the patients?—I should say a great number.

21,883. Out of those 1,891, how many were reported to you by the relatives of the patients, how many by doctors, and how many by your volunteer committee?—(The following figures were supplied later by the witness:—)

There were reported by relatives -	402
" " " doctors - -	54
" " " volunteers -	1,435
	1,891

21,884. What penalty was there for not reporting?—The penalty has varied very considerably. The penalty used to be that the people were put into the segregation camp or under surveillance. Practically, however, we have never, or very rarely, exacted the penalty within the last three months. We wish to diminish all motive for not reporting or concealment of any sort.

21,885. But the exaction of a penalty is a motive for reporting cases, and a remission of the penalty is a motive for concealing cases, is it not?—I suppose it is.

21,886. How many cases of sickness do you think escape detection in Poona in a year?—I do not know.

21,887. How many unreported cases of sickness, do you think, occur in a year in Poona city? I make out that 1,900 cases were reported to you, and of those 600 died. That is a proportion of one case of death to three cases of sickness. With your total of 3,000 deaths, I take it that you would have 9,000 cases of sickness. Am I right in that?—Yes.

21,888. How many cases of plague were detected during life, during 1898, by means of the visitation of the sick? Am I right in saying that there were 25?—Twenty-one by visitation of sick, from May 1898 to January 1899.

21,889. What object was achieved by the detection of these 21 cases in life?—To get the case early—to take early measures. The sooner you get an infected unit the less likely he is to spread infection.

21,890. Were these 21 cases removed to hospital; you did not, did you, bring them all to the hospital?—They were brought to hospital.

21,891. What is the advantage you are going to obtain from your sick inspections in the future? I understand that you are going to leave moribund patients in their houses?—None of the cases detected from May 1898 to January 1899 were moribund.

21,892. A large number of the cases which you will detect by your sick inspection will be moribund cases, and, therefore, you will leave a large majority of the plague patients whom you discover in their houses?—Arguing from our previous experience I should say not. This system has not been tried.

21,893. And you propose to isolate certain other plague patients in their own houses?—No, I am prepared try it.

21,894. Those, also, you propose to leave in their houses?—No.

21,895. And, again, certain of the plague cases which you detect will come under the category of suspicious cases?—Yes.

21,896. Those, also, you will be compelled, will you not, to leave behind in their houses?—We have an Observation Hospital.

21,897. Will you take these merely suspicious cases away from their houses?—It is not an absolute rule about people being treated in their own houses. Each house will be treated on its own merits. We will take into consideration whether it is a suitable house for a patient to be treated in, or not.

21,898. The point that strikes me is this. There are very few cases detected during life and most of these you will be compelled to leave in their quarters for one reason or another. You will leave them behind, either because the diagnosis is not certain, or because the house is good enough to permit of the treatment of the sick in it, or, lastly, you will have to leave your plague patients in their houses because they are already moribund when you find them. In view of these facts I want to learn what advantage will accrue from your system of sick visitation?—Up to date no trial has been made of treating patients in their own houses, and out of 25 cases which occurred during 1898, 23 were seen during life and all were sent to hospital. One advantage will be that in a large town we hope to diminish panic.

21,899. How can you diminish panic by going to see sick persons in their houses?—By allowing certain cases to be treated in their houses.

21,900. I am not doubting the wisdom of your plague policy; I want merely to learn how you are advantaged by finding a plague patient while he is alive, if you have, from one cause or another, to leave that plague patient in his own house?—By antiseptic precautions the conditions existing in hospital might be approximated.

21,901. (*Mr. Hewett.*) In these cases in which plague was detected during lifetime, would it not be an advantage that you would also find the contacts?—Yes, undoubtedly.

21,902. (*Prof. Wright.*) I will now go to corpse inspection. You have a table here (*see* Questions No. 21,416 and 21,788) in which deaths are classified according to causes; is there any one of these diseases, with the exception of plague, which is enumerated in that table, which can be detected by mere inspection of the corpse?—As vital statistics, I do not think the table is worth anything. Cholera, small pox, and leprosy, are diseases which could be diagnosed after death.

21,903. Why are the cases not classified simply under the headings of "plague" and "not plague"?—It is the nearest approach that one can get to some sort of registration. We tried to get it in that way—we were only concerned with plague. There was no particular reason why we adopted this course. At first we commenced with "plague" and "not plague."

21,904. On what principle is a person put down as dying from asthma or bronchial pneumonia?—I admit that the table is of no value from a vital statistical point of view; it is the best approximation to accuracy.

21,905. What is the best you can arrive at in the matter of a determination of the cause of death? Can you arrive at anything more than a classification of deaths into a class of "certainly plague," "certainly not plague," and "suspicious"; is not that the most important classification which can be based upon corpse inspection?—Practically that is all, except cholera, small pox, and leprosy.

21,906. With regard to these cases, when you classify a case as "due to plague," I presume that this will happen only when a corpse is found to have buboes?—Yes.

21,907. Further, I presume, that if a corpse has no buboes, you put that down as "suspicious," whenever you find that another death has occurred in the same house?—Yes.

21,908. And if you find a case of sudden death, you put that down also as "suspicious"?—Yes.

21,909. If you find that a body is well-nourished and that the period of illness has been very short, you also put down that case as "suspicious"?—Yes.

21,910. And if, further, you see that the tongue is furred like a plague tongue, and if you see that the conjunctivæ are injected, you would put that down as "suspicious"?—Yes.

21,911. So that you would have quite a large number of cases which you would classify as "suspicious"?—Yes.

21,912. With regard to disinfection, you would disinfect, would you not, all the houses where deaths had occurred which were certainly due to plague?—Yes.

21,913. What would you do with houses in which suspicious deaths had occurred, would you disinfect these houses also?—We have done so up to now.

21,914. I find that in certain months, for instance, in November and December last, no plague cases were detected in Poona: that means, I presume, that no corpses with buboes were detected? But there may have been suspicious cases, may there not, in those months?—Yes.

21,915. Was disinfection carried on all through November and December, or was disinfection suspended?—Instructions were given to the Inspecting Medical Officer that in all suspicious cases the certificates were to be marked "suspicious," and they were treated as plague cases. With regard to the measures taken, the place was disinfected and the people were placed under surveillance.

21,916. Did suspicious cases occur right throughout or was there any month in which no suspicious cases occurred?—We have been getting a large number of cases the last few months.

21,917. Was there any month in which you had not suspicious cases, any month in which you can assert that there was no plague in Poona?—I could not say.

21,918. We have, for instance, a great number of cases of bronchial pneumonia and bronchitis set down for the months of November and December, and I understand that there were such deaths in January?—Yes.

21,919. Were all these suspicious cases from your point of view?—Yes.

21,920. Was disinfection carried out in all those cases?—In every case marked.

21,921. In view of the fact that a large number of cases can only be classified as "suspicious," and in view of the fact that you cannot in these cases arrive at a definite opinion as to whether you are or are not dealing with plague, do you not think that corpse inspection, as at present carried out, is unsatisfactory?—No, I do not think it is.

21,922. I presume you can never, by your present methods, certify the town to be free from plague?—I think you may arrive at it by bacteriological examination.

21,923. Do you think that corpse inspection ought to be supplemented by bacteriological examination of the the suspicious corpses?—Yes.

21,924. Have you any means of supplementing your corpse inspection by bacteriological examination?—No, not in Poona.

21,925. Have you represented the great importance of such a bacteriological examination of all the suspicious cases?—We sent down to Bombay to have some examined.

21,926. Do you know how many specimens derived from corpses were sent down to Bombay?—Seven altogether.

21,927. Do you not think that there would be a great advantage in having the bacteriological examinations done here?—Yes.

21,928. And, further, that it would be a great advantage to have these bacteriological examinations made in all suspicious cases?—Yes.

21,929. Do you think that corpse inspection, combined with the bacteriological examination of suspicious cases, would be the most valuable agency that could be employed in detecting plague?—Yes. I think it would diminish the necessity for our measures of disinfection which we have to take now.

21,930. I understand you have to waste a lot of disinfectant now simply because you do not know whether suspicious cases are really cases of plague or not?—We disinfect the houses almost immediately, and we put the inmates under surveillance. Directly the diagnosis is cleared up, and we know that the case is not one of plague, they are no longer put under surveillance.

21,931. You think that you might stop disinfecting operations altogether as soon as bacteriological examination showed that no more cases of plague were occurring in the town?—Undoubtedly.

21,932. (*Mr. Cumine.*) With regard to the present infection of Poona, was not the mischief done before you ever saw the corpse at all? Was not Poona infected before the man died?—Yes.

21,933. In this case the damage was done before the man was a corpse?—I have admitted that you could not repair that particular damage by the system we have now.

21,934. (*Prof. Wright.*) But the fact that this case of plague was detected immediately after the man died is evidence, is it not, of the utility of your system of corpse inspection?—Yes.

21,935. (*The President.*) What is the largest number of people you have evacuated in Poona at one time?—We evacuated 4,800 people into the health camp at Bhamburda.

21,936. What is the largest number of people you have had evacuated in one given time?—962 was the maximum.

21,937. I do not understand your views about home segregation. I thought you said that it was attended

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with very considerable risk?—We have never tried home segregation. I said one did not know what minimum one might eventually come to with regard to segregation. It is a system of which I have had no experience. One is prepared with plague, however, to have an open mind with regard to any particular measure.

21,938. You think there are so many risks that the experiment would be unadvisable unless you had some further experience?—Quite so.

21,939. Do I understand that your plague cases are not being treated at home at the present moment?—Home treatment is on its trial.

21,940. What are you doing?—The regulation passed recently on the subject was to the effect that cases which were reported early, and were found to be in airy houses, would be allowed to be treated in their own homes.

21,941. Are there not the same risks, or nearly the same risks, in that measure as there would be in home segregation?—There is risk to the attendant or relative, undoubtedly. One tries to diminish that by putting disinfectants freely on the floors.

21,942. Is there a risk of outside conveyance by friends?—There is a possibility of it, but from one's experience in Plague Hospitals I do not think it is very great.

21,943. I do not refer to Plague Hospitals; I refer to houses in the centre of the town where a plague patient is being treated. Are there not great risks of infection being conveyed outside that house, which you cannot keep under control in the same manner as a Plague Hospital, where friends come in and friends go out? Have you had much experience of this treatment?—It is on its trial. That is the regulation now in force, and we are waiting results.

21,944. With reference to the registration of deaths, I think you said you considered that that was a very important measure, and should be carried out effectively; that you should have an improved system of registration of deaths so as to obtain early information of the cause of death. Is that not so?—Undoubtedly.

21,945. You said that that was the greatest requirement at the present time?—Undoubtedly.

21,946. With regard to what?—Plague, and epidemic diseases generally.

21,947. What do you say now with regard to sanitary measures? How far would your opinion be modified if you also took into consideration sanitary measures and their influence in reducing the likelihood of epidemic diseases?—I think they have much greater influence in some diseases than in others.

21,948. With regard to generally prevalent diseases, sanitation takes an important place?—With regard to plague, and such diseases, I think it has a very vast influence; but with regard to small-pox, and so on, I do not think it has the same influence.

(Witness withdrew.)

Capt. C. K Morgan, R.A.M.C.

Captain C. K. Morgan, R.A.M.C., called and examined.

21,949. (*The President.*) You are in the Royal Army Medical Corps?—Yes.

21,950. (*Prof. Wright.*) How long have you been on plague duty?—About two years.

21,951. Have you any information with regard to the occurrence of neuralgia in people who are engaged in plague work, and in Medical Officers who are attending on plague patients?—Yes; in Poona, about the middle of the epidemic of 1897–98 my attention was first drawn, I think by Colonel Fawcett, to neuralgic pains in the glandular regions occurring in those doing plague duty. I inquired of the other officers, and also of the soldiers, and the majority of them admitted that they suffered from the same symptoms. They had neuralgic pains in the glandular regions of the body, in the arm, groin, and in the neck.

21,952. Have you seen any cases in which these neuralgic pains were severe?—Yes, they were very severe in several cases. I am not quite certain, but I think two or three cases went to hospital complaining of the pain.

21,953. Do people who have been a long time on plague work cease to suffer from these pains?—They do not. I have been for two years amongst the plague, and I still have them.

21,954. If you cease to do plague work for a time and then go back to it, do the pains return?—Yes, they come again when one sees patients.

21,955. Does the seeing of a single case of plague suffice to cause the reappearance of the neuralgics?—Yes, when I have seen only one case in a day I have had those neuralgic pains.

21,956. Do you think any element of hypochondriasis can contribute to the causation of these pains?—It may be so, but they are very general.

21,957. Have you heard of the occurrence of these pains among soldiers who knew nothing about pathology?—Yes, they did not generally complain of it, but admitted it when I asked them.

21,958. With regard to corpse inspection, do you think that, in a great number of instances, it is difficult to detect cases of plague by mere inspection of the corpse?—In many cases it is extremely difficult, in fact impossible.

21,959. Have you found in the course of your sick-visitation that it is often very difficult to diagnose a case of plague?—Yes, it is impossible in many cases.

21,960. Do you think bacteriological examination would help you very much in that which you diagnose?—Yes, very materially.

21,961. Have you seen cases resembling mumps which seemed to you to be possible cases of plague?—Yes.

21,962. Have you seen any cases of *pestis ambulans?* I mean cases of swollen glands unaccompanied by serious symptoms which are seen in plague?—Yes, I think I can recall a very small number of cases, in fact only two.

21,963. (*Mr. Cumine.*) You get a history of the case from the friends of the patient, do you not?—Yes, but they give very erroneous statements. We had a case yesterday where we were told that a child had had dysentery for fifteen days. The child was far too well nourished for that to have been the case, and it was put down as a suspicious case of plague.

21,964. (*The President.*) You have always an opportunity of learning the patient's history from friends or outsiders?—We get very erroneous information from them.

21,965. Still, you have an opportunity of examining them and asking them questions?—Invariably.

21,966. Your opinion is not arrived at by inspection?—No, not by inspection alone.

(Witness withdrew.)

Lieut.-Col. W. J. Fawcett, R.A.M.C.

Lieut.-Colonel W. J. Fawcett, R.A.M.C., recalled and further examined.

21,967. (*Prof. Wright.*) Can you tell us something about the neuralgias which develop in medical officers and men who are engaged in plague work?—I presume you allude to what one may call plague neuralgia. I think I first observed it in Poona in the latter part of 1897. Probably the best way of describing the affection is by giving the details of a case. I came across a very well marked case, perhaps one of the most severe, in an officer whose duties did not always bring him in contact with plague, but sometimes for days together he had occasion to be in contact with plague cases and handle them. On each occasion that he handled a plague case, within half an hour afterwards, he had symptoms exactly similar to those which follow the immersion of one's hands in carbolic acid solution, a tingling in the hands extending over the lymphatics of the forearm and before many minutes reaching the lymphatic glands in the axilla. Sometimes it passed off after a few hours, at other times the pains were intensified until the lymphatics felt like burning cords, and the glands in the axilla became extremely painful—a neuralgic pain not increased on pressure. At other

times there were very violent neuralgic pains in the lymphatic glands in the back of the neck, so violent that they quite prevented rest and sleep. This officer never had neuralgia of any kind or description before or since. When I first became aware of this case it led me to inquire among other officers, and I found one medical officer who described of his own accord exactly similar symptoms in himself. At least one Captain and one subaltern on plague duty in the cantonment suffered in the same way. The subaltern suffered aching pains in the groin for the first 10 days after he went on plague duty. He had been on plague duty in two or three different places in India. He had sought relief in remedies of various kinds, but without the slightest result. I did not like to suggest these symptoms to the men, so I asked the medical officers in charge of the two camps, and they told me they frequently had men coming in sick complaining of neuralgia. Two of those men were so bad that they were sent to the Station Hospital. There I observed them, and I have not the slightest doubt that they were suffering from an exactly similar disease. I should like to say that this plague neuralgia is alluded to in print in a popular lecture of mine dated 19th January 1898.

21,968. Will you read us the paragraph?—"I have "found that of those Europeans who have been most "exposed to plague, many have told me that during "the first week or so of their exposure they suffered "from neuralgic pains and stiffness in the groins, "armpits and neck."

21,969. Have you suffered from it yourself?—Yes.

21,970. When you go away from plague work do the pains cease?—Yes. They never last more than a few hours at a time. Many of the officers told me that they became immune after some days. I never did, because my work was not continuous. I frequently did not handle a plague case for two or three weeks, and then mixed with them again during the following two or three weeks. Then I always got the pains again. In fact, so accurate was it, that I used it as a means of diagnosis.

21,971. (*The President.*) Do you mean you could apply this power any time you wished?—Not in every case. I was alluding specially to the first case of plague in a European soldier which occurred in the cantonments. It was a doubtful case, and I examined him with great care, and spent some time in the ward with him. When I reached home I found the tingling had set in in my hand. That was within half an hour. I happened to meet the man who was attending on the patient and I said, "That is undoubtedly a plague case." A bacteriological examination of the bubo made that afternoon confirmed the fact that it was a case of plague. We went back again in the afternoon and he had all the usual symptoms of plague.

21,972. Did you have a similar experience in other cases?—That was almost the last in the cantonment. I noticed it in one or two other cases. I undoubtedly used it as a means of diagnosis in suspicious cases.

21,973. Have you done so lately?—No.

21,974. Would it not be advisable to take an opportunity of doing so?—I do not desire to suffer from neuralgia of that kind again. The power does not last for very long. It would very soon be blunted.

21,975. (*Prof. Wright.*) You said you would make some inquiries with regard to the statistics of the inoculations which were done at Kirki?—I regret to say that although I hold a receipt for a bundle of papers, of which this was one, in the Assistant-Adjutant-General's Office in the Poona district, they are not to be found, but inquiries are being made. Perhaps the Commission might apply to the Assistant-Adjutant-General.

21,976. Before you gave up these reports of the result of the inoculations, had you made a careful study of them?—Yes, and I considered them so untrustworthy that I was unable to draw any inference of any value, or I would have had great pleasure in bringing them to the notice of my superiors at an early date.

21,977. Do you know if a special report other than this untrustworthy report that you speak of was sent to Professor Haffkine with respect to the result of the inoculations?—This was not submitted to Professor Haffkine; it never went further than myself.

21,978. Had Professor Haffkine a special report on the result of the inoculation?—Yes, by another medical officer.

21,979. Do you think the report of the medical officer in question was based on these statistics which you speak of as untrustworthy?—I do not know; I rather think not.

21,980. Were there two medical officers on the spot, one who made the statistics which were submitted to you and another who made a report for Professor Haffkine?—I believe a medical man came down here specially to make a report for Professor Haffkine.

21,981. These statistics which you speak of were made for the information of Government?—I originally obtained these statistics with a view of submitting them to Government, but I found I could not compile anything of any value from them. The young officer had been quite overworked, he was not to blame; he could not possibly keep up with his work at the time, and he seems to have entirely neglected anything like statistical work.

(Witness withdrew.)

Dr. W. D. Betenson called and examined.

21,982. (*The President.*) You are on Plague Duty in Poona, I think?—Yes.

21,983. You wish to make a statement?—I wish to say that I was responsible, to a great extent, for the table (*see* Questions No. 21,416 and 21,788) which was drawn up, grouping the diseases under different headings, to which exception has been taken, and to offer any information that I can with regard to that grouping.

21,984. (*Prof. Wright.*) Was the classification of the cause of death which appears in this table established only on facts observed at the corpse inspection?—No, certainly not.

21,985. It was also made on information received during life?—Yes.

21,986. I find that in your sick visitation you certified 1,900 cases during the year?—There were 1,900 cases of sickness certified by us personally.

21,987. And 600 of those died?—Yes.

21,988. I suppose that those 600 cases with respect to which you obtained information during life by sick visitation are enumerated among the 3,000 cases which came before you in your corpse inspection?—Yes.

21,989. And, therefore, they are included in the table among the 3,000 odd corpses which you examined?—They are included in the table.

21,990. In addition to that there were 2,400 corpses which you had not seen during life?—Yes.

21,991. Then, with regard to the 2,400 corpses, you had no information except that obtained after death?—No, that is not so. The respectable native practitioners sent in to our office, on forms supplied to them for that purpose, a report of all their cases of serious illness, and we received 1,063 of those in the course of the year. As they report serious cases only, the probabilities are that quite three-quarters of the cases which the doctors reported would be found among the 2,400 of which we had no personal knowledge.

21,992. With regard to the 2,400, you have no personal knowledge, but you have had information from native doctors?—To the extent of 1,063. The 1,800 cases are our own, we saw them individually, and each of them had a certificate on the regular form. Then if you add the native doctors' 1,000, you get 2,800 people who were seen in the course of the year by some doctor or other out of 3,035 deaths.

21,993. But some of the 1,063 reported by native doctors you saw while living, and, therefore, they are included in the 1,800 whom you saw during your sick visitation?—Some, but very few. We see on an average, perhaps, three or four a month, and then they would be cases of high fever. We generally write to the native practitioner, if we know him to be a respectable man, and say "kindly send us a longer description of the symptoms of this case," because it is essential not to interfere with the practice of the native practitioners or they would not back us up.

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21,994. Of the 1,063 cases of sickness reported by native practitioners, very few were seen by you?—That is so.

21,995. Then the medical practitioners reported their cases when the patients were practically moribund, is that so?—Most of the native practitioners sent in a report of all serious cases day by day.

21,996. Are the patients, in the majority of cases, already dead when the report reaches you? — Oh, no.

21,997. You say you only saw a few of those cases during life?—We did not take the trouble to see them.

21,998. I thought that every case of sickness which was reported was visited by you?—Not those reported by the native practitioners.

21,999. My difficulty is, that I to-day saw a case visited which had been reported by a native practitioner, and I understood it was your practice to see every such case?—We are more energetic now; if we hear of anybody ill we go and see the case. But, in ordinary times, when there is no plague, unless a man reports a case of high fever we do not go and see it.

22,000. This case which I saw was, as a matter of fact, reported as a case of high fever?—If we have any reason to doubt the report we see the case. I can give you particulars of two cases which were stopped in the city, which are included in our returns of corpse inspection. No details of cases which were sent in were kept until July. After that there were two cases found by corpse inspection.

22,001. You mean the two sporadic cases of plague what were found by you—one in August and one in October?—Yes, they were imported cases.

22,002. The inference you draw from the fact that these two were unreported cases is, that there was plague lingering on at that date in the town?—We have been able to trace every case, I think, up to the 12th February, and we found they were imported from outside.

22,003. You infer from that that the infection died out in the town?—Yes, in May. We had 39 cases imported since. but we can trace them all.

22,004. (*Mr. Hewett.*) Of the 1,063 cases reported by private doctors to you as sick, how many were after-inspected as corpses?—I do not know.

22,005. Will you please get the information?—(The witness subsequently stated that 371 cases were so inspected.)

22,006. Were any of the 106 cases which were found after death to be plague reported to you by doctors in private practice?—No.

22,007. What number of persons whose deaths are recorded in the table do you estimate were not seen in life, either by Government doctors or by private practitioners?—I should think quite two-thirds, 2,000 out of 3,000.

22,008. In case of those 2,000 persons, what means had you of ascertaining whether the record of death which is entered in the table was accurate or not?—We had visual inspection and enquiry.

22,009. What is the nature of your inquiries?—We enquire how long they were ill, and whether they had fever, vomited, or coughed, &c.

22,010. Who made the inquiries?—The medical officer on the spot, through the interpreter.

22,011. Then they were not made direct to the natives of the country by a person who understood the language?—Yes, the interpreter is a native of the country.

22,012. Did the doctor who made the inquiries understand the language?—No.

22,013. (*Prof. Wright.*) Do you think it would be possible, by that method, to distinguish, as you do in your table, between bronchial pneumonia and bronchitis?—The bronchitis in this table includes the bronchitis of old people.

22,014. (*The President.*) But all tables of mortality classified according to the cause of death are full of errors in every country in the world?— Yes, of course this is only rough. The bronchitis includes bronchial catarrh and bronchitis in children.

22,015. (*Prof. Wright.*) Has not the table a fallacious appearance of accuracy? Would it not impose upon anyone who is not a medical man?—Yes, but I maintain they are comparatively accurate.

22,016. Is there not a fallacious suggestion in the table that all these cases are seen during life, whereas, as a matter of fact, two-thirds of them have not been seen till after death?—Yes, certainly. (Note added by witness on correcting proof of his evidence.—No, it is a table of corpse inspection.)

22,017. (*The President.*) You admit the table must contain errors?—Yes, but it is approximately correct.

(Witness withdrew.)

(Adjourned till Monday, February 27, at Satara.)

At The Collector's Office, Satara.

FIFTY-SIXTH DAY.

Monday, 27th February 1899.

PRESENT:

Prof. T. R. FRASER, M.D., LL.D., F.R.S., (*President*).
Mr. J. P. Hewett. Mr. A. Cumine.
Mr. C. J. Hallifax (*Secretary*).

Mr. C. G. Dodgson, I.C.S., called and examined.

Mr. C. G. Dodgson, I.C.S.
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22,018. (*The President.*) You are Collector of Satara?—Yes.

22,019. You have had experience of plague at Hyderabad in Sind and at Satara?—Yes.

22,020. And have formed some opinions with reference to methods of dealing with plague and the causes of plague?—Yes.

22,021. Can you tell us what your views are about the causes of plague?—I know nothing about the disease bacteriologically. I do not know what plague is beyond that it is a bacillus of some kind or other, but I believe plague may be spread by rats. I do not know that it is, but I suppose it is, because rats frequently die before human beings; but I think that, probably, the spread of plague is mainly due to human agency. I think people take it about in their clothes, and possibly, for all I know, they may eat it. I mean that I have seen people sitting alongside a plague patient in a hut eating food off the ground, with the patient lying within a few inches of the food; it is therefore quite possible that they are eating bacilli at the time.

22,022. Have you any case which seems clearly to show that the disease can be communicated by human agency?—I have known a man go to an infected village, stay there for two or three days, or even less, and come back to his own place and at once infect his family.

22,023. That would include his clothing, probably?—Yes, he had probably brought the infection in his clothing. I have known cases of a person coming in that way from an infected place to his own house, and his own family getting the plague, he himself not getting it.

22,024. Have you any case in which clothing, apart from persons, appears to be the conveying agency?—There was a case two or three hundred yards from this bungalow. A man last year, who was a storekeeper in the camp at Satara, went, after the plague was over, and lived in this village of Kamatpura. A month, or two months afterwards, plague broke out in Kamatpura, and the general opinion was that this man had been stealing the clothes from the dead bodies of women in the camp, and then giving them to his own wife. His wife was the first person who got the plague—he himself did not get it, but the rest of his family got it, and they nearly all died.

22,025. What evidence was there that he had really been taking infected clothes?—The clothes were found in his house, and he could not account for them. Of course, there was no real evidence, on which we could have secured a conviction in a criminal court, that he had stolen them, but that was the general opinion among his relatives and neighbours.

22,026. Do you think it is possible to prevent the spread of plague from one place to another?—No.

22,027. Why?—Because you cannot thoroughly control the people. You might, if you had an unlimited staff and unlimited money, but we have no money and next to no staff here, and we cannot, therefore, control them.

22,028. The people will not voluntarily do what is required?—No. We have very frequently to prosecute a man for leaving an infected place and going to another village without permission, but it is only one case in 50, or one in a hundred, that we actually get hold of.

22,029. In regard to dealing with plague when it has occurred, what, in your opinion, is the most effective of the measures which can be adopted?—If the object merely is to stop the plague, I should say the only thing was evacuation, and segregation of the contacts and the patients.

22,030. Evacuation of the infected area?—Yes, of the infected quarters, or whatever it might be; that is, if the only object is to stop the plague, quite regardless of the feelings of the people and their wishes.

22,031. But as a practical measure, what do you consider the most effective for stopping the plague?—Trying to evacuate, and evacuating as much as possible.

22,032. The more evacuation, the more effective this measure is, in your opinion?—The more evacuation, the more effective I think it is.

22,033. Have you any instance in which, by evacuation, you have been able to stop the extension of plague in an infected village?—I have some figures which I have had worked out which show that, in some cases where plague has broken out in a village and the village has been evacuated, plague has stopped at an early date.

22,034. Would you illustrate that with your figures?—There are many other figures, I admit, which show that if you evacuate the place plague does not stop at an early date; it goes on for a long time.

22,035. Would you give us the figures?—There is a village, Taruk, where the occurrence of the first case of plague is shown as on the 25th November 1897. The village was evacuated, and all the people are reported to have finally left the village on the 2nd of December. There had only been three cases there, and no more occurred after the village had been evacuated. There is also another case of the village of Nandgaon, which was infected on the 15th of November 1897. The people were all out of the village on the 22nd—there had only been four cases previous to, and three more during, evacuation, and no more occurred after evacuation.

22,036. How large are these villages?—Taruk has a population of 1,559, and Nandgaon has a population of 1,402. Then, again, Garawadi, with a population of 1,599, was infected on the 5th October 1898, and the people were all out on the 10th October 1898. There were three cases in the village, and three cases after they went out, and that was the end of it.

22,037. Do you know at what interval of time the majority of cases after evacuation occurred, and whether any of those which occurred two weeks and afterwards could be accounted for by fresh importations?—I produce a statement which shows villages in which the number of cases began to decrease soon after evacuation:—

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1.	2.	3.	4.	5.	6.							
Village.	Population.	Remarks.	No. of Cases before Evacuation.	No. of Cases during Evacuation.	No. of Cases after Evacuation.							
					1st Week.	2nd Week.	3rd Week.	4th Week.	5th Week.	6th Week.	7th Week.	8th Week.
Taluka Karad.												
Vathar	1,646	During the rainy weather, evacuation not thorough.	7	12	18	18	19	29	24	18	19	18
Atke	2,038	—	3	80	30	16	10	4	1	3	1	5
Malkhed	792	—	6	2	2	9	11	6	6	1	1	3
Sarli	972	—	6	—	1	14	9	3	—	—	1	—
Saqier	739	—	3	3	6	—	—	—	—	—	—	—
Kalawada	1,717	—	3	—	1	2	—	—	—	—	—	—
Karve	3,500	—	6	11	3	2	8	2	9	2	3	—
Kirpe	606	—	1	—	—	—	—	—	—	—	—	—
Taluka Valva.												
Takari	878	—	15	11	1	3	1	2	1	—	—	—
Bahe	2,592	—	5	6	10	5	11	7	3	7	3	1
Shirgaon	576	—	2	—	—	—	—	5	—	—	—	—
Retre Harnaksh	2,493	—	5	—	—	5	4	8	4	3	1	—
Gatadwad under Gotkhind.	3,238	—	6	—	7	5	9	—	—	—	—	—
Lonarwadi under Gotkhind.	3,238	—	5	9	10	2	1	—	—	—	—	—
Tujarpur	607	—	6	1	10	7	1	—	—	—	—	—
Ozarde	1,120	—	10	1	16	3	9	3	4	1	4	2
Valva	5,625	—	1	1	—	—	—	—	—	—	—	—
Shiralapeta.												
Shirala	4,011	—	2	—	—	—	—	—	—	—	—	—
Bhikarwad under Takve	270	—	6	—	2	—	2	—	—	—	—	—
Dhamwade	483	—	6	—	8	1	1	—	—	—	—	—
Karmale	476	—	4	—	6	2	2	—	2	—	—	—
Taluka Patan.												
Garwde	1,181	During the rainy weather, evacuation not thorough.	4	19	19	18	13	5	6	3	1	5
Paneri	440	—	10	—	—	—	—	—	—	—	—	—
Taluka Tasgaon.												
Akalkop	395	—	3	—	—	—	—	—	—	—	—	—
Andumberwad under Akalkop.	3,395	—	2	—	—	—	2	1	1	—	—	3
Khandobachiwad under Bhilawd.	6,985	—	28	—	—	6	4	—	—	—	—	—
Sahlarde	1,543	—	5	—	9	6	9	—	—	—	—	—
Brahmanal	769	—	3	—	11	5	7	2	4	—	—	—
Bhose	2,445	—	3	—	2	—	3	1	2	2	—	—
Bisur	1,410	—	3	—	8	13	2	—	—	—	—	—
Taluka Khanapur.												
Ghoti Bk.	715	—	7	—	—	—	—	—	—	—	—	—
Khambale	658	—	1	17	4	2	4	2	2	—	—	—
Mohi	418	—	3	—	4	3	1	—	—	—	—	—
Hamnant Vadiye	693	—	4	8	6	—	2	1	1	—	—	—
Vazar	346	—	4	1	6	—	—	1	2	1	—	—
Pare	1,656	—	2	—	5	1	1	3	—	—	—	—
Kurli	779	—	2	—	—	—	—	—	—	—	—	—
Ghoti Kd.	794	—	3	—	2	—	—	—	—	—	—	—
Taluka Khatav.												
Landewad under Trimali.	736	—	1	—	5	2	6	3	2	1	—	—
Trimali	—	—	1	2	3	13	7	12	7	5	3	—
Taluka Koregaon.												
Eksal	761	—	2	14	18	13	2	1	3	—	1	—
Bobdewad under Tadawle	1,282	—	3	5	1	1	2	—	—	—	—	—
Kathapur	1,483	—	5	37	6	5	1	—	—	—	—	—
Jalgaon	1,566	—	3	9	4	1	—	—	—	—	—	—
Shirdhon	1,671	—	2	22	8	5	5	5	1	3	2	—
Losurne	1,392	—	2	—	1	3	16	5	4	1	1	—
Satara Road Station	*	—	1	2	—	—	—	—	—	—	—	—
Ekambe	1,492	—	1	10	6	1	1	4	1	4	3	1

* Not available.

1. Village.	2. Population.	3. Remarks.	4. No. of Cases before Evacuation.	5. No. of Cases during Evacuation.	6. No. of Cases after Evacuation. 1st Week.	2nd Week.	3rd Week.	4th Week.	5th Week.	6th Week.	7th Week.	8th Week.
PETA KHANDALLA.												
Ving	2,157	—	6	3	5	14	8	6	4	1	—	—
Palshi	1,227	—	6	8	10	2	5	3	—	—	—	—
Vadgaon	498	—	12	—	5	—	2	2	—	—	—	—
TALUKA JAVLI.												
Nipani	248	—	1	—	—	—	—	—	—	—	—	—
Saygaon	1,333	—	2	1	8	8	3	8	6	5	—	—
Parbhuchiwad under Kharshi.	703	—	1	3	3	3	3	—	—	1	—	—
Raygaon	883	—	1	—	—	—	—	—	—	—	—	—
TALUKA SATARA.												
Parli	1,889	During very rainy weatherthroughout.	1	—	14	16	20	20	20	19	16	1
Gajawdi	491	—	6	—	24	20	5	3	2	1	2	—
Kasarsthal	38	—	3	—	8	3	—	2	—	—	—	—
Dhawb	193	—	7	4	—	6	2	—	—	—	—	—
Banghar	221	—	1	—	1	2	—	2	2	2	1	—
Atit	1,871	—	2	39	29	13	5	—	2	1	—	—
Sangaon	634	—	2	16	8	23	21	8	3	2	—	—
Jike	1,431	During rainy weather.	6	28	13	13	11	40	12	6	24	4
Ashte	329	—	2	—	1	2	3	2	—	—	—	—
Borkhal	1,050	—	4	—	41	5	—	6	3	1	—	—
Nisrale	513	—	4	—	5	2	14	24	30	7	5	—
Panawdi	110	—	—	3	6	6	—	2	—	—	—	—
Dahiwadi	412	—	—	4	5	8	—	—	—	—	—	—
Are	682	—	11	—	24	5	8	2	1	—	1	1
Rewli	167	—	—	—	12	7	—	—	—	—	—	—
Sonawdi	434	—	8	—	7	7	9	6	9	3	—	—
Chinchuer	1,136	—	3	20	2	—	4	2	2	2	1	—
Dhendewad under Tasgaon.	1,835	—	—	2	27	2	—	—	—	—	—	—
umnekhol	199	—	2	—	1	—	—	—	—	—	—	—
Degaon	2,509	—	—	3	—	—	—	—	—	—	—	—
Vadgaon	87	—	1	—	—	—	—	—	—	—	—	—
Kamthi	246	—	1	—	—	—	—	—	—	—	—	—
Ambawdi Bk.	589	—	2	—	2	2	2	—	—	—	—	—
Kus Kd.	171	—	2	—	6	—	3	2	—	—	—	—
Kari	880	—	5	—	12	—	—	—	—	—	—	—
Kashi	561	—	5	—	15	2	—	—	—	—	—	—
Kaloshi	465	—	1	—	2	1	—	—	3	—	—	—
Total	—	—	324	417	555	364	314	255	189	106	94	45

22,038. Can you give us examples of other villages in which evacuation was not successful?—I may point to Mahuli. Mahuli has a population of 1,959. It had 17 cases before evacuation. The people went out two days after the outbreak, and they had 138 cases afterwards, but that was the end of the epidemic. And there is another case here—I do not know what happened in this village, but it is rather peculiar. The village is Ving, with a population of 4,424. Plague broke out on the 4th September 1897, and the village is said to have been completely evacuated on the 13th September; there was one case before they went out, and 295 after they went out.

22,039. So that evacuation was not successful. Have you any explanation of that?—No. I do not know the exact circumstances of the village. I have been to it lately, but the epidemic I speak of occurred in the winter of 1897-98, and I was not in the district then. I came to this district in February 1898.

22,040. Who could tell us about this case?—The Collector then was Mr. Winchester, but he has retired now. There is also a Staff Corps officer, Lieutenant Steen, who was here then.

22,041. Have you other instances in regard to which you have more detailed information?—Here is the case of Vathar, with a population of 1,646. Plague broke out on the 1st June 1898, and the village was reported to be evacuated on the 5th. There were six cases before the people went out, and 204 cases after they went out. I know about Vathar. I went there myself last July. It was raining very hard at the time, and although the people were nominally out in the fields, they were not living permanently in them; they could not do so owing to the rain, and they kept going back to their houses. The day I walked through the village we found a lot of houses occupied that were supposed to be empty.

22,042. This was not really an instance of evacuation?—No. The large number of cases after nominal evacuation undoubtedly was due to the people continually returning to their houses.

22,043. Have you any other case?—Aundh is a Native State village. Plague broke out on the 11th October, and evacuation is said to have been complete on the 15th October. There were five cases before completion of the evacuation, and 165 cases afterwards.

22,044. What was the total population?—3,248.

22,045. How do you explain this failure?—That being a Native State village, it was very badly looked after, and it was never really evacuated, although it was officially reported as having been—all the upper classes, the Brahmans, for instance, were never turned out of their houses.

22,046. What has been the general impression produced in your mind in regard to complete evacuation where it has been properly carried out?—That the

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plague undoubtedly does begin to decrease soon after the people have gone out—within, I should say, a fortnight.

22,047. In regard to some of the villages where the evacuation was unsuccessful, have you any knowledge of the nature of the huts in the camp to which the people had removed?—They are generally huts of a most primitive type. In many cases they consist merely of a few sticks of karbi or grass put up as a lean-to for protection against the sun.

22,048. Is there any exceptional condition with regard to the huts in those camps where you had no success?—The absence of success is chiefly due to the rain, not to the nature of the huts. The people build their huts much more solidly during the rains than they do at this time of the year. During the rains they are tighter and closer, and are like little bee-hives, with a little door which you can just walk in at, and there is, practically, no ventilation whatever.

22,049. This is confirmatory of what you have expressed, that unless you secure free ventilation you do not greatly lessen the progress of plague?—Exactly.

22,050. In addition to evacuation, what other measures do you consider important?—I think you have to make a distinction between large towns—I mean a place like Hyderabad, in Sind, for instance, with a population of about 60,000, and one of these little district villages, with populations of from one to three or four thousand. In these small villages, I think, the patients and their relatives ought to be segregated. They can be segregated fairly effectively.

22,051. That is, in addition to evacuation?—Yes. Without segregation there is no knowing where the members of an infected family go to; they wander all about, and get mixed up with their friends and relatives, and spread the infection.

22,052. In the evacuation camp, you mean?—Yes. But in a large town it is very much more difficult to segregate the contacts and the patients. If it can be done, I think it is undoubtedly a good measure, but, of course, there are great difficulties in the way, because the people hate the idea of being segregated. They do not generally resist actively—they do sometimes, and then the only means is to use force.

22,053. Which would be objectionable?—Yes. It may be a good thing for stopping the plague, but it is a bad thing for anything else.

22,054. In regard to evacuation, in dealing with large towns, what opinion have you formed?—The same idea; if you can get the people out, well and good.

22,055. It is desirable, but not always practicable?—Yes, without a great deal of opposition.

22,056. Are there any other plague measures that you consider valuable?—I believe in letting in fresh air and sunlight into the houses, keeping the windows open if there are any, and opening the roofs.

22,057. If there are no windows?—If there are no windows, in the majority of houses one might make holes in the wall, but sometimes that is not possible, owing to the walls being too thick, or the houses being too badly built.

22,058. What is your view about disinfection by chemical substances?—I have tried it in Sind, and here last year; and we did it here on a very large scale. I do not think it is necessary—I do not mean to say that it does not kill the germ—I do not know whether it does, or does not; but the germ, I think, certainly dies without it.

22,059. If ventilation has been freely done, you think, then, that chemical disinfectants are not required?—Yes, I think so. It may take longer for the house to become habitable; but the house does become habitable sooner or later.

22,060. Have you any examples in which, without opening up a house so as to admit light and air, chemical agencies only were trusted to?—Yes, we did it in Sind. Some of the houses could not be opened up owing to their thick walls and entire absence of windows. There we used only chemical disinfectants, and the house was not opened up; but the plague did not occur again.

22,061. At what interval of time after disinfection were the houses reoccupied?—I was not there when the people were readmitted, but I think it was about two months after the people had been turned out. I have seen the Sind report, and, as far as I can remember, this is what I gathered from it.

22,062. You have had no examples of re-occupation following chemical disinfection at a shorter interval than two or three months?—We have had a case in the town here quite recently. There was a very well-built house outside the gaol, belonging to the gaoler—a house built very much like this, with plenty of windows. There was a case of plague there, and the house was disinfected with perchloride of mercury, and the people went out. On their return, after 10 days, there was a case of plague almost immediately, and the patient died.

22,063. You think the interval should be a long one?—Yes; but I would not trust to that alone.

22,064. What is your experience with regard to the influence of rats in propagating or causing the disease?—Rats undoubtedly die of plague, because one finds hundreds of them that have died in infected places; and I think they certainly die before human beings. I suppose that they carry the plague about from one house to another; otherwise, I do not see how it can spread, because it certainly spreads in an evacuated village. I mean, after the village has been evacuated, the infection does spread from one house to another.

22,065. What is the evidence of its spreading?—You can take a village with a few infected houses at one end, or in one quarter, and turn all the people out; but you constantly find that when the people go back to the other end of the village, or to a hitherto uninfected quarter, and sleep there without permission, they get the plague. There are many cases of this kind. I suppose rats have, in such cases, carried the infection, because I cannot think of any other means of locomotion for the plague.

22,066. Do you know if plague is spread by any other animals?—It may, perhaps, be spread by squirrels and monkeys. I have been told that dead monkeys and squirrels have been found, but I have not seen any.

22,067. Have you any opinion to offer as to the means by which infection seems to extend in certain directions from the originally-infected area?—I have seen it go across one side of the street, but it is rather slow in crossing.

22,068. Do you mean that it usually extends by continuity of dwellings?—Yes.

22,069. Supposing there were an interval between two dwellings on one side of the street, would you expect it to stop at that interval?—No; it would get across, but it might be delayed by this interval.

22,070. Why, then, would it not get across a narrow road to the other side of the street?—It would cross it finally, but it would be delayed. If there was traffic up and down the road, this would probably make the rats more chary in crossing the street.

22,071. That would apply more to the day-time than to the night?—Yes.

22,072. Rats are more likely to move about at night?—I suppose they are; but rats burrow from house to house at closer intervals than the breadth of a broad street, with probably traffic going on over their heads.

22,073. Have you any example to show that grain has been so infected with plague as to convey infection to man or the lower animals?—No. I have never heard of a case.

22,074. (*Mr. Hewett.*) What was the maximum number of villages which you had infected in the Satara district at any one time?—It was 114 on the 25th November 1898.

22,075. At what period of the year do you find that plague is most prevalent?—I do not think it matters much. As far as we have seen hitherto in the two epidemics in this district, there is less plague in the hot weather. For instance, it began to die out last year about the month of February, and practically stopped in April, when the weather got hot. This year it seems to be doing the same; but in both years it was very bad from July up till February, *i.e.*, during the rains and the cold weather.

22,076. Is it not practically impossible to evacuate a village on a black cotton soil during the rains?—We did evacuate such villages, but the evacuation was not thorough; and the people constantly went back to their houses.

22,077. You cannot keep the people out of the village site?—No, not in rainy weather; it was practically

impossible, as they cannot continue to live up to their knees in mud and water. If they do that, then the plague is probably just as bad outside as it would be inside the village.

22,078. When the plague broke out afresh after it died down to a certain extent in the hot weather last year, did it reappear in the same villages as had previously been infected, or in new ones?—Mostly in others, I think.

22,079. Do you remember whether those others were in the immediate neighbourhood of the villages which had been infected before?—Yes, they were, but the infected and the non-infected villages were fairly distributed all over the country.

22,080. Is it your general experience that a village adjoining a village infected in the first epidemic got infected in the second epidemic?—Not necessarily, but plague has run down the western part of this district fairly regularly. Excluding the hill portion of the west, it has run down a broad strip, perhaps 30 miles wide from north to south, and it has been mostly confined to that strip in both epidemics.

22,081. Has a village infected in the first epidemic, as a rule, escaped in the second?—Yes, I think that is so; if they have been attacked in the second epidemic, it has generally been in a milder form than in the first epidemic.

22,082. (*Mr. Cumine.*) Do you remember what the population of the district is?—About 1,225,989.

22,083. How many Staff Corps officers or other special European officers have you for plague work at the present time?—Five Staff Corps officers and one English doctor.

22,084. With so few European officers to work against plague, and with such a large population and so many infected villages to deal with, do you think it possible to work plague measures against the active opposition or even the passive opposition of the people?—No, not satisfactorily. You can do a great deal in the way of evacuation and segregation, but it is never perfect. I mean, supposing the European officer has perhaps 12 or 15 villages, actually reporting plague at the same time, and scattered over an area of, perhaps, 200 square miles, he can only go to each of these villages once or twice a week. While he is there he does the best he can, and gets arrangements more or less into working order. Then he goes away and does not return for, perhaps, a week, and he then probably finds his contact camp empty and his village practically re-occupied, and he has to begin all over again.

22,085. To make evacuation yield its maximum amount of good, it has to be accompanied by two or three complementary measures, such as cutting off the people from the infected village site and getting them to isolate their sick, has it not?—Yes, certainly.

22,086. Where a Staff Corps officer has about 15 villages to look after, can he carry out these complementary measures which are necessary to make evacuation have its full effect?—He can only start them, and then trust to his subordinates, the Hospital Assistant, and the Plague Circle Inspectors, and the Mamlatdar. He must trust to them to try and keep the measures in working order.

22,087. But, personally, he cannot see to them?—No; he can only start the arrangements and must trust to his subordinates to carry them out, and keep them going regularly.

22,088. If every plague stricken village is evacuated, the people are unable to contrast what happens in a village which is evacuated and where the people are prevented from communicating with the infected site and made to isolate their sick, with a village in which the people do not go out at all and nothing whatever is done. That contrast is not before their eyes, is it?—No.

22,089. Do you think it would be better, and have a greater educational value, if each Staff Corps officer confined his attention to two or three villages alone close to one another, and made evacuation completely effective in them, and managed to stop the plague in them within a week or ten days, and thus enabled the villagers all round to see what the maximum amount of good is that evacuation can do? Do you think that would be a better plan than the present one?—In some ways it would; but as against that you would have the fact that the people frequently evacuate themselves. As soon as they really get scared, and realise that there is a dangerous epidemic among them, they go out of their own accord without any driving on the part of the European officer. Of course, having gone out, they would not segregate in the sense that we would; they would not bring all their sick into one field and call it their segregation camp; each family would move its patients away, very likely, from the family hut, and put them in a hut 10 or 12 yards away.

22,090. Would they boycott anybody who went into the village and brought back infection, thus nullifying all the good results achieved?—They would give information to the authorities if he was a low caste man, but not if he was a Brahman.

22,091. The extent to which evacuation produces good results depends largely on the season, I think I understood you to say—that is to say, in the rains evacuation produces hardly any good results whatever?—Hardly any.

22,092. Does it depend to some extent also on the employment of the inhabitants? That is to say, if a village is a purely agricultural one, I suppose the people do not feel going out so much as if they were Banniahs and Marwaris?—No, the ryots go out quite readily, but the traders object.

22,093. Do you remember inwhat villages the plague appeared first of all, what month it was, and in what part of the district? The reason I ask is that we have had a theory laid before us that the germ, if imported straight from Bombay into a dry open *desh* village, appears to be unable to infect the village; that it requires a damp place like Lanauli as a half-way house to get partially acclimatised in before it can infect a dry *desh* village?—We have had cases brought from Bombay here, but the present epidemic came from the Sangli State, a Native State in the South. The first infected village in this district was a village called Sangwi. That was infected in October, 1896, and the infection came from Bombay. I was not there, but I think it is so. The present epidemic certainly came from Sangli, although villages have got the infection from Bombay. The infection lingered on in one or two villages in the south of the district during the hot weather of 1898. Then it got worse in Sangli, which is just over the border, and it gradually spread northwards into our district. But there have been cases of plague coming up from Bombay, and villages have been reported to have been infected owing to this. It is very difficult, however, to get the exact truth as to how a village does get infected. The following is a short account of the manner in which this district has suffered from plague. The first village to be infected was, as I have already stated, Sangwi, which has a population of about 700. A man came up from Bombay with plague and the result was that in a few days the disease spread in the village and 38 people were attacked. The first case in this village occurred in October, 1896, and the last case in February, 1897, but the disease did not spread from here to any other village. On the 16th February 1897 the village of Akhade, in the Javli Taluka, was found to be infected, and on the 27th February 1897 a neighbouring village called Waluth was found to have been attacked. Both these villages were infected from Bombay and not from Sangwi, which is more than 20 miles from either of them. About the same time cases began to occur in Phaltan, a small Native State in the north of this district. This was practically the whole of the epidemic of 1896-97, the total number of cases being only 88. The last case in 1896-97 occurred in the week ending 10th April 1897 at the village of Bahule (Taluka Patan). The district then remained free till the 9th July 1897 when plague broke out at Karad, a town with a population of 13,209. As a matter of fact, there is reason to believe that there was plague at Karad before this date, but it was not officially reported. From Karad a case went to Kaledhon, a village some 35 or 40 miles further east, and from these two centres the epidemic spread rapidly in all directions, especially around Karad. The weather was raining at the time, and owing partly to want of staff, partly to the weather, little could be done for a long time. From July to December 1897, the epidemic was very severe. It was chiefly confined to the two talukas first attacked—Karad and Khanapur. The town of Karad alone had 1,457 cases, and the Karad

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Taluka had 6,448. The total number of cases in the district during 1897–98 was 15,186. The town of Satara with a population of about 25,000, was infected from Karad, although a case was also imported from Bombay. The epidemic began in the month of October 1897, and lasted until April 1898, the total number of cases being 720, with a death-roll of 612. Evacuation was freely resorted to everywhere, both in towns like Satara and Karad, and small villages and everywhere it seemed to be the only effective measure. In other words, there was nothing for it, but for people to run away from the infected spot. The last case in Satara Town occurred on the 4th April 1898. No further indigenous cases occurred in Satara City until 7th October 1898. Since then the disease has lingered on making but little progress, the cases varying from about 3 to 10 every week, and every fresh centre being met by the evacuation of the surrounding houses. The district has practically been never quite free since its severe attack in 1897–98. No cases were reported for a few days in April and May, but plague lingered on in two or three villages in the south throughout the hot season of 1898. It gradually became worse in June and July and spread northwards until at the end of November over 100 villages and hamlets were infected. We have throughout tried to meet it by means of evacuation of the infected quarters and segregation of the sick and their relatives, and in 1897–98 we made a free use of perchloride of mercury as a disinfectant, many villages being disinfected from end to end. This year, we have used very little in the way of chemical disinfectants as they are too expensive, and their benefits open to question. We have also always insisted on ventilating all infected houses, making holes in the walls and opening the roofs. A little inoculation with Prof. Haffkine's serum was done, chiefly in Satara Town, but the people did not take to it readily, and the experience we had with it has been too small to justify any conclusions being drawn.

22,094. You spoke, I think, of the advantages of segregation in a town, but you also said that even after a village has been thoroughly evacuated, the plague seems to have the power of spreading by itself from house to house. If that be so, does that not discount to a great extent the advantage of segregation?—It does to a certain extent, certainly, but the fewer people there are in the town, the fewer there are liable to get attacked, and the fewer the plague centres from which the plague may spread.

22,095. Has it been noticed at all whether the second epidemic in this district was milder or more severe than the first one?—I think the percentage of deaths on attacks is very much the same as it was last year, but the number of cases is somewhat less; for instance, in the epidemic of 1897–98 there were 15,906 cases in the district, whereas during the present epidemic there have been up till now only a little over 10,000 cases. The cases have been fewer, but the death-rate is about the same.

22,096. (*The President.*) More virulent?—About the same virulence, I should think, as last year. About 75 or 80 per cent. of the infected cases die, I should think.

22,097. (*Mr. Cumine.*) You have a Civil Surgeon in the district. Have you any other European doctor permanently stationed in the district?—No. We have one on plague duty temporarily, and there is a Civil doctor at Mahableshwar. He is Superintendent of Mahableshwar, but he does not help us in the district.

22,098. As regards the inspection of corpses, do you think that the Natives would have much objection to inspection of corpses by European doctors?—I do not think they would have.

22,099. Do you think the Musalmans would have any objection to corpse inspection by a Hindu doctor, and the Hindus to corpse inspection by a Musalman doctor?—They would not in this district, because the ordinary Muhammadan here is of a very low type, he is half a Hindu; but in Sind, I think, they certainly would object.

22,100. Have you any reason to suppose they would object in this district?—I do not think the Muhammadans here attach very much importance to their religion, they are much the same as Hindus.

22,101. Do you think they would object to having the corpses of their women, for instance, examined by anybody, whoever he might be?—A doctor can answer that question better than I can, but I think several Muhammadan womens' corpses have been examined by European doctors. Here, in Satara, they generally are examined by the lady doctor.

22,102. If it were to be done by European doctors alone, the town of Satara is the only place where it could possibly be done, is not that so?—That is the only place where we have a doctor to do it.

22,103. The Civil Surgeon of Satara has a great deal of other work to do, has he not, in looking after the Civil Hospital and the Jail and the town?—Yes.

(Witness withdrew.)

Lieut.-Col. H. Hay, I.S.C.

Lieut.-Col. H. Hay, I.S.C., called and examined.

22,104. (*The President.*) You are in the Staff Corps?—Yes.

22,105. And Chief Plague Authority in Satara?—Yes.

22,106. How long have you been here?—Since the 7th of October 1898.

22,107. (*Mr. Hewett.*) You had some prior experience of plague elsewhere, had you not, before coming here?—Yes, I had four months in the Thana district at Bhiwandi.

22,108. What is the population of Bhiwandi?—15,000 is the normal population, but during the epidemic it went down to 7,800. I took the population by census.

22,109. When did you go there?—I went there on the 28th of May 1898.

22,110. At that time what was the strength of the population?—About 12,000.

22,111. About how long had plague been present when you got there?—They had a few straggling cases, within the month of May, for about a fortnight.

22,112. When did the epidemic begin to be bad?—About the 15th June 1898.

22,113. And how long did it continue?—It continued till the end of September.

22,114. During that time what measure did you take?—We were instructed by the authorities to deal gently with the people, without the application of force, and to try and get them to co-operate with us in checking the spread of the disease.

22,115. Were the people mainly Hindus?—No, a large number of them were Muhammadans—Momins.

22,116. Did you evacuate the town, or did you restrict yourself to removing patients from infected houses?—We treated most of the patients in their houses at their earnest request.

22,117. Did you find that that was a good measure or not?—I found that it was a very bad measure.

22,118. Why do you think so?—I have some carefully collected figures here of Dr. Ernest Hill's, the Chief Medical Officer at Bhiwandi. The collection was made at the end of the epidemic. From these figures it appears that, supposing one case occurred in a house, it was 100 to 3 against an inmate getting plague if he went at once into segregation, or if he attended his friends in hospital outside; but that if he remained there, out of every hundred, at least 16 were taken.

22,119. How many people went to hospital?—153 persons actually went to hospital, and 1,625 were treated in their own houses.

22,120. Can you say how many people got the plague by reason of being attendants on sick persons?—I cannot say that.

22,121. Did you disinfect the houses in which these people lived?—All those houses were disinfected.

22,122. While the people were actually suffering?—No, not until they either died, or were discharged.

22,123. Was there no attempt at disinfection in a house while a case was being treated?—Never.

22,124. Did you notice in Bhiwandi any mortality among rats?—A great deal.

22,125. Did that occur immediately after your arrival there?—I think I observed it about a fortnight or so

after my arrival, but others had remarked it before I arrived. In one of the worst infected quarters of Bhiwandi I remarked a bad smell emanating from a shop which had been closed for two months. I forced the door open, and on entering, discovered several dead rats. One rat was actually dying. It was crawling across the floor, and I killed it with a stick. I ordered the rat holes to be opened up, and I discovered no less than 57 dead rats in various stages of decomposition. Some of them were skeletons. They had burrowed into the walls and into the floors. Plague was raging on all sides of this shop which apparently was the centre of infection from which diseases radiated. It seemed to be the exact centre, as plague was very bad all round there. I thought that a notable case.

22,126. Were you able, by your inquiries there, to make out that the mortality among the rats began before any imported case of plague occurred?—No, I could not say that.

22,127. Did you notice that any particular classes of persons were immune?—I did. I noticed that the rice cultivators were a very strong example of that. They numbered 600 or 700, and they lived in very small huts in the midst of this epidemic. Their occupation took them out from morning till night in the heavy rains—transplanting rice. They had not a single case amongst them. They were continually in the open air, while the other people were shut up in their houses with the windows and doors closed. These people were working outside the whole time.

22,128. Did you notice that any particular class was especially subject to attack?—No; I only noticed those who lived in huts, and in inferior dwellings with bad ventilation. The worst portion of the town from a sanitary point of view was the worst infected spot. I noticed that the fakirs were immune. A very large number of fakirs come to Bhiwandi. They pass through it on their way to somewhere. I never heard of a single case amongst them.

22,129. Muhammadan fakirs?—Both Muhammadan and Hindu. They live an out-door life; and I think that is the reason why they do not get attacked.

22,130. Did you come from Bhiwandi to Satara?—Yes, straight.

22,131. What is the population here that you have had to deal with?—I have had to deal with about 19,000.

22,132. In the town?—Yes, in the town.

22,133. How many cases of plague have occurred since you came?—Only 146.

22,134. Have cases occurred among any particular class of people, or in any particular section of the town?—Cases have occurred chiefly amongst the poorer classes of people who live in the poor houses.

22,135. Have cases occurred in houses in which there were plague cases last year?—That is the remarkable thing about it. Many of the divisions which were badly infected last year are not infected at all this year.

22,136. Were the houses in which plague cases occurred ast year disinfected?—They were all disinfect

22,137. Has any instance come to your knowledge of a case of plague recurring in a house which was disinfected last year?—Yes; I can give you a case. This is the case Mr. Dodgson referred to—the Head-Jailer's house. That, I think, was a very strong example. That house became infected on three separate occasions, after having been thoroughly disinfected three times with a solution of perchloride of mercury, 1 in 1,000. It is a large stone house, built by the Public Works Department, with a galvanised iron roof, and very well ventilated. The Jailer's daughter was seized with plague on the 9th January 1898. The house was immediately evacuated, and disinfected with perchloride of mercury. When general disinfection of the whole town took place in the following month by the order of the Collector, this house was again disinfected with perchloride of mercury, but, notwithstanding this double disinfection, a second case of plague occurred on the 20th of December 1898, when another daughter of the Jailer was seized with plague and died. Again the house was evacuated, and disinfected most thoroughly with perchloride of mercury under my personal superintendence. The Jailer and his family returned to the house on the 31st December 1898, and 10 days after both the Jailer and his wife were seized with plague and succumbed, after an illness of 24 hours in the case of the husband, and 48 hours in the case of the wife. As a result of a most searching inquiry, these cases are not traceable to any other cause than the house. The family kept practically aloof from outside infection, and they were very particular about it.

22,138. Do you think it possible that any clothes remained in the house which were not disinfected?—I do not think so. Everything was cleared out, and all the belongings of the deceased were burnt.

22,139. Were all the belongings of the daughter burned in the first instance?—I believe so. It is the practice. I, however, was not here then.

22,140. Were all the clothes of these people disinfected when they went out?—They were, before they left hospital to return to their house.

22,141. I mean when they left their house; were all the clothes of the people who went out disinfected?—The clothes which they took with them to the hospital were disinfected before they went back.

22,142. Did they leave any clothes inside the house?—I am not aware of it. I saw the house was completely gutted out.

22,143. On the other hand, I understand that you have noticed that the houses which were infected last year have escaped this year?—I have noticed that in a very marked degree.

22,144. Although they are in the neighbourhood of places which were infected?—Yes, in the neighbourhood.

22,145. In addition to your work in the city, you have had to do with villages in this taluka?—Yes, and the suburbs, which include cantonments.

22,146. How many villages have been infected in the taluka?—57 villages have been infected this year.

22,147. Have you noticed that the villages in a particular situation have been specially liable to infection?—I have noticed all the towns and villages on the banks of the river Krishna in particular. I have brought a map* with me to show the Commissioners what I mean. All the villages on both banks which were infected this year are marked in red. You can contrast it with the other portion of the taluka away from the rivers, where there has been very little plague.

22,148. It would appear from the map that many of the villages on both sides of the river have been infected, but comparatively few away from the river?—Practically, it seems to have confined itself to the Yenna and the Krishna.

22,149. The chief difficulty which you find is in getting early notification of the existence of the disease, is it not?—That is the difficulty which we have; but as time goes on, and the people become educated, and understand the advantages of giving early information, we have not had that difficulty so much.

22,150. Is that difficulty due to the people not wishing te report, or to the fact that they do not know when plague is among them?—The reason is because they do not wish to turn out of the village—the inconvenience.

22,151. Do you think that is the only reason?—That is the chief reason.

22,152. Do you think that they generally know at once that plague exists among them?—I think nearly all the villagers know it now.

22,153. Have you had pneumonic plague here?—I have only seen two cases of it. I do not think there has been much pneumonic plague.

22,154. Have you tried inoculation?—Yes, we have; but to a very small extent compared with the size of the place. We have only 691 people inoculated in the city out of a population of between 18,000 and 19,000.

22,155. Have people shown no desire to be inoculated?—No. I have tried every means in my power to persuade them, even to the extent of promising readmission to their houses; they have been promised that those who have been evacuated should be allowed to return if they would be inoculated; but it has had no effect upon them at all.

22,156. Have you any results as to the effect of inoculation?—I have a few. The Pet (or ward) called Rawiwar which is inhabited by Mahars and other low caste people, has 68 houses and a population of 292. In December 1898 it was found necessary to evacuate the Pet, as cases had occurred in 13 houses. Out of this population of 292, 107 had been inoculated. As an

* Not published with the Proceedings of the Commission.

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experimental measure, at the suggestion of Dr. Thomson, I allowed those who had been inoculated to remain. No case has occurred amongst them up to date, although the area in which they have been living must have been full of plague germs, infected houses being on all sides of them, some actually touching.

22,157. Have the houses been disinfected?—Yes, with perchloride of mercury; and the roofs have been taken off—or, rather, a portion of the roofs—about 3 feet broad.

22,158. Did the people who went away without being inoculated have any plague among them?—Yes, they had; but I could not give you the numbers.

22,159. Did you place those people whom you took out of their houses in camp?—Yes, they were placed in camp at Godoli.

22,160. Do you know whether they had plague in the camp?—Yes, they had; but I do not know how many cases.

22,161. Was anybody supervising them while in camp?—Yes, we had a Hospital Attendant, and the doctor visited it daily. The medical officer would know the exact number of those people who were afterwards attacked with plague. Dr. Smith will be able to give you the information.

22,162. Do you think that disinfection is a useful measure?—Yes, as a repellent, but not as a destructive agent. The second epidemic in Satara, in my opinion, might have been as bad as the first (it is very much less this year, about one-third, or something like that) had it not been for this general disinfection ordered by the Collector of all houses, which was carried out early in 1898, before I came to Satara. I think that has had the effect of stopping the spread of plague in every house that was disinfected. I think disinfection makes the area less palatable for the microbe. That is to say, if you took two houses (one which had been saturated with perchloride of mercury, and the other had not), I think it would prefer the one which had not been disinfected.

22,163. How can this be if the bacillus does not travel about at a rapid rate?—I think it has been kept aloof, to a certain extent, by perchloride.

22,164. Would you not expect that, if a disinfectant had this repellent effect, it would also have a destructive effect?—It does not seem to have been strong enough in a case like the Jailer's house.

22,165. Can that be in any way due to your solution of perchloride of mercury not having been of the strength you thought it was?—No; I am very particular about mixing it. I have it done under the superintendence of a medical officer once a week. The strength is 1 in 1,000.

22,166. One of the measures which you think of extreme importance in dealing with plague is the system of what you have described as perfect surveillance; what do you mean by that?—The town is divided into 24 Pets, and each Pet has an honorary secretary: and under him he has so many voluntary helpers. Every morning they go round to the houses. They have a little census on the wall outside, with the number of the occupants of each house, and their names. They have a sort of roll-call every morning; and if they find an additional member of the family they inquire into the newcomer's antecedents, where he or she came from. The medical officer goes round periodically and examines those people; and in that way it has been found that plague cannot get to any great dimensions without our knowledge. I am kept informed every morning of the result of these house-to-house visitations. For 10 days after people arrive from Bombay they are under this surveillance—visited daily.

22,167. In order to carry that out you have to rely upon the active co-operation of the people themselves?—Yes, upon the active and intelligent co-operation of the people themselves.

22,168. I suppose they are moved to that by the fact that they had these two outbreaks in the town?—Yes; they work very willingly.

22,169. Supposing you were dealing with a large town which had not been unfortunate enough to have suffered already from plague, do you think that the people would realise the extreme importance of keeping an eye upon new arrivals?—No; I think this is the result of the education which the first epidemic has given them.

22,170. That would be a great difficulty in dealing with a newly-infected town, would it not?—Yes, it would.

22,171. (*Mr. Cumine.*) Were there two epidemics in Bhiwandi?—Yes, two.

22,172. Were you present at both?—No, only the second.

22,173. During what months did the first epidemic prevail?—The first epidemic was imported from Bombay, and began in December, 1896.

22,174. How long did it go on?—It lasted up to the middle of April, 1897.

22,175. How many deaths were there in all?—491 cases and 434 deaths.

22,176. Do you know to what extent the people left the town?—Nearly the whole of them left. Dr. Dalal, who was in Bhiwandi, in comparing the two epidemics, said that the mortality being so small in 1896, and so great in 1898, one would naturally ask the reason for the difference. In 1896 there were 491 cases, and 434 deaths, and in 1898, 1,788 cases, and 1,255 deaths. He explains this by the fact that plague broke out at a different season of the year in the second epidemic; and that the people of their own accord left the town and went into huts during the first epidemic. During the second epidemic we were prevented by the rains, which confined the people to the town, and which, consequenty, made a great difference in the mortality.

22,177. Is it within your knowledge to what extent the plague in the second epidemic visited the same houses as it did in the first one?—It is within my knowledge that it visited the same houses and the same quarters.

22,178. I do not quite understand your ground for saying that the treatment of the sick in their houses had a bad effect at Bhiwandi?—I will tell you. The reason was this. These houses were so close together and thickly populated that it was utterly impossible to isolate the sick. I had to institute prosecutions in several cases of surreptitious visitations—generally by night—of the friends of the patients, who would go and sit alongide the bed, and in that way help to spread the disease into the town. Constant intercourse between the sick and the healthy outside is the evil effect of house treatment. I found I could not stop it by prosecution.

22,179. Did you distinctly trace cases of new infection to a visit to a sick person?—Yes, very often.

22,180. Did you put the patients on beds?—When we could.

22,181. But not always?—No, not always.

22,182. If you left the patient lying on the floor, and if you did not disinfect the floor daily, then the treatment of the sick in the house did not have a fair chance, did it?—I do not think it had a fair chance. In many cases they were lying on cow-dung floors.

22,183. You did not disinfect the floors daily?—Not until the patient was disposed of, either by death or discharge.

22,184. You spoke of rice planters being exempt. Are the rice planters out working in their fields during the night?—No, they are not; but they spend a very short time in their houses. They get up very early in the morning, before daybreak, but they sleep in the houses.

22,185. Did you carry out any corpse inspection at Bhiwandi?—Yes, we carried it out.

22,186. Did you strip the bodies?—They were not all actually stripped; they were examined to see whether they had plague or not.

22,187. By whom?—By the Hospital Assistant. The bodies were not allowed to be disposed of until a certificate was sent by the Hospital Assistant.

22,188. Did a European always accompany the Hospital Assistant?—Not always.

22,189. Supposing no Europeans had been there, do you think the people would have permitted the inspection of their dead by a native Hospital Assistant?—I do not think that the Memons would. They would not allow their women corpses to be inspected. I have a chart* here, which I made yesterday, on the Bhiwandi epidemic, showing where the people were treated in their houses.

* Not printed with the Commission's Proceedings.

22,190. (*The President.*) Which year is that?—Last year, during the rainy season. This chart shows the result of home treatment.

22,191. Have you any other chart which contrasts with that, showing other treatment?—Not a chart of sufficient contrast. I have two charts* of the Satara city and the taluka up to date.

22,192. (*Mr. Cumine.*) Had you any cases of plague amongst the Musalmans of the better classes in Bhiwandi?—No, hardly any at all.

22,193. Had there been many cases amongst the superior Musalmans, do you think there would have been a difficulty in effecting corpse inspection amongst them?—If there had been amongst the women, there would have been great difficulty, because they were zanana women—pardahnashin.

22,194. Do you think corpse inspection could have been carried out amongst them at all unless there had been Europeans present?—I do not think so.

22,195. Supposing you had the town of Bhiwandi to deal with over again, what measures would you, with your present experience, attempt to carry out?—I could not do otherwise than I did then if it was in the rainy season. In the dry season I should make every one of them go out.

22,196. And in the rainy season?—In the rainy season I should have to use force to make them go out. A large amount of accommodation would be required. Houses would have to be built upon piles, because it is a rice-growing country, and very wet.

22,197. It is not practicable?—It is not practicable; it is impossible.

22,198. What would you do?—I should, of necessity, have recourse to the same system.

22,199. Will the inhabitants of villages which have been infected once or twice do their best to keep infection out?—Yes; they will evacuate the place of their own accord. Now, even on the discovery of dead rats, some of the inhabitants of the villages under me do not wait for plague to break out, but evacuate the place at once—immediately on the discovery being made.

22,200. When they have evacuated do they abstain from re-visiting an infected house?—We cannot always keep them from revisiting the house—that is the difficulty.

22,201. Will they go back into the infected village site?—Yes.

22,202. They have not realised the danger of doing that?—No.

22,203. Will they isolate their own sick?—They will do that to a certain extent, but not enough. They will not keep sufficiently away from the sick.

22,204. Do they think that danger lies in the clothing of infected persons?—No; I think they believe in the poisoned atmosphere of the houses.

22,205. Do they believe in having their houses perchlorided?—They are very sceptical about its benefit. They believe in sunlight and air. They have very little objection to stripping the roofs off; they believe it does good.

22,206. Would they strip off the roofs of houses of their own accord?—I think they would now—all those who have had experience of plague.

22,207. Have you tried the kiln method of disinfection?—In one case only. Yesterday I had the floor of the Jailer's house dug up, preparatory to burning it.

22,208. You have no experience of it?—No; I am trying it now.

22,209. Have you noticed in this district a clear case of recrudescence of plague as opposed to re-infection from without, say from six to nine months afterwards?—Only in the way of a second epidemic the next year.

22,210. Are the villages which are being attacked this year the same that were attacked in the former epidemic, or are they for the most part a different set of villages?—They are a different set. That is the curious thing about this epidemic. This year it passed over most of the villages which were attacked last year, and *vice versâ*.

22,211. Have you noticed that even when it does get into a village which was badly infected in a former season it does not seem to spread much in that village?—No; I have not remarked that. During the first epidemic in the taluka there were 15 villages infected, with 192 cases and 149 deaths. During the second epidemic, which began in August 1898, there were 57 villages infected, and they are at present all evacuated. Up to that date we have had 2,560 cases, against 190 the year before; and many of those 15 villages which were infected last year have not been infected this year at all.

22,212. How many of those 15 villages have been infected this year?—I am sorry to say that I have not got the figures.

22,213. Are those villages so geographically situated that you would have expected them to be attacked this year?—Yes. I can give the instance of a village called Anewadi, which was very badly infected last year, and three other villages within a mile of it, Parbuchiwadi, Saygaon, and Kharshi. Those three villages are within a mile of Anewadi, and they are all very badly infected this year. There was not a single case in Anewadi until two days ago, when there was one solitary case, which, I think, was imported. Those three villages are very badly infected, and other villages which were uninfected last year have been badly infected this year.

22,214. Have you any idea whether the freedom of this village is due to the sharp look-out kept by the inhabitants?—To a great extent. I have found them turning people back of their own accord. They have barriers across the entrance to the village. It is one of the few instances in which they have really taken the initiative themselves, and taken an interest in it.

22,215. Do you think that the plague measures which are to be successful in the future must be worked with the co-operation of the villagers, and indeed by the villagers themselves?—I think we have to a very great extent to be dependent upon their co-operation, and the education they receive from the experience of plague induces them to take the initiative.

22,216. Which do you think is of the most educational value for the people, one European Staff Corps Officer endeavouring to look after 15 villages or his taking two small manageable ones thoroughly in hand personally?—I think it depends a great deal upon the man, whether his movements are active, and whether the villages are at a distance. For instance, the group of villages which I mentioned are almost within a mile of one another. It would not be enough work for him to look after those four villages. He could take a great many more than that, and visit them morning and evening.

22,217. (*The President.*) What are the huts of the rice cultivators like?—They are mud huts, with very heavy thatch. They are very badly ventilated, and there are no windows.

22,218. They stay in them only a very short time, is not that so?—Yes.

22,219. That is, relatively to other people?—Yes. They are very early risers. They rise before daylight.

22,220. In the case of villages which escaped infection this year, and were infected last year, was thorough disinfection carried out in the huts?—Those which were infected this year simply had strips of the roof taken off.

22,221. What was done with those which were infected last year?—Only the roofs stripped off. I was not here then. I believe that is all that was done.

22,222. Was there no chemical disinfection?—I do not hink so—not in the villages.

22,223. Has it been noticed whether rats have appeared in the villages which have become infected this year?—In a great many cases rats have appeared. The village of Umbowda Budruk is a good example of a village where the disease has attacked rats before it has attacked human beings. Rats were found in a house in the village on the 23rd of November 1898, and seven days after this discovery plague broke out in the house in which these rats were found. Six members of the family died of the disease. The village of Areh also, in the Parli valley, is another instance. On the 1st and 2nd of October dead rats were found in two houses, and on the 3rd of October plague broke out in both houses, and in those adjoining them. Nine people were seized on that date. I think those are good examples. I can give a great many others.

22,224. Have any dead rats been found in the houses which were infected last year, but which have escaped infection this year?—I could not tell you.

* Not printed with the Commission's Proceedings.

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22,225. Not to your knowledge?—Not to my knowledge.

22,226. (*Mr. Hewett.*) In those villages you mention in which dead rats were found, are you convinced that there had been no imported cases?—I am quite convinced. I made a searching inquiry.

22,227. How far off was the nearest infected village in each case?—The village of Parli was nearest—I should say within four miles.

22,228. Do you think rats would travel that distance?—It is quite possible. I have seen them go long distances.

22,229. That is how you would account for the infection?—It is quite possible they brought it from another village to Parli.

22,230. Was there any communication whatever from this village of Parli except by means of rats?—Yes, people go backwards and forwards continually—the same class of people.

22,231. (*The President.*) What is the nature of the water supply of the district we are considering?—All those villages on the banks of the Krishna get their water supply from that river, as a rule.

22,232. Those villages which have been chiefly affected are in the neighbourhood of the river, and are supplied by the river?—To a certain extent from the river.

22,233. How are those villages which are not near the river supplied with water?—By wells.

22,234. Taking the supply of the villages chiefly affected, it is from the river, is it not?—Yes, from the river.

22,235. And the water supply of the villages which have largely escaped is independent of the river?—Yes.

22,236. Is there any system of sewage disposal here?—No; it is all hand lifted into the carts, as far as I know.

22,237. What is done with it after that?—Then it is carted out, I do not know exactly where—trenched.

22,238. Is there any chance of the admission of sewage into this river?—There is no discharge of sewage into this river.

22,239. The only possibility is that an infected village might discharge part of its sewage into it?—Yes, perhaps so. There is a good deal of intercourse between the villages on this river.

22,240. Is there a also good deal of intercourse between the people living in the villages on the river banks and the people who are in villages removed from the river?—There is.

22,241. I believe there are a good many temples and bathing ghats on the river, are there not?—Yes.

(Witness withdrew.)

Capt. G. S. Thompson, I.M.S.

Capt. G. S. Thompson, I.M.S., called and examined.

22,242. (*The President.*) You are in the Indian Medical Service?—Yes.

22,243. Will you state your medical qualifications?—I am M.B., M., Ch., M.A.O., of the Royal University of Ireland.

22,244. In what places have your duties been in connection with plague, and of what kind have they been?—For about seven months I was in sole medical charge of the Parel Plague Hospital in Bombay, and since 31st July 1897 I have been on plague and other duty here.

22,245. You have, therefore, come in contact with a large number of plague cases, I presume?—Yes.

22,246. Have you inquired into the question of the infection of Bombay?—That came before us very often.

22,247. What was the result of your inquiry?—For many reasons, I came to the conclusion that most probably the infection was conveyed to Bombay from Hong Kong. My reasons for thinking so are as follows. As not every ship that left Bombay during the epidemic of plague since it broke out there carried away plague patients or plague infected rats, so it is probable that not every ship which left Hong Kong carried away infected rats. Possibly some particular ship may have cleared from Hong Kong to Bombay. I think the distance all through would cover the incubation period of plague, so it is not likely that a patient can have brought it from Hong Kong to Bombay.

22,248. You think it would be too distant?—Yes, without plague symptoms being developed on the voyage. Of course, there is the possibility of concealment and mistaken diagnosis. But the infection may have been brought by plague infected clothing, or by rats. In this connexion I refer to a number of ships which left Bombay, I think about ten only, on which plague had broken out, and only one barque which left Calcutta during the time Calcutta was infected. I think, therefore, plague rats brought plague into Bombay and landed at Mandvi, where grain is stored, and which is near to the docks, and thereby infected Bombay rats, which conveyed the infection to the houses of the people who afterwards became infected.

22,249. Mandvi was the district first infected?—It is considered to be the first place which was infected. At any rate, that is where plague cases were first noticed. The objections to that are, that intermediate ports, such as Singapore and Rangoon, were not infected before Bombay. My explanation of that is that they are not grain exporting ports, and of course, it is possible that the particular plague-infected ship which infected Bombay may not have called there, supposing it was only one, whereas Bombay for five months of the year does export grain. Therefore, there was an inducement when the plague-infected ship came to Bombay for the rats to land there, to forsake their dead, and try to seek new quarters.

22,250. You have considered the other possible sources of importation, I believe?—First, I may mention that it can hardly have been imported from one of the Himalayan villages by pilgrims. That possibility has been mentioned to you. The towns and villages which the pilgrims visited *en route* must be equally, if not more, insanitary, and as fitted to propagate the epidemic as Bombay, and the inhabitants must be equally susceptible to it, and yet they did not become infected. On the contrary, we find that plague spread from Bombay northwards towards those towns and villages. Then again, I know, as a matter of fact, that those pilgrims are particularly immune to plague by sleeping in well ventilated temples or in the open air.

22,251. What would be the shortest period of time in which a person might come from that district to Bombay?—These people would naturally travel on foot and take months.

22,252. We will take the shortest possible time. Suppose that some generous person were to pay their railway fares?—Perhaps they could do it in seven days, but I am open to correction upon that statement.

22,253. You think that it might be done within the incubation period of ten days?—It might be, but, as I have stated, it is not at all likely that these people would come by that means.

22,254. Have you considered how plague was imported into Satara?—It was introduced from the districts. The districts were affected probably in the month of July, especially about Karad. Before July 1897 there was a missionary on a visit there, and he reported to the then Collector that there were many deaths of suspicious fever, but steps were not taken until later on, in fact, not until two months after that was reported. I was not here at the time. The place at which plague was introduced, according to the official account, is, as the Collector, I think, mentioned, Mani Rajuri. It spread to the talukas, and then it was brought in definitely to Satara.

22,255. How many cases occurred in the first epidemic?—We had 719 attacks up to the end of February 1898. I can complete the figures up to the end of the first epidemic.

22,256. Will you complete the mortality also?—I have the table of both.

22,257. Can you give the numbers now?—Up to date there have been 1,160.

22,258. Can you give us the totals of the first and second epidemics separately?—I can do that, but I

have not the figures now. I consider the first epidemic ended on the 1st April, and then we had dropping cases in May and June. Afterwards, we began to have others in the rainy months, and then the second epidemic began. I can produce a statement. (The figures afterwards supplied by witness are as follows:—)

SATARA MUNICIPAL LIMITS.

First Epidemic, 873 cases; 727 deaths,
Second Epidemic, 287 cases; 209 deaths,
up to 31st March 1899.)

22,259. Can you tell us the total number of attacks during the first epidemic, and the case mortality?—The case mortality was about 66 per cent, I think.

22,260. When did the second epidemic commence?—We had one case on the 8th August, another on the 10th, and another on the 16th, and so on. The second epidemic really began about the 27th September 1898. It is rather curious that that is the same date on which the first epidemic began.

22,261. Were the cases which occurred in the interval imported cases, or cases which seemed to have originated here?—They were indigenous cases.

22,262. Were your means for preventing importation so perfect that you could be certain that there was no further importation?—We had a definite history of plague rats travelling in the direction in which the first cases occurred, and we had no history of importation. The patients first attacked certainly did not come from outside, and we had no history of persons visiting or sleeping in their houses. The roads were well guarded on all occasions. We had quarantine to keep out plague.

22,263. Have you observed any facts with regard to immunity from plague amongst human beings?—Amongst Europeans and Natives, I think, I have noticed it. In the last epidemic, although we had 45 adult Europeans, 20 European children, and 125 European soldiers, not one was attacked, although 20 Europeans and one lady nurse were daily engaged in plague operations. They remained completely immune. I should like to mention that Europeans now and at former times are not equally susceptible.

22,264. Will you state your views about that?—I can bring proof that in Surat in 1689 Europeans were not attacked; that is stated in Snow's Report,* and in Bombay we know they were reduced to almost one half. Regarding the comparative immunity of Europeans now and in the past, I am unable to prove that Europeans lived under the same sanitary conditions as regards ventilation (although from the Barracks Commission Enquiry, and the old Barracks, *e.g.*, in Bombay, now used as Commissariat godowns, we may infer that ventilation was not provided for in the past as now), but we do know from certain annexe to bungalows that the relations of Europeans with certain classes of natives was one of greater intimacy than happily prevail now-a-days. This may have led (as in Parel Hospital it undoubtedly did to three sepoy ward-orderlies being attacked) to Europeans falling victims in previous plague epidemics. My explanation is from the historical evidence in a well-known book, Moor's "Hindoo Pantheon."

"'In Bombay and Western India,' by James Douglas, vol. ii. p. 253. (Sampson, Low, Marston, & Co.), there is a footnote as follows:—

"Bombay: Change of Fashion (1810):—'This 'pleasant and salutary article (cow dung) is falling 'into disuse with the English, who, in their habitations 'and habits, are departing more and more from the 'sober dictates of nature, and the obedient usages of 'the natives.

"'We now, for instance, build lofty rooms, admitting 'insufferable glare and heat through long glazed windows fronting the sun, reflected by marble or polished 'floors; domestic comfort is sacrificed to exterior 'decoration.

"'No man of taste would now build a low sun-excluding verandah, nor mitigate the intensity of the heat 'by a cow-dung flooring. In Bombay the delectable light 'that, 20 or 30 years ago, was so commonly admitted 'through thin, semi-transparent panes, composed of 'oyster shells, is no longer known among the English 'except in the church; and these, perhaps, will, when 'the present worthy clergyman shall vacate his cure, 'give way to the superior transparency of glass. The 'church will then be like our new houses, insufferably 'hot, and the adaptation of punkahs . . . will be 'necessary.'"

In marked contrast to this, the houses of the first factors or settlers at Surat, as attested by the ruins of some, and the continued standing of others, were large, roomy, and well ventilated. Europeans at the beginning of this century everywhere in India may reasonably be supposed to have maintained the same standard of personal cleanliness.

Then we had no Eurasians, Native Christians, missionaries, or their families attacked, and there were 527 such in Satara city and cantonment. Thirdly, no Parsees were attacked. They comprised 50 souls. Of the hospital servants, ward boys, sweepers, corpse bearers, ayahs, &c., of whom there were from 60 to 65 in the first epidemic, only one was attacked, and that is mentioned because it can be clearly shown how he became attacked. Again, none of the jail population were attacked.

22,265. How many prisoners were there?—There were 198 prisoners. Then, again, the depôt sepoys, the 3rd Bombay Light Infantry, with their wives and families, remained immune; total 385. In the first epidemic the Sadr Bazar remained free.

22,266. That is a locality. You were speaking with regard to people?—Two other classes of people who, I think, remained practically free were the Buruds (cane workers), of whom there were 1,060, and the Dhangars (shepherds). The Buruds are said to be a dirty and degraded class, living in miserable huts and very poor, according to the Gazetteer. The Dhangars generally go out and attend their flocks during the epidemic months, and the Buruds lived in the outskirts of the village, and it is for that reason, I think, they were exempt, because they lived in the open air during the epidemic months. When we had the plague here they left the city. The sweeper caste are not so much affected as one would expect. On the theory that it is a filth disease, one would expect a great many more sweepers to be attacked. In Satara, during the first epidemic, we had 214 Brahmans attacked out of a Brahman population of 5,249. I quote from a table giving the plague attack by castes in Satara city from the first epidemic.

22,267. Did you ascertain the number of the members of each of these castes?—The Buruds in Satara number 203, and there are 186 wandering beggars actually living in Satara. They visited the town every day, and they come here twice a week for alms into the town—Tuesdays and Thursdays, I believe—and not one of them has been attacked; and I have frequently inquired into the matter.

22,268. Is there any other occupation which seems to affect the incidence?—No.

22,269. Will you kindly put your table in?—Yes, it is as follows:—

PLAGUE ATTACKS and DEATHS, by CASTES, in the SATARA CITY, 1897-98.

Castes, with English Equivalents.	Population.	Attacks.	Male Deaths.	Female Deaths.
Brahmans	5,249	214	102	81
Mahrattas	6,048	232	96	86
Musalmans	2,789	64	31	25
Mangs, night-soil labourers	656	6	3	2
Dhers, tanners	408	4	1	0
Malis, gardeners	589	17	8	6
Rajputs	25	19	2	6
Janjams, worshippers	05	2	0	
Banniahs, money-lenders	437	18	8	7
Goldsmiths	621	5	2	3
Shoemakers	440	3	1	2
Gujars, money-lenders	333	24	11	6
Washermen	126	4	3	1
Tailors	1,772	31	10	15
Kasars, brassfounders	527	11	5	6
Butchers	18	2	1	1

* *See* p. 66 of "Report on the Outbreak of Bubonic Plague in Bombay, 1896-7," by P. C. H. Snow, I.C.S.

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PLAGUE ATTACKS and DEATHS, by CASTES, &c.—*cont.*

Castes, with English Equivalent.	Population.	Attacks.	Male Deaths.	Female Deaths.
Marwaris, money-lenders	321	5	2	3
Barbers	444	14	4	8
Mahars, night-soil labourers	525	6	4	0
Shepherds	104	4	0	2
Sangars, blanket sellers	221	11	4	5
Telis, oil pressers	527	11	7	3
Karanjkirs, saddlers	45	7	4	3
Tinmen	57	9	0	0
Parbhu, Gujarati Brahman	113	4	2	2
Tambolis, betel-leaf seller	62	3	1	2
Carpenters	212	6	2	4
Banjaris, grain traders	197	6	0	1
Mehtars, sweepers	140	9	6	1
Alari, metal moulders	43	1	0	1
Christians	57	1	0	0
Pedlars	11	1	1	0
Blacksmiths	84	1	0	1
Gosain, beggars	128	—	3	1
Masans	60	—	1	1
Jewellers	13	—	1	0
Lonari, coal merchants and burners.	210	—	0	1
Coppersmiths	57	—	6	3
Fishermen	367	—	2	2
Lakheri, bangle seller	40	—	1	1
Hymn singers	71	—	1	0
Palwegars, silk-workers	—	—	5	3
Shenni, fish-eating Brahman	—	—	1	0
Weavers	149	—	1	2
Potters	172	—	1	2
Totals	—	—	344	300

The under-noted castes furnished no plague cases:—

Castes.	Population.
Parsee	63
Jews	15
Buruds, basket-makers	203
European Christians	155
Goanese	70
Gaolis, milkmen	126
Europeans	102
Madrasis	13
Arabs	17
Eurasians	36
Naikinis, prostitutes	33
Bairagis, wandering beggars	186
Total	1,018

22,270. You spoke about immunity with regard to locality?—I refer to Bombay. The Halalkhors and the people who live on ships have been particularly immune. That I can account for, according to my theory.

22,271. What about the Halalkhor class?—They are a sturdy class, and live in municipally constructed huts or houses, which are better ventilated. Dr. Weir has referred to that.

22,272. With regard to ships?—Of course, they are well ventilated too.

22,273. Have you anything to say about localities?—Last year the Sadr Bazar here, only a mile distant from the plague-stricken city, remained immune, with a population of 2,335. There was constant communication between the city and the Sadr Bazar. The reason for that was the fighting ahead of plague in those places; the houses were unroofed in strips, to let in sun and air, and, therefore, they were well ventilated. That was not done this year, and the Sadr Bazar has had an epidemic. There was no fighting ahead of plague this year, although I advised it as regards the Sadr Bazar, and they had a severe epidemic about six weeks afterwards.

22,274. Can you give us any explanation which would cover all these cases of immunity?—It depends on the habits of the people. Referring to the castes, it depends on the condition of ventilation of the houses and the habits of the people as regards their knowledge of the necessity for fresh air and cleanliness in and about their houses. Of course, the districts which were exempt lost year were compulsorily ventilated by unroofing the houses by strips at a time, and holes being knocked into the walls before plague came.

22,275. What have you to say with regard to cleanliness?—I do not say that filth has nothing to do with the causation of plague, because we know that filth spoils the air and makes it unfresh equally with deficiency of fresh air, overcrowding, or bad ventilation.

22,276. You think that the explanation common to the several instances of special liability to plague is to be found in impurity of the air?—Yes.

22,277. And conversely, that the relative purity of the air explains the instances of impunity from plague which you have mentioned?—Yes.

22,278. Have you any views to advance with regard to the diffusion of plague?—I think it is chiefly spread by human agency, including under that term infected clothing and bedding. I think, also, it is spread by animals to which the germ is pathogenic — rats, squirrels, and, to a lesser extent, monkeys.

22,279. Have you any clear cases in which the clothing appeared to be the medium of conveyance?—As regards Satara I have not a specific instance to give.

22,280. Have you any instance elsewhere?—I think not, except, historically, from the literature of plague.

22,281. Can you give us an instance in which rats conveyed plague?—I think rats conveyed the plague to Satara city from the first infected places here this year. Before the plague began in the city, I used to get 20 rats in a week to examine from the houses in the city next to the Post Office and next to the infected suburbs. I examined these by bacteriological examination and *post-mortem*, and found invariably that they had died of, or were subject to, plague.

22,282. What did you find in the *post-mortem* examination?—Slightly inflamed glands in the inner side of the thigh, and enlarged spleen. Those are the signs we used to go on in Bombay. That was confirmed by cultures.

22,283. What did your bacteriological examination include?—Growth on agar.

22,284. And the ordinary morphological characteristics, I suppose?—Yes; I had no means of going into it thoroughly with a microscope. We had not an oil-immersion lens.

22,285. Therefore, you trusted chiefly to cultures?—Yes; the characteristic culture on agar.

22,286. I think you have some evidence to give with regard to the possibility of animals in a lower biological scale being infected?—I made *post-mortems* with the cultures on squirrels and also on leeches which had been applied to plague buboes before they had suppurated, and I have made what I thought characteristic growths that I got from those leeches afterwards. I made a *post-mortem* on two monkeys, only one of which suffered from plague; the other had not died of plague.

22,287. You did not do anything more with regard to leeches beyond making cultures?—I had no means of doing anything else. I wrote to Dr. James and he could not account for a village, a great distance outside the cordon, being infected by plague, and I suggested that perhaps the leeches used in the plague infected camps were taken outside the cordon and used for other diseases. He inquired into the matter and said that he had strong reason to believe that that was the cause of the villages at a great distance becoming infected.

22,288. What measures have you found most effective in dealing with plague in Satara?—Evacuation is the first and most important to be carried out quickly and effectually. But one must have an idea of what it means. The word is in everybody's mouth. The reason, I think, it is successful is, that first it removes the people from their infected houses and thereby diminishes overcrowding, and if they are put into Government constructed huts, or huts which are

properly constructed, they get into fresh air, which neutralises the plague. Only under those circumstances will the measures be successful.

22,289. You wish to point out that the mere fact of evacuation is not sufficient, but there must be certain things attended to in that evacuation; what do you think is the most important?—That the Government huts should be ready for the whole population of the area to be evacuated. It is useless removing them from structurally insanitary houses to huts equally air-tight, because that would defeat the benefit of evacuation almost entirely.

22,290. Is that the result of your experience?—It is from my experience of Satara, and some villages which I know continued to have plague after evacuation.

22,291. Can you narrate your experience?—The town of Limb was completely evacuated, and yet plague continued for months afterwards, because the people were allowed to hut themselves in an area of 5 miles round the village, and they did not go into Government constructed huts. They continued to have plague for months after evacuation. Plague lasted eight weeks before evacuation with 45 deaths, and 22 weeks after with 521 deaths.

22,292. How do you account for that?—Because they went into badly ventilated huts.

22,293. What were the characteristics of these huts which produced the bad ventilation?—First of all their structure. They were made of bamboo poles, and they used matting for the walls, while the roof was constructed of grass. We leave air spaces on the top, but if the people are allowed to build their own huts they will build them of karbi, and daub them inside and outside, and make them as air-tight as possible, with very small openings, and they will also overcrowd them.

22,294. That was the condition of the huts in the instance which you have given?—Yes, of those huts in which plague continued to occur. I only visited those places occasionally, and had not many opportunities of verifying these facts. There were other huts constructed as the Collector described, with only a few stalks of this karbi and those remained free from plague so far as I can remember.

22,295. Have you any definite instance in which the evacuated people continued to have plague among them where that continuation was, in your opinion, due to want of ventilation in the huts they occupied?—The town of Rahmatpur, of 6,670 inhabitants, had plague for 15 weeks after complete evacuation owing to the insanitary huts to which they went. Before evacuation plague had lasted seven weeks, and there were 106 deaths before, and 162 deaths after evacuation.

22,296. Will you describe those huts?—They have the same characteristics as those of Limb which I have described. I have seen them.

22,297. Have you any experience of partial evacuation?—We had only partial evacuation of a large town like Satara, and yet plague continued to spread to non-evacuated districts.

22,298. Was that partial evacuation accompanied by disinfection?—Case disinfection only, that is to say, we disinfected the houses in which plague cases had occurred, as soon as possible after a case of plague was reported.

22,299. Were the cases of plague removed?—Yes. Isolation and segregation were carried on at the same time.

22,300. Within what time were the plague houses disinfected after the removal of the patients?—In the beginning the houses were disinfected the same day or the next day, but as the number of cases increased the work, fell greatly behind; we could not get coolies here.

22,301. Partial evacuation with disinfection failed to stop the progress of plague?—Yes.

22,302. What do you mean by disinfection?—Chemical disinfection was used on this occasion.

22,303. Were there no instances in which disinfection was carried out by sunlight?—I tried to get the Collector to allow me to have that done, but he insisted, according to the order of the Plague Commissioner, that chemical disinfection must be carried out in every house in which a plague case had occurred.

22,304. To the exclusion of every other form of disinfection?—Both measures were adopted.

22,305. You opened up the roof as well as applied chemical disinfectants?—Yes.

22,306. What was the chemical disinfectant you used?—A prepared solution of perchloride of mercury, one in 1,000.

22,307. Who is responsible for the preparation?—At first we had men who came from another district who had had experience in this matter. They were natives. Then about the month of February we had a European who came from Karad, and he superintended the mixing of it under my supervision.

22,308. Will you tell me exactly what was done?—First, a solution of perchloride of mercury dissolved in water, with ammonium chloride, was made up and stored in stone jars and corked, and from those a certain measure was taken and added to a kerosine-oil-tin full of water, and that made up a strength of one in 1,000.

22,309. It was stored in those jars until the perchloride of mercury had been entirely dissolved?—Yes. It was made at night in order to be used the next day—a strong solution.

22,310. Are you certain that the perchloride of mercury had been dissolved?—Yes.

22,311. How do you know?—Because I emptied out the contents of the jar the next morning in the first few instances, to see if it had really been dissolved.

22,312. It was an opaque jar, was it not?—Yes, but we emptied it into a vessel so that we could see it.

22,313. And anything solid would be removed from the jar, you think?—Yes.

22,314. Will you describe the process?—We have a certain measure of water, we put it into the jar, and then added chloride of ammonium to it, and then perchloride of mercury in the proper proportion.

22,315. In what form was the perchloride of mercury?—Solid.

22,316. Was it in crystals or powder?—Powder.

22,317. Was the jar shaken?—It was mixed up with a wooden stick with a glass tied to the end of it.

22,318. So that you made certain it was all dissolved?—As far as we could possibly do so.

22,319. There could be no doubt about it?—I should think not. A certain glass measure of that was added to one kerosine oil-tin full of water, and that made it one in 1,000.

22,320. How was this solution of one in 1,000 applied?—We had a supply of wooden pumps and tubs, which were supposed always to be the only things used in applying it. But when the pressure came we had to resort to brass pumps.

22,321. You pumped it with considerable force?—Yes.

22,322. Where?—On the wall, roof, ceilings, and especially the floor. I laid great emphasis on that, that the floor was to be done more carefully than anything else.

22,323. Nothing else was added to this solution?—Very often the coolies did defeat my object, because they put the pump on the ground and then took it up covered with mud, and then put it in. I had, at last, to tie the pump to the tub; in fact, I took every precaution, although I did not believe in disinfection, to give it every chance in a practical way, and I recommended the re-disinfection of plague houses.

22,324. When you discovered this possible source of error you had it corrected as far as possible?—Yes.

22,325. Why do you not believe in disinfection?—Because although experiments in laboratories will kill the plague germ, I was doubtful, from the practical experience I had had, that it would kill the germ under the natural conditions in which we found it in native houses mixed with cow dung floors and so on, and further experiments in Haffkine's and Hankin's laboratories have fully confirmed that view.

22,326. At present, you do not believe in chemical disinfection for plague?—I do not say it is of no use, but it is unreliable.

22,327. It is not a certain measure?—It is not to be relied upon at all. There are better disinfectants, such as sunlight and fresh air, which are more cheaply available.

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22,328. Have you anything further to say with regard to chemical disinfection?—Experiment's from Haffkine's and Hankin's laboratories are before you, and I have nothing more to say except from practical experience. I have some cases in which plague occurred in houses in Satara which were disinfected, and I can put a statement of them in. I have also some cases which occurred outside what is considered the ordinary incubation period of plague, that is to say, more than 10 days.

22,329. Does that deal with chemical disinfection?—The houses were disinfected and the people were allowed to go back, and 15 or more days afterwards plague cases occurred in the same house.

22,330. Of course, you eliminated all other sources of infection?—Yes, the cases are given in the following table:—

Some Instances of Plague in Disinfected Houses.

Name.	Pet.	Date of Disinfection.	Date of Attack.	Days after Chemical Disinfection.
N. M. Bhosale -	Somwar -	15.2.98	22.2.98	7
P. Kodha -	„ - -	15.2.98	21.2.98	6
K. K. Maruti -	Yadagopal -	18.2.98	23.2.98	5
A. K. Krishnaji -	Pratapganj -	20.2.98	25.2.98	5
S. R. Lawzekar -	„ -	12.2.98	26.2.98	14
S. K. Sitaram -	„ -	21.2.98	3.3.98	10
P. K. Gulabsing -	„ -	21.2.98	16.3.98	23
B. W. Subhana -	Durga -	27.2.98	26.3.98	27
D. N. Jagatap -	Mangalwar	15.2.98	8.3.98	20
G. Rupachaud -	„ -	23.1.98	27.2.98	35

The Jailer's house was disinfected by one in 1,000 solution of perchloride of mercury, ceiling, walls, and floors, including the verandahs, front and back, on 18th and 19th January 1899, once; and again on 31st January 1899; yet the Jailer and his wife were attacked by fatal plague on 14th February 1899. This house had been disinfected in 1897, in the month of December, by mercuric perchloride solution, so that the effect of that disinfection was not permanent, although it appeared to be effectual, and by some was credited with stopping further attacks there, as three plague attacks took place in this house in the 1898-99 epidemic. The house was disinfected in the presence of the Chief Plague Authority in January 1899, and the floors twice, the second time with fresh perchloride solution.

22,331. What are the clinical varieties of plague which you have encountered?—I divide them (in addition to primary plague pneumonia or malignant plague) into cerebral, or nervous, and that, has two sub-varieties, quick and slow. I mean by "quick" that it kills a person in a few hours or a day, and by "slow" that the patient lingers on for perhaps three or five days, or recovers. Secondly, there is the septicæmic form, which is also quick and slow as regards its progress. In both these the infection, I think, goes into the blood.

22,332. Why do you think so?—It is only theoretical. We have a supposition of how these diseases had affected people, and we try to account for the fact that they kill people quicker than others.

22,333. By what kind of reasoning have you arrived at your opinion?—From these two diseases occurring without any previous symptoms, and affecting people quite suddenly and killing them off very quickly indeed.

22,334. You mean the rapid development of the disease?—Yes.

22,335. Are there any other forms?—Thirdly, there is the ordinary mild bubonic form, which probably infects through the lymphatics. Then there is a fourth variety which we have identified, which is very rare; that is the abdominal or typhoid, which is also quick and slow.

22,336. Why do you think the third variety is caused by conveyance through the lymphatics especially?—That is a generally held opinion, but I myself have objections to its being conveyed through the lymphatics, because I think the most common method of infection of plague is through the air, although I cannot deny that plague may be conveyed through inoculation from the lymphatic channel, and rarely by cuts and abrasions in the extremities of natives. I have had instances of that.

22,337. Your opinion is not very strong on that?—I think that is the rarest form of infection.

22,338. What is the evidence of the existence of the abdominal or typhoid form?—I have had some cases among the Natives here. Altogether, I have seen one European case infected, and three Natives out of about 750 cases of which I have clinical notes.

22,339. What are the distinguishing clinical features of this form?—General typhoid symptoms and slow development of the buboes. A case is fully reported in the first epidemic. In General Gatacre's Report* I have recorded one case in full, and I have notes of others which I have seen here.

22,340. Could you state what you think are the characteristic features?—The details of this case are confirmed by the other cases which I have seen here of plague, simulating enteric fever. The patient was attacked with giddiness, trembling, chilliness, headache, repeated vomiting of bilious matter, and prostration. This continued for a few days. Then there was diarrhœa of a thin, yellowish, watery, fœtid character, six motions in 24 hours, with griping and gurgling; he was delirious, with an elevated temperature, of course. There was repeated vomiting, and nausea was constantly present. The tongue had a yellowish-brown fur with moist tip and edges clean, the skin hot and clammy, bad odour from the breath, peculiar earthly smell from the bedclothes when turned down. There was a headache and drowsiness. The patient wanted to sleep but could not. The eyes were injected.

22,341. There is nothing there, except diarrhœa, which is distinctive, is there?—The general appearance of the typhoid state, that is to say, that the patient waved his arms aimlessly before his face, sinking down in the bed and fumbling and picking at the bedclothes, with enteric motions which were acid to reaction. There were no typhoid spots, although they were looked for.

22,342. To bring it into a group of plague cases there appears to be nothing but diarrhœa?—And the typhoid symptoms.

22,343. Typhoid symptoms in as far as there are some nervous symptoms?—Yes.

22,344. Have you seen any pneumonic cases?—I have seen 19 cases of primary pneumonic plague, and only one recovery. I wish to mention the importance of marking thoroughly the difference between that and the other varieties of plague.

22,345. What are the distinctive symptoms of plague pneumonia as distinguished from ordinary pneumonia?—A very small area of the lung is affected, as is shown by clinical examination, and the sputum is frothy and bloody, and in culture contains myriads of plague bacilli. It is the malignant form of plague.

22,346. How is it a malignant form?—From comparing it with small-pox, where we have discrete and confluent, and so on.

22,347. Have you found the pneumo-coccus of pneumonia in the sputum?—The German Commission worked out this in Parel, but did not find it.

22,348. Have you made any examination?—No, I have no microscopes.

22,349. Have you seen any very mild cases of plague?—In Parel one very mild case I saw in which the patient was all right in three days and attended to his work. He was a sweeper in the Parel Hospital. I have not seen any here.

22,350. Was any bacteriological examination made?—Yes.

22,351. Have you seen any cases of enlarged glands, which in some respects simulate plague, and could not from the symptons be distinguished from plague?—In Bombay some cases during the first epidemic were sent in as plague which I was able to prove by bacteriological examination were not plague, although they had enlarged glands.

22,352. Do you think that proof can be obtained only by bacteriological examination?—Now, I am of that opinion.

* *Vide* pp. 86–7 of "Report on the Bubonic Plague in Bombay 1896–97," by Brigadier-General W. F. Gatacre, C.B., D.S.O., Chairman, Plague Committee.

22,353. You think there is a disease involving the lymphatic glands, sometimes also accompanied with high temperature, which is indistinguishable from plague except by bacteriological examination? — Quite so.

22,354. Have you any evidence as to the incubation period of this disease?—We take the incubation period from the first case we see generally, and count from that, but it is obvious that patients who have subsequently developed plague have been equally exposed in the plague-infected house as the first patient. Counting in that way we generally find that the attacks develop within ten days, but I have some examples in which it went beyond that period.

22,355. With regard to the etiology, do you think there is any relationship with such circumstances as famine, rainfall or subsoil water?—As regards war and famine, these visitations drive the people into the towns and they overcrowd the towns, and then, if plague happens to come along, naturally the overcrowded town will be more subject to attack.

22,356. Have you any facts with regard to rainfall?—No; I do not believe there is much relation between the two except that the life of the germ outside the human body seems to have something to do with the state of the moisture of the ground. It is in a hybernating condition during the rain, and after the rain it becomes virulent, and becomes able to attack people. The idea is that it is always in the soil, and is alive in the soil, and in rats and other animals outside the human body, and thus the epidemic is continued after perhaps a few dropping cases, and then, subsequently, next year, when the weather conditions are favourable, and when the people go into the ill-ventilated houses, the plague is able to attack them, as we found in Satara.

22,357. Why do you think it occurs so much in seaport towns and large towns?—That is natural, because we know that cities have been built for the sake of commerce on the banks of the river and that these are the emporiums for import and export into which plague will naturally be brought first by communication with other infected places.

22,358. How do you think the virus actually enters the body?—Principally through the air, although I cannot deny that there are other methods by which it may enter.

22,359. You mean, it is inhaled into the lungs?—Yes; that is the most common method, I think.

22,360. Why do you say that?—From the absence of evidence of other means of infection, except in very exceptional instances. One other possible means mentioned is inoculation through cuts and wounds in the extremities, but very few cases confirmatory of that have been found, and in some of those I have been able to recover the plague bacillus from a blister raised on the opposite limb, showing that it was a case of generalised septicæmia, and that this was really an instance of black blister, of which I have seen five or six examples here.

22,361. It did not, in your opinion, indicate the entrance point at all?—Not at all. Besides that, I have many reasons against that theory, and my objections are stated together as follows:—

1. The inoculation theory cannot explain the fact, that fever and general symptoms precede the appearance of the local expression of the lesion of infection, if such exist, and this is the sequence in all the cases of plague that I have seen.

2. In those cases where plague bacilli had been isolated from the supposed initial lesion I was able, at the same time, to demonstrate the presence of plague bacilli in the serum of a blister raised upon the opposite limb; proving that the patients had plague bacilli generalized in their blood, and were, in fact, suffering from septicæmic plague, plague with black blister. All those patients died, which is confirmatory of this view.

3. On this theory of cutaneous infection the occurrence of buboes in sequence, with intervals, and in sites in no way anatomically related with the first bubo or site of supposed inoculation cannot be explained satisfactorily.

4. Whole families are attacked simultaneously or within ten days or so of each other, and it is difficult to believe that all of them should exhibit cutaneous lesions sufficient for inoculation at or about the same time.

5. The rarity of such lesions, although carefully looked for, is an important objection to this view. At Parel Hospital only five such instances were discovered amongst 304 patients, and many medical officers studying plague there were daily examining patients to discover evidence of this mode of infection; and the medical subordinates were instructed to critically examine all admissions for slight wounds, &c., below the affected lymph gland. In the Satara hospitals such lesions were even rarer than at Parel.

6. "*a.* It is difficult to believe that any large proportion of the inhabitants habitually have lesions of the skin—even though minute ones suffice—suitable for the absorption of the infection.

"*b.* We know that in septicæmias and other external infections, a decided lesion is required, and that, even though this be present, infection does not always occur. It seems very improbable that, in any large proportion of those having the smallest lesions, it would chance that those should become infected.

"*c.* Were inoculation by very slight cutaneous lesions the usual mode of infection, the plague would be even more destructive than it is.

"*d.* When once established in a place, the disease has spread more rapidly than if it were diffused mainly by external inoculation.

"*e.* The rarity of the occurrence of plague among those of the upper classes of natives who live in houses of a superior class, and who do not ordinarily frequent insanitary places, yet whose habits as regards the infrequent use of shoes, and in certain other ways, resemble those of the poorer classes.

"*f.* Women's duties as attendants on the sick must have exposed them to the dangers of cutaneous inoculation far more than the men; yet they were (and generally are) attacked in nearly equal numbers. For, of those attacked in Karachi between the ages of 20 and 55, one in every 21·5 of the population were men, and one in every 23·5 were women. Presuming external inoculation to be the rule, it, therefore, appears that it is not more frequent among women who tend and handle those sick with plague, than among men who do less of those duties, which is scarcely credible.

"*g.* The still greater infrequency of inoculation among the staff and attendants in a Plague Hospital; were external inoculation the common mode of infection, it would be reasonable to think that, in Indian hospitals at least, the paid attendants, and even more commonly, the relatives of patients, would, owing to their ingrained disregard of precautions, have frequently succumbed. This immunity appears to prove the truth of aërial infection and to seriously weaken any evidence that exists in favour of internal inoculation. For whilst the conditions favouring the one are, in a hospital, very greatly diminished, those conducing to the other continue to exist for all alike with but slightly diminished strength.

"*h.* The occurrence of infection by way of the lungs is acknowledged by the supporters of external inoculation; and the possibility of the former being granted, it seems somewhat unreasonable, looking at the greater facility with which it would occur, to question its universality. It may be conceded that pulmonary inoculation does cause, in certain instances, a special type of the disease, the chief feature of which is primary pneumonia. But it would seem that it has yet to be proved that the ordinary form of the disease is not commonly conveyed through the lungs. That a general affection of the lymphatic glands cannot result from pulmonary infection does not seem probable.

"*i.* The analogies of other contagions do not seem to favour the external inoculation theory. In none of the other specific fevers does this, normally, occur; whereas in those diseases in which it is possible or usual, local manifestations of infection are the rule (tubercle, leprosy, syphilis, septicæmia, anthrax, diphtheria), or they are entirely absent (tetanus, hydrophobia). But, in their general character, the infections above-named differ entirely from those eruptive fevers (typhus, relapsing fever, mumps) which plague most resembles; and there is no reason to believe that, in any of the latter, infection takes place by external inoculation. In mumps one special gland becomes inflamed, yet it can scarcely be argued that this is due to external inoculation. Why, in a similar manner, should not the whole lymphatic system be affected in plague through pulmonary inoculation?

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"*j*. The rapidity with which plague spreads among families, and among occupants of the same dwelling, seems to support causation by aërial infection. Were external inoculation the usual mode of infection, it seems very improbable that the coincidence of the time of exposure of cutaneous lesions to infection would occur in so large a proportion of cases. Whereas these are easily explained by the theory of pulmonary infection, to which all those inhaling plague-poisoned air are exposed from the first moment of its inspiration.

"*k*. Though cutaneous inoculation may occasionally happen, its general occurrence would seem improbable." The arguments 6(*a*) to (*k*) are quoted from a report* by Surgeon-General Bainbridge.

Professor Aoyoma, in Hong Kong, inoculated himself accidentally and died of plague, and Dr. Stricker, in Bombay, during a *post-mortem* examination, cut his finger and got a mild attack of plague from which he recovered. Animals are easily inoculated with plague. Surgeon Wilm, in reference to the Hong Kong epidemic of 1894, relates that "About 300 Europeans "were employed in disinfecting operations and all "wore boots and putties, yet the few who were affected "all had enlarged glands in the groin. It is extremely "unlikely with such precautions that they had cuts, "&c., on their feet and legs, but they must frequently "have had cuts, &c., on their hands and came in "contact with infected articles; yet none developed a "bubo in the armpit. This shows that direct inocula-"tion through cuts or abrasions was not, and is not, the "usual mode of infection. Infection may take place "by the lungs, or alimentary canal, but aërial "infection is, in my opinion, the most common."

22,362. Do you know of any experiment showing the results of introducing plague virus into the respiratory passages?—There are experiments by the two Russian observers, of the first Imperial Russian Commission, on monkeys: they found they could give plague pneumonia to susceptible monkeys very easily by spraying the solution of the virus in the air in the cage in which they lived.

22,363. Judging from these experiments, would you not expect plague pneumonia to be more frequent than it is, as you think the virus is principally introduced through the air?—There is the possibility to be considered that it may pass the lungs and produce an infection of the lymphatic glands, the ordinary septicæmic or bubonic form, or cerebral varieties even, without specially infecting the lungs, which we would recognise clinically.

22,364. This is very hypothetical?—Yes.

22,365. Have you any opinion as to whether the virus may be absorbed from the alimentary canal?—No; that is a supposition also from the so-called abdominal cases. The natives have a habit of eating food off the ground; of course, their nails are not always clean; the sputum of the patients, especially in pneumonic cases, would infect the ground from which they eat their food.

22,366. That is a possibility, but have you any observed facts?—No, I have no cases to prove it.

22,367. How do you think the virus proceeds out of the body?—Only by those excreta which will contain blood at any time. I have evidence to show that it cannot be excreted by the fæces or urine, unless they contain blood. The German Commission and myself examined all excreta, tears, and everything else, to find the germ, and failed to do so, except in cases where the excreta also contained blood and the sputum.

22,368. Did you find it in the urine?—We never found the germ in the urine unless there was blood also.

22,369. Was that hæmaturia or hæmaglobinuria?—Hæmaturia.

22,370. And the fæcal matter?—Never when it did not also contain blood; in fact, it was extremely rare; I never saw a case in Parel.

22,371. You think, therefore, the virus can only be passed out of the body through an actual lesion of some surface?—So I believe, or from the sputum of a plague pneumonic case, which, I think, is the most important means by which plague infection is given off by the patient.

22,372. Have you any experience with regard to the treatment of plague which you wish to state?—Only the antiseptic treatment, which I introduced. Perchloride of mercury was tried in very large doses, and seemed, along with stimulants and ordinary treatment, like nursing, dieting, and keeping the patient lying down, to give better results than any other treatment I have tried.

22,373. Can you state that in a statistical form?—From a report regarding the Parel Hospital, which I have completed up to that date, and from evidence which I have procured since, I shall go into that question fully. In my précis I ask permission to put in the full statistics. These I have excluded at present, because I thought I had enough evidence on other points.

22,374. Your experience is in favour of antiseptic treatment, I understand?—Yes, combined with stimulants and ordinary nursing.

22,375. Aiming at the source of the illness—the actual cause—I suppose, you mean?—Yes.

22,376. And cardiac feebleness you treat at the same time with cardiac stimulants?—Yes, quite so.

22,377. What are the plague measures which you consider effectual?—Looking at it from a broad view, the only measure which would be effectual is an alteration in the habits of the people from the etiological standpoint and historical standpoint, and from my experience, I believe that plague is a want of fresh air disease, and that it can be very easily destroyed by fresh air. In that way it is in marked contradistinction to typhus fever. No matter what you do with regard to sanitary houses, like Lord Sandhurst's improvement scheme, if the people do not understand the precautions which have to be taken they will defeat your measures. They will not get fresh air at night, and they will continue to do those things which will tend to continue plague.

22,378. Of course, in a big house, even if the means of ventilation were far from perfect, so long as the house was not overcrowded, the air would remain pure for a long time?—Yes.

22,379. Have you any other measures to mention which you consider useful?—Yes, as temporary expedients. I call all those measures which we have adopted, namely, evacuation, which is a strong measure, and isolation, temporary expedients.

22,380. Evacuation you have already discussed as a means of securing fresh air?—Yes, and that is the ground upon which it is useful. Isolation of the sick naturally comes in, and segregation of contacts in health camps, and disinfection by the real disinfectants, viz., fresh air and sunlight. Of course there are other disinfectants—not chemical—viz., steam and boiling water. Then quarantine comes in to keep plague out.

22,381. You have already expressed an opinion that disinfection by chemical agencies is not a very effective measure in itself?—Quite so. I do not condemn it entirely.

22,382. Have you some illustrations in favour of your view—that fresh air is the most important thing?—I have mentioned the facts in my evidence in connection with instances of immunity. Then there are those which I have already stated with regard to castes, people, and localities, and also from the failure of evacuation and facts regarding the floating population everywhere, not only in Bombay, but also in Canton. Clifford Allbutt's book mentions 250,000 people living on the river. Then I have particular instances. For instance, Commander G. S. Hewett (March 5th, 1898) has forwarded to the Press the following remarks with regard to the Tardeo section to the Secretary to the Bombay Plague Committee: "An extraordinary outbreak of "plague took place in chawl No. 202, Bapty road, on "the night of 2nd instant. At 11 p.m. there was no "sick in the house, and at 4 a.m. on the 3rd (the day "following) there were four deaths, and at 10 a.m. "seven other cases were removed to hospitals. On the "morning of the 4th instant nine more cases were "removed to hospitals, making a total of 20 cases in "24 hours. This remarkable outbreak appears to have "been caused by the extreme cold of the night of the "3rd instant, which caused people to *close all the rooms* "inhabited. The chawl, which is in a fairly sanitary "condition, is situate in an open compound, *with* "*plenty of light and air*, and no cases of plague had "occurred in it." That was quoted against me as

* "Report on the Plague in Sind, 1896–97," by the Principal Medical Officer, Sind District—not reprinted in the Proceedings of the Commission.

showing that plague could not occur in a well-ventilated shawl, but the reason is plain. The people shut all the doors and windows on account of the high wind, and immediately in one night they got 20 cases, although for two years previously, although they were surrounded with plague, they remained plague-proof.

22,383. Do you know that during the two years in which they were plague-proof they did not on any occasions shut the doors and windows?—With regard to that, I have no information.

22,384. Have you any other examples?—The Jailer's house is a well-ventilated house. The Jailer's house is an ideally perfect house, even for Europeans to live in, but as he only used the centre room, which may be 14 feet by 12 feet, for himself, his wife, his mother-in-law, and six children, who all sleep on the floor, and in the cold weather they have a charcoal brazier in the centre of the floor, and cover their heads with razais, blankets, and so on, and as all the doors and windows are shut, they have made this naturally well-ventilated house perfectly insanitary, and constantly there was plague last year, and there was also plague this year. The jail, which is next door to it, remained free, although there were plague rats in it, and this year a plague case was introduced into the jail, and there were no further cases, because ventilation is enforced there at night on the prisoners.

22,385. Did not the lesson gained on the first occurrence of plague prevent a continuance of this habit of closing all ventilation openings?—No; the Jailer would not be convinced, although his daughter was attacked. I diagnosed the case on the 17th January this year before I got ill, and I said, "Now, surely, you will give up your bad habits and preserve your health?" And he replied: "God will take care of my health."

22,386. You have had some experience of Yersin's curative serum, I think?—The total number of cases inoculated by me, or by Yersin, was 27. He injected the first three, and I injected the other 24, in Parel Hospital. Of the 25 cases of undoubted plague, 13 died and 5 recovered, the percentage of mortality being 76·5.

22,387. How does that contrast with the mortality of cases not treated?—The general mortality in Parel was 64·5 per cent., but 66 were moribund on admission and 17 died during the first day in hospital and 38 during the second day. If those cases are excluded, the mortality was 41 with cases treated; but then we must further exclude Yersin's cases, and then the rate of mortality under hospital treatment, separate from serum treatment, was 30·8 per cent. of the admissions who lived after 28 hours in hospital and actually got some hospital treatment.

22,388. This serum was not applied to cases obviously moribund, I suppose?—No; in fact, at the end, Yersin advised me to make selections of cases which had been ill under 24 hours. Even then, the mortality amongst those was, if anything, worse.

22,389. Have you had experience of Lustig's serum?—No, I have not used it myself. We tried it on animals in Hankin's laboratory in Bombay, and did not find it had any protective effect. But then it was not actually the serum, but what he first published in the *British Medical Journal*.

22,390. You have had some experience of Haffkine's prophylactic fluid, have you not?—I have injected 708 cases here, and afterwards only three deaths occurred among them from plague.

22,391. Did these people voluntarily come forward?—More or less. Some compulsion was used, that is to say, they had privileges held out to them if they submitted to inoculation. If they were living in infected places, they were told that unless they became inoculated they would be moved out into segregation and health camps. In one Pet which I quote in my précis, Pantachakote, the total number of houses was 75 with a population of 160. There were 12 infected houses, and 3 people were inoculated between 11th October and the 11th November. There were only two plague deaths, and those two occurred on the first day after inoculation. I know that they themselves resorted to inoculation, because they had had a plague death in their house which they concealed until the next day.

22,392. How many deaths had occurred among the non-inoculated at that time?—The non-inoculated were compulsorily removed out of the locality. I believe there were cases and deaths among them. The whole locality had been infected, and perhaps 35 cases of plague had occurred in this district before inoculation was resorted to, so that the plague had got a grip of the place. The people were allowed to go back into their houses, and no other measures were taken, as far as I remember, as regards non-infected houses. The infected houses were opened up, and the others have since been opened up all over the place, because we could not distinguish between those which had been infected and those which had not.

22,393. Did you experience much difficulty in inducing people to be inoculated?—The people of Satara only resorted to it when we brought statistics before them to prove its great value. The personal factor enters into this matter a great deal, that is to say, the influence of the Collector or the doctor who is trying to get them to resort to this measure. Fortunately also, by the co-operation of some intelligent members of the community, we got the people to resort to inoculation this year, but last year we could not.

22,394. Is it proceeding now?—Inoculation has stopped now altogether; but plague is almost at vanishing point. The last Pet which was infected with plague has been completely evacuated, and that has practically stopped the plague for the last week or so.

22,395. The people are not now so much afraid of plague?—I think they have been educated a good deal.

22,396. The total number of cases which you appear to have inoculated is very small relatively to the population; were serious efforts made to induce the people to become inoculated?—At first we had a public meeting which was announced long beforehand. There were addresses by the Collector, the Deputy Collector, and myself, and we inoculated some Europeans and a child of a public officer in the presence of the assemblage, about 2,000 or 3,000 people. That had a great effect. Then, afterwards, the police offered themselves for inoculation.

22,397. Did you take other means to induce people to be inoculated?—Yes, and to give them an opportunity of judging of its value for themselves.

22,398. And they had ample opportunities for being inoculated?—Yes, three operators were ready to inoculate.

22,399. More than was required?—We used to wait for them two hours in the morning and one hour in the evening.

22,400. That is the utmost you effected in this town?—Yes, but this year plague has not been so bad.

22,401. Have you noticed if any effect is produced upon the progress of other diseases by inoculation?—I have had experience. Where once the reactionary fever has occurred gonorrhœa and gleet have been cured. It has also a great modifying effect upon syphilis.

22,402. How many cases of venereal disease have you seen cured?—Twelve cases of gonorrhœa, two of gleet and only four of syphilis. Unfortunately, I became ill when this was discovered, and so I could not investigate it further.

22,403. How long had the gonorrhœa lasted?—One has to trust the word of the patient; they say for two or three months.

22,404. How many injections were required?—Only one full injection.

22,405. How soon after were the symptoms stopped?—Up to the third day.

22,406. Were these cases verified?—Yes, I have inquired into them, and had the people come up before me.

22,407. Have you seen any cases in which inoculation has done harm?—In Satara I had seven cases which I inoculated, in which what one might call abscesses formed, although I did not actually see pus but only a clean watery serum exude. That was due to the fact that the contents of the bottle had become deteriorated.

22,408. Have you any other facts bearing upon the value or otherwise of inoculation?—Compared with evacuation, I would say that, if everybody could be induced to become inoculated, inoculation would seem to have a greater effect than evacuation, as evacuation is carried out now; but ideally, evacuation with the precautions I have mentioned, would be equally successful with wholesale inoculation.

22,409. Do you speak of partial or complete evacuation?—Complete, and the person should have full advantage of fresh air, on which the value of that evacuation depends.

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22,410. You think that there are limitations to the value of inoculation as a plague measure?—Yes. You cannot get the whole of the people to agree to inoculation, and, therefore, you must resort to other temporary measures in addition. It would be a very good thing if you could get the people to resort to inoculation wholesale.

22,411. Do you think it is hopeless to get them to do so?—I think so. We cannot trust to it alone, it is only a temporary measure for dealing with plague, because although vaccination is of use for small-pox, it has not stamped out small-pox. In the same way we cannot trust to inoculation alone to stamp out plague. No one, in the face of an epidemic, has expressed an opinion that it will check the further progress of plague for the time being. I look on it only as a temporary measure.

22,412. What do you regard as the most fundamental?—Radical sanitary reform, teaching the people to alter their habits, which is the secret of our exemption, except in the case of plague pneumonia, which any of us might get any day.

22,413. Even in the open air, you mean?—Yes.

22,414. Will you give us your views with regard to the incubation period of plague?—I have known cases where it has been above the ordinary accepted period of ten days. This was amongst families in which more than one attack occurred. There were:—

3	instances	11 days	after removal to segregation camp.	
1	,,	13	,,	,,
1	,,	14	,,	,,
3	,,	15	,,	,,
1	,,	19	,,	,,
2	,,	20	,,	,,
1	,,	26	,,	,,

22,415. Let us take the last. What was the cause of infection in that case?—I have noticed about that the possibility of the man having revisited his house to bring away cooking pots.

22,416. Those are cases which occurred in the segregation camp?—Yes, with the only exception of the man on the 26th day.

22,417. I suppose you apply that possible source of error to all the cases?—There is a possibility of that, but we were quite certain of this particular case; there was a definite history that he actually had visited his plague-infected house to remove cooking pots, but the others were very carefully watched, and the seals of their houses were not allowed to be opened. We had all the locks sealed, and the people were not allowed to break the seals except by permission of the Divisional Officer, and in the presence of the authorities.

22,418. Was there a roll-call at any time of the day?—Yes, that is the practice.

22,419. At what times?—In the morning and evening, and sometimes surprise visits are paid into the huts, in order to find if all the segregated people are in the huts assigned to them.

22,420. The evidence, on the whole, would seem to show that these men, other than the case you mentioned, had, at least, not slept in any hut outside the camp?—Quite so.

22,421. Further than that you cannot go, I suppose?—No.

22,422. Have you any tabulated statement respecting the duration of the incubation period?—I have, and I will put it in, as follows:—

ADDITIONAL EXAMPLES of PROLONGED INCUBATION, 1898-99 EPIDEMIC.

GODOLI HEALTH CAMPS.

No.	——	Admitted.	Attacked.	Incubation.
				Days.
1	Dhondi K. Sidhu -	22.11.98	2.12.98	11
2	Shanker Vishnaranath.	22.11.98	5.12.98	14
3	Case No. 73 -	14.10.98	27.10.98	13
4	,, 74 - -	13.10.98	27.10.98	14
5	,, 83 -	14.10.98	2.11.98	18
6	,, 96 - -	13.10.98	7.11.98	24
7	,, 110 -	1.11.98	14.11.98	13
8	,, 113 - -	5.11.98	16.11.98	11
9	,, 114 -	26.10.98	18.11.98	23

RANCHATRI PLAGUE HOSPITAL, 1898-99.

10	Case No. 11 -	18.1.99	1.2.99	16
11	Prisoner in jail camp "under guard."	10.1.99	21.1.99	11

Such examples occurred in about one per cent. of the people segregated as contacts.

22,423. It there anything further which you wish to bring under our notice?—The only thing I wish to refer to is, the inverse relation between the height of the temperature and the intensity of the plague epidemic "in a place with a variable climate." That is the most important point in the statement. I have observed the minimum temperature at 8 a.m. We have not self-registering thermometers. At 4 a.m. it would be more telling still. In a place with a variable climate like Satara, when the temperature falls the plague increases. I have prepared a chart, which I produce.* When the temperature is generally lowest, or soon after that, you have a rise in the plague attacks. The chart shows a rise of temperature up to 82, which is our maximum in the hot weather, and there are no cases; and again, in the epidemic of this year, a fall in temperature, which corresponds to a rise of plague attacks. Generally, throughout the chart, the same thing occurs. Of course, there is a time when the epidemic will wear itself out, and then, although you have a fall of temperature, you may not have a great rise of deaths, owing also, perhaps, to measures which have been taken. Perhaps the town is half evacuated for instance. The only objection to that is places like Karachi, where they have had an epidemic of plague in the middle of the hot weather. But I can explain that, because there is no very variable climate, it is very equable, and the habits of the people are not altered by the cold or hot weather.

22,424. Therefore, you come back to the old proposition, that temperature has nothing to do with it, but that it is a matter of fresh air?—This fact of the temperature has also been noticed in the *British Medical Journal* by two doctors. Dr. Polverini and Dr. Galeotti, of Bombay, have written a book noticing the same point.

22,425. And giving the same explanation?—I do not know that.

22,426. In your opinion, plague is an easily prevented disease in civilised communities; what is your reason for saying so?—The knowledge of sanitation and our increased knowledge of plague and the conversion of medical men to the real, true, etiological theory of plague will put everyone on their guard as to how plague may be avoided. It is easily avoided, because we know that Europeans, and the other classes I have mentioned, are immune, and the reason is that they enjoy, and habitually provide themselves with, fresh air. When everyone knows how to do that, they never need be afraid of plague at all, except plague pneumonia.

22,427. (*Mr. Hewett.*) Have you noticed that persons of any particular ages have escaped plague here?—I do not think any definite statement could be made upon that point.

22,428. What is the mortality among the very young—children under five years of age?—33 under the age of five out of 1,152, but one would require to know the number of children living under that age in the town.

22,429. But you know there is a large population, and the fact remains that only 33 got plague out of 1,152?—The impression remains upon my mind that children are not so liable to plague. The adult age, in fact, is the age for plague, but, naturally, there are a great many adults among the population, and their work probably exposes them more to infection.

22,430. Have you observed any places which were infected last year, and have escaped the infection this year?—My work has only to do with Satara.

22,431. Have you observed that any portion of the town escaped last year, and got infected this year?—The Pets which were most infected last year have almost entirely escaped this year. I have tried to account for this, firstly, on the ground of general disinfection; secondly, the people being better educated; thirdly, their not visiting their sick relatives so much; and fourthly, that they have obeyed the rules

* Not published in the Commission's Proceedings.

which, I say, are the primary sanitary safeguards which will enable them to resist plague.

22,432. Do you think that the wards of the town which have escaped this year have enjoyed more fresh air than other people?—Yes, from both example and inducement of the leaders of their community, they are following these supreme sanitary safeguards, and running the risk of dying of diseases which they thought were caused by cold air rather than die of plague.

22,433. Are we to understand that the great majority of the people in places infected last year have altered their mode of living?—Not more than in Pets that were being affected, but native doctors have told me that many in the city are following these safeguards with regard to ventilation and cleanliness of their houses.

22,434. But not more in the uninfected than in infected localities?—I could not state that as a fact.

22,435. Do you feel confident that where you have noticed infection by rats, you have been able to exclude the possibility of cases of plague having occurred among human beings before the rats began to die?—Yes. I have an example in which we very nearly cleared out the infected district, and a large area round it in the first epidemic.

22,436. I was not speaking so much of places within a town, but of a village or town which has got infected from another place?—I had that experience this year, because plague was definitely found to be conveyed by rats, I think, from the little hamlets outside the city into the city by the road which the plague afterwards travelled.

22,437. What distance would that be?—About 600 yards from the village, and there were intermediate places which had dead rats.

22,438. You have no proof of rats having carried the plague a further distance than that?—No, that is my limit.

22,439. At what time of the year did these two places, Limb and Rahmatpur, get infected?—I think, perhaps, in July, but I am only speaking from memory.

22,440. Was it during the rainy season?—Just at the end of the rainy season we began to have dropping cases, but my connection with the villages was naturally not close, because I have been wholly on plague duty in the city.

22,441. Do you think that the people were in the habit of visiting the infected sites after they went out into these huts?—That is possible, but so far as police protection and visiting at night to see that the people were not living in the town are concerned, that was excluded.

22,442. Do you know how many police were looking after the 6,670 inhabitants of Rahmatpur?—I do not know about Rahmatpur.

22,443. How many people were there in Limb?—About 4,587.

22,444. How many police were there to look after them?—Perhaps 30 at most.

22,445. What do you exactly mean by partial evacuation?—In a commercial town, or in a large city, you cannot have wholesale evacuation of the place. There are obvious objections to it, such as trade, &c., and therefore blocks are evacuated, as we did in the beginning of the 1897 epidemic. The infected houses and those for 100 yards round, say, were evacuated, and also the houses which were closely packed together.

22,446. Did I understand you to say that partial evacuation at Satara had led to the dispersal of the plague?—No; I said it failed to stamp it out. The rats conveyed plague from the infected to the non-infected area, and therefore, I think, quarantine and isolation cannot stop plague, unless you can quarantine the rats and other animals.

22,447. Was there plague here in February and March 1898?—Yes, I think so.

22,448. To what extent?—About 30 cases during the whole month of March.

22,449. In the last half of February?—About 60 from the 15th to the 28th February.

22,450. There appear to be ten instances in which plague cases occurred in houses that had been disinfected during February and March 1898?—Yes.

22,451. A certain number of them appeared during the period of incubation calculated from the date when the persons affected are known to have been in contact with persons suffering from plague, but there are others which occurred after that period. Do you contend that the cases which occurred after the period of incubation could not have been contracted by communication with persons living outside those houses?—That was not likely at that time, because the population of the town was very much reduced. Instead of 25,700, it was about 4,000, certainly under 5,000. Therefore, the friends of those people were out of the town, and there was not so much intercourse between them or between others who are not friends as at ordinary times.

22,452. During the time in which these ten cases occurred you had 90 cases in the town altogether; is it not possible that those people got infected otherwise than through the infection being in the houses which they re-occupied?—I do not believe that merely visiting a house during the day will give a person plague if he has been living and sleeping in sanitary surroundings for some time. For instance, we, who are visiting Plague Hospitals, do not carry away infection to our houses, nor did the natives who visited the hospital.

22,453. You do not exclude the possibility of these people having been in the habit of visiting other plague patients in their own houses?—That may be possible, and I do not exclude that, but there would be very few instances.

22,454. Still, it is possible?—I admit that it is possible.

22,455. Are you satisfied that on the occasion of the disinfection of the houses in February, 1898, the solution of perchloride of mercury was actually one in 1,000?—I can make this statement that so far as human agency and foresight—by Europeans as well as Natives—could make it perfect, disinfection by perchloride of mercury, even in the last months, was so. The perchloride of mercury was acidulated with hydrochloric acid, as Hankin recommended. It was carried out as early as it could be. I made it a point to give disinfection by an acidulated solution of perchloride of mercury, the best possible chance of proving its efficacy, although I did not believe in it.

22,456. Who prepared the paper you have just handed me on the subject of the period of incubation?—I, myself, did that.

22,457. Are you responsible for the letter-press?—It is in my own handwriting.

22,458. You say in it: "It is to be noted that re-"infection and concealment accounted for attacks "beyond ten days from the initial case in a family "amongst segregated families. No case of undoubted "plague developed beyond the tenth day of observa-"tion except in one instance, and this person had "been allowed to visit his house after segregation, "and had been known to remove some articles for "use"?—That was a case in which I could not find positive proof, but it was possible in the others.

22,459. I understand you to say a great deal more than that?—There I may have, but I have modified that opinion. That was written a long time ago.

22,460. (*The President.*) You do not agree with what you have written here and you wish an opportunity of re-writing it?—Yes; my opinion may have altered.

22,461. (*Mr. Hewett.*) I wish to know why it has altered?—It has altered from experience. I have found instances since then which extend definitely beyond ten days.

22,462. None of the cases which you have mentioned later are referred to in this letter-press?—No, I have added those cases.

22,463. Where did these cases occur?—In Satara during the first epidemic.

22,464. Do these figures which you gave in answer a question of the President, relate to the people who are referred to in the letter-press?—Yes.

22,465. Why have you changed your opinion here?—Because I have had larger experience since I wrote it. I think in those cases it was not possible for the people to visit those houses, in fact, the seals were not broken.

22,466. Why did you think it was possible when you wrote this opinion?—At that time I did not enquire as to whether the seals had been broken or whether the people had permits.

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22,467. How long after these people had been in the segregation camp did you enquire as to whether the seals had been broken?—Before the end of the epidemic, when I was making up my figures for the whole epidemic again.

22,468. How long afterwards was that?—At the beginning of April.

22,469. When did you write this opinion?—On the 12th March. I wrote this before, in Bombay.

22,470. When did you write your opinion which I have quoted to you in a previous question?—Before the 12th March.

22,471. And when did you write down the figures which you gave to the President?—Before the 12th March also.

22,472. How long after writing your opinion did you change your views?—About four weeks.

22,473. Do you think that it is possible you might change your opinion again?—One always thinks the last opinion the best just as one thinks one's last religion is the best.

22,474. Do you think that none of these persons who were infected in the contact camp could have been infected from being in the neighbourhood of the contacts who became ill while they were in the camp?—I do not think they could, because if what I hold is true, and I believe from my experience that it is true, one cannot catch plague, so to speak, by coming into contact with a plague patient in our sanitary camps.

22,475. Then your deduction as to the period of incubation is entirely dependent on your view that you cannot catch plague by coming into contact with people in the camps?—I think that is so.

22,476. I understand that you wish to place before the Commission facts with regard to certain cases which led you to change your view with reference to the period of incubation?—Yes.

22,477. In these cases are you able entirely to exclude the possibility of the infection having been caught outside the camp in which the affected persons were located?—No, but these people only have left the camps, say from 12 to 2, under a system of passes.

22,478. Why could not they have evaded the guard at night?—We were all supposed to control that under the guard.

22,479. Do you consider that it is possible to keep people in under those circumstances?—No, I do not.

22,480. A certain number of people were taken out of that Pet, and out into the segregation camp. Sixty persons who had been inoculated were allowed to remain in the Pet, and none got plague; but among those who went into segregation a certain number developed plague. I want to know the number?—(The following statement, containing particulars as to plague among persons segregated and inoculated, was afterwards furnished by witness:—

Inoculation Statistics in three Divisions of Satara City 1898–99.

Name of Division.	No. of Houses.	No. of Infected Houses.	Population.	No. of Persons Inoculated.	After Segregation among the Non-inoculated.		After Inoculation among Inoculated.		Date of Evacuation and Inoculation. Week ending.	Remarks.
					No. of Plague Cases.	No. of Plague Deaths.	No. of Plague Cases.	No. of Plague Deaths.		
Durga - -	78	2	333	60	3	2	—	—	7th Nov.	No case since 17th January.
Raviwar -	68	13	292	107	13	10	1	1	8th Dec.	No case since 6th December.
Pantachakote	75	12	160	83	10	7	2	2	10th Nov.	No case since 24th November.
Totals -	221	27	785	250	26	9	3	3	—	—

The three attacks and deaths among the inoculated were on the 1st day of inoculation.

There have been no further attacks among the inoculated, all under strict observation, up till 31st March 1899. Some of the non-inoculated went out, after ten days detention under observation, into fields and to other villages or towns where they are not under strict observation and it is not known if any of them were attacked by plague.)

22,481. Have you had any corpse inspection here?—Yes.

22,482. Who inspected the bodies here?—In the first instance the Hospital Assistant. I was alone here during the height of the epidemic, and when he was in any doubt as to whether he could declare a case to be plague or not, I was sent for. I have even gone to the Muhammadan cemetery at half-past eleven at night to examine bodies.

22,483. Did you find any difficulty in carrying out corpse inspection?—There was no objection as far as I was concerned nor with regard to the medical subordinates.

22,484. Are there many Muhammadans here?—There are 2,789 Muhammadans.

22,485. That is a very small proportion of the total population?—Yes.

22,486. Are the Muhammadans an influential community here?—Very few of them are of any standing.

22,487. Have you noticed anything as to the effect of plague among the ordinary diseases of the population?—I am of opinion that a person who has had syphilis suffers much less from plague, or rather, survives a much more severe type of plague than a person who has not.

22,488. Have you observed that an outbreak of plague affected the general mortality from other causes?—I have not noticed that.

22,489. Perhaps you will put on record the facts which came to your notice with regard to the immunity of persons in the Parel Hospital, of which you had charge in Bombay?—In upwards of 240 instances the friends of the patients attended their sick, and in 20 instances scarcely ever left the bedside, and in not a single instance did the disease spread to the friends. Out of more than 140 attendants on the sick, belonging to the hospital staff from time to time, only one sweeper was attacked, and he had been constantly helping in the *post-mortem* room, and had a very mild attack with small axillary bubo.

I have also statistics to show the non-infectiousness of plague amongst servants in Plague Hospitals, when they are properly hutted on the premises, at Satara, in 1897–9, as follows:—

Ranchatri Plague Hospital, 1897–8:—27 servants and one plague attack and death, disease *not* contracted in hospital, *vide* "Bombay Medical and Physical Society's Transaction," March, No., p. 11.

Ranchatri Plague Hospital, 1898–9:—14 servants; no plague.

Wadha Camp Hospital, 1898–9:—22 servants with one attack and death which was in the person of a corpse bearer, and the source of inspection could *not* be traced as he was in the habit of going outside the hospital camp. The Medical Officer in Charge was able to exclude any recent or special source of hospital infection where the man had been on duty for seven months.

Budhwar Hospital, 1897–8:—27 servants; no plague.

Godoli Camp Hospital, 1898–9:—14 servants; no plague.

The above figures include only *bonâ fide* attendants upon the sick and not policeman, 22 in number, who were engaged at and in the Hospital Compound, and were completly immune. No notice is taken of changes in the establishments (add 20 per cent., say), in this return, which is compiled from the designations of the offices. Total servants 104, with two attacks and two deaths. The conclusion is that special exposure to plague miasm (?) in freely ventilated Plague Hospitals is not specially liable to lead to an attack of plague. It is to be borne in mind that it is not the *locâle* of a man that saves him from plague, but the habits as regards hygiene that he follows. No doubt those attacked had insanitary habits, and slept covered up in their blankets, and endeavoured to shut out the cold air in the early mornings, especially of the cold season, and rendered themselves equally liable to attack as the patients they attended had done in their hovels. Occupation *per se* is no preventive of plague. To find complete immunity among such a number of attendants, specially exposed to infection, would argue their possession of habits of living to which they had been strangers hitherto; but that so few were attacked is contrary to what we would have expected.

22,490. Have you noticed a case of a person having plague more than once?—Only in one instance, and there, I think, it was rather a relapse.

22,491. You had one case of a second attack of plague?—Yes, and another patient had a relapse.

22,492. Can you give us some details of the second attack of plague?—It was in a woman, aged 40. She had been convalescent 18 days, and was attacked 27 days after the initial symptoms of the primary attack, and died five days afterwards.

22,493. (*The President.*) Was the woman you speak of as having had plague twice still in the hospital when she was attacked the second time?—Yes, but in the convalescent ward, nearly separated.

22,494. She did not quite recover?—She was able to walk about.

22,495. But you say that she relapsed?—I consider the interval too long, but that was a matter of opinion.

22,496. Was it a case of bubonic plague?—Yes.

22,497. (*Mr. Hewett.*) Have you examined the registers of deaths in the town of Satara?—From the 31st July 1897.

22,498. Do you feel certain that every death is reported?—Yes.

22,499. Are you certain that the cause of death is properly reported?—It was not verified in each case. There was no necessity, because there was no indigenous plague.

22,500. How could it be verified if a death had not to be reported until 72 hours after it occurred?—It could not; the body would be disposed of.

22,501. Did your inquiries lead you to believe that deaths were all reported within 72 hours, and never afterwards?—No. That point did not come up.

22,502. Was it the practice for a medical man to examine the register of deaths?—It was submitted to me daily. The Secretary brought it either to my bungalow, or I went to the Municipal Office to look at it.

22,503. From what date?—I arrived here on the 30th July, and I immediately started to watch the daily mortality. That was the point I was keen on.

22,504. Was it the custom for the Civil Surgeon to examine the register before plague occurred?—Only for a short period I, myself, was here.

22,505. You began it on account of plague?—Because of my apprehension of plague—that the plague was within striking distance, so to speak.

22,506. In the ordinary course, does a Civil Surgeon in the Bombay Presidency look at the registration of deaths within Municipal areas?—I should say that is a very exceptional custom.

22,507. Has he any duties as Health Officer in ordinary circumstances?—No, except in a very cursory manner, looking after the streets and latrines, and that sort of thing. I do not think his duties amount to anything more than, perhaps, suggesting means, and that there should be more filth carts—sanitary arrangements.

22,508. (*Mr. Cumine.*) You are going to give us the figures in Satara town and Satara district of the first and second epidemics. Will you give us the periods, in order that we may see whether the town or the district was ever completely cleared of plague for any appreciable period?—(The following statement was supplied by witness subsequently):—

SATARA DISTRICT PLAGUE RETURNS, 1897–99.

Blank Dates.	Cases.	Deaths.
13.4.98 - - -	0	4
19.4.98 - - -	0	1
22.4.98 - - -	0	0
28.4.98 - - -	0	0
29.4.98 - - -	0	0
5.5.98 - - -	0	1
2.6.98 - - -	0	1
8.6.98 - - -	0	2
10.6.98 - - -	0	0
22.6.98 - - -	0	1
6.7.98* - - -	0	4

* *I.e.*, the last fortnight of April and middle fortnight of June were nearly free.

——	Cases.	Deaths.	Mortality.
Satara City:—Free from indigenous plague, 4th April 1898–10th October 1898:—			Per cent.
In the 1st epidemic -	729	610	83·7
„ 2nd „ till date 31.3.99.	143	118	83·1
Satara Municipalities, including Sadr Bazar, &c., indigenous and imported:—			
1st epidemic - -	873	727	83·27
2nd „ - - -	287	209	72·8
Grand totals:—			
Satara District, till 17th March 1899:			
Imported - -	914	764	83·58
Indigenous - -	25,082	19,647	73·33

22,509. In the rains, in a non-evacuable town, what are the best practicable measures to adopt, according to your experience?—In the absence of evacuation, wholesale inoculation is the next best thing.

22,510. What is the best thing to do with the patients?—If you can, you should isolate them with one attendant in a well-ventilated room in their own house. There is no danger of attending them there. I have on my own responsibility tried that in one case here, in which the family were allowed to remain in their own house. The room in which the patient remained was plague-proof, as I call it, and no case followed. Of course, it was a dangerous experiment. The house was well ventilated, and not overcrowded.

22,511. Did you put the patient on a bed?—Yes.

22,512. Did you throw disinfectants on the floor every day?—There were no chemical disinfectants, but the house was disinfected with sunlight and air. I wanted to try the effects of my own theory as far as possible.

22,513. Were the people in the house attacked?—No

22,514. You did this with one patient only?—Yes. There were five other members of the family. Of course, it was done on my own responsibility.

22,515. Have you any information of outbreaks of mumps appearing in children in a village in which the plague epidemic had died out?—I have no instance of that.

22,516. When the perchloride of mercury solution had been prepared, and was about to be applied to the houses, did you ever take a sample and test it to see what its strength was?—No.

(Witness withdrew.)

(Adjourned till to-morrow.)

At The Collector's Office, Satara.

FIFTY-SEVENTH DAY.

Tuesday, 28th February, 1899.

PRESENT:

PROF. T. R. FRASER, M.D., LL.D., F.R.S. (*President.*)
Mr. J. P. HEWETT. | Mr. A. CUMINE.
Mr. C. J. HALLIFAX (*Secretary*).

Mr. V. G. Ghanekar.

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Mr. V. G. GHANEKAR called and examined.

22,517. (*The President.*) You are a pleader at Satara?—Yes.

22,518. (*Mr. Cumine.*) In what town have you had experience of plague?—In Satara city.

22,519. In Satara city Municipal limits only?—Yes.

22,520. What were the preventive measures taken when plague had been discovered in Bombay?—Observation camps and detention camps were first established on the roads leading from the railway station and Poona to Satara city.

22,521. Medical inspection was introduced?—Yes, and names of strangers who used to visit the city were every day registered on different posts, with their addresses. These persons were to be visited by inspecting Karkuns every day.

22,522. How was infection brought into Satara city?—From Karad, I think.

22,523. By human agency?—Yes.

22,524. And having regard to this fact, you think, I understand, that persons from infected localities should not be allowed to come to the limits of an uninfected locality?—Yes.

22,525. Putting on one side human agency, what other agency is there by which infection is carried from one village to another?—By the clothes of infected persons.

22,526. Have you any case where infection was clearly carried by clothes?—From Satara city infection was carried to Yawateshwar.

22,527. How did the clothes go there?—A Mahar was engaged where the dead bodies were to be burnt, and there was a report to the effect that he took clothes from the dead bodies to his relations on the hill, and by those means the plague was carried from Satara city to Yawateshwar.

22,528. Was that verified?—That is what was said, but it was verified also.

22,529. Could the Mahar not have caught plague himself?—That Mahar who carried these clothes did not catch plague.

22,530. Do you think that in an infected village rats may carry plague from house to house?—Yes, in Satara city I observed that. I saw from my own personal observation that rats carried infection from house to house.

22,531. Have you seen them going from house to house?—No; but if one house was infected the next neighbouring house was also infected.

22,532. When plague breaks out what do you think the most valuable remedy is?—Evacuation is the most successful remedy.

22,533. How long would you keep the people out after evacuation?—From my last experience I think that they should be asked to leave the town for four months.

22,534. If they went back to their houses sooner, what would happen?—Perhaps they would be attacked with plague.

22,535. Have you any instances where people went back within a shorter period and got plague?—Yes. I have instances of some disinfected houses, and as soon as they went back into those houses they were attacked with plague, and they died. The Jailer's house is an example.

22,536. We do not want a disinfected house, but one that has not been disinfected. We want to see how long the germ takes to die. You say four months; have you any instances of people who went back within a shorter period than four months to a house that had not been disinfected, and got plague?—I have no particular instance.

22,537. Why do you say four months?—The last particular time all the inhabitants of the city were out for four months; and afterwards they were allowed to go in. We did not notice any plague case occurring in the houses that were inhabited after four months.

22,538. What would you do with the patients?—According to the present Government orders they have to be segregated.

22,539. But what would you do with them?—If the house is well ventilated I would allow the person to remain in it.

22,540. Has that been tried at all?—No; because according to Government standing orders we were not at liberty to keep a patient in his house.

22,541. It is not within your experience whether it would be a dangerous course or not?—I could not get experience at that time, because we were not allowed to leave the patient in the house, although the houses were well ventilated.

22,542. Is the carrying away of the patient to the Plague Hospital offensive to the feelings of the people?—It is very offensive. In one case that came under my notice the patient, as soon as he was told that he was to be taken to the Plague Hospital, died. It was I who went to that patient and told him that the order was passed that he should be taken to the Plague Hospital. As soon as the ambulance came he was told that it had arrived, and he died.

22,543. If you leave the patient in the house what would you do with the rest of the people in the house?—I would allow one or two of his relations to attend on the patient, and the others I would ask to go into the segregation camp in the open air.

22,544. Would they object very much to that, do you think?—They would not object, I think, if the huts are well built and well ventilated.

22,545. How would you manage in the rains?—In the rains it is impossible.

22,546. What is your experience of disinfection of infected houses with chemicals?—I have no belief in disinfection, because it is very costly, and it cannot be properly done. The houses are not pucka built, and in the tiles and roofs, and other places, the germs will not be killed by disinfection.

22,547. What is your evidence of this?—Because many cases have re-occurred in houses which have been disinfected.

22,548. Are you quite sure that the chemicals were properly prepared?—There were special officers appointed to make the mixture, and I must assume that the chemicals were properly prepared.

22,549. Have you many actual instances where people returning to a disinfected house got plague?—Yes, I have about 10 cases.

22,550. Can you exclude the possibility of their having caught infection from some friends outside on the road, or from other houses?—They were not allowed to go into other houses.

22,551. Not after their return to their own houses?—Yes, after their return they were allowed; but they stealthily went to their houses without permission, and they died there.

22,552. But may they not have gone stealthily to other people's houses too?—I cannot say that.

22,553. You think, I understand, that poorly-fed people were, to a large extent, victims of plague?—Yes.

22,554. Do you think that plague is got much by touching plague patients?—No, because every day I used to visit the Plague Hospital, and touch the patients, and I never got plague.

22,555. But I suppose it is necessary to have boots, or something, on one's feet?—Yes, that is necessary.

22,556. Is sleeping on the ground dangerous?—Yes; sleeping on the ground is dangerous, even in the day or in the night: they got plague.

22,557. What are your views regarding the co-operation of the people, as being necessary for the successful carrying out of plague measures?—I believe that the people should be induced to act according to the wishes of Government. If they do not co-operate, then these measures will not come into proper operation. It is necessary that they should co-operate.

22,558. Is there any marked difference in the behaviour of the people now, as contrasted with their behaviour in the first epidemic?—Yes. As regards evacuation, last time, when they were asked to evacuate their houses, they were unwilling; but now that they have received their education with regard to plague, as soon as they see any infection is coming, they willingly leave their houses.

22,559. But do they understand the danger of their going back stealthily to their houses to sleep?—Some do, not all.

22,560. (*Mr. Hewett.*) Was it at about the worst period of the outbreak of plague that these ten cases occurred of people going back to disinfected houses, and getting plague?—No, it was not at all during the worst time; most of the cases occurred in February.

22,561. How many cases were there in February?—99 deaths.

22,562. There was then a considerable amount of plague in the place at the time?—Yes; there was a certain amount of plague in the place.

22,563. Are you a Municipal Commissioner?—I am a Municipal Commissioner; and, at that time, I was a Municipal Commissioner too.

22,564. Is there a byelaw of the Satara Municipal Committee which requires a householder to report every case of death in his house?—There is a byelaw that all persons are required to give information to the City Municipality within 72 hours after death. But that was not made with regard to plague.

22,565. I am not speaking about the plague, I am speaking about a byelaw before plague occurred?—There is a byelaw to that effect, that every person in whose house a death occurs, should give information to the Municipality within 72 hours, and if he does not give that information he will be liable to a fine of five rupees.

22,566. Has everybody in Satara always complied with this byelaw?—As regards death everybody complies with it.

22,567. Has any instance come to your notice in which it has not been complied with?—No.

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22,568. Every death since that byelaw was passed has been properly and punctually reported?—Yes.

22,569. Was the mortality rising when plague was detected here?—Yes, day by day it was rising.

22,570. Was the first case that was found out to be plague reported by the householder?—No.

22,571. How was that?—He could not say it was plague, the death only was reported.

22,572. Was the death reported?—Yes, the death was reported to the Municipality.

22,573. When a report is made that a death has occurred from a particular cause, does any medical officer examine the report?—No.

22,574. It is merely filed in the Municipal office?—Yes.

22,575. What is done with the reports of plague?—When a report comes to the Municipal office, then a Hospital Assistant is asked to examine the dead body, and if he says it is not plague, then the body is allowed to be taken to the burial ground. The register of deaths is kept at the Municipal office.

22,576. (*The President.*) You are in favour of allowing patients to be kept in their houses?—Yes.

22,577. In the case where you allow a patient to remain in his house, you allow some people, of course, to remain to look after the patient?—Yes.

22,578. How do you prevent visitors from outside coming in?—They should be asked not to come in.

22,579. There are no special means adopted?—No.

22,580. Do you not recognise that to be a possible danger?—As regards a well ventilated house, I do not see any possible danger.

22,581. You think it is quite safe?—Yes; and besides it is against our Hindu notions to take a patient away from his house to a Plague Hospital; our habits of living and our notions are against it.

22,582. One would not compel it to be done unless it were required, and if it came to be a public danger, habits would have to be set aside to some extent, would they not?—Yes, that is so.

22,583. Is it part of the difficulty that in the hospital they are isolated from their friends?—Yes.

22,584. Could that not be got over by allowing a certain number of their friends to come and see them just as if they were in their own houses?—That difficulty can be got over by allowing friends to see the patients in hospital, but when the patient himself is unwilling to go to hospital, how can the difficulty be got over?

22,585. Do you find that difficulty as great now as it was at the commencement of the epidemic?—The difficulty is still in existence.

22,586. You do remove certain patients?—Yes, we remove them, but they are unwilling.

22,587. You have a hospital, I think, for plague patients, have you not?—Yes.

22,588. And patients are taken there?—Yes.

22,589. Are they only the patients from houses where the accommodation is insufficient, do you think?—Almost all patients are taken.

22,590. You are not now adopting the plan of treating patients in their houses?—No.

22,591. Therefore, it is a practical measure, and you can carry it out?—Yes, we can.

(Witness withdrew.)

Lieutenant N. B. Anderson, I.S.C., called and examined.

Lieut. N. B. Anderson, I.S.C.

22,592. (*The President.*) I believe you are in the Indian Staff Corps, and have had a good deal to do with plague work in Karachi and Satara?—Yes.

22,593. You are now engaged here?—Yes; I am Plague Supervising Officer.

22,594. (*Mr. Cumine.*) I will ask you about the Satara district first. How many infected villages have you now under your management?—Nineteen, spread over an area of about 1,000 square miles.

22,595. What do you think is the great remedy when a village is infected?—Entire evacuation, immediately.

22,596. When plague has regularly "caught on" in a village, do you find that partial evacuation is successful?—No.

22,597. When is it successful?—If there is a wadi some distance from the village, it might possibly not go to the village.

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22,598. I suppose if you take the epidemic early enough, when there have only been two or three cases, you might succeed with partial evacuation?—Yes; but I do not think so if a case occurred in the village proper.

22,599. Do you attach great importance to carrying out evacuation promptly?—Yes.

22,600. Does the duration of the epidemic depend upon the time the evacuation occupies?—Yes.

22,601. When, after evacuation, cases still keep going on amongst the evacuated people, after the expiry of the period of incubation, to what do you think that is traceable?—It is generally due to the people visiting their houses surreptitiously.

22,602. Supposing that the plague had been only in one corner of the village when the people went out, and after some time a man whose house is in the opposite corner goes back to his house, do you find that he is liable to get the plague?—He is practically certain to do so. Infection spreads through the emptied village just as it would do if people were there, only more slowly.

22,603. In cases where you leave a few human barometers in their houses in the villages, do you find those people especially liable, or do they escape?—I think they are especially liable to infection.

22,604. From the fact that cases keep on occurring amongst evacuated people in consequence of their surreptitiously visiting their houses, I suppose that evacuation must be accompanied by the preventing of people from visiting their evacuated houses, must it not?—Yes; all should be done that is possible to prevent them.

22,605. Do you find that with so many villages to manage you are able to do this in the case of each village?—Where there are good village officers, it can be done; but where there are not, it cannot be done.

22,606. Do you think evacuation is practicable in the rains?—No; I think not.

22,607. Even when you do evacuate in the rains, is there in the huts in which the evacuated people are living sufficient ventilation to prevent the plague "catching on" to the floor in the same way as it does in a house in the village?—I think the huts are frequently worse than the houses in the village.

22,608. In what way?—In being worse ventilated.

22,609. What is the result of that?—The result is that a large number in one hut will frequently get plague.

22,610. When the hut is apparently infected in that manner, what would you do with it?—Remove it to another place.

22,611. What is your opinion with regard to the effectiveness of disinfecting with liquid disinfectants—for instance, perchloride of mercury?—As it has been done it has been of no use.

22,612. How do you draw that deduction?—Cases have occurred in houses which have been disinfected.

22,613. Who prepared your perchloride of mercury for you?—I have not done any disinfection with perchloride of mercury here. It has been stopped during this cold weather.

22,614. Have you tried the kiln system of disinfection at all?—No.

22,615. You have had no experience of it?—No; but I burned the floors with grass in one village.

22,616. Have you any experience of inoculation in the Satara district?—None.

22,617. How do think plague is carried from one village to another?—I think, practically, it is always by human agency.

22,618. How do you think it is carried from one house to another within the same village?—By rats, and also it seems to travel of itself; there are very few rats in some villages.

22,619. Do you evacuate all villages as soon as they are found to be infected?—Yes; immediately.

22,620. But I suppose all villages become practically unevacuable in the rains, do they not?—Yes.

22,621. The effectiveness of evacuation depends to some extent upon the size of the village, does it not? For instance, you could not evacuate Bombay town?—No.

22,622. Does it depend also, to some extent, upon the employment of the inhabitants?—That sometimes adds to the difficulty of getting them out of the village.

22,623. In what way? Who are the most difficult to get out?—Brahmans, Banniahs, and Sahukars.

22,624. Have you noticed whether there is a specially high mortality amongst children just before an epidemic, or just after one?—I have not kept any figures with regard to it, but there has been a high mortality amongst children in several villages just before an outbreak of plague.

22,625. Have you noticed that the same villages are getting infected this year as were infected last year, or in the two previous years, or does the plague this year seem to choose villages it did not attack before?—It has attacked both.

22,626. You have nothing to observe upon the matter?—No. The plague shows no preference for one or the other.

22,627. Have you noticed any case in which there appears to have been a genuine recrudescence of the old germ within the village itself?—Not that could be called a recrudescence.

22,628. It has been re-infection?—Yes. Cases have occurred on paper as recrudescent, but they were not really so.

22,629. Do you do any corpse inspection?—Yes; in the larger villages it has been done.

22,630. Is it offensive to the feelings of the people?—Yes, I think it is.

22,631. Have you noticed whether there is any decrease in the virulence towards the end of an epidemic?—The epidemic is always finished up in the fields; but I think just as many people die amongst those who get plague as in the earlier stages; the proportion of deaths is not smaller.

22,632. Have you noticed whether swellings in the necks of children seem to occur to a remarkable extent after the close of an epidemic when plague is supposed to be extinct?—No, I have not noticed that.

22,633. Have any cases come under your observation of people getting plague twice?—None.

22,634. In the fair weather, amongst the evacuated people living in huts, do you find that if one plague case occurs in a hut it is liable to infect the neighbouring huts?—It does not follow; it occasionally occurs.

22,635. Which of the measures are people so convinced of the usefulness of, that they will adopt them themselves in the future?—Evacuation, certainly.

22,636. Do they do their best to keep out infectious strangers?—It depends on whether they have influence or not. A man with influence will get into the village.

22,637. Do they appreciate the danger of returning to sleep in their infected houses after they themselves have been evacuated?—I think they do, but they prefer to run the risk, and make sure that their goods are all safe.

22,638. Is there great difficulty in guarding their property in the empty village site?—There is a great deal.

22,639. Do you find that dacoities begin?—Yes, they do.

22,640. Do the people ever boycott those who insist on going to their houses and laying the rest of them open to re-infection?—No.

22,641. Do you find that they readily report the fact of their village being infected, or do they conceal it up to the last moment?—That entirely depends upon the village officers.

22,642. Have any cases come to your notice where they have sent off an express messenger to you to let you know of the first appearance of plague?—Yes, in one or two villages they have done so. The majority of the villages, however, do not do so.

22,643. Coming to Karachi, I believe you served there during the greater part of the first epidemic?—Yes.

22,644. And during the whole of the second?—Yes. In Karachi I was during the greater part of the first outbreak, and during the whole of the second, in charge of the Jail and Market Quarters which include the Joria Bazar, in which quarter the plague was more severe than elsewhere.

22,645. Were there any reasons for this?—There were, I think, three principal reasons for this:—

I. The plague originated in this part, and when it had become severe the inhabitants of other parts left before the full strength of the infection had time to reach there.

II. The Joria Bazar is the grain bazar of the city; and rats therefore abound in this neighbourhood in enormous numbers. No less than 105 were found dead in one house.

III. The narrowness of the streets and insanitary condition of the houses, especially the existence in them of wells and cesspools, which attracted the rats to them from the grain godowns.

22,646. Did the disease spread very rapidly in the Market Quarter?—In the Market Quarter the spread of the disease was exceedingly rapid, and was I believe conveyed entirely by rats, dead ones being found in almost every house in the quarter. The number (105) mentioned above was the largest found in any single house, but 75 were found in another and 65 in a third.

22,647. What reason had you to believe that rats moved about from one house to another?—In one or two houses where the walls were touching, I have seen them go from one house to another.

22,648. What reason have you to believe that they carried infection to human beings when they did go there?—Because cases have occurred in very many of these houses, and the first cases occurred where there were most rats, round the grain bazar.

22,649. How was it, in your opinion, that rats were enabled to spread the disease to such an extent?—That the rats were enabled to spread the disease to such an extent was due, I believe, to the insanitary condition of the houses and the wretched way in which they are built. In that portion of the Market Quarter in which the disease was most severe, the houses are mostly very old and built of mud plastered over a framework of sticks. Latterly, stone fronts have been built to some of them, but the house behind its imposing exterior remains the same as before. The foundations of these houses are saturated with sewage and filth of every description. There is generally a cesspool below the house which has not been emptied since the house was built and the neighbourhood of which is honeycombed with rat holes, and which is probably situated close to a well from which water is drawn for washing, if not for drinking purposes. This evil was aggravated by the narrowness of the streets and the scarcity of windows. The Municipality have followed a fatal policy of allowing householders to encroach upon the streets and selling them strips of land in front of their houses until it has been absolutely impossible to sell any more. Overcrowding, moreover, exists to a terrible extent, especially amongst the Meghwars (weavers), who are, I should imagine, the filthiest class of people in India. So dirty are they and such outcastes, that in their own country they used to be, and I am not sure are not still, branded each with the initial letter of his village, and wherever he may go he is never allowed to sleep within the limits of the town or village. It is, therefore, not surprising that the plague was so severe in this quarter. Any epidemic disease would, I think, be the same. It is a notable fact that in the really good houses, some of which were situated in the very heart of the most highly infected quarter, practically no cases occurred.

22,650. Did you notice in what kind of floors the plague seemed to flourish best?—Most of the floors were cow-dung floors. Houses with cemented and tiled floors did not suffer so badly.

22,651. Do you think that the drainage system of the town had anything to do with the excessive severity of the plague?—No, nothing whatever.

22,652. Does a drainage system really exist at all?—Only so far as carrying away dirty water from a very small percentage of the houses is concerned.

22,653. Are the houses connected with the sewers?—No, very few; perhaps some half dozen.

22,654. Has much been done in Karachi since the epidemic to improve the ventilation of the houses?—Yes, a very great deal.

22,655. What is the value, from your experience, of sleeping upon a bed as contrasted with sleeping on the floor?—People who sleep on beds run far less risk of catching plague, I think, than those who sleep on the floor. In one house in the Market Quarter everyone in the house got plague except one old man, who lay on the only bed.

22,656. Do you think that the people still have a great dread of the hospitals?—Yes, I do. There is, no doubt, that a very large number still have a dread of hospitals, especially Government Hospitals, and also of segregation, but the greatest dread of all is that of being carried away in stretchers, which is very genuine, and I think it is best never to use them at all.

22,657. Did you disinfect with perchloride of mercury in Karachi?—Yes; all the houses were disinfected.

22,658. Did you find it effective there?—No; cases occurred in some of the houses which had been disinfected, which were re-occupied; no further re-occupation was allowed.

22,659. Could you exclude the possibility of infection having been brought in again from some neighbour?—I think it was probably brought in by rats again.

22,660. So that you cannot say that disinfection did not perform its duty properly?—No; but if so, its effects were counteracted immediately afterwards.

22,661. Do you think that after a certain time the plague germ will die of itself without any disinfection?—Yes.

22,662. At what period do you put that?—Between three and four months.

22,663. Why do you select that particular period?—Because I have known people going back after nearly three months, and they have got the plague.

22,664. Do you find a shorter time is necessary in the extremely hot weather?—That I have not noticed.

22,665. Though disinfection may be of no use when there are a great many rats infected, and plague is very severe in a town, can you say it would not be of use where only two or three cases have occurred in a town?—No.

22,666. Do you think it would be useful then?—I think it might be useful then.

22,667. If you do not use disinfection to an infected house, what would you do to it?—Evacuation is all you can do. The entire disroofing of infected houses is by far the most effective measure. The opening of holes in the roof and walls, though it is doubtful, I think, if it has much effect at the time, forms, provided the openings are transformed into ventilators and windows, a vast permanent improvement.

22,668. What is your experience of inoculation at Karachi?—I had coolies working in my division, at a daily strength of about 50. Twenty cases of plague occurred amongst them up to the end of the first week in May. There were probably more, as more men disappeared; but there were certainly 20 cases which I was able to trace. I had half the men inoculated on the 7th of May and the other half a week later. One man developed high fever after he was inoculated, and died on the fifth day. After this, no case ever occurred amongst these men, who worked for two months subsequently in the most highly infected quarter of the town.

22,669. Are those the men who had already come unscathed through the plague, while the other 20 men died or disappeared?—Half of them would be; the other half would be fresh men.

22,670. Besides these coolies, have you any experience of inoculation of any other people in Karachi?—Yes.

22,671. Who?—Some 900 people living in my division were inoculated, and there was only one case among them.

22,672. But were those 900 people of all classes?—People of all classes. Some of them were actual contacts, and they did not get plague afterwards.

22,673. Were there amongst them representatives of classes amongst whom you think plague would probably come? Or did they all belong to the upper classes who would not be so likely to be infected?—They belonged to all classes; but many of them belonged to the Borahs and upper classes, who are less liable to be infected.

(Witness withdrew.)

Lieutenant H. C. Steen, I.S.C., called and examined.

Lieut. H. C. Steen, I.S.C.

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22,674. (*The President.*) I believe you have been occupied in connection with plague matters at Satara?—Yes, in the southern part of the district.

22,675. You are a Staff Corps Officer, are you not?—Yes.

22,676. (*Mr. Hewett.*) How long have you been employed on plague duty in the Satara district?—Since the 19th of November, 1897.

22,677. Have you been continuously in the villages ever since that period at all seasons of the year?—Yes, I have been in the district ever since then.

22,678. Will you tell us what your experience has been as to the way in which a village gets infection?—I think it is almost entirely by the visits of people from infected villages to non-infected villages. I have had one case in which that has not been so.

22,679. When you say you have had one case in which it was not so, do you mean one case in which you were not able to establish that it was so?—I mean a case in which the infection was brought by other means.

22,680. Would you specify those other means?—It was supposed to have been brought to a village called Bambavda in the Valva taluka, by imported tobacco. A cartload of tobacco from Bhatavda, a neighbouring village, is said to have brought the infection.

22,681. Did you investigate this theory?—I was not there at the time, and I did not hear of it until afterwards. Dead rats were reported to have been found in the house, and three cases of plague occurred.

22,682. In the house to which tobacco had been brought?—Yes. Those three cases were amongst the first cases in that village.

22,683. How far is this village from the one from which tobacco was brought?—About two miles.

22,684. Was there any communication between the residents of the two villages?—I think there is communication between those villages.

22,685. What do you yourself think of the theory of communication by means of tobacco?—The theory is, I suppose: possible.

22,686. Do you attach any importance to it?—I do, provided infection can be imported by means of tobacco, in which case, this being the only theory of the infection of the village, I accept it in the absence of any other. In any case it appears that tobacco was brought from the infected village to a non-infected village.

22,687. And also that the plague occurred in the non-infected village?—Yes.

22,688. But do you think that it is impossible that plague got to the non-infected village by other means?—I think it is quite possible.

22,689. Have you a number of villages in which you have been able to trace the infection to people coming from infected villages?—Yes.

22,690. How many villages?—Seven villages at least. I have been able to trace it, more or less clearly, in about seven villages, and without doubt in one or two.

22,691. And you have had no village in which you have been able to clearly discover any other source of infection?—No.

22,692. Have you been able to trace any source of infection from clothes?—No.

22,693. Have you anything to say as to the part which rats play in disseminating plague?—Rats, I think there can be little doubt, disseminate the plague in a village, once the infection has been imported by human agency. Communication may exist, and probably often does, amongst the careless villagers, but when cases of plague are found occurring in totally different, and well separated, parts of a village, and amongst the highest and lowest castes, at the same time, there can, I think, be little doubt that the rats have spread the plague over the village, especially as large numbers of these vermin are, as a rule, found dead in the village, either before or at the same time as the occurrence of cases amongst the inhabitants. As a rule, they disseminated the plague in the village site very rapidly indeed, especially if it has been concealed, and little or no evacuation has taken place; but sometimes, as was the case in Islampur, which really forms one village with Uran, the plague develops slowly.

22,694. As a rule it has been your experience that you have found local cases of plague before you have found mortality among rats?—I have experienced both sets of circumstances.

22,695. Which is the more common?—I think rats dying first. There have been a good many cases of the other sort.

22,696. When you say rats dying first, do you mean rats dying between an imported case and a local case?—Yes.

22,697. Have you any instances in which you have moved people out of any infected portion of a village site, and then when the village has been re-occupied the people in another part of the village which was not infected at the time when it was evacuated have got the plague?—I have not had any examples of that.

22,698. Have you, as a rule, evacuated the villages in which plague has occurred?—Yes, always.

22,699. Even during the rainy season?—Yes.

22,700. Have you found any difficulty in doing so during the rainy season in this country?—It is very difficult, of course, during the rains, but I have generally got the people out.

22,701. Do you think that the people in the black cotton soil districts when they are turned out of their houses in the rains suffer considerable hardship?—Yes, I think they do. I can instance two villages especially, the village of Marul in Patan taluka and the village of Vathar in the Karad taluka. I used to visit the inhabitants of these villages after they had been evacuated, and go round their huts. They were suffering very great hardships indeed. They were all living more or less in mud. Some of them had good huts, but a great many of them had very uncomfortable huts.

22,702. Do you think that the hardship of living in huts during the rains led people to go back to their villages at nights?—It is quite possible. It is hard to say definitely what they do. Even if one seals the village, you find the seals broken. It is difficult to be certain in the majority of cases whether they go back or not.

22,703. Do you think that it is likely that they go back?—I think it is extremely likely that they go back. About the end of January 1898, in the village of Kale in Karad taluka, which had then been evacuated for about three months, I found, as the result of a night search that I made, considerably over a hundred persons surreptitiously sleeping in houses that had not been disinfected. Kale was, after Karad, the worst infected village in the taluka, and many cases of plague had occurred all over the village owing to the impossibility of evacuating it quickly in the rains. These persons may have been indulging in such practices for a considerable time, as the village was not sealed, but no fresh cases of plague were traced to them. The plague, in fact, had been dead at Kale for some weeks previous to the time that I made the search.

22,704. Did you find that when you evacuated people during the rains the plague took longer to bring under when the people were in the huts than it would if you had evacuated them in the dry season?—In the two instances I have named it did take a very long time. I have had other cases since then in which it took quite as long, if not longer, when the village was evacuated in the dry season.

22,705. But in both cases—both in wet and dry seasons—the establishment under your control does not enable you to be certain that the people do not get back to infected sites?—No, it does not.

22,706. How many infected villages have you to deal with at the present time?—Four villages in one taluka, and nine in another, about thirteen altogether, of which six are fully infected with plague, the other seven not being very bad.

22,707. Have you noticed a circumstance which Colonel Hay referred to yesterday, that villages on the banks of a river are especially liable to infection?—I noticed that in Valva taluka recently—last autumn. The fact is, that a very large proportion of the villages in that taluka are on the banks of the river, but I do not altogether connect the fact of their being on the banks of the river with the fact of their having become infected.

22,708. Have you resorted to disinfection in the villages?—I was doing that in Karad taluka shortly after I first arrived, and until I left there about six months afterwards.

22,709. What did you disinfect with?—With a solution of perchloride of mercury.

22,710. Do you do it now?—No, I have not been doing it since last July.

22,711. Is that in consequence of orders having been given to stop it?—No; orders were not given to stop it; but I was told to do it to a very much lesser extent. I made up my mind that it was not very much good, and so I practically did not do it at all.

22,712. Why do you think that it is of no use?—My experience of disinfection in Karad taluka first led me to doubt its value, but I do not feel competent to say that it is of no use. I did not begin until the villages had been evacuated for some considerable time. The shortest period was two months. In some cases I did not begin until they had been evacuated for five or even six months. In the meantime there had been a certain amount of re-occupation of those villages.

22,713. Is your reason for not believing in the efficacy of disinfection by perchloride of mercury that you think that the germ would be otherwise disposed of within a certain period, or that you think that the solution of perchloride of mercury cannot be efficiently used so as to destroy the infection throughout the house?—I think, in the first place, that the solution does not kill all the germs; in the second place if the village is left alone long enough the germs will probably disappear in some other way. My experience in Karad taluka tends to convince me that the germs did actually disappear somehow or other in a great many of the villages which I disinfected. I was led to believe that the germs had disappeared before disinfection was begun.

22,714. How long do you think that it takes for the germ to disappear?—I think two months is the least time.

22,715. Have you noticed that any of the villages have been infected in both epidemics?—Yes.

22,716. Are those villages which had been disinfected or had not been disinfected?—Villages which had been disinfected, and others also. It seems to take fresh villages more or less haphazard. When, however, the second epidemic began in the southern half of this district it attacked villages which had not been attacked before; but since then it has attacked others. No recrudescence of plague occurred in any of the disinfected villages in Karad taluka for months, and only then through re-importation. This seems to speak in favour of disinfection. At Andumbar Wadi, however, in Tasgaon taluka, which was disinfected under the "kiln-burning" process, but which was evacuated only six weeks, three fresh cases of plague at once occurred as the result of the inhabitants merely revisiting their houses.

22,717. Could it attack others by reason of infected people having come from infected villages?—It could, of course, and that is the probable reason; but it is impossible to be certain, as the people never, or seldom, assist such investigations. There is so much communication between the people that it is difficult to say what they do and where they go, and they practically never tell you. I think in nearly every case the infection of a village is due to re-importation.

22,718. You have not observed anything to lead you to believe in a recrudescence?—No, I do not think so. There has always been some story attached to the re-infection of a village to the effect that some man had come from some infected village, and in his house or in the adjoining houses cases have almost invariably occurred.

22,719. Have you observed a village which lies next to a village which was infected in the first epidemic get infected in the second epidemic while the village infected in the first epidemic escaped?—I have noticed instances of this, but consider it largely pure chance; though, at the same time, it is, I suppose, a likely theory, that the formerly infected village will be less susceptible to fresh infection than the one which has never been infected at all. Such a theory might be explained by the possible extermination or diminution of the rat community, as to which I have heard several affirmative opinions. It seems to me, however, purely a matter of what the people do, where they go, and what communications they have.

22,720. Did any of your disinfecting coolies get plague?—I did not have a single case of plague amongst my disinfecting coolies during the four months that I was engaged in disinfection work in Karad taluka, though, as I afterwards discovered, they were utterly careless of the possible risks they ran. I ascribe this immunity to the fact that, when I began disinfection, early in January, 1898, the first villages treated had been evacuated and partially unroofed for close on three months, while some of those taken in hand later had been evacuated four, five, and even six months. In fact, I came to the conclusion that the infection in these villages had somehow or other disappeared of itself, and that there was, therefore, none left to disinfect. Later, in July, 1898, when I sent the same coolies to Khanapur taluka to disinfect the small village of Upala-wangi, which, however, had only just got infected, their carelessness of the risks they ran resulted in no less than seven of them, out of twenty-four, being attacked with plague.

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22,721. In what respect were the coolies careless?—I heard from the Hospital Assistant in charge that they had been particularly careless. I was a long way off and I had not visited them for some time. I heard they had been in the habit of buying clothes they found in the infected houses, and eating food similarly found, and, in fact, doing all sorts of careless things.

22,722. How long after that village had been evacuated was it that they went to disinfect it?—Less than a month, three weeks.

22,723. Have you recently, under your orders, tried disinfection by means of the kiln process?—Yes; it was tried in Tasgaon last August and September.

22,724. In how many villages was it tried?—It was tried in three wadis in Tasgaon taluka.

22,725. Did you employ the villagers themselves, or coolies, to do it?—I employed coolies permanently. When they had once learned the work they were more useful than other fresh coolies whom we should have had to be constantly teaching.

22,726. Who did the work?—Assistant-Surgeon Merchant personally supervised the work, I supervised the disinfection work generally.

22,727. Did you find that the coolies employed in disinfecting in this manner suffered?—The coolies must be careful, in whitewashing the dusty floor, not to raise the infected dust and so inoculate themselves with plague, a physically impossible task. A coolie with a whitewash brush first raises the dust, afterwards he may, or may not, whiten the floor. The coolies must also be careful to remove, without a doubt, the whole of the whitened top layer of the floor. When, however, they enter a room with picks and shovels, confusion at once reigns. The supposed dangerous top layer of the floor is cut up, flung about, mixed with lower layers, and the probability is that a very great deal of it is never removed from the house at all. For these reasons I consider the "kiln-burning" process a more dangerous one than that I employed. During the disinfection of three small wadies in Tasgaon taluka in September, 1898, four coolies died.

22,728. They exposed themselves to all sorts of risks, apparently?—Yes. My experience of coolies is that they are utterly careless.

22,729. Were the coolies as careless when they went to disinfect by means of the kiln process?—I fancy so. There was so much work on hand, and so many villages got infected at that time that I fancy they were left a good deal to themselves; they were always careless, and more so when not closely looked after.

22,730. How long after those three wadis had been disinfected by the kiln process were the people allowed to re-occupy them?—In the case of Andumbar wadi, it was six weeks, and in the case of Khandoba wadi a little over two months.

22,731. Did any case of plague occur in houses which had been disinfected in this way?—In Andumbar wadi three cases of plague occurred.

22,732. Were you able to trace the infection in this instance to any other place?—No; it was reported to me by Mr. Merchant, the Medical Officer in charge of operations, and he said that there was no evidence that infection had been imported in any other way. The people were allowed to revisit their houses for the purpose of cleaning them after disinfection, preparatory to re-occupation. Almost immediately, the same day or the next day, three cases of plague occurred amongst the people who had so revisited their houses.

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22,733. Do you infer from that that the kiln process was not efficient in this particular case?—It seemed to point so. I am not, however, prepared to say so definitely. It is just possible that there was some other reason for the occurrence of these three cases. One never knows what may have happened, what the people may have done. Some man may have gone to an infected village and returned.

22,734. Do you think that the unroofing of houses is a desirable measure?—I cannot say whether it kills plague germs or not: but I think it is an advisable thing to ventilate a house in this or in some other way. A good many of the houses I have had to deal with have probably never had any ventilation for years. Mud-roofed houses, especially, are so constructed that it is almost impossible to ventilate the interior.

22,735. Have you had any experience with regard to the use of passes to try and check the movements of the people?—Yes; I have been issuing passes myself, and supervising the pass system, but I do not think they have ever been effectual. Hundreds and thousands of people are going about without passes every day. It is almost impossible to deal with these people except under a very complicated system of passes.

22,736. Do you think that the people in the villages understand evacuation; do they think it is a good measure against plague?—They only appreciate it after a certain point. In nearly every case they will stay in the village till the last moment. When plague gets very bad they will go out of their own account.

22,737. Do you think that when plague has got really bad in that manner that evacuation will stop it in a short time?—It must be prompt, otherwise the village population quickly become infected, and then evacuation does not stop the plague, but only diminishes it. The case of Uran is an example of this. In the rains, however, I was almost forced to the conclusion that evacuation was a failure, even to diminish an outbreak. The hardships and extreme discomfort to which the people are exposed in the cold, damp, and unhealthy fields seem to render them so susceptible to infection, that they readily fall victims in large number to any plague that may be about. Vathar and Marul in Karad and Patan talukas respectively are good examples of this. Both villages were evacuated in very bad weather, and necessarily slowly; in the former village almost 20 per cent. of the population was exterminated. Subsequent experience, however, went to prove that even in the rains evacuation is better that non-evacuation. The village of Atke in Karad taluka became infected in September last year, when the rains were exceptionally heavy. The inhabitants could not be induced to evacuate the village, with the result that the daily attacks so increased that six or seven were common, a state of things were common, a state of things worse than prevailed at Vathar at any time. Atke is, however, a good example of where, plague being severe, complete evacuation stopped it. During the first half of October the plague was at its height. On the 14th of that month the village was completely evacuated, and by the end of the month the plague had almost completly died away. The village of Takari in Valva taluka is another, though not so good. Plague broke out on 4th October 1898. The plague was concealed for some time, but by the 14th the village was completely evacuated and by the end of the month the plague had stopped entirely. The village of Padmal in Tasgaon taluka is a good example of where, plague having been discovered early, complete evacuation stopped the outbreak in a few days. Plague appeared in the village on the 22nd June 1898. By the morning of the 23rd it was completely evacuated, and after six cases had occurred the epidemic stopped. This village also well evidences the fact of the locality being infected. On the 29th June disinfection was begun, the people had necessarily to revisit their houses, and on the 4th July the plague began again, resulting in 30 fresh cases. But the village of Ashta in Valva taluka bids fair to be the best example of all of the value of evacuation in stopping an incipient epidemic. Two cases of plague occurred in the very middle of this large and hitherto uninfected town on the 16th December 1898. On the 17th and 18th a large block was evacuated, which by the 23rd was extended so as to include no less than 90 houses. Four cases have occurred amongst the inmates of the first and an adjoining house, but with these the epidemic appears to have ended. There is little doubt, however, that two or three houses are infected, and so the whole block is rigorously sealed, and no disinfection or other interference with any of the evacuated houses is permitted.

22,738. Please give us the facts as regards Uran?—The outbreak was first discovered on the 25th of September; the infection was supposed to have been imported about five days before, about the 20th.

22,739. Did you evacuate the whole village then?—I evacuated it by large blocks. It was all evacuated by the middle of October.

22,740. Is plague still in existence there?—There is an odd case from time to time even now.

22,741. When was plague at its worst there in the interval between evacuation and the present time?—It was at its worst during the whole of October and November. During both those months plague was very bad.

22,742. Practically it continued to be very bad for two months after it started?—From about the beginning of October till very nearly the middle of December.

22,743. Have you anything to say as regards segregation of the sick and contacts in villages?—Segregation of all sick and contacts in a particular place ought to be an excellent measure for a village, provided it is thorough, as in this way the infection will be gradually weeded out of a community until the epidemic stops. But for carrying out this measure thoroughly at a large number of infected villages resources must be ample. I have been engaged in this work for over a year now, but am afraid I cannot say much for the results. My resources were necesarily limited, and in fact the arrangements have been practically in the hands of the people themselves. They are of the following rough and ready description. I choose a suitable piece of ground, and order the infected families to live there for the required time, building their own huts, as they refuse to live in huts erected for them. They are required to make proper arrangements for comfortable residence during their stay. I allow the patient and one attendant to remain behind in the fields. Separate camps, and sometimes even separate huts, for sick and contacts have been found impossible, as families are, as a rule, too small to admit of being broken up. An instance of the latter is where a family consists of a man, his wife, and two or three very small children. If the mother gets attacked with plague, the husband attends on her, and the children cannot be separated from their mother. Consequently the whole family live together in the same hut. Such cases are common.

22,744. (*Mr. Cumine.*) I believe you have only six villages infected now?—That is about the number of the *badly* infected villages; the total number of infected villages in my charge is more than double that.

22,745. What is the approximate area that they cover?—Thirty-five miles long, by about ten miles broad.

22,746. During the rains I suppose the people, in order to make themselves comfortable in their huts, have to close up the huts almost as much as their houses in the village?—Yes, they have to keep out the rains and so the top part of the hut must be more or less watertight. The door of the hut, when there is one, is almost always open. I consider that there is always more ventilation in the huts than in most of the mud roofed houses. There is sometimes a door to the hut, which is practically never closed, and would not make much difference if it were, as it is generally of loose construction.

22,747. Have you done anything in the way of corpse inspection in the villages?—Yes, I have inspected corpses myself; and they are always inspected either by the village officers, or the Circle Inspectors.

22,748. Is the inspection of female corpses offensive to the feelings of the villagers?—I do not think so, as I have never had any trouble on this head except with gosha families.

22,749. Though the people may not have made any actual opposition, have you any reason to suppose that it is offensive to their feelings?—No, as a rule I do not think it is. They show their corpses quite freely, both male and female. Before death, also, I have often examined both male and female patients, and have never noticed that my doing so was offensive to their relatives. The people are often quite callous as to what is done with their sick and dead.

22,750. Have you ever tried treating patients in their houses, instead of removing them to hospital?—No; I have had very few patients left in the houses, occasionally one or two. The village has always been evacuated more or less promptly, and most of the cases have occurred in the fields.

22,751. Have you noticed whether an excessive mortality amongst children precedes or follows an outbreak of plague in a village?—No; I cannot say that I have.

22,752. (*The President.*) What is the population of the village of Uran?—About 6,000.

22,753. I understood you to say that plague had been developed for several weeks before evacuation was attempted?—No, I did not mean to say that, if I did.

22,754. After evacuation, do you mean to say that the development was not checked, but rather increased?—I cannot say to what extent the plague would have developed if the village had not been evacuated, but considering that two, three, four, and even five and six cases of plague occurred daily in the fields for no less than six weeks or so after evacuation had been completed, the extent to which this measure checked the development of the epidemic can hardly be said to have been marked.

22,755. When was evacuation commenced?—The infected house was at once evacuated.

22,756. On the same day the first case was discovered?—Yes.

22,757. How long did evacuation continue before it was completed?—For about a fortnight.

22,758. That would be early in October?—Towards the middle of October.

22,759. Prior to this period in the middle of October how many cases had occurred?—I think only four in the village, but upwards of 50 in all in the village and fields, I will not be absolutely certain.

22,760. It was a small number?—Yes, inside half-a-dozen in the village only.

22,761. After evacuation was accomplished in the middle of October, how many cases occurred among those people who were evacuated?—I think upwards of 200, if not more than that.

22,762. Where were they evacuated to?—Into the fields. Each man went to his own field.

22,763. What house or hut did he go to?—Each man erected his own hut in his own field.

22,764. Did he construct the hut inside or outside the village?—He constructed his hut outside the village.

22,765. Under what directions or superintendence?—His own.

22,766. He put it wherever it liked?—Yes, in his own field.

22,767. In the open country?—Yes.

22,768. As near any other hut as he pleased?—Yes.

22,769. Were any directions given as to there being sufficient air space in these huts?—No.

22,770. As a matter of fact was there any ventilation?—There was a certain amount of ventilation through these huts, by the doors, and their loose construction generally.

22,771. Is there more than one door?—As a rule there is only one door, but in some cases there are two doors. In the majority of cases, however, there is only the door space, but no door. When the door space is required to be closed, a blanket is hung across it.

22,772. Of what material are the doors made?—The same material as the huts, karbi.

22,773. Were the doors kept open all night or shut?—I cannot say for certain, though as a rule, they were probably kept open. The majority of the huts were however doorless. A certain number of the huts had loose doors.

22,774. Do you know whether the huts were ever overcrowded or not?—I think in the majority of cases they are generally pretty full of occupants.

22,775. Overcrowded?—Yes.

22,776. Was any precaution taken to prevent communication between those people who had gone out, or who had been removed, and the villages or houses from which they had been removed?—The doors of the houses were sealed.

22,777. Did the people go back?—Many seals were broken, and so possibly some did go back, but as they were never found living in the village, except perhaps an odd family or so, those who did go back can only have paid a hasty visit. No considerable number ever went back and stayed.

22,778. The probability is that they did go back because these houses would not be broken into by other people, there are not so many burglars?—Most of the seals were broken by children and mischievous people. I several times found that the seals on the doors of the compounds were broken, while the seals of the houses themselves were intact.

22,779. Were there many such cases?—Yes, a great many were like this. The evidence clearly pointed that the seals had been broken by mischievous persons.

22,780. You examined a good many houses and found this out?—Yes, I examined the villages regularly after evacuation.

22,781. And in nearly all these cases the outside seals had been broken, while the inside seals remained intact?—Yes; in the large majority of cases. The inside seal was not broken, but the seal on the large outside door leading into the courtyard was broken. Children and other mischievous persons did not seem to care to take the trouble to climb over walls for the purpose of breaking the inside seals.

22,782. The actual living rooms could not be entered without breaking the seals?—No.

22,783. Were there any windows by means of which the rooms could be entered?—Practically none.

22,784. I suppose in a certain number of these cases of seal-breaking, it was not done with mischievous intent?—In a certain number of cases it might have been done out of spite. The people knew generally that if seals were broken somebody would be punished. If a man had an enemy he would probably break the seal on his door.

22,785. Do you attribute any of the seal-breaking to the people themselves attempting to go into their houses?—I think only a very small proportion, if any, was due to this cause, the majority of cases were, apparently, due to either mischief or spite.

22,786. Can you tell me whether, in your opinion, in any instance whatever the people went back into these houses?—In a very few, odd instances, I have found persons paying temporary visits to their houses, and in an equally small number of cases I have found persons come back to stay.

22,787. From the precautions which you took can you say with confidence that they never did go back?—No, I cannot.

(Witness withdrew.)

(Adjourned till Wednesday, 1st March, at Belgaum.)

At The Collector's Office, Belgaum.

FIFTY-EIGHTH DAY.

Wednesday, 1st March 1899.

PRESENT:

PROF. T. R. FRASER, M.D., LL.D., F.R.S. (*President*).

MR. J. P. HEWETT. | MR. C. CUMINE.

MR. C. J. HALLIFAX (*Secretary*).

Mr. G. Carmichael, I.C.S.

1 March 1899.

MR. G. CARMICHAEL, I.C.S., called and examined.

22,788. (*The President.*) You are the Collector of Belgaum?—Yes.

22,789. You have had experience of plague in Sholapur, Kaira, and Belgaum?—Yes.

22,790. In regard to Sholapur, at what time did you have experience of plague?—In September 1897.

22,791. Were you there long?—I was only there for a month and a half after the plague began, and then I was transferred.

22,792. You cannot say much about it?—No.

22,793. Do you know how it originated in Sholapur?—I think that it was brought by Mr. Thatcher's servant.

22,794. Did you leave before the epidemic had ceased?—Yes.

22,795. Can you tell us, generally, what were the measures adopted while you were there?—Evacuation was not attempted, and we only tried to prevent the people from leaving Sholapur. We took the sick to hospital, and then the segregation camp was started.

22,796. While you were there, was the epidemic being brought down markedly?—Oh, no, it was going up rapidly.

22,797. The measures that you speak of do not appear to have been effective?—They were not effective.

22,798. With regard to Belgaum, is your experience only of the city, or of the cantonment and the city?—Only of the city.

22,799. Did you see both epidemics?—No, I came at the height of the second epidemic only.

22,800. Do you know how it originated?—In the city it began on the outskirts next the cantonment. There is nothing in between the city and the cantonment but a ditch and a wall. All the streets on the side of the city next the cantonment were first infected.

22,801. What did you infer from that?—That rats carried it from one to the other.

22,802. Was the city infected from the cantonment?—Yes.

22,803. At what date did the second epidemic begin?—It began about August 1898. There were a few cases in July. It has not really been clear, except for about four weeks after the first epidemic. It has not been quite clear, without a single case, for a whole month.

22,804. You think it was spread by rats?—I think it was brought over from the cantonment by rats, and also by the people, because we could not keep the people from the cantonment out of the town. From the fact that so many cases in the beginning occurred in the streets next the cantonment, I thought that not the people only, but the rats carried it over.

22,805. Were there many dead rats found in the city during the previous epidemic?—There were, but I cannot say from personal knowledge, as I was not here at that time.

22,806. That was in the rainy season?—Yes.

22,807. What were the measures which you adopted?—When I came here the people were left in their houses, and the only measures that were being carried out was disinfection of houses, and disinfection of burial parties.

22,808. Were the people segregated?—The people were not segregated, except for the time it was necessary to disinfect their houses. After their houses had been disinfected, and their clothes disinfected, they were allowed to go back to their houses again, and the sick were not removed to hospital unless they went voluntarily.

22,809. What time did that occupy?—From one to two days at most.

22,810. Did the sick generally live in houses with more than one room?—It was the usual house here, a long, deep, narrow house, with doors at the narrow ends. There are usually three rooms in it, each opening into the other. The two outside rooms have doors, the middle one being quite dark, with no windows.

22,811. So that they were able to move a patient to one of the other rooms while the room that had been occupied by the sick person was disinfected?—Yes.

22,812. Was that done?—Of course they were kept in one of the rooms, but you could not possibly keep the people separate.

22,813. While disinfection was going on, what was done with the people in the house?—They were taken to the segregation camp, and there their clothes were disinfected, and meantime the house and everything in the house was disinfected.

22,814. You say the people who were not infected in this house were removed to the camp?—Yes, the people who were not patients.

22,815. What about the patients?—The patient was left in the house, and the people were left in the house until he had either died or recovered.

22,816. In the meantime no disinfection was done?—No, that was done afterwards. If you had several cases occurring one after the other, sometimes it was a fortnight before the disinfection was done.

22,817. Was any special means taken to prevent communication from outsiders with this house?—We tried to do it, but it was of very little good.

22,818. What were the means?—Only advising the people not to leave the house.

22,819. There was nothing like a cordon?—No.

22,820. Was that effective in keeping the people from going into the house, or not?—No, I do not think so.

22,821. As a general result, were those measures successful or not successful?—We had a large death-rate, but I do not know that we had a larger death-rate than we would have had if we had segregated the people.

22,822. You did not segregate the people, but as a matter of fact did the plague go on growing, or did it not increase?—It went on growing. In October it went up to 60 attacks and 50 deaths for one or two days, and then it went down.

22,823. Whatever your method was, it failed to stop the epidemic?—Yes.

22,824. Did the epidemic continue into the dry weather?—It continued into the dry weather. Of course we persuaded a large number of people to go out, and at the end of the year there were only about 18,000 people in the town out of a population of 30,000.

22,825. Did the epidemic still continue?—Yes, we were still getting occasional cases here and there in the town, about three a week or four a week.

22,826. What was done with the people who went out? Did they go where they liked, or was there any kind of regulation as to where they should go?—We helped them to build huts in different places round the town, and they went out into them.

22,827. In certain prescribed places?—We helped them to select places.

22,828. Were there fewer cases among those who went out than among those who remained in?—There were.

22,829. How did you disinfect?—We disinfected with perchloride of mercury. We had disinfecting gangs who opened up the house and sprinkled the whole of the walls and floors with the solution, and steeped the clothes in the tub of perchloride of mercury, and then swept out the house and burnt up all the rubbish outside.

22,830. That was only the infected house?—In one or two cases we did the streets, whether the house had a plague case or not, but we did not attempt to do it all over the town. In the other part of the town we only did it in houses where plague cases had occurred.

22,831. In that one street did you remove the people altogether, or what did you do with them?—They went out. I ordered it to be evacuated.

22,832. How long did they generally stay out?—Many of them are out still.

22,833. There is no rule in the matter?—No.

22,834. Within what time might they return to Belgaum? Would any of them come back within a few days, or did they all stay out?—I cannot say definitely. Once they go out they all stay out, except the weavers, who get permission to come in and work in their houses in the daytime but not to sleep in them.

22,835. Do you know of any cases in which a disinfected house having become re-occupied has become again an infected house?—We have had several cases of that.

22,836. Can you give us an instance?—I cannot quote you an instance, but I think there are several stated in Mr. Uchgaonkar's précis of evidence.

22,837. At any rate, you know they did occur?—Yes.

22,838. You also did a good deal of inoculation with Haffkine's fluid?—That was done before my time, practically at the beginning of the first epidemic it was done. In December and January most of it was done.

22,839. That was before you came here?—Yes.

22,840. Have you no personal knowledge of it?—No, but I know the way they collected the statistics. Mr. Uchgaonkar has given the inoculation statistics in his précis of evidence.

22,841. Have you any opinion as to how long a village ought to be evacuated?—I cannot make a rule as to the time of it, but about three months after the last case, if possible.

22,842. You have not sufficient grounds to go on?—I have only dealt with one or two villages yet. It is only three months after the rains, and we could not evacuate during the rains.

22,843. I think you state in your précis that after that period of three months re-entering the house appears to have been safe?—If the house had been ventilated and thoroughly cleaned out.

22,844. That is only an opinion, you have no reasons for it?—I cannot give you any facts except judging from the villages that have had an epidemic, and it has passed over in the rains without any evacuation.

22,845. The epidemic ceased in those villages?—Yes, without anything being done; without much evacuation, and, in some cases, without any evacuation.

22,846. You have some cases without any evacuation, and without any disinfection?—Yes.

22,847. Do you remember those cases?—Neginhal was one instance.

22,848. What is the population of that village?—2,949. It was infected on the 21st July 1898, and the disease died out at the end of September.

22,849. How many cases were there?—496 cases and 391 deaths.

22,850. When this large number of plague cases was occurring, did the people not remove themselves from the village?—Some of them did.

22,851. To a large extent?—About half of them went out.

22,852. Therefore you had about 496 cases among 1,500 people?—I do not say that those that went out did not get cases.

22,853. They may have got cases?—Yes.

22,854. Do you know where they went to?—They went into huts round the village site.

22,855. Are you sure that they did not go to other villages?—Some of them may have done so; probably a few did.

22,856. Not a large proportion?—Not a large proportion of the whole.

22,857. Had you any means of knowing that—I mean by a census of the people who had gone out?—A census was kept by the Supervisor.

22,858. In the camp?—No, in the village. He made a census of the village.

22,859. I am talking of the people who left the village?—I am talking of the village as a whole, not of the village site. We make a census of the villagers, and those that leave have to get permission from the subordinate officer in charge of the operations there. In his daily statement he puts down how many people left the village, and we have to depend upon him.

22,860. It is recorded?—Yes.

22,861. Can you give us the number of villagers who went into other villages?—54 families, or, say, 300 people at most.

22,862. I suppose your view is not, however, that an infected village ought to be left to itself?—No, I do not think so.

22,863. If an infected village is left alone, experience shows that it suffers a very high mortality?—Yes.

22,864. What are your views about the importance of disinfection?—Disinfection with perchloride of mercury I do not think has much effect, except that it cleans out the house and airs it out for the time being, but if it is shut up afterwards it is just as likely to get infected by rats as it was before.

22,865. I asked you if you had instances in a town of where houses were re-occupied after disinfection. That would be the proof?—It is no proof, because we cannot tell where the infection was caught, whether it was in the town, or whether it was caught elsewhere, or in the house.

22,866. Why do you think disinfection was not effective, and that it was of no value?—It was done so much in the jail, and it did not stop the disease there.

22,867. Would you tell me the facts that came under your observation of what happened with regard to plague in the jail?—We first had a case on the 17th November. The man was found sick one morning and died at 9 o'clock at night, and two days after that we had another case. We had 10 cases altogether—four deaths and six recoveries.

22,868. Would you tell me how that bears on disinfection? You have not referred to that yet?—After each case we disinfected the whole of the jail. It is a small jail, and the clothes and everything about the prisoners were disinfected.

22,869. Nevertheless you had a succession of cases following each other?—We kept on getting cases.

22,870. Within what time did these cases occur?—The most of them occurred within one month, and then there were two isolated ones—after we had inoculated a good number—at the end of December and the middle of January. In the meantime the prisoners, after a week in the jail where they got the plague first, were removed to the small Civil jail, and some of them got it there.

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22,871. Did many of these cases occur in the small Civil jail, or nearly all?—The first case was on the 17th. There were two cases on the 18th, two on the 20th, one on the 22nd, one on the 24th November. These were all in November.

22,872. But why do you think that is a case proving that disinfection was of no use?—We disinfected every day the whole of the wards.

22,873. You have given me seven at least out of these 10, which all occurred within the incubation period. They might have been all infected at the same moment. There were only three left, which may have occurred in the other jail?—The next one was on the 1st December, and the next on the 23rd December. When the jail was vacated we moved into the new one on the 26th December.

22,874. The last case that occurred in the small Civil jail had nothing to do with the disinfection which you carried out in the other jail?—No.

22,875. Do you think it is important to disinfect clothing or other movables?—Yes, I think it is of great importance.

22,876. Have you any proof that it is of great value?—No, I do not think I can give any case.

22,877. It is just an opinion?—Yes.

22,878. You state in your précis that you think that clothes carry the infection. Have you any cases that seem to show that?—There is a case at Ugargol. There were six cases all in one house on the 2nd of October, and the man had come from Dharwar in the middle of August, and he had not gone back to Dharwar, as far as I could ascertain.

22,879. I do not quite see how the clothes carried the infection in this case?—It could not be incubating all the time. I say the usual way in which the villagers are infected is that some of them bring in infected clothes or other articles into the villages. It is not necessarily incubating on the man himself when he comes to the village.

22,880. Can you give us examples of clothes or other articles, apart from a human being, having brought plague?—I cannot mention any particular article that brought it.

22,881. How do you separate the person from the article?—Because plague is said to incubate at 10 days. If you had a man who was living in a village for a month—if he is going to have plague—if he conveyed the bacillus in his person—it would have come out and he would have had plague before.

22,882. I do not quite understand how that applies to this case?—This man came to Ugargol from Dharwar, which was, at that time, infected with plague.

22,883. When did plague occur in Ugargol?—He went to Dharwar on the 2nd of August. He came back in the middle of August, and the case occurred on the 2nd October—a month and a half afterwards.

22,884. But were not there other opportunities for infection?—That was the only infected village at all. It was the first infected in that part.

22,885. You think this shows he carried some article which was infected?—Yes, I think so.

22,886. Have you any other cases?—Then there is the case of Yedur. Dead rats were found in a coolie's house. That was the first intimation that plague was in this part of the village. The coolie himself did not die or any of his family until 15 days after the dead rats were found.

22,887. You think the rats were the cause of it?—I think rats got it first before human beings got it.

22,888. How do you think the infection originated in the house?—The man must have gone to some other village and infected the rats.

22,889. Do you know of your own knowledge of any inoculated persons acquiring plague?—Yes; there was the case of two persons in the jail.

22,890. Have you any others?—I cannot vouch for others.

22,891. What is your view as to the best measures to adopt? What, in your experience, is the most effective step to take when plague has occurred in a district or village?—For the people to go out and evacuate a village.

22,892. You have said you do not think disinfection was sufficient?—I think disinfection is a very good thing in dealing with imported cases, and segregation also. If you have an imported case, before plague had spread in a village, I think disinfection is good.

22,893. There are two kinds of conditions in the villages: one is where the village is largely infected—where there is a large number of cases in that village, then you think the best plan is evacuation?—Yes.

22,894. And on the other hand, where you have only one or two cases?—If they are indigenous, it does not matter—evacuation still is the only hope of saving the place.

22,895. If they are not explainable by importation, then you also think evacuation is necessary?—Yes.

22,896. But if they are explained by importation, what would you do?—I would have the man out and disinfect his house thoroughly.

22,897. On what theory do you draw the distinction?—I think it is spread by rats. If the rats in the village are not infected, and if they do not get the plague in houses in which imported cases occur, then it is not likely to spread to the village.

22,898. You do not know of any case occurring in a village in which there was no mortality among the rats?—No, not as far as I know.

22,899. (*Mr. Hewett.*) In Sholapur was there a byelaw for the registration of deaths?—Yes.

22,900. Did the Municipal Committee carry out the byelaw thoroughly?—I do not think so.

22,901. Is there a similar byelaw in the Belgaum Municipality?—There is.

22,902. Can you give us an estimate of the proportion of the population who ran away from Belgaum city during the second outbreak?—I am afraid I could not give you an estimate of those who ran away.

22,903. Do you think that many ran away?—Not a large proportion of them. At the beginning of the first epidemic there were a lot of strangers.

22,904. At the beginning of the first epidemic you had a large extra population?—Yes, I believe so.

22,905. What proportion of the population here at the beginning of the first epidemic ran away?—Less than one-fourth, I should think.

22,906. What I want to try and make out is, whether in your opinion a larger proportion of the population ran away during the first epidemic than in the second epidemic?—I do not know—I could not say.

22,907. Do you think that the fact that rigorous measures were not taken during the second epidemic encouraged the people to stay in the town rather than to run away?—Yes, in the case of the poorer people.

22,908. Could you state exactly when you first began to be able to get the people out of the town during the second epidemic?—It was well into November before we got an appreciable number out.

22,909. Was the disease on the decline when you got them out?—Yes, it was going down. The highest month was October and part of November.

22,910. By the time you were able to get them out, the disease had begun to decline somewhat rapidly?—Yes, of itself. The fall was comparatively rapid in November.

22,911. Could you state the number of villages which are infected in your district at the present time?—About 60.

22,912. Are they scattered all over the district?—They are mostly in the south.

22,913. How many European officers are employed on plague duty in the villages at present?—Six.

22,914. With that number can you insist on the effective carrying out of the measure of evacuation under European supervision in each individual village?—It is enough if we have good circle men under them. They instruct the circle men, and the circle officers go round.

22,915. Do you regard any agency except European agency as really reliable in the matter of evacuating a village, and seeing that people do not get back to the village site?—I think it is necessary; I do not think native agency is sufficient.

22,916. I suppose these European officers who are working the district have to move long distances from one infected village to another?—Yes, they have.

22,917. In addition to the villages which are now infected, you have had a large number infected at different times; could you give us a statement to show the number that have been infected in each month from the beginning up to February?—(The following statement was subsequently supplied by witness):—

	Villages.
Newly infected in 1897:—	
November	3
December	1
Newly infected in 1898:—	
January	12
February	8
March	8
April	3
May	3
June	5
July	7
August	28
September	33
October	37
November	34
December	27
Newly infected in 1899:—	
January	22
February	21

22,918. When did the villages in your district begin to be infected?—In January and February 1898.

22,919. Have certain portions of the district remained continuously infected ever since then?—Yes; certain portions of the district have always been infected since then.

22,920. Do you think that the evacuation of a village in the Belgaum district is accompanied during the rainy season with great hardship to the people who have to be evacuated?—I think it is a very great hardship.

22,921. Do you think that the hardship involved in evacuation is likely to make the people more liable to other kinds of illness; I am speaking of exposure in the rains?—I know that the Deputy Collector reported that the health of the only village which did evacuate during the rains was good.

22,922. Is it your opinion, the infection having been brought to a particular area by either a person, or clothes, or other effects, the person himself not being infected, that the disease passes into the rats, and that it takes some little time for it to become localized among the human beings of the place?—Yes, I think so.

22,923. I see from a report made by the Superintendent of the jail, that he seemed to connect the outbreak in it with the fact that the first man who got plague had been taken out from the jail to be placed under trial?—Yes.

22,924. Why do you think that this was not the method in which the jail became infected?—Other prisoners began to get plague immediately after the man was infected; and dead rats were found before. I cannot say that for certain: that is my impression.

22,925. The Superintendent says that the first one was found on the 19th; do you think they were found earlier than that?—I believe they were; but I have no proof of that. I only know that they removed the grain to a different place from where the rats were found. Shortly before, I believe, rats were dying, and they removed the grain; but they did not report it.

22,926. Was there plague in Gokak at the time?—Yes.

22,927. How far from the jail was plague in existence at the time in Belgaum?—The jail is on the top of the hill, about 300 yards from the nearest house in the town where there was plague.

22,928. Was there any plague among the warders in the jail?—No, I do not think there was. None of the warders took it. The jailer was living in the corner of the jail. He moved his family shortly before the outbreak—about the same time as the grain was removed.

22,929. I believe you were going to say something about the statistics of inoculation which have been separately furnished to the Commission. The statistics that have been prepared were prepared under your general directions, were they not?—Yes, under the directions of Colonel Peters, I.M.S., and myself.

22,930. Could you describe the manner in which they were prepared?—The record is not very complete. It is very difficult to identify the inoculated people in several cases, because there was often a single name entered, and very little else. A certain proportion is like that. In other cases the name, the number of the house, and the occupation of the person are entered in our Inoculation Register. They are arranged alphabetically; and we went over our Attack Register and Death Register and checked the names. That is the way we did it.

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22,931. Did the Attack and Death Register contain any particulars beyond the actual names of persons who died?—It gives the streets and houses; and it also is supposed to say whether a man was inoculated or not. That, however, has not always been given.

22,932. Has it sometimes happened that nothing but the name of the person inoculated has been entered in the register of inoculated persons?—That has occurred in some cases.

22,933. In what proportion of cases did you find only the name?—I could not say.

22,934. Would the proportion be sufficiently large to seriously affect the results?—No, I do not think so. Most of those without names were the servants of gentlemen who were not living in the cantonment, and people in the cantonment.

22,935. When you say that you have only heard of two inoculated persons acquiring plague, I understand you to mean two who have come under your personal observation?—Yes.

22,936. But you know that a considerable number died of plague?—Yes. There was the case of a man who came to a village in Parasgad taluka. He had been twice inoculated and he died. He came from Dharwar.

22,937. The villages now infected are in the south portion of your district?—Yes, mostly on the south side.

22,938. Adjoining what district?—Adjoining Dharwar.

22,939. Do you attribute the infection of the talukas at present infected to infection from your own district or from another district?—Partly from our own, and and partly from other districts. There are a lot of natives from the Native States. We have part of Sangli State in the centre of the suburbs of the Municipality.

22,940. Do you think that in order to make plague measures effective, it is essential that you should have the people on your side?—I certainly think so.

22,941. Do the people generally in your district recognize the benefits of any particular measures that are taken to suppress plague?—I think they do now.

22,942. What particular measure do you think they recognize as being useful?—They evacuate the villages of their own accord when they find rats. They do not understand why they should clean their clothes, and disinfect their clothes. It is very difficult to make them understand that.

22,943. But they do understand that they should leave a plague infected site?—Yes. Rats were found in the village of Holihosur in December; the people went out for a whole month, and they had no plague case until, I suppose, they thought they were safe, and some of them went back. We are having a few cases there now.

22,944. (*The President.*) I think you expressed the opinion that early information of plague cases was very important?—I certainly think so.

22,945. Do you think it is of primary importance?—Yes.

22,946. Otherwise, the alternative which you mentioned to removing the people is of no benefit; you cannot find out imported cases?—No.

22,947. If you always had early information, you would always be able to deal sufficiently early with imported cases?—Yes.

22,948. And, therefore, all other measures would be unnecessary, according to your views?—Yes; but my point is, that you cannot get information in many of the cases because the people do not know.

22,949. Have you any suggestion to make by which early information could be obtained as a practical measure?—I do not think you can get it. Very often you cannot tell whether a village has been infected, because it is infected before deaths occur and before sickness occurs.

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22,950. Do you mean that you cannot know until it is very strongly infected?—It may be infected without your knowing it, without deaths occurring amongst human beings. It is there that the difficulty of giving the first information arises. That is why I think it is important to disinfect clothes.

22,951. Even if you did hear of the first case in a human subject, that might not be sufficiently early information, because the place may be already infected?—It is usually too late when we hear of the first case.

22,952. What do you think has happened in that village?—I cannot say. It is only shown that rats are dying there.

22,953. You base your opinion upon the fact that rats may have died there?—Yes.

22,954. With reference to the distribution of the villages in your district in which plague has occurred, you have already given us the numbers; have you noticed any relationship to the larger rivers? Are the villages which have been infected near the rivers?—We have one large river going across. I do not think the nearness of the village to the river has any connection with infection.

22,955. The largest number of cases has not occurred near the rivers?—We have one or two villages near the large rivers.

22,956. Of all the villages infected, do you think the larger number are villages on the banks or near the banks of rivers; or, on the other hand, do you think that the larger number of infected villages are distant from the rivers?—I should think the majority of the villages are distant.

22,957. (*Mr. Hewett.*) Do you hold the opinion that the site of a village may become infected?—I do not know whether it is the site; the infection is there, in animals, or in the floor.

22,958. The only method of getting information is to get it from the village Headman?—Yes.

22,959. Although he is living in the immediate area, he may, in reality, be just as ignorant of the first case of plague as the Collector at the headquarters of his district?—Yes.

22,960. It may be quite impossible for him to give first information?—Yes, to give information that will be in time.

22,961. Even if he ascertains the first case of plague, and the time of the year happens to be the rainy season, may it not, in some instances, take considerable time for the information to come to you by the speediest manner in which it could reach you?—I should think the Patel, in the first place, would turn the man out if he was a villager.

22,962. If the Patel went with the utmost speed, it is possible that he might have a considerable distance to go to the Mamlatdar, and it would be some days before he would hear of it?—He might be on the wrong side of the river.

22,963. Does it not happen every day during the rains that a report takes a long time to reach the Collector?—Yes.

22,964. (*The President.*) I think you were going to explain that the delay need not necessarily occur on that account, as the authorities in the village would know how to act from directions which they had previously received; do I understand that to be the case?—If the Patel thought it was a case of plague, indigenous or imported, the man would be put outside the village.

22,965. (*Mr. Cumine.*) I should like to ask you a question about the medical establishment of the district. You have at the Sadr Station a Civil Surgeon, who is purely a Government servant, have you not?—Yes.

22,966. Is there any other purely Government medical servant in the district—purely Government as opposed to Municipal or Local Board?—There is none.

22,967. In each taluka would there be more than one Municipal medical man, as a rule?—No.

22,968. His duties would be restricted to the chief town of the taluka?—Yes.

22,969. So that you may say that throughout the villages of the whole district there is no Government medical man at all?—No.

(Witness withdrew.)

Lieut. G. B. Kidd, I.S.C.

Lieut. G. B. Kidd, I.S.C., called and examined.

22,970. (*The President.*) You are Plague Officer at Belgaum?—At present.

22,971. How long have you been Plague Officer in Belgaum?—Since the 6th November of last year.

22,972. Is this the only experience you have had?—I was 11 months in Sholapur before I came here.

22,973. (*Mr. Cumine.*) Your work in this district lies amongst the villages?—Yes.

22,974. And in Sholapur did it lie amongst the villages?—In the city at first.

22,975. For how many months were you in the villages in Sholapur?—About eight.

22,976. At present how many plague-stricken villages have you to look after?—Only two.

22,977. What is approximately the largest number of villages you have had to look after in this district?—About 22.

22,978. Could you give us any idea of the length and breadth of the area they occupied?—About 150 square miles.

22,979. Were you able to pay complete attention to any one village?—I had my head-quarters at one village, and attended there personally, and visited villages within 5 miles radius of that, generally one a day.

22,980. With 22 villages you would be able to visit each village about once in three weeks?—I think once in 15 days.

22,981. What establishment have you under you at the present moment?—I have one Abkari Inspector and one Karkun.

22,982. When you evacuate a village and you have to go on to another village, whom do you leave to look after the evacuated village?—If I am supplied with a Karkun or any outside man I leave him in charge, otherwise I leave the schoolmaster or the Kulkarni or Patel, whichever I think best.

22,983. Do you generally have a special officer to spare to look after the evacuated village?—I did not; one man had four or five villages to look after.

22,984. Mr. Carmichael has told us that though the people go out without much pressure, they require constant watching to prevent their going back into the village site and getting themselves re-infected there. Is that your experience?—I dare say in the village four or five people might go back. In fact, in one village I have just given them permission to go back, but they are rather chary about it.

22,985. Are they villages which have had experience of plague before, or are they infected now for the first time?—Now for the first time.

22,986. Do you find it is easier, in the matter of evacuation, to deal with villages which contain a purely agricultural population than with villages which contain a commercial population?—It is easier with the agricultural population. I have only met with very few commercial people in the villages.

22,987. What are your views with regard to having the co-operation of the people with us in any plague measures we wish to get carried out?—Nothing can be done unless the people be got to work with the Government. Any attempt to force them to go to hospital or to segregate contacts only drives them to conceal cases, and escape to other places carrying the infection with them. The same result would follow the inoculation operations carried on, even with mild compulsion.

22,988. What are your views with regard to evacuation in the rainy season in this district? Have you any experience of it?—I went out just at the tail end of the rains. It only rained perhaps once or twice a week for a fortnight. In Sholapur taluka we turned them out in the middle of the rains. There the rains are not so excessive as they are here, I believe.

22,989. Do you think turning them out in the rains entails great hardship upon them?—It depends upon the extent of the rainfall. In Belgaum taluka, I think, it would be a great hardship, but it was not in Sholapur.

22,990. Did you find that in order to protect themselves against the rain they barricaded their huts and stopped ventilation so thoroughly as to reproduce almost the same conditions as obtained in their houses?—They tried to, of course, but I do not think they had that success in their huts which they always had in their houses.

22,991. Did you find that evacuation in the rains succeeded in stopping plague in eight or ten days?—In the only village which I evacuated during the rains plague did not stop till about 25 days.

22,992. Did you notice whether the exposure had any prejudicial effect upon the health of the young or the very old?—Not on the very young, but perhaps one or two old people died on account of the exposure.

22,993. To what extent do you think the people will protect themselves in the way of keeping out suspicious strangers? Are they on the alert to do that?—The majority of them do keep out strangers if the village is in an infected area, but, of course, some always get in.

22,994. Those are the relations of influential people, I suppose?—Yes.

22,995. When plague has got into a village in this way, do you think the people who have had previous experience of plague will evacuate a village entirely of their own accord?—They do the infected gali, but they do not the remainder of the village. They never go out of the galis in which no deaths have occurred of their own accord; at least, I have never found it so.

22,996. Have you noticed whether imported cases from Bombay seem not to infect the local people here?—No cases have come to my knowledge from Bombay.

22,997. Have you noticed what the interval is between the arrival of an imported case and some local person getting infected? Is it four or five days, or a month?—Generally three or four days.

22,998. Have you specific instances in your memory?—I am afraid I have not any special case which I could bring forward.

22,999. Have you noticed whether rats invariably die between the arrival of the imported case and the occurrence of the first local case, as though the imported case was unable to infect the local case directly, but had to do it through some medium?—I have not noticed that.

23,000. Have you any system of passes so that a man is not allowed to go from one village to another without getting a pass?—He has to get a pass from the Resident Plague Authority.

23,001. How far may he have to go to get that?—At the outside it would be 3 miles from the village, and it is probably only one, but he need not go so far as that, because the fields are divided up into sections and each section has a supervisor.

23,002. He has not to come to you to get it?—He cannot. My camp may be 15 miles away. The Resident Plague Authority would be one of the native Karkuns or an Abkari Inspector.

23,003. The standing order does not require him to come to you for a pass?—No.

23,004. You mention in your précis a case of infection being carried by grain. You say that at Mohol village many Banniahs had the plague: some Mangs stole their grain, and a few days after many Mangs got plague. Are you able to exclude the possibility of the Mangs having caught the plague themselves when they came to steal the grain?—I am afraid I have not down the date of the evacuation of the house. The house had been evacuated by the Banniahs. Of course, they might have caught it through going into an infected house.

23,005. Do not you think it is more likely that they caught it from the infected house rather than from the grain?—No, because, in the village I came to about 10 days ago, a Banniah had got the plague, and a woman who bought some grain from him got plague four days afterwards. The Banniah's daughter had died of plague, and I could not trace how this woman had got plague in any other way, as she was living in a hut at least 100 yards away from any other hut, and she had not been in the village for a month, and none of her relations had died of plague. The Banniah's daughter had died before the woman went to the house.

23,006. But might not that woman have gone to the house a few days before while the daughter was still alive?—Yes, that is possible. This Banniah was living in a hut, and he had all his stores outside the hut on a little verandah, and I do not think the woman who went to buy grain went into the hut: she would have stayed outside.

23,007. With regard to the comparative cleanliness of the people, what are the people that you think are the most liable to be attacked by plague?—There is no distinction, I think.

23,008. You mention that in two villages no Mahars were attacked, and that Maharwaras were not evacuated, although the villages were. But did not the Mahars live apart from the rest of the villagers?—In both those villages there is only a street between them. One was the high road about 50 yards, and the other was less than 10 yards.

23,009. The Mahars are considered impure by the natives, and are not allowed to enter the houses of the natives; is not that so?—Yes.

23,010. You speak in your précis of instances having come to your notice where people have stolen clothing from plague corpses and contracted the disease. Will you describe those to us?—Only two cases came clearly under my notice. The first was one in which the Mahars, who went to bury a corpse, were found with the clothing of the corpse in their hut. In the other case the man died of plague in a hut alone, and a Mahar stole his clothing.

23,011. I suppose, in order to steal the clothing, he had to go into the hut, had he not?—Yes, but only for a minute. I consider he caught the plague from the clothing.

23,012. Do you think it is dangerous for people after having evacuated a village to go and live too near it, and, if so, why?—In this cotton soil, when they are gathering their harvest, I have found two villages where the rats came out of the village and the people caught plague: in fact they moved their huts two or three times, and the rats followed them.

23,013. What makes you conclude that the rats moved?—Because I take it that the rats do not live in empty fields.

23,014. Were they house rats?—I did not actually see the rats myself.

23,015. Do you know whether they were house or field rats?—I do not know.

23,016. Have you noticed whether the villages infected this season are the same villages which were infected last season, that is, last rains?—I was not here during the last rains. I see from the Government "Gazette" that none of the villages I was working in in Sholapur have got plague this year.

23,017. Have you noticed any case where a village after having become perfectly free of plague has got attacked by plague again months afterwards, and, so far as could be traced, there has been no second infection, but apparently a recrudescence from the original germ in the village itself?—Not months afterwards—20 days afterwards.

23,018. (*The President.*) You have always traced another importation of plague in those cases which would account for the recrudescence?—Yes; 20 days was the longest interval between the two.

23,019. Have you any system of corpse inspection in your villages?—No.

23,020. Do you think it would be offensive to the people to have the corpses of their female relations examined? — Yes, I think it would be, especially among the pardah women.

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23,021. Would it be practicable to examine the corpses of such women?—It would be impossible, I think.

23,022. Have you had any experience?—I had not even seen them alive.

23,023. You are not allowed to see them?—I presume they do not allow one to see them when dead, as one is not allowed to see them when alive.

23,024. (*Mr. Hewett.*) When you say you have not seen them alive you mean that you have not been permitted to do so on account of the customs of the people?—Yes.

(Witness withdrew.)

Lieut.-Col. C. T. Peters I.M.S.

Lieut.-Col. C. T. Peters, I.M.S., called and examined.

23,025. (*The President.*) You are Acting Civil Surgeon in Belgaum?—Yes, and Acting Deputy Sanitary Commissioner.

23,026. How long have you been here?—Since February 1897.

23,027. Can you inform us what was done to guard against the importation of plague?—In consequence of the prevalence of plague in the City of Bombay arrangements were made so early as the 22nd January 1897 to inspect and report on the arrivals in Belgaum by rail, agreeably with Government orders, and the Municipality detailed the Medical Officer in charge of the Municipal Dispensary to do this duty, who was instructed to take any persons suffering from plague to the Civil Hospital for segregation and treatment, and the first case of plague was discovered on the 24th January 1897, a passenger from Bombay to Goa, who was detained at the Civil Hospital in the infectious ward (which is set apart for the treatment of cholera or smallpox cases in ordinary years), and was discharged cured on the 10th February 1897. The Collector of Belgaum issued orders to the Railway Police "to detain passengers at Belgaum reasonably suspected "of being infected with plague until they have been "medically examined under the orders of the Civil "Surgeon to whom such cases should be instantly "reported *direct* as they occur." By direction of the Surgeon-General a separate hut was built for the reception of plague cases by the Municipality in the Civil Hospital compound.

23,028. I want to understand how plague was introduced?—In the month of September 1897 a pensioned Naik returned from Sirur with the family of another pensioned Naik, who had died of plague in the Cantonment Bazar Sirur. According to his statement, they were segregated at Sirur from the 20th to 31st August 1897, and their clothing was boiled in the presence of the Medical Officer before they left the camp. However, on the 11th September one of the children got plague and died in the Cantonment Bazar. On the 22nd September another child sickened, and died on the 11th October, and the third on the 19th October. Shortly after, his neighbours, the Kakars, became infected in the Cantonment Bazar, where he kept a grain shop. On examining the Cantonment Death Register kept by the Cantonment Police, it appears that Jainabbee walad Silar, aged two years, died on the 7th September, the cause of death being put down under "other causes." On the 13th October 1897 another child named Gafur walad Silarbux, aged five years, died, the cause of death being put down as "fever"; and on the 21st October 1897 another child named Abdulrahman walad Silarbux died in the Native Infantry Lines Bazar, the cause of death being recorded as "fever" All these children, pensioned Naik Silarbux informed me, had glandular swellings. Thus it appears that the infected family arrived in Belgaum on the 3rd or 4th September 1897, and on the 7th September 1897 the first death had occurred.

23,029. It was an imported case which was not discovered promptly?—That is so.

23,030. And no discovery was made until a good number of cases had occurred?—That is so.

23,031. How do you account for the extension of the disease to the town of Belgaum?—In the town of Belgaum the first case of plague occurred in the family of a store lascar. The child was admitted into the Civil Hospital on the 22nd October 1897, and no doubt he had received the infection in the Cantonment. There were no further cases in the town until the 6th November 1897, when a Brahman gentleman arrived from Poona from an infected part of that city and was admitted into the Kanburgi Camp. He died there on the following morning. The first indigenous case occurred on the 23rd November 1897, when a Mahratta took ill. He was serving under a Marwari who had a shop in the Cantonment Bazar, and was found dead in the town. Shortly after his master's death, this man was infected and was removed to the Kanburgi Camp. So it appears to have come from the Cantonments. The Brahman gentleman from Poona was removed within a few hours after his arrival.

23,032. Of the several measures which you employed, which do you think are the most effective?—Evacuation, segregation, and disinfection, in the order named. In an infected locality the first thing to do would be to evacuate the place, then segregate those who had been taken out and disinfect them, their clothing, all their belongings and their houses.

23,033. Have you had much experience of evacuation?—I had nothing to do with villages. I have had only charge of camps in connection with my hospital, where hundreds of people were brought in from infected houses. Some of them were sick, and some of them were contacts, amongst whom very few cases of plague appeared afterwards.

23,034. You had nothing to do with the actual process of evacuation?—No.

23,035. How do you account for the beneficial results of evacuation?—As soon as the people are taken out into the fresh air, and removed from the infected places, the number of cases ceased altogether, or only one or two cases occurred amongst them. I remember in December 1897 a Brahman woman was confined. Her baby was 14 days old, and the question was whether she was to be removed to the health camp or not, a case having occurred in the family. Her husband went in charge of the sick child into the health camp, and the child died, but the husband remained alive. This poor woman was allowed to remain in her house, with her child and two attendants, that is to say, her sister-in-law and her husband's brother. Both the sister-in-law and the husband's brother got plague in the house. The brother died in the house and the sister-in-law died in my camp, where she was removed after her husband's death, along with this woman and her baby. A large number of people were taken away, but with the exception of the sick child, who died in hospital, there was no other case amongst them.

23,036. What are the conditions which account for the benefits of evacuation?—Principally fresh air and sunlight.

23,037. Have you had much experience of disinfection?—Yes. I have used nothing but disinfection in my hospital and the health camp.

23,038. Not in the town?—Not in the town.

23,039. In the second epidemic the measures which might have been desirable were somewhat obstructed by the heavy rains?—Yes.

23,040. What was the result?—At first the sick and contacts were segregated, but subsequently neither the cases nor the families were removed, because we had no more space after my segregation camp had become crowded.

23,041. At first you did remove them?—Yes, and then when we got overcrowded we could not remove them. The consequence was instead of only the person first attacked dying, three, four or five cases died in the

houses. I can only say that from hearsay; the people have told me that when they came to my camp. There was a large family of Pattawallas, Peons, in the different offices here. Only two of the brothers came to the camp and four other brothers perished in the house. After they were brought into my health camp, 13 in all, there was not a single case among them.

23,042. You think that because you could not sufficiently evacuate, the epidemic spread all through the town?—Yes. The way the epidemic spread through the town was this. A person got sick in one house. Perhaps the sick person and an attendant remained there, and all the rest went into a comparatively healthy locality; they carried the infection there and died there. Sometimes it was not the sick, but rats which went from one place to another.

23,043. How do you account for the benefit of evacuation if the rats move?—Evacuation alone is not efficacious, but must be accompanied with disinfection. If the people go back to their houses, they would get plague again unless the houses are thoroughly disinfected. That is why I believe disinfection measures should be strictly carried out.

23,044. Have you had any experience of disinfection?—In the case of the first village which was attacked in November, a place called Kagwad, I went over to it with the Collector. That was thoroughly disinfected, because I sent over large quantities of perchloride of mercury at the request of the Hospital Assistant. To this day there has not been another case of plague in that village. I think the last case of plague was somewhere about March, but I am speaking from memory. After the first epidemic was over the people were allowed to go back to their houses, and to this day there has not been another case.

23,045. How long was the interval?—The epidemic lasted from November 1897 to March 1898. There have been no cases since.

23,046. Have you charge of the Civil Jail?—Yes. I am Medical Visitor of the sub-jail.

23,047. Had you any plague there?—We had in November 1898. I think about the 16th November an under-trial prisoner was reported to have had fever. My Hospital Assistant had gone to see him and the next day he was better, and therefore he did not go that day to see him. The fact of the matter is this. As there is no Hospital Assistant now attached to the jail, the patients are usually sent to the Civil Hospital when their cases are supposed to be serious. But this man suddenly got worse and died about 9 o'clock in the evening, on the 17th November. The next morning the Hospital Assistant, on viewing the body, found plague glands. We at first thought that as he had just returned after giving evidence at Gokak he had brought the infection from there. I went to see Mr. Carmichael and he rode up on his bicycle much quicker than myself, and found on going into one of the rooms which was formerly used as the hospital, but which is now used as a godown for grain and other things, that there was a very offensive smell in that place. Accordingly, the next day he took a disinfecting party there himself, and on digging up the place a dead rat was found. I heard subsequently that about five days or a week before this man died there were dead rats found in the godown. The grain was removed from there. The man who got plague was cooking his meals in the room next to it. My own impression is that the man got the infection, as well as the whole jail, from those rats. Afterwards rats spread all over the place, in the kitchen and offices and all the wards.

23,048. Dead rats?—Some of them were dying; they were crawling about and hardly able to move.

23,049. You say this man was under trial?—Yes.

23,050. How long had he been in jail?—I forget now; the jailer will be able to give that information.

23,051. Do you know whether it was a fortnight or a week?—Within a week.

23,052. When was he admitted into the jail? Will you kindly find out exact particulars?—Yes. (Witness subsequently noted as follows:—He was admitted into the sub-jail as an under-trial prisoner on the 25th October 1898; was sent to Gokak for trial on the 2nd November 1898; and returned thence to the jail on the 12th November 1898. He had fever on the 16th, and died in the jail at 9.30 p.m. on the 17th November 1898.)

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23,053. Do you think the rats may be infected by grain?—Yes.

23,054. Have you any observations which seem to support that view?—In the case of the pensioned Naik (Corporal) he was selling grain to sepoys and other people, who bought provisions from him. I believe the 26th Madras Infantry got it from him, because a child died in the Native Infantry Lines Bazar.

23,055. How do you know that there was no human conveyance; can you exclude that?—Of course there was human conveyance as well.

23,056. Have you any case in which you think there was clear evidence that the grain was so infected as to be able to produce plague?—No, I have no clear evidence.

23,057. Or with regard to grain bags?—No.

23,058. You have observed a difference in plague mortality at different periods of life?—Yes.

23,059. What do you say about that?—The only observations I have made chiefly are with regard to children under one year.

23,060. Is there any immunity or proneness with regard to plague at any period of life?—Children are more or less immune, either from plague attacks or fatal terminations.

23,061. Why do you say that?—In a place called Khanapur there were 38 deaths from plague and no deaths under four years of age, and only one child aged four years.

23,062. I suppose there is the normal number of children in this place?—I could not say that, but I presume that the proportion was the normal one. We always assume, under the vaccination figures, that about 32 children under one year are alive, out of the total births, viz., 40 per 1,000 of population.

23,063. You have no reason to suppose it was different here?—No. Again, in the village of Kagwad, there is no single child under one year of age who died of plague.

23,064. Were all these children vaccinated or not?—By the Vaccination Register we assume that about 32 per 1,000 of the population is available for vaccination and nearly three-quarters of them are vaccinated. I think it may have some protective influence.

23,065. Simply because the children under a certain age are not affected so much as older persons?—Yes.

23,066. With regard to rats, you place great importance upon the diffusion of plague by rats, do you not?—Yes.

23,067. Can you remember whether there were many diseased rats in the first epidemic of plague in Belgaum?—Not one was known when the epidemic first broke out; at least, I never heard of any.

23,068. Was there throughout the epidemic the same absence of dead rats, or was it most remarkable at some period in the epidemic?—Since May dead rats have been heard of, but not before; the first epidemic extended from October, 1897 to the end of April, 1898.

23,069. Therefore, throughout the first epidemic, there were, at any rate, not sufficient dead rats to attract attention?—That is so.

23,070. Nevertheless, plague was virulent?—Yes.

23,071. How does that bear upon your view of the great importance of the spread of plague by rats?—Families began to evacuate the town and went into the fields. Within a short time after they arrived there, they began to get dead rats in their huts.

23,072. Were there many cases of plague among the evacuated relatively to the number among the non-evacuated?—No.

23,073. (*Mr. Hewett.*) Do you consider that the jail was infected by means of rats?—That is my impression, because five days or a week before this prisoner died, I am told that dead or diseased rats were found.

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23,074. Did you see any diseased rats?—I did not see any myself.

23,075. What number of people went away from Belgaum?—I have no figures.

23,076. Can you form any estimate?—It varied in different periods. I think that, about January 1898, only one quarter of the population remained in Belgaum.

23,077. How many people went away from the neighbourhood altogether?—I do not think that figure can be obtained.

23,078. Had you anything to do with the inoculation here?—No.

23,079. Have you seen the records of the inoculations here?—Yes.

23,080. Where are they?—Some are kept by the Municipality and some by the Cantonment authorities.

23,081. Who has them now?—I think they will be found in the proper places.

23,082. In the case of the people who were inoculated in December 1897 and January 1898, what facts were included in the register?—The names were given in the Cantonment Register, but the professions were not given.

23,083. Have you seen the books in which the records are contained?—I have seen the Town Register once and the Cantonment Register oftener.

23,084. Have you looked to see what facts are contained in those registers?—Yes; the names, age, residence, the profession of the man, and the date of inoculation. Those are the principal things recorded in the Municipal Records. I have already stated what are contained in the Cantonment Register.

23,085. Is the residence recorded in the case of those people who were inoculated in December 1897 and January 1898?—In a general way.

23,086. What do you mean by residence in a general way?—In the town the street has been entered, and sometimes the number of the house also, so that you can find out exactly where they came from.

23,087. Is the place of residence recorded in every instance in the City Register?—I could not say that; I have not examined the books so carefully. In the cantonment some names were not put down at all, but it was simply stated that a certificate was given to a sepoy of the 26th Madras Infantry.

23,088. Can you state the number of those inoculated in the cantonment and in the Municipality?—No. I should like to state that in one case a woman left the camp with a certificate of Major Minchin that she had been inoculated, and it was found on inquiry that her name was not on the register. In other instances, I have recently found that transport mule drivers and dooly-bearers, of the Commissariat, who had been inoculated, are not recorded in the Cantonment Registers, that is to say, they had not been entered up to that date.

23,089. When were they inoculated last year?—Those papers I saw were dated September, October, and November, 1898.

23,090. And they have not been entered yet?—Probably they have by this time. I got them in the Cantonment Magistrate's office, when we were trying to fill up inoculation figures and verify the number of deaths.

23,091. How long after inoculation?—Only last month—February.

23,092. (*Mr. Cumine.*) What was the approximate number of those people who had been omitted in this way in the register at the time of inoculation? 5 or 500?—I could not say.

23,093. (*The President.*) How many do you know?—In the nominal roll of inoculated persons in the Belgaum Cantonment who have got plague, I have a number of names which have been recorded who came to the Civil Hospital after having plague, and from inquiries I found out that they had been inoculated, but the names cannot be found in the Inoculation Register.

23,094. Can you tell us how many?—46.

23,095. (*Mr. Hewett.*) How many people came into the Civil Hospital suffering from plague after having been inoculated?—11 cases from the town and 49 from the cantonment—altogether 60.

23,096. In how many of those cases were you unable to find a record of inoculation in the registers?—Only three have been identified in the Cantonment out of 49.

23,097. Forty-six people stated they had been inoculated, but you could not find out whether they had or had not been from the register?—That is so.

23,098. How many did you identify in the town?—3 were identified out of 11.

23,099. Three persons said they were inoculated, but you could not discover their names in the town register?—Yes. One came from Gadag.

23,100. The remaining two came from the town?—Yes.

23,101. Did you have any cases of inoculated persons getting plague who were not treated for plague? You have 60 down here?—These 60 names were sent to me by the Chairman of the Municipality; they have been verified cases.

23,102. You do not put them forward?—No; I only put the hospital cases forward.

23,103. (*The President.*) I believe you can give us a case in which plague occurred twice in the same person?—Yes. A boy named Miran Sahib, father's name Hussan, was admitted to hospital on the 8th November 1897. He had several glandular swellings, and his temperature, at one time, went up to 105·4. The high temperature lasted for eight days, and after that it went down, but the buboes took a long time to heal. He was discharged cured on the 28th of December 1897.

23,104. Did you ascertain how he acquired plague?—Yes. Major Minchin told me about this case. The boy was the son of Major Minchin's bhisti (water carrier). When he went to search the house, he found that the child was being secreted by the back door. It was taken up and was found with fever, and was sent to the Civil Hospital for treatment. The rest of the family were sent for segregation to the segregation camp of the Cantonment.

23,105. There had been already a case of plague in the house?—Yes.

23,106. When did you see this person again?—A note from the Hospital Assistant on plague duty in the Cantonment Bazar states that the child had been ill for seven days, as a hidden case, with fever and a bubo, on the 22nd of February 1899. It was a hidden case the second time also. He came over to the Civil Hospital on the 22nd of February 1899, with a temperature of 102·4, which went up to 102·6 on the 23rd. Then on the 24th and 25th, it gradually came down till on the 25th it became normal, and the temperature now continues normal. He has a swelling on the same groin as before; on the previous occasion he had also a swelling on the neck. This boy is still in hospital.

23,107. Was this boy inoculated?—No.

23,108. You wish to put in a statement to show that in houses containing both inoculated and non-inoculated persons some of the inoculated acquired plague, while some of the non-inoculated escaped. How many cases are there?—Four. I put those cases in, as follows:—

OCCURRENCES of PLAGUE in HOUSES INHABITED by PERSONS INOCULATED AGAINST the PLAGUE.

Full Address.	Names, Sexes, and Ages of the *inoculated* Persons who were living in the House on the Date of Attack, with the Dates of their Inoculation. Name, Sex, Age of the Attacked, if he is amongst the *inoculated*; Date of Onset of Disease, Symptoms, Issue.	Names, Sexes, and Ages of the *un-inoculated* Persons who were living in the same House on the Date of Attack. Name, Sex, Age of the Attacked, if he is amongst the *un-inoculated*; Date of Onset of Disease, Symptoms, Issue.
	HOUSE No. 1.	
Jalgar Gali, Khadakwada	Gangubai F. Vithoba. Female. Age 10 years. Not attacked. Sorabai K. Vithoba. Female. Age 30 years. Inoculated on the 8th January, 1898. Date of onset of disease, on or about the 19th January, 1898. Fever temp. 100°. Pulse 96, full and strong. Glandular swelling about the size of a large lemon behind the left ear; only one, which did not suppurate. Admitted 22nd January, 1898, discharged cured, 10th February, 1898.	Govinda F. Vithoba. Male child. Age 3 years. Not attacked.
	HOUSE No. 2.	
Cantonment, Belgaum, Madras gali.	Francis F. Rayapa.* Agnes K. Francis Rayapa.* Francis F. Francis Raya. Inoculated 8th January, 1898. Dose $\frac{1}{2}$ cc. Chowriapa Raya. 8th January, 1898. Dose 1 cc. †Mari F. Raya. 8th January, 1898. Dose 2·10 cc. Arakswamo F. Raya. 8th January, 1898, Dose 1·5 cc.	Yesu Manwell F. Francis Raya. Baby. Age 8 months.
	HOUSE No. 3.	
Konwalgali, Town of Belgaum	Tulsabai K. Narayan. Female. Age 40. Inoculated 10th January, 1898. Got ill on the 3rd February, 1898. Noticed a swelling in right groin, after being ill, accompanied with pneumonia. When brought to hospital on the 6th February, 1898, was rather delirious. Tongue dry and brown. Bowels said to be not moved for three days. Discharged cured, on the 3rd April, 1898.	Krishnabai kom Hanmant Female. Age 20 years. Not attacked.
	HOUSE No. 4.	
Aditwar Pet, House No. (not known).	Adiwewa K. Baseteppa. Female. Age 35. Got fever 13 days after inoculation. Saw glandular swelling in left inguinal region after three days of fever. Only one—was opened on the 18th January, 1898. Discharged cured, on the 7th February, 1898.	Shivalingawa K. Timapa. Female. Age 75. Pitrawa K. Mallapa Gonje. Female. Age 35. None attacked.

* Names cannot be found in register: state that they were inoculated at the Hindalgi Camp.
† Mari was admitted on the 29th August, 1898, with a swelling in right groin. Discharged cured on the 15th September, 1898.

23,109. In the case of house No. 1, what was the age of the person who escaped?—Three years.

23,110. This is a person who was not inoculated?—Yes.

23,111. What was the age of the non-inoculated person in house No. 2?—Six or eight months old.

23,112. In the third house there were two persons?—Yes; one was a mother and the other the daughter; the mother, aged 40, was inoculated, and the daughter, aged 20, was not. The latter did not get plague.

23,113. What about the fourth house?—In the fourth house there were two persons who were not attacked; the first was 75 years old, and the other was 35 years old. Neither of them was inoculated. There was another person who was also 35 years old, who was inoculated, and got fever 13 days after inoculation, with glandular swellings. She was discharged cured.

23,114. In the first two houses and in one instance in the fourth, the persons were, I think, on account of their age, not very liable to plague?—That is so.

23,115. In the households where these cases occurred, there were other cases I think?—I cannot say that. That information is not supplied to us by the Supervisors who send them over.

(Witness withdrew.)

Mr. G. UCHGAONKAR called and examined.

Mr. G. Uchgaonkar.

(Evidence translated).

23,116. (*The President.*) You are Chairman of the Municipality of Belgaum?—Yes.

23,117. (*Mr. Hewett.*) Is there any system of registration of deaths here?—Yes.

23,118. Is there a byelaw requiring a householder to register every death that occurs in the house?—Yes.

23,119. Is that byelaw carefully observed?—Yes.

23,120. Do you think that cases of death ever fail to be registered?—All deaths have probably been duly registered.

23,121. Do you think that cases of death escaped registration at times or not?—I do not think so.

Mr. G. Uchgaonkar.

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23,122. What is the population of Belgaum?—27,952 according to the census of 1891.

23,123. Is the population larger now than it was then?—The present population is 18,948.

23,124. When did the first epidemic of plague begin?—The outbreak first commenced in October 1897. In April and May 1898 the number decreased, and the epidemic finally stopped in June 1898.

23,125. When did the second epidemic begin and terminate?—It began in July 1898; it is not yet ended.

23,126. Are casual cases happening at the present time?—There was one case to-day.

23,127. Do you put in a table showing the number of cases from October 1897 to the end of 1898?—Yes.

Weeks.	1897.						1898.											
	October.		November.		December.		January.		February.		March.		April.		May.		June.	
	Attacks.	Deaths.	Attacks.	Deaths.	Attacks.	Deaths.	Attacks.	Deaths.	Attacks.	Deaths.	Attacks.	Deaths.	Attacks.	Deaths.	Attacks.	Deaths.	Attacks.	Deaths.
1st -	—	—	1	1	10	6	56	53	24	22	7	8	1	1	2	2	—	—
2nd -	—	—	1	1	12	9	76	74	7	4	3	3	1	—	1	1	1	1
3rd -	—	—	2	2	32	25	56	49	12	10	1	1	3	3	—	—	5	2
4th -	1	1	7	5	40	38	37	31	7	5	4	4	1	1	—	—	2	2
Total -	1	1	11	9	94	78	225	207	50	41	15	16	6	5	3	3	8	5

Weeks.	1898.															
	July.		August.		September.		October.		November.		December.		Total 1897.		Total 1898.	
	Attacks.	Deaths.	Attacks.	Deaths.	Attacks.	Deaths.	Attacks.	Deaths.	Attacks.	Deaths	Attacks.	Deaths.	Attacks.	Deaths.	Attacks.	Deaths.
1st -	5	2	11	7	106	67	327	257	144	105	22	20	11	7	107	544
2nd -	7	6	18	16	211	149	394	299	102	68	26	21	13	10	847	642
3rd -	12	8	33	32	206	148	346	264	72	62	8	9	34	27	754	588
4th	11	8	58	44	307	223	237	157	35	32	9	8	48	44	708	515
Total -	35	24	120	99	830	587	1,304	977	353	267	65	58	106	88	3,014	2,289

23,128. Do you know the total number of cases in January of this year?—The number of cases in January 1899 is:—

Attacks.	Deaths.
14	13

23,129. What were the main measures adopted in the first epidemic?—The measures adopted to suppress the epidemic were as follows:—

(1.) The first and foremost was the segregation of the sick and of their families in hospitals and quarantine camps. This was rigorously enforced.

(2.) The second—disinfection of the infected area—was effected by removal of the roof or part of it, and by squirting a solution of carbolic acid (1 in 20), or perchloride of mercury (1 in 1,000), by means of garden syringes over the floor and all the interior walls and roof. Much light and air were let in.

(3.) All the clothes and beddings of infected persons were invariably burnt at his death or recovery, and the clothes of the rest of the family were soaked in a solution of carbolic acid or perchloride of mercury.

(4.) Families were not allowed to return to their houses until the houses had been thoroughly disinfected, and until at least 10 days had elapsed from the time any of the members of the family were exposed to some kind of infection, and until all their clothes had been disinfected in the presence of persons specially appointed for the purpose.

(5.) When a house was once vacated it was rarely allowed to be reoccupied within a fortnight or so.

(6.) Houses in the immediate neighbourhood of infected houses were treated as suspected, the inmates were segregated, and the buildings disinfected.

(7.) Houses where rats were found dead, apparently by plague, were treated as infected places, and were disinfected in the same way.

(8.) Thorough cleaning up of all the houses infected by plague and dead rats ultimately led to the wholesale disinfection of all the buildings situated within the Municipal limits.

(9.) People were induced and encouraged to erect their own sheds in fields, and to remove themselves there.

(10.) Number of huts were erected on the fort glacis for the convenience of those who were unable to have their own huts.

(11.) Until the setting in of the rains the weekly Saturday bazar was held outside the town.

(12.) Re-occupation of houses was complete by the end of June 1898.

23,130. Did you superintend the work of inoculation in the town?—I did not.

23,131. Can you say when inoculation in the town began?—On the 26th December 1897.

23,132. What privileges were given to the inoculated?—These persons appeared themselves for inoculation on the assurance given to them that no inoculated person would be liable to be removed to hospital or segregation camp against his wish. This privilege was subsequently withdrawn, and the following special rules were given to persons inoculated:—"A person "inoculated before January 16th is not liable to "segregation, even if a case of plague occur in the "house in which he is living, unless he himself gets "plague. In that case he will be sent to the Civil "Hospital, if he prefers, instead of to the Kanburgi "Hospital. Persons inoculated after January 15th "will still be liable to segregation in case plague occurs "in their houses." The result of the withdrawal of the privileges was to put almost a complete check to further progress of inoculation. The per-centage of inoculated persons over the population was 12·2.

23,133. Do you put in a statement showing the number of persons inoculated week by week?—Yes. It is as follows:—

STATEMENT showing the NUMBER of PERSONS INOCULATING, week by week at different places from 26th December 1897 to 31st December 1898, at the BELGAUM MUNICIPAL OFFICE and OTHER INOCULATION STATIONS.

Weeks.	Belgaum Town.	Belgaum Cantonment.	Shahápur.	Anigole.	Vadgaon.	Khánápur.	District.
26th Dec. 1897 to 2nd Jan. 1898	805	67	73	4	8	2	6
3rd Jan. 1898 to 9th „ „	415	8	16	—	4	—	16
10th „ „ to 16th „ „	246	20	10	—	—	—	1
17th „ „ to 23rd „ „	120	1	15	—	1	—	5
24th „ „ to 30th „ „	21	1	22	—	21	—	—
7th Feb. „ to 13th Feb. „	43	—	18	—	13	—	—
18th July „ to 23rd July „	9	—	—	—	—	—	—
25th „ „ to 31st „ „	3	9	5	—	—	—	—
1st Aug. „ to 7th Aug. „	20	14	18	—	—	—	—
8th „ „ to 14th „ „	24	3	—	—	—	—	—
15th „ „ to 21st „ „	72	12	17	—	4	—	10
22nd „ „ to 28th „ „	229	15	18	—	2	—	7
29th „ „ to 4th Sept. „	218	33	32	2	3	—	4
5th Sept. „ to 11th „ „	23	49	189	—	—	—	12
12th „ „ to 18th „ „	50	—	40	—	—	—	8
19th „ „ to 25th „ „	52	—	28	—	7	—	—
26th „ „ to 2nd Oct. „	41	—	14	—	2	—	—
17th Oct. „ to 23rd „ „	41	23	23	32	—	—	—
24th „ „ to 30th „ „	11	13	—	—	—	—	—
31st „ „ to 6th Nov. „	15	1	—	—	—	—	—
14th Nov. „ to 20th „ „	2	—	1	—	—	—	—
19th Dec. „ to 25th Dec. „	—	23	—	—	—	—	—
26th „ „ to 31st „ „	1	—	—	—	—	—	—
Total	2,565	292	448	6	65	2	69
Grand total	3,447						

23,134. How many cases of plague, and how many deaths occurred among the inoculated persons?—There were 62 attacks and 33 deaths.

Mr. G. Uchgaonkar.

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23,135. Do you think that the people were very much afraid during the first epidemic?—The people got frightened, partly on account of the Government measures and partly on account of the epidemic.

23,136. Did they report the number of cases accurately in the first epidemic?—They were not reported accurately.

23,137. Were the cases accurately reported in the second epidemic?—They gave information.

23,138. Why did they do that in the second epidemic?—During the second epidemic the patients, as well as the people, were allowed to remain in their houses, and therefore they quite accurately gave information and they had no fear.

23,139. Do you think that during the first epidemic many persons ran away from Belgaum suffering from plague or while they were in the incubation period of plague?—I think persons with sickness must have gone out of Belgaum.

23,140. Did they do so also in the second epidemic?—No.

23,141. Did the people here show any objection to being treated with European medicines?—Some illiterate persons have objected to Europeon medicines, and said they would prefer native treatment.

23,142. Who prepared the statement showing the names of inoculated persons who had attacks of plague, but subsequently recovered, and also the list showing those inoculated persons who died?—I prepared this statement.

23,143. Will you put it in?—Yes.

LIST showing the NAMES of INOCULATED PERSONS who had ATTACKS of PLAGUE but subsequently RECOVERED.

Serial No.	Street.	House No.	Name in Full.	Age.	Caste.	Occupation.	Number and Date of Inoculation.		Number and Date of Attack.		Remarks.
1	Khasbag Baswan.	324	Dhondubai Govind	40	Hindu	—	447	30th Dec. 1897	280	7th Jan. 1898.	
2	Tahsildar Gali	181	Rama Balu Tahsildar	25	Mahratta	—	64	26th „ „	289	8th „ „	
3	Gondhali Gali	3260	Meghasham Purshotam Redker.	—	Bráhman	—	979	5th Jan. 1898	345	17th „ „	
4	Kangral Gali	3892	Abdul Rahman valad Hussen.	25	Musalmán	—	142	28th Dec. 1897	392	27th „ „	
5	Kolkar Gali	887	Abdul Kadir	35	„	Hamál	1473	10th Jan. 1898	753	20th June 1898.	
6	Bogarwes	1447	Sorabji Edulji Burjorji	10	Pársee	—	101	28th Dec. 1897	893	20th July 1898.	
7	Mujawar Gali	295	Laxmi Monappa	30	Mahratta	—	792	4th Jan. 1898	1246	4th Sept. 1898.	
8	Math Gali	519	Bhimaji Govind Harpanhali.	15	Bráhman	Student	206	28th Dec. 1897	1264	6th „ „	
9	Mahadeo Gali	1381	Abaji Ganesh Kulkarni	30	„	Kárkun	26	„ „	1463	16th „ „	
10	Deshpande Gali.	1282	Bhaurao Ramchandra	38	„	Stamp-Vendor.	250	29th „	1618	22nd „ „	
11	Gondhali Gali	3179	Deopa Laxman	26	Mahratta	—	177	28th „	1684	24th „ „	
12	Burud Gali	1670	Dhodappa Sannappa	14	Burud	Burud	388	30th „	1806	28th „ „	
13	Senginkera	—	Datto Kallappa	8	Hindu	Shimpi	743	2nd Jan. 1898	1927	1st Oct. 1898.	
14	Resaldar Gali	3398	Laxman Nagesh Moghe	14	Bráhman	—	270	30th Aug. 1898	2217	11th „	
15	Aditvar Bazar	622	Sakharam Appaji	50	„	—	139	26th Dec. 1897	2227	„ „	
16	Khanjar Gali	3408	Ramchandra Appaji	42	Mahratta	—	997	24th Sept. 1898	2279	12th „	
17	Samadeoti	3355	Chandrabai Govind	12	Hindu	—	1021	30th „	2281	13th „	
18	Uppar Gali	2047	Drupatibai Monappa	7	Mahratta	—	973	4th Jan. 1898	2301	„ „	
19	Kasai Gali	2559	Ibrahim valad Kashim	14	Musalmán	—	20	28th Dec. 1897	2559	20th „	
20	Mujawar Gali	253	Jaya bin Shidappa	—	Mahratta	—	48	„ „	2547	„ „	
21	Pangul Gali	1911	Awali kom Shiwappa	30	„	—	858	3rd Jan. 1898	2667	24th „	
22	Bogarwes	1463	Parwati Shankarrao	17	Bráhman	—	698	4th Sept. 1898	2811	29th „	
23	Konwal	1246	Dongaru Nagappa	20	Mahratta	—	362	26th Dec. 1897	2921	3rd Nov. 1898.	
24	Jalgar Gali	4086	Shidappa Wamanna	9	„	—	486	30th „	2920	„ „	
25	Uppar Gali	2043	Subrao Topanna Killekar	9	„	—	1809	20th Jan. 1898	2955	5th „	
26	Khanjar Gali	3410	Krishnaji Ramchandra Gaikwad.	11	„	—	1180	8th „	2968	6th „	
27	Khasbag Baswan.	318	Kalawa Irbhadrappa	10	Lingáyat	—	1197	„ „	3039	11th „	
28	Bapat Gali	1639	Chandrabai Rajaram	20	Hindu	—	438	30th Aug. 1898	3322	29th Dec. 1898.	

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List showing Names of the Inoculated Persons who have Died of Plague during 1897-98.

Serial No.	Street.	House No.	Name in Full.	Age	Caste.	Occupation.	Number and Date of Inoculation.		Number and Date of Attack		Remarks.
1	Kangral Gali	3973	Ningappa Bhannanna	30	Mahratta	Farmer	78	26th Dec. 1897	101	30th Dec. 1897	"Contact."
2	„	3973	Radhabai Krishna	11	„	—	539	30th „	166	7th Jan. 1898	„
3	Pangul Gali	1911	Shattu Shiwappa Undre	7	„	—	859	3rd Jan. 1898	192	9th „	„
4	Bogarwes	1497	Annaji Ganesh	12	„	Student	698	4th „	204	11th „	„
5	Sherkhan Gali	3534	Gawsu Khadersaheb	22	Musalmán	—	1063	5th „	214	12th „	„
6	„	3534	Khader valad Madarsaheb	62	„	Clerk	1175	8th „	217	„ „	„
7	Jalgar Gali	4060	Somaji Wittoba	12	Mahratta	—	1265	„ „	220	13th „	„
8	Tangadi Gali	51	Topaka Bhairu	80	„	—	1064	12th „	251	18th „	„
9	Kakatiwes	3708	Ibraham Kassimsaheb	15	Musalmán	—	20	28th Dec, 1897	287	28th „	
10	Bogarwes	1465	Shamlal Ramlal	6	Rájput	—	450	30th Aug. 1898	871	5th Sept. 1898	"Contact."
11	Math Gali	566	Jinnappa Sangappa	23	Jain	Farmer	553	31st „	924	8th „	„
12	Ganpat Gali	1757	Nagappa Shidraya	8	Lingáyat	—	530	30th Dec. 1897	1065	15th „	
13	Anantsein Gali	921	Ambabai Annabhat	20	Bráhman	—	749	3rd. Sept. 1898	1102	16th „	
14	Gheewada	4158	Gawasu valad Hassansaheb.	5	Musalmán	—	668	1st Jan. 1898	1313	25th „	
15	Khadu Bazár	2959	Krishna Raghunath	20	Jingar	Bookbinder	1550	11th „	1329	„ „	
16	Gondhali Gali	3196	Dada Ananda	45	Narwekar	Trader	507	30th Dec. 1897	1338	26th „	
17	Khasbag Baswan.	332	Basuppa Kallappa	15	Lingáyat	—	1312	8th Jan. 1898	1648	3rd Oct. 1898.	
18	Gondhali Gali	3279	Khachu Umanna	60	Mahratta	Farmer	43	28th Dec. 1897	1636	4th „	
19	Bhandur Gali	77	Ishwar Ningappa Chougla	21	„	„	1751	17th Jan. 1898	1737	5th „	
20	Chandu Gali	4002	Kassim Mahidinsaheb	6	Musalmán	—	727	2nd „	1743	„ „	
21	Ramling Khind	1164	Payappa Deoppa	20	Jain	Farmer	505	28th Aug. 1898	1696	„ „	
22	Baswan Gali	1039	Shesho Janardhan Konur	35	Bráhman	Schoolmaster	991	23rd Sept. 1898	1765	6th „	
23	„ „	1086	Padmanna Chandappa	50	Jain	Balgar	1078	6th Jan. 1898	1792	7th „	
24	Bhoi Gali	1964	Channappa Womappa	55	Bhoi	Fishmonger	1493	10th „	1862	9th „	
25	Ganpat Gali	1798	Chhaya Rama	10	Burud	—	433	30th Aug. 1898	1937	11th „	
26	Khada Bazár	2958	Yamnabai Appuna	7	Lingáyat	—	1447	9th Jan. 1898	1917	„ „	
27	Aditvár Bazár	677	Parwati Balappa	40	Mahratta	Tailor	1013	5th „	2141	15th „	
28	Gheewada	4178	Mahamad Khatal	9	Musalmán	—	430	30th Dec. 1897	2304	20th „	
29	Khasbag Bazár	28	Gangawa kom Basuppa	45	Lingáyat	Weaver	1209	8th Jan. 1898	2648	2nd Nov. 1898.	
30	Kelkarbag	1557	Tulsa Tambdappa	50	Mahratta	—	1073	6th „	2760	11th „	
31	Jelgar Gali	4085	Ningappa Laxman	30	„	—	1567	11th „	2860	21st „	
32	Khasbag Bazár	42	Nagappa Dada	6	„	Tailor	1224	8th „	2955	4th Dec. 1898.	
33	Jalgar Gali	4124	Laxman Bharmanna	45	„	—	1592	11th „	2857	21st „	

23,144. Did you prepare the statement* which shows the social position and name of the inoculated people?—I did not. My head clerk prepared it, not by my orders, but at the direction of Dr. Boyce.

23,145. What is the meaning of this statement which is appended to the précis of your evidence headed, "Statement* showing instances where the whole "families and the majority of members of families "have died of plague within the limits of the Belgaum "Municipality?"—In that statement I show the largest number of persons dying in a family.

23,146. In some instances three out of 14 died; that does not show that the majority of the family died of plague?—That is so.

23,147. Had you anything to do with the preparation of the report* on inoculation which has been separately sent to the Commission; do you know who prepared it?—I had nothing whatever to do with that report; it was drafted by my head clerk.

23,148. When was the census taken that gave the population as 37,800?—During the months of October and November 1897.

23,149. Was it a census of the people resident in the town, or also of the people living in huts outside the town?—Only of persons living in the town.

23,150. How many, do you think, are residents in huts outside the town?—No complete census has been taken of the people living in the huts. Some of them lived in huts outside the Municipal limits.

23,151. Can you estimate the number of townspeople who are at the present moment living in huts outside the town?—Nearly 10,000.

23,152. (*Mr. Cumine.*) I do not understand how you reconcile these inoculation figures in paragraph 14 of your précis (which is as follows:—"14. The work of "inoculation in the native town of Belgaum com-"menced on the 26th of December 1897, and in "1 month and 19 days 2,094 persons were inoculated, "and from 23rd July 1898 to 17th November 1898 "the number of persons inoculated was 1,344; thus "the number of persons inoculated was 3,438,") with those in the table which you have put in?—I cannot give any explanation about the inoculation figures.

23,153. Which may I take as correct?—The figures in the table are correct.

23,154. (*The President.*) What is the intention of the table in your précis from page 10 to page 29, headed "Statement* showing instances where the whole "families and majority of members of families have "died of the plague?"—My intention was to show how many cases occurred in each house, and how many of those cases occurred in one day, and how many in each house which was disinfected.

23,155. What conclusions do you draw from this table?—I think there is no benefit whatever from disinfection.

23,156. In some houses there were many more deaths than in other houses; can you explain the difference?—I cannot explain that.

* Not printed with the Proceedings of the Commission.

(Witness withdrew.)

(Adjourned till to-morrow.)

At The Collector's Office, Belgaum.

FIFTY-NINTH DAY.

Thursday, 2nd March 1899.

PRESENT:

PROF. T. R. FRASER, M.D., LL.D., F.R.S. (*President*).

Mr. J. P. HEWETT. | Mr. A. CUMINE.

Mr. C. J. HALLIFAX (*Secretary*).

Major A. V. ANDERSON, I.M.S., called and examined.

Major A. V. Anderson, I.M.S.

2 March 1899.

23,157. (*The President.*) You are in the Indian Medical Service?—Yes.

23,158. What are your medical qualifications?—M.B.C.M., Aberdeen.

23,159. You have had a good deal of experience of the plague in the Dharwar district?—In Dharwar district for the last three months.

23,160. In which taluka?—In the Navalgund taluka, and the Nargund-petta.

23,161. How many villages are contained in that area?—53 belonging to the British Government.

23,162. And those which are not British?—They belong to the Native State. I have no charge of Native State villages.

23,163. You carried out evacuation more or less completely?—Yes, the following is a general statement of the plague work carried out:—

I began plague operations in Dharwar district on 5th December 1898, and was placed in charge of Navalgund taluka, west of the Benni Halla and Nargund Pettas, an area of about 30 miles by 10, which comprised 53 villages with a total population of between 50,000 and 60,000. To work this district in which were, from first to last, 23 plague infected villages, I had a subordinate staff of one plague karkun, two Circle Inspectors, two vaccinators, and one office clerk. In order to ascertain how many villages were actually infected, I placed one Circle Inspector in charge of the uninfected villages of Navalgund, and the two vaccinators in charge of the uninfected villages of Nargund. The number of villages in charge of each being from 8 to 10, with orders to fill in a special report form with regard to each village. These men visited one, two, and sometimes three villages each day, and as soon as they had done all in their charge they began over again, so that I had frequent and definite information regarding the uninfected villages. The plague karkun I placed in charge of the infected villages of the Navalgund and the other Circle Inspector of those in Nargund. In addition I had four policemen on plague duty, two in Nargund and two with my camp, but I had no occasion to use them for plague work. The people at each infected village were ordered to vacate the village and go into huts within three days. A general order for evacuation had already been issued, but as will be seen from the detailed report of each village, some took a month to evacuate, and even after definite orders were issued, one village took 12 days. Later, however, evacuation was always effected with in three days. A month after complete evacuation the people were allowed to return to the village to open up the roofs of the houses.

In this district, fortunately, the houses with few exceptions are single storeyed, the roofs consisting of bamboos laid on beams and covered with earth to a depth of one foot. The roof was opened by removing the earth and lifting up the bamboos, the whole length of each room for a width of two feet. After all the roofs in the village had been thus opened, the doors were opened and the houses swept out, the doors lifted off their hinges and left open, thus securing thorough ventilation.

The number of cases for the week ending 10th December was 191; for the week ending 17th December, 145; for the week ending 24th December, 172; for the week ending 30th December, 135; for the week ending 6th January, 117; for the week ending 13th January, 119; for the week ending 20th January, 60; for the week ending 27th January, 59; for the week ending 3rd February, 33; for the week ending 10th February, 31; for the week ending 17th February, 28; for the week ending 24th February (report not made up when I left the district on 28th February 1899).

The total number of villages was 26, with a population numbering 34,669.

The first village reoccupied was Talimorab, population 633, which reported plague on 27th November 1898; was totally vacated on 12th December; last case occurred 20th January; began to be opened up 9th January; readmitted 2nd February.

The next village reoccupied was Javur, population 858. Plague first reported 26th November; totally vacated on 7th December; last case reported on 2nd January; began to be opened up 14th January; reoccupied on 14th February 1899.

Hanasi was next reoccupied, population 1,097. Plague first reported 27th October; vacated 29th November; last case occurred 17th December; began to be opened up 17th December; reoccupied 16th February 1899.

These plague measures have so far entailed no public expenditure, all work being done by the villagers themselves.

23,164. Can you divide the villages into any groups?—In my précis of evidence I divide them into three groups.

23,165. What groups?—These villages might be divided into three classes,—

1. Those infected, while evacuation was not in practice, and concealment of the outbreak was kept up as long as possible.
2. Those infected at the beginning of December, when evacuation was carried out against the wish of the people and when concealment was still practised.
3. Those infected when evacuation was in full operation, and was not opposed by the villagers, and when systematic means were adopted for ascertaining the outbreak of plague on its first appearance in each village.

In the 1st class, I would include Morab, Hanasi, Javur, Yamanur.

In the 2nd class, Shirur, Talimorab, Amargol, and Nargund.

In the 3rd class, the remaining thirteen villages, viz., Padesur, Arekurhatti, Kálvád, Tirlápur, Shirkol, Gumgol, Karlvad, Hal, Rasugal, Jagapur, Kalkeri, Kurlgeri, and Banhatti.

Major A. V. Anderson, I.M.S.

2 March 1899.

23,166. You give examples of the results in the villages which you have grouped in each of these classes, do you not?—Yes.

23,167. Will you classify these results in tabular form? — The following table gives the required information:—

No.	Name of Village.	Population.	Date of First Case.	Date of Commencement of Evacuation.	Date of Completion of Evacuation.	Date of last Case.	Number of Cases between Dates entered in 4 and 6.	Number of Cases between Dates entered in 6 & 7 for each Period of 10 Days after Evacuation was completed.		Remarks.
1.	2.	3.	4.	5.	6	7.	8.	9.		10.
1	Hanasi	1,097	27.10.98	22.11.98	29.11.98	18.12.98	224	A. 15	B. 1	A.—Cases occurring within 10 days after evacuation. B.—Number of cases occurring after 10 days in camp.
2	Yamanur	984	27.10.98	13.11.98	20.12.98	12.1.99	159	A. 8	B. 11	
3	Morab	4,146	30.10.98	27.11.08	18.12.98	1.2.99	607	A. 22	B. 23	
4	Javur	858	6.11.98	27.11.98	7.12.98	3.1.99	83	A. 17	B. 10	
5	Nargund	9,597	5.11.98	25.11.98	25.12.98	23.2.99	157	A. 53	B. 71	
6	Talimorab	633	27.11.98	10.12.98	12.12.98	20.1.99	66	A. 22	B. 10	From 20th September to 12th December 108 deaths occurred in this village. Plague not reported.
7	Shirur	1,307	12.12.98	27.11.98	12.12.98	4.2.99	0	A. 25	B. 38	
8	Amargol	1,891	19.12.98	20.12.98	25.12.98	4.2.99	60	A. 45	B. 54	
9	Padesur	1,161	26.11.98	1.12.98	14.12.98	16.2.99	9	A. 3	B. 21	Rats died, 1.12.98.
10	Kalkeri	311	13.12.98	20.12.98	23.12.98	23.2.99	3	A. 1	B. 10	
11	Kurlgeri	750	17.12.98	5.12.98	20.12.98	20.2.99	3	A. 5	B. 38	
12	Shirkol	1,640	21.12.98	26.12.98	29.12.98	3.2.99	29	A. 20	B. 19	
13	Arekurhatti	1,343	30.12.98	30.12.98	31.12.98	21.2.99	4	A. 7	B. 14	
14	Halkasugal	1,202	31.12 98	7.1.99	11.1.99	20.2.99	1	A. 0	B. 2	
15	Jegapur	633	31.12.98	4.1.99	8.1.99	13.2.99	11	A. 10	B. 8	
16	Hunsikatti	716	31.12.98	22.1.99	24.1.99	17.2.99	0	A. 0	B. 3	31.12.98, date of imported case.
17	Karlwad	726	4.1.99	8.1.99	10.1.99	20.2.99	0	A. 0	B. 17	Rats died, 4.1.99.
18	Kalwad	1,335	4.1.99	6.1.99	9.1.99	13.2.99	2	A. 6	B. 6	
19	Gumgol	977	6.1.99	6.1.99	8.1.99	5.2.99	2	A. 3	B. 5	
20	Tirlapur	2,162	6.1.99	6.1.99	10.1.99	7.2.99	4	A. 8	B. 14	
21	Banhatti	951	17.1.98	30.12.98	7.12.98	17.2.99	0	A. 1	B. 37	Cases had occurred but were not reported before 17.1.28.
22	Madgunki	262	15.2.99	16.2.99	17.2.99	15.2.99	1	A. 0	B. 0	Imported cases occurred 15.2.99.
23	Mugaur	380	17.2.99	9.2.99	10.2.99	23.2.99	1	A. 2	B. 0	

Talimorab re-occupied 2.2.99. Javur re-occupied 14.2.99. Hanasi re-occupied 16.2.99.

23,168. Will you kindly state what opinion you have formed as to the results of evacuation?—I think evacuation has been very successful in the Dharwar district in the part of which I have been in charge.

23,169. What has been the general result of your experience?—The benefit of prompt evacuation of the infected site consists not only in lessening the severity of the outbreak in each individual village, but in lessening the tendency to spread throughout the other villages of the district, no new villages having been infected since the 6th January; while before active measures were undertaken the disease was spreading rapidly. The number of cases would undoubtedly have been fewer had it been possible to entirely prevent people from visiting the village site and surreptitiously breaking the seals and entering their houses.

23,170. The operation of evacuation had not, in all cases, a fair chance, had it?—No, it had not.

23,171. What was the nature of the difficulties which you had to encounter which probably made the results less beneficial than they otherwise might have been?—The people were very prone to return to the village to remove articles of furniture to their huts in the field, and as the staff at my disposal was limited, I could only check them by examining the seals on the houses.

23,172. Had you a sufficient staff to carry on all the operations of cleansing and disinfection and evacuation?—The cleansing had to be done by the people themselves entirely, I had no actual cleansing staff.

23,173. That also operated injuriously?—Yes.

23,174. In what way?—Because the people had to be allowed into the village to open the roofs of their houses, and while doing so they entered their houses, and were exposed to the danger of the infected site. If they had not done this work within the houses, then this cause of infection among a certain number would not have existed.

23,175. In carrying out evacuation in the most beneficial manner what conditions do you consider to be requisite?—My system in the Dharwar district has been to remove the roofs. I may as well explain that the houses there are one storey houses with bamboo roofs laid on beams. Over the bamboo they have a covering of earth to a depth of about 1 foot. I caused the earth to be removed from the roof the whole length of each room, and for a width of about 2 feet. I then lifted up the bamboos, which exposed an opening of about 2 feet by the whole length of the room, to light and air. When this had been done in every room in a village, I then opened the doors, took them off their hinges and removed them and left them open for a few days. I then allowed the people to return.

23,176. In obtaining the best results, I presume it is very necessary to get early information?—Yes.

23,177. In your treatment of plague cases, you, I suppose, draw some distinction between the requirements where there are only imported cases, as contrasted with the requirements where there are actually indigenous cases?—Yes.

23,178. Would you state what you think are the differences which are required?—In dealing with an imported case of plague, if we remove it before it has infected the locality to an uninhabited and sanitary area, and disinfect all articles likely to hold the infection, we destroy the originating cause of the plague outbreak. On the other hand the removal of an indigenous case is merely removing the sign, or symptom, of the already existing disease in the infected locality, and disinfection of the house, and its surroundings, even if done effectively and at once, does not necessarily destroy the existing disease, as, during the incubation

period of the first case, the disease has already spread we know not how far or in what direction.

23,179. And, therefore, in this latter instance, you recommend, as far as you can, complete evacuation?—Complete evacuation of the infected locality.

23,180. What was the general sanitary condition of the houses that you are now referring to?—Very bad. The people keep their cattle in the dwelling-house in all these villages in the Dharwar district.

23,181. What defects in connection with the sanitary conditions do you think operated most deleteriously in regard to plague? Was there any special defect?—I should think that this habit of keeping cattle in the house is the greatest sanitary difficulty in this district.

23,182. How does that operate with regard to plague?—Because there is always a large amount of organic matter on the floors of the houses.

23,183. How do you think that operates? Why should the large quantity of organic matter have a bad effect?—The filthy condition of the houses favours the spread of the disease.

23,184. Are the means of lighting sufficient or defective?—Very defective.

23,185. Is the ventilation and the fresh air supply to the inhabitants of the houses also very bad?—Yes, the ventilation is also very bad.

23,186. The presence of the cattle in the houses is then deleterious, in that it reduces the pure air, especially if the ventilation is very bad?—Yes.

23,187. What opinions have you formed as to the value of disinfection?—I think the disinfection of personal clothing is useful and it should always be carried out if possible; but the actual disinfection of houses, in my experience, I have found to be impracticable.

23,188. You could not carry it out?—No.

23,189. Why?—Because it would require a very large staff to work over such an extensive area as we had in the Dharwar district, and disinfection requires very skilled and constant supervision. Each individual gang of coolies must have a skilled and thoroughly reliable supervisor, who perfectly understands what we are trying to do by disinfection; and that is extremely difficult to get in this country.

23,190. Have you any cases that came under your observation in which disinfection was satisfactorily carried out?—No, not under my own observation. My opinion as to when disinfection of a house proves successful or not is when the people are taken out, and the house disinfected, and the people re-admitted at once.

23,191. You have had experience of that?—That is my idea of what disinfection ought to do. As soon as the disinfection is finished, the people should be re-admitted into the house immediately, and not kept out for 10 days or a month, because if disinfection destroys the germ, it destroys it at once.

23,192. You think that disinfection does not operate in that way at present?—No, the corrosive sublimate should destroy the germ if it is there.

23,193. You mean if disinfection is successful, that should be the result?—The people should be re-admitted and there should be no further case.

23,194. Have you any facts which show that that did occur?—Our experience in Igatpuri was against that.

23,195. I understand you mean if disinfection is a satisfactory measure it ought to operate in such a way that the people can return to their houses as soon as it is finished. It may be a few hours afterwards?—Yes.

23,196. I understand you have cases in which that did not occur?—Yes. Plague broke out in Igatpuri of the Nasik district in August 1897. At that time the theory of disinfecting the house, its inhabitants and their belongings, and allowing them to re-occupy the house at once was in favour, and as the rainfall at Igatpuri is extremely heavy it was impossible to vacate the village, population 4,000. A disinfecting party under an Assistant Surgeon, who had been working at disinfection work in Bombay, was obtained at once, and thorough disinfection with corrosive sublimate solution was carried on. After some time, as no good results appeared, I tested the solution used by the working party, and found that by the mistake of a decimal point the Assistant Surgeon in charge had been making up his solution 10 times too weak. I mention this to show that even if you have a good man in charge, such mistakes occur in working with a disinfectant. From that time the regulation 1 in 1,000 was used, but I cannot say with any better result. Igatpuri is a railway town, and the railway authorities having provided a large number of empty waggons all railway employés were moved into these with most satisfactory results. I may instance the case of two chawls, one inhabited by railway police and one by railway labourers; each of these had been disinfected four or five times and still cases occurred, when the inhabitants were moved into waggons, and no other case occurred amongst them.

23,197. With regard to the towns or villages which you evacuated what has been the population?—In the Dharwar district it varied from over 9,000 in Nargund to a small village of 300.

23,198. What was the largest town in your experience?—Ahmednagar or Sholapur, which are both of the same size, with a population of 60,000.

23,199. Which was the largest town in which you found complete evacuation could be effected?—I should think up to about 60,000 or 70,000.

23,200. Have you experience of other large towns?—I have no experience of anything over. I have been to Ahmednagar and Sholapur.

23,201. Was complete evacuation carried out in these two towns promptly or gradually?—Gradually, particularly in Sholapur. The disease obtained a strong hold of Sholapur before evacuation was carried out. In Ahmednagar on the other hand, the town was more quickly evacuated.

23,202. Does gradual evacuation act just as well as prompt evacuation?—No. My idea is that the people are still exposed to contagion—that the contagion spreads through an evacuated area, but it takes time to spread, and where we have slow evacuation the results are not as definitely beneficial as where we have prompt evacuation.

23,203. In this large place which you speak of, would it have been possible to have effected prompt evacuation?—No, I think not.

23,204. In a place of such a size that rapid evacuation is impossible, is evacuation by instalments, in your opinion, the best measure you can adopt?—In my opinion it is the best measure to adopt.

23,205. Even although it is not altogether satisfactory?—Yes.

23,206. What is the result of your consideration, founded upon experience, of the means by which plague is conveyed from one place to another?—By human intercourse mainly—by clothing. I find that in towns or villages at a distances of several miles from each other, the disease was invariably introduced by intercourse, either by persons suffering from plague or persons themselves in good health coming from an infected locality. My opinion with regard to this is that they carry the infection in their kit or clothing. When an uninfected person, I mean a man who is not suffering from disease, infects a locality, it is by his clothing and not by his person.

23,207. Have you any clear instance of that kind of infection?—In many instances which I have investigated, we find a distinct history of the introduction of the disease by human intercourse. In most places, as in Kurla, Bassein, Chembur, Trombay, Malad, Chinchni, Thana, Bhiwandi, Bhayndar, &c., we have a history of persons coming sick from an infected locality, or falling sick a day or two after their arrival prior to the occurrence of local cases. In other instances we find the disease introduced by persons themselves in good health coming from an infected locality. In Agashi the first local case occurred in the person of a Shimpi woman, to whose house some friends from Bombay had come to live. None of these Bombay people were sick or were afterwards attacked. In Kelwa also the first local cases occurred in the person of two Shimpis to whose house some Shimpis from Bombay had come five days before. These Bombay people were in good health, and after staying three days returned to Bombay. In both the above instances rats died in the houses after the arrival of the people from Bombay, and before the persons living in the house were attacked. It would appear that rats are extremely susceptible to the disease, have a short incubation period, and propagate the disease

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locally. It is improbable that they intensify the poison, as in Mahim rats died in at least three different houses and no local cases occurred in the town. At Versova a striking instance of the disease being carried by an apparently healthy person occurred. The first imported case at Versova occurred on 30th January in the person of a Brahman who came sick from Bombay. The Brahman schoolmaster of Versova visited this man while he was sick, and attended his funeral on 31st January. The schoolmaster lived in the village Talati's house. On the 2nd February the Talati's nephew, who lived in this house, was attacked by plague, while the schoolmaster himself was not attacked till the 5th of February. In Marol also the first case occurred in a house to which a number of people had come from Bombay to attend a wedding. These people were in good health, and after staying some days they returned to Bombay in good health. Here also rats died in the house after the arrival of the Bombay people and before the first human occupant was attacked. Again in the village of Madhan, an isolated case occurred prior to the outbreak there, in the person of a man to whose house a man from Bombay had come to live. Four or five days after the man's arrival rats died in the house, and the people moved out into sheds. A day or two afterwards the local occupant of the house was attacked and died, while the Bombay man escaped. In Jegapur, of the Dharwar district, a man went to visit his brother's wife, who was suffering from plague at Shelawdi, nine miles distant. Two or three days after his return rats died in his house, and three days after his daughter was attacked by plague and died. Rats also died in the adjoining houses shortly after the girl fell sick, and 29 cases occurred in the village which was promptly vacated.

23,208. In addition to the conveyance by articles belonging to human beings, what other means are there?—Within inhabited areas, and in towns or villages in close proximity to one another, the disease was often spread by rats. This was well seen in the Bandra group of villages—Bandra, Pali, Chuim, Danda, Santa Cruz, and Joo, and also in the infection of old Kurla from new Kurla—where in every case the death of rats preceded attacks among the people.

23,209. You refer to a group of villages where you yourself made inquiries?—Yes, it is my own personal investigation on the spot.

23,210. How long before a case occurred had dead rats been found?—In Bandra, which is practically a suburb of Bombay, the first local case occurred on 3rd December in a house in which dead rats had been found about one week before, and the disease at once spread in this locality. On the same date another local case occurred in another part of the town in a house where no rats had died. The disease did not spread in this locality, and it is possible that the infection had been caught elsewhere. In Pali dead rats were seen on the 6th or 7th of January, and the first case occurred on 13th January. In Chuim rats died in the beginning of February, and the first case occurred on 9th of February, although it was not reported till the 12th February. In Danda in the same way dead rats were found in several houses prior to the outbreak of the disease on 11th February. The above four villages, separated from each other by a distance of about half a mile, are included in the Bandra Municipality. In Santa Cruz, two miles distant from Bandra, dead rats were found on 12th February, and on 15th February the two first cases occurred in the house in which the dead rats were found. In Joo, which is separated from Danda by a creek, dead rats were found in a house about the 10th of April. The people thereupon removed to a shed for 10 days, and then returned to the house, which had not been disinfected, and three days after their return the first occurred in this house. It would appear that the Bandra rats spread the disease in the adjacent villages, as they entirely disappeared from the town, and were only, in the beginning of June, re-appearing. Dead rats were in the same way discovered at an early stage of the local outbreak in the villages of Vela Parla, Andheri, Goregaon, and Padali, which form an almost continuous line of houses along the Bandra-Ghodbandar Road, the intervals between the various villages being at that time filled up by the sheds of Bombay refugees. The disease appears to have been carried along this line of road as it was carried in Bombay itself. In old Kurla village, which is only separated from new Kurla town by less than half a mile of open fields, no case had occurred, though the disease was very prevalent in New Kurla during January, February, and March. On the 4th of April rats died in the house nearest New Kurla, and although that and some of the adjoining houses were vacated and disinfected, the first cases occurred in the village on 8th April. Here the presumption is that rats came from New Kurla and carried the disease with them.

23,211. How far do you think rats would have to travel from an infected place before a new place became infected?—Under a mile.

23,212. How do you know there was no human intercourse between these two places which infected each other by persons or articles, other than rats, capable of conveying the plague?—The coincidence is rather remarkable in the case of these particular villages. With regard to others at a greater distance from one another, we found we could trace the personal intercourse, but in these villages we were unable to do so.

23,213. It was more likely to occur if they were closer to each other than if they were distant?—Certainly that is an objection. That is possible.

23,214. At any rate, you have a distinct opinion that rats may infect a place?—Yes, I had an instance in Dharwar.

24,215. Will you give us that?—In Hunsikatti, a village in the Nargund Petta of the Dharwar district, two dead rats were found in a house on the 20th of January, and on the 22nd of January one sick rat was seen by me. This village is within one mile of a village called Jegapur, where cases of plague existed at the time.

23,216. Rats became affected, but how about human beings?—I was unable to trace any connexion between the two villages.

23,217. You mean the rats had become infected from this village?—Yes, rats had died in Jegapur.

23,218. I do not think that anyone doubts that rats become infected with plague. There is no question about that. That is all your case shows. In regard to the etiology of the plague, referring to human beings, how do you say that rats bear on that in the communication to human beings? I do not wish you to show whether they themselves become infected or not, but how they become factors in the communication of plague to human beings. Were the infected rats the productive cause of the plague in this new village?—Rats became factors in the communication of plague to human beings by their excreta.

23,219. Have you any instances in which an outbreak of plague appeared to be clearly produced by a previous infection of rats?—In this instance I can only say that the rats died on these dates in the village, and no case of plague had occurred. No person was then affected.

23,220. After the death of rats were they affected?—Then I moved the people out of the village, and after they went out, three cases occurred in the huts.

23,221. How long after?—The people began evacuating the village on the 22nd of January and were all out on the 24th January. On the 13th of February one case of plague occurred among the people in the fields, and on the 17th February two more cases occurred, one being a member of the family first affected. On inspecting the houses of these two people in the village, I found that the seals had been tampered with, and the doors opened.

23,222. How does that prove infection of human beings by rats? That is the point?—From that I concluded that the rats infected the village site, but that the people had gone out before any of them were infected. When these two people came back and entered their houses in the infected site they got the disease, which shows that the plague was there although not amongst the people.

23,223. Assuming they had friends in a village where there was plague, and that there had been intercommunication—personal communication—between them, that itself, without rats, would have been quite sufficient, and that might have occurred, might it not?—I formed my opinion because of the rats dying originally.

23,224. It might have had nothing nothing to do with rats, because another plague patient in another village might easily have been visited, for example, by some of these people, and direct communication without rats might have occurred?—Yes, that is so.

23,225. I want cases which exclude as far as possible obvious fallacies, and show distinctly that rats were the communicating media?—I have no further evidence than that.

23,226. Did you notice any particular distribution of the affected villages in your district in regard to plague infection?—They occurred in circles as it were, round, or in the neighbourhood of, some larger villages in the first instance; they occurred in groups.

23,227. Had this occurrence in any part of your district any association with rivers?—It seemed to follow a course along the bank of the river.

23,228. The majority of the affected villages?—Yes, but it did not cross over from one side of the river to the other very readily. The river seemed to protect the villages on the opposite bank, but the disease seemed to spread along the bank of the river on the side that was infected.

23,229. Did you find many villages distant from the river affected, or were the great majority on the banks of the river?—I would not say the great majority, but about two-thirds.

23,230. Have you formed any idea as to why that should be?—No, I cannot say that I have.

23,231. Can you recollect any conditions in the manner of living on the river bank, as contrasted with the manner of living in the villages remoter from the river, or in the habitations, or in the water supply? The water supply is the same? — The people do not use the river water for drinking purposes.

23,232. What do they use it for?—For washing their clothes and their bodies. The higher class natives go to the river and bathe before taking their food.

23,233. It is not a sacred river?—No.

23,234. In washing do not the natives take the water in their mouths during the process of cleaning their teeth, and sometimes unintentionally swallow a portion of it?—Certainly they do.

23,235. Therefore they would be drinking it to that extent?—Yes.

23,236. You cannot remember any other distinguishing feature?—No, the banks are high, and there is not much difference in the sub-soil water.

23,237. Was the character of the houses worse or better?—I did not see any difference in the state of the houses.

23,238. Not with regard to overcrowding or lighting? —There was no distinction, but, as a rule, along the bank of the river villages are closer together.

23,239. That is to say, if one house happens to become infected, the possibilities of infection of others are greater?—Yes.

23,240. That would go a good way in explaining it? —Yes, certainly.

23,241. I think you have many cases in which partial evacuation has, in the first place, been carried out, and found to be unsuccessful, and then, afterwards, complete evacuation was put into effect?—Yes, I make the following remarks regarding evacuation:— Plague having appeared in Nasik Town in October, I advised the evacuation of the town, population 25,000; 10,000 people were moved out in two or three days, and the rest of the town gradually evacuated as cases occurred. The town escaped with a mortality of between 400 or 500, though plague undoubtedly existed some time before it was discovered, and evacuation was only carried out as areas became affected. In December 1897, while working at Malegaon in the Nasik district, I tried the evacuation of large areas as cases occurred in them, keeping the people out in a segregation camp for 10 days while their houses were disinfected and limewashed, and then allowing them to return while their places in the segregation camp were taken by persons from another newly infected area. It was hoped that even should the disease spread through the whole town—population 20,000—that it would not be necessary to keep more than 2,000 or 3,000 out at a time. This failed, because the people of the supposed unaffected portion began to move their sick about and conceal cases, and the whole town had to be evacuated, after which the disease quickly ceased. During January 1898, I was employed on plague work in the Native State of Palanpur where up to then partial evacuation, as cases occurred, was being carried on. I advised certain measures, namely, the taking of an immediate census of the people, and the evacuation of the whole village on the occurrence of the first indigenous case. Although, owing to concealment of cases, we seldom heard of the first actual case, still the villages were in most cases totally evacuated before the disease obtained any stronghold, and in a few months the Palanpur Political Agency was entirely free from the disease. I afterwards visited Cutch and Kathiawar to advise on plague measures. In Kathiawar, prior to my visit, there was no plague; but while I was there a large village named Kutiana became affected, a nominal roll of the people was at once taken, and they were moved out into camp and mustered daily. Several other towns and villages of Kathiawar have since been affected, but the disease has been kept in check, and I understand that Kathiawar has got rid of the disease altogether.

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23,242. In these examples, what result followed the after adoption of complete evacuation?—It had the result of getting rid of the disease.

23,243. You have also some instances in which partial evacuation alone was successful?—In 1897, indigenous cases occurred in Wadhwan, a large town of Kathiawar, partial evacuation of the affected locality was carried out and the disease stopped. This result has also been seen in Ahmedabad and in Kutiana in 1897. At that time there was no indigenous plague in the districts concerned. It appears that an isolated outbreak at a great distance from any badly infected district is more easily dealt with than an outbreak in a town or village of an already affected district, although plague measures may be as quickly undertaken in one case as in the other. It would appear that the disease requires time to become acclimatised, as it were, in any new district, and the measures which succeed in such a case will not be equally successful should the disease again appear in that town or village when the adjoining districts have become affected. In the same way the further we go, even within the same district from a bad centre of infection, the more amenable to measures of suppression does the disease become. Might I add that in the Dharwar district, I came across an instance of that in the case of a village called Alur, which was affected in July 1898, at which time plague was only beginning to get a firm hold of the Dharwar district. Twelve cases occurred in this village. The people of the affected locality were moved out, and no more cases occurred. This village has become affected again in February this year, and four cases only have occurred, but in different parts all over the village; complete evacuation has been carried out.

23,244. What has been the result?—We have not had a case for ten days. It occurred in February, but we cannot yet tell what the result will be; they have been ten days free. This village is in Mr. Lushington's charge who can probably give more definite information.

23,245. You have said more than once that you think the disease is a very local one?—Intensely local.

23,246. What is the definite locality to which, as it were, the bacillus seems to confine itself? — The inhabited area.

23,247. What part of it?—The dwelling-house.

23,248. What part of the house?—I should say the floor almost entirely.

23,249. Is there any particular characteristic in the floors of these houses from which this opinion of yours has been drawn?—They are all cow-dunged.

23,250. You have, therefore, no experience of stone floors, have you?—Not in the Dharwar district.

23,251. In any other district?—I have no experience, practically, of stone floors.

23,252. You speak only from your knowledge of mud and cow-dung floors?—Yes.

23,253. I understand you to say that if a case is imported into any district or town, however populous, if you get early information, and can absolutely segregate that patient and all contacts, the disease ought not to spread?—The disease ought not to spread.

23,254. But on the other hand, if it has already spread even to a small extent, if that extent includes distant parts of the town, such treatment is probably ineffective?—Yes, ineffective.

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23,255. (*Mr. Hewett.*) How many villages are infected in the Dharwar district altogether now?—I cannot tell that.

23,256. Have you formed any opinion as to how the villages in the Dharwar district became infected?—From the towns of Dharwar and Hubli.

23,257. Do you mean that the result of the inhabitants of the towns of Dharwar and Hubli being infected, has been that the infection has been scattered all over the district?—Certainly.

23,258. A number of the people from each of those towns left the town carrying infection with them?—Yes.

23,259. And the district of Dharwar has been extremely badly infected, has it not?—Yes, very badly infected.

23,260. Have you known any other instance in which a district has been so badly infected?—There is no other instance that I know of.

23,261. You have under your charge, at present, 70 villages, have you not?—53 is the correct number, 70 includes the Native State villages.

23,262. Have you any other European officer working in these villages under you?—I had a European officer assisting me last month to whom I handed over charge.

23,263. Practically, there is one officer dealing with all this tract?—Yes.

23,264. What is the area of it?—Thirty miles by ten.

23,265. And some of these villages, I suppose, are a great distance from each other?—Yes.

23,266. Do you think that one officer can manage so many villages satisfactorily?—No, not satisfactorily.

23,267. How many badly infected villages do you think that one European officer can satisfactorily manage, provided that he is personally responsible for seeing that the people remain away from the village sites?—It depends on the distance these villages are from one another.

23,268. Exactly, but how many villages in the immediate neighbourhood of one another, can one officer properly supervise in that respect?—Not more than two large villages of over 2,000 population.

23,269. Would you be disposed to place reliance on the efficacy of Native agency in preventing villagers from coming back to an infected site?—To a limited extent.

23,270. Would you require them to be supervised by British officers?—Certainly, always.

23,271. You say in your précis of evidence that no villages had become infected after re-occupation before January the 6th. Have any of them become infected since that date?—Only one.

23,272. Do you consider that the evacuation of villages during the rains, in the Dharwar district, was accompanied by great hardship to the people?—Certainly, there is hardship during the rains in a black cotton soil.

23,273. Do you think that the evacuation and the exposure to which people are subjected during the rains, render them more liable to sickness generally?—During the rains it does.

23,274. And that if you evacuate the people during the rains, in order to escape plague, you make them more liable to other diseases?—Yes.

23,275. Do you think that the people when influenced by the exposure to which they are subjected when out of the village during the rains, are more liable to take the plague?—I do not think so.

23,276. Do you think that when people are turned out of their villages during the rains, and subjected to this exposure, that there is a strong temptation for them to go back at night and sleep in the infected site?—Certainly.

23,277. And that consequently your measures may be to a great extent defeated owing to their doing this?—They require closer supervision.

23,278. If then you had a district infected as Dharwar was during the past rains, you would require an enormous staff of European officers working in the district?—Certainly, at the end of the rains.

23,279. Do you not think that in the Dharwar district there were many cases in which villages were infected during the rains, and the infection was not discovered until a long time afterwards?—Certainly, I am quite sure of that.

23,280. So that probably you would have required during the rains a very large staff?—Yes.

23,281. Then you speak of the fact that cattle are kept in the same house as the people, do you partially mean that the cow-dung falls upon the floor and thus affords the bacillus a soil which is favourable for its growth?—I think so.

23,282. I understand you to believe in the theory of the efficacy of disinfection, if it is properly carried out?—Yes, if it is properly carried out.

23,283. And that you would be quite satisfied with a disinfectant which effectively disinfected the floor?—No, chiefly.

23,284. I understood you to think that the infection exists almost entirely in the floor?—Almost entirely in the floor.

23,285. Where else do you think the infection exists?—It may exist in the walls; it may exist in any bundle of rags which may be overlooked, not actually in the room, but in some covered place within the wall, such as a hole in the wall which cannot be reached by any disinfectant we use.

23,286. Is it your opinion that the walls as such, and the roof as such, are likely often to contain infection?—They are not likely to do so.

23,287. Even if disinfection of the villages is carried out, and it is not always thoroughly done, it does a certain amount of good, does it not?—Yes, a certain amount of good.

23,288. Do you think that one of the causes of the infection of villages situated on a river bank may be that outsiders come more to those villages?—No, I do not think they do, because they do not use the water for drinking purposes.

23,289. Have you noticed in your experience that an outbreak of plague results in the general mortality (apart from plague) falling below the normal?—I think that is due to a defect of registration. When village officers once report plague they are very apt to put down every death that occurs as plague. If death occurs within a week of a man falling sick it is put down as a plague case. All cases of fever which terminate fatally are put down as plague cases after plague has been declared.

23,290. Then you find that if plague has been declared the village authorities who are responsible for notification notify all the cases of plague, and some notify as cases of plague sickness which is not plague at all?—In most instances; of course there are certain exceptions

23,291. Where there is concealment?—Yes.

23,292. Under ordinary circumstances would it not be rather difficult for the village officer to be certain that the original case which occurred in his village was a case of plague?—Yes.

23,293. With the best of intentions he might be deceived?—Yes.

23,294. Village Headmen are fairly reliable, as a rule, in reporting cases of cholera, are they not?—They are.

23,295. Do you attribute that to the fact that they have no difficulty in recognising the disease, owing to their familiarity with it?—I do.

23,296. Do you think that they can be expected to do the same thing as regards plague at present?—No, I do not think so.

23,297. Do you think that there is any organisation which you could substitute for them over the country generally which would help you to get immediate notification of cases of plague in rural areas?—Yes, certainly.

23,298. How would you do that?—By preparing a special form of report to be filled in by inspecting officers, who will fill in the facts, and not their own opinions as to what any particular disease may be, that is the mere fact that there are so many sick persons in

the village, and so on. The following is the form of Report.

Inspection Report Form of Circle Inspector.

Population.	Mortality during the last six months.	Cause of death, during the last four weeks.	Age and caste of death during the last four weeks.	Were any of these related to each other.	Did any two or more die in the same house.	Houses in which deaths occurred inspected.	Grave-yards and burning grounds inspected.
1.	2.	3.	4.	5.	6.	7.	8.

23,299. What class of men would you put on as inspecting officers?—Men of the class of Circle Inspectors, or vaccinators.

23,300. Who are the Circle Inspectors?—I understand that Circle Inspectors are subordinates of the Land Records and Agricultural Department.

23,301. And the vaccinators are subordinates of the Sanitary Department?—Yes.

23,302. You are a Deputy Sanitary Commissioner?—I am.

23,303. Do you think that the efforts to get early and proper notification of disease might be improved if you could import into the Sanitary Department some Assistant Surgeons to supervise the work of vaccinating?—Certainly.

23,304. Do you think that it would be a good thing to have a certain number of Hospital Assistants employed as vaccinators?—Certainly.

23,305. Do you think that these measures would help you to get early notification?—They would.

23,306. Do you know anything about the byelaws in Municipalities requiring a householder to report the occurrence of death? Is it the case generally that in Municipalities in the Bombay Presidency there is such a byelaw?—They are supposed to collect and keep a return of all deaths occurring within a Municipality. This is generally done by a special clerk, who usually collects his information from the village Mahars.

23,307. Is there no obligation placed by the law, or by a byelaw, upon the householder to report a case of death himself?—In most Municipalities there is such an obligation.

23,308. Is that byelaw usually carried out?—I do not think so.

23,309. What is the reason that it is not carried out?—It is so seldom enforced.

23,310. By the Municipality?—Yes.

23,311. It is your opinion that it is allowed to fall into practical disuse?—That is my opinion.

23,312. What was the form of seal with which you used to seal up a house?—A slip of paper with the Municipal seal stamped on it, and dated. Nargund being a Municipal town, we had a Municipal seal. Elsewhere we have simply a piece of paper with the name of the house and the date, signed by one of the subordinate officers. I had five native subordinates. These seals were applied to the doors, but as there were so many villages affected at the time I first began working, I had to rely on the village officers putting the seals on and dating them, and I had to rely on my subordinate seeing that this was done.

23,313. When you say putting them on, how were they fixed on?—With gum.

23,314. Would not some other method be required in the rains?—Certainly, in the rains.

23,315. Do you find that the people remove them?—They do.

23,316. Can they remove them and put them back again?—In some instances I believe they have done that, but it has only recently been detected.

23,317. In referring to your operations at Igatpuri, you said that after you had resorted to the proper solution of perchloride of mercury, you could not say that better results accrued. Can you tell us a little more definitely what actually did happen there?—I have no record of the exact date on which I discovered the mistake that was being made in Igatpuri of using a solution of perchloride of mercury 10 times too weak, but it was some time towards the end of August when the flying column had been working about a month. I append a statement of the daily returns from Igatpuri, which show that no improvement took place during September.

Major A. V. Anderson, I.M.S.

2 March 1899.

For Igatpuri Town for 1897.

Date.	Attacks and Deaths.		Total of the Month.		Date.	Attacks and Deaths.		Total of the Month.	
	A.	D.	A.	D.		A.	D.	A.	D.
*2.8.97		1	—	—	17.9.97	13	8	—	—
3.8.97	—	3	—	—	18.9.97	11	5	—	—
4.8.97	3	2	—	—	19.9.97	8	5	—	—
5.8.97	1	—	—	—	20.9.97	6	9	—	—
6.8.97	2	1	—	—	21.9.97	11	6	—	—
7.8.97	1	3	—	—	22.9.97	8	7	—	—
10.8.97	—	1	—	—	23.9.97	6	4	—	—
11.8.97	2	1	—	—	24.9.97	7	6	—	—
12.8.97	5	1	—	—	25.9.97	9	4	—	—
14.8.97	3	2	—	—	26.9.97	2	5	—	—
15.8.97	1	1	—	—	27.9.97	6	3	—	—
16.8.97	3	1	—	—	28.9.87	8	9	—	—
17.8.97	2	1	—	—	29.9.97	1	3	—	—
18.8.97	4	4	—	—	30.9.97	6	7	292	251
19.8.97	6	1	—	—	2.10.97	8	7	—	—
20.8.97	3	5	—	—	3.10.97	4	8	—	—
21.8.97	1	2	—	—	4.10.97	8	8	—	—
22.8.97	3	1	—	—	5.10.97	8	8	—	—
23.8.97	1	2	—	—	6.10.97	6	—	—	—
24.8.97	9	2	—	—	7.10.97	4	1	—	—
25.8.97	10	3	—	—	8.10.97	4	5	—	—
26.8.97	8	10	—	—	9.10.97	2	1	—	—
27.8.97	6	5	—	—	10.10.97	4	3	—	—
28.8.97	6	3	—	—	11.10.97	4	1	—	—
29.8.97	8	7	—	—	12.10.97	4	3	—	—
30.8.97	14	13	—	—	13.10.97	4	4	—	—
31.8.97	15	9	123	85	14.10.97	4	4	—	—
1.9.97	16	18	—	—	15.10.97	4	5	—	—
2.9.97	13	10	—	—	16.10.97	1	1	—	—
3.9.97	21	19	—	—	17.10.97	1	1	—	—
4.9.97	10	11	—	—	18.10.97	4	1	—	—
5.9.97	14	16	—	—	20.10.97	—	4	—	—
6.9.97	14	12	—	—	21.10.97	—	1	—	—
7.9.97	8	12	—	—	23.10.97	1	1	—	—
8.9.97	20	9	—	—	25.10.97	4	2	—	—
9.9.87	2	4	—	—	26.10.97	1	—	—	—
10.9.97	6	8	—	—	29.10.97	4	—	84	6
11.9.97	12	5	—	—	2.11.97	2	—	—	—
12.9.97	12	6	—	—	3.11.97	—	1	—	—
13.9.97	13	12	—	—	4.11.97	—	1	—	—
14.9.97	9	6	—	—	6.11.97	1	—	—	—
15.9.97	6	12	—	—	26.11.97	—	1	3	3
16.9.97	14	10	—	—					

* From this date all local cases occurred.

23,318. You thought at the beginning of the epidemic that the disease was a coast disease?—Yes.

23,319. Have you anything further to say upon that now?—I think it was a coast disease, because it had great difficulty in securing a hold in any new town or village. We know that many cases escaped from

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Bombay at the beginning of the epidemic, and even after railway inspection was put on still a large number of cases got through. For example, in Nasik before indigenous plague occurred there we had 35 imported cases. Those 35 imported cases, I think, did no harm.

23,320. Do you think that bubonic cases are usually likely to cause spread of the disease?—I think so.

23,321. For what reason?—Because the germ, I understand, is in the excreta.

23,322. You think, then, that the infection is carried in the clothes of the bubonic patient?—Yes; in all probability it is in the men's clothes.

23,323. When you speak of plague as a coast disease, would that mean that you think it is a disease which requires some time before it can assimilate itself to an inland place?—Yes, certainly. One other point which struck me concerning the outbreak in the Thana district was that no indigenous case occurred in any town or village more than five miles from tidal water. The disease, when it first appeared in Bombay, was undoubtedly a coast disease, and required a high sub-soil water for its development; a town with a good water supply and no drainage suffering more than an equally dirty town with a deficient water supply.

23,324. Have you found in your subsequent experience that this theory still holds good, that the towns with a good water supply are attacked, and those with a bad water supply are not attacked?—Not so much now.

23,325. Do you think, now the disease has spread about in the interior a good deal, that it more easily attacks a fresh place than it would at the beginning?—Within a certain area. Places in the Deccan are much more easily infected now. Nasik, I understand, this year has been infected from one single imported case, whereas the previous year it took at least 40 imported cases to infect the town.

23,326. Might not that depend to a certain extent upon the character of the imported case?—That is what I mean.

23,327. Supposing you had a pneumonic case, would it not be more likely to infect the locality?—I do not mean that.

23,328. But might it not be so?—No; I think it is more dependent upon the locality where the man comes from—the locality from where the man comes with relation to the place he goes to. If they both belong to the same district there is greater danger from a single case than if that case had gone to an entirely different district, as, we will say, from the Nasik district to the Central Provinces; that case would not have been so liable to infect a town there as to infect a town in the man's own district.

23,329. And in the same way a case from Bombay would not be so likely to infect Nasik as a case from some other place in the Nasik district?—That is my opinion.

23,330. The town of Sholapur has a population of 60,000 inhabitants, has it not?—Yes.

23,331. When you speak of the complete evacuation of the town of Sholapur, what do you exactly mean?—I would rather not give evidence upon Sholapur, because my experience was limited.

23,332. I should like to have an answer to the question, because you have expressed an opinion in favour of evacuation upon a large scale; did you mean that the whole 60,000 people were kept under the eye of the authorities?—Not the whole 60,000. Many went away. At least 20,000 left the district altogether, and we did not know where they went.

23,333. But did they not go about the country carrying plague with them?—Undoubtedly they did.

23,334. Then the outbreak at Sholapur was put under, to a great extent, at the expense of other places?—Yes, because the evacuation was not carried out at once. By "at once" I mean as the different areas of the town become infected. Sholapur was infected by degrees; one area was infected, and then another, and another. It had spread over several areas before any attempt whatever was made to evacuate the town.

23,335. To effectively evacuate a town of 60,000 persons (or whatever the population may be) so as to prevent the spread of plague it would be necessary to see that the people did not get away first, would it not?—Certainly.

23,336. Do you think you will ever be able to do that in a big town?—Not altogether, but my contention is that if a part of a town with a population of, perhaps, 2,000 people becomes infected, if we clear those 2,000 people out before they are badly infected they will do very little harm, even if they go away anywhere.

23,337. Then you think that the people went out from Sholapur after they were badly infected?—Yes.

23,338. And they certainly did a great deal of harm?—Yes.

23,339. What is the population of Ahmednagar?—Forty thousand.

23,340. The plague was never bad there, was it?—No.

23,341. Were the whole of those 40,000 people moved out of the town of Ahmednagar?—I cannot say from my own personal knowledge, but I understand the whole town was vacated.

23,342. The whole town was vacated, and I presume the same result as regards the going out of the people happened as at Sholapur?—No.

23,343. The same proportion of people did not fly away?—The same proportion of people did not fly away; and the disease was not spread by Ahmednagar.

23,344. I want to know whether, in your opinion, the same number of people did go away?—I do not think so.

23,345. The tendency to run away would be greater as the disease was worse?—Yes.

23,346. Have you formed any opinion as to the necessity for close segregation in the case of people who are evacuated from the town as contacts?—I consider that contacts ought to be kept in camp by themselves, if possible.

23,347. Do you think that when you have evacuated a town you should keep all the contacts in close segregation, that is to say, prevent them from going about their ordinary business, and keep them in a camp the whole 24 hours of each day?—Yes, I think it should be done, if possible.

23,348. You would not allow them to go out of the camp during the day, and simply require them to sleep in the camp during the night?—It would be preferable certainly to keep them by themselves. I do not think the risk of allowing them to go out would be very great, but it would be more efficacious to keep them under close supervision during the entire day.

23,349. Is it your experience that, when you have people who have been subjected to infection in a camp, there is a danger of the disease spreading amongst a certain number of the inmates of the camp by contact?—Individual cases do occur. It is rather exceptional, but they do occur.

23,350. As a general rule the health of the people who come into a contact camp is good, is it not?—Yes.

23,351. (*Mr. Cumine.*) Do you not think that a sketch of the village site with the deaths marked with crosses against the houses in which they occurred would be even of more value in telling whether plague was there or not, than the mere numbers of deaths occurring in the village?—Yes, certainly.

23,352. As regards the tidal rivers in the Konkan, they are great highways, are they not?—Yes.

23,353. While railways and roads are watched (sometimes at any rate) are the tidal rivers watched in the same way?—An attempt was made, at any rate in the Thana district, to watch the native craft.

23,354. When a village is evacuated in the rains, do the people, in order to protect themselves against the weather, make their huts almost as airtight as the houses in the village site?—They do.

23,355. Are those the conditions which favour the infection of the floors, and the consequent spread of plague to other members of the family where one person is attacked?—Yes.

23,356. Have you experience of the treatment of patients in their own houses in an unevacuated town or village, the leaving patients in their houses, putting them upon beds, and disinfecting the floors daily?—No. I have experience of their being left in their houses in Bhiwandi. That was done during the last

outbreak. The people were not compulsorily moved to hospital.

23,357. Were they put upon beds?—I think not.

23,358. Were the floors disinfected daily?—No.

23,359. What was the result of the leaving the patients in their own houses?—There was a very severe outbreak of plague. Originally the population of Bhiwandi was about 15,000, but it was certainly reduced to considerably under 10,000 by the flight of the people. There were 2,000 cases.

23,360. (*The President.*) You expressed the opinion that people who are removed in the rainy season to huts are affected by general disease to a larger extent than if they had not been removed?—That is my opinion.

23,361. Have you any experience that it ever happened?—No, I have had no experience.

23,562. You have, however, carried out evacuation during the rainy season?—Yes.

23,363. You made a remark about the effect of the water supply in two places. In one place there was a good water supply, but in the other place there was a deficient supply. I understood you to say that where there was a good water supply there was much plague, and where there was a deficient water supply there was less plague. Do you attribute the difference to the water supply?—I attribute it to the moisture of the soil, because in that town there is defective drainage.

(Witness withdrew.)

Lieutenant G. Thomas, I.S.C., called and examined.

Lieut. G. Thomas, I.S.C.

23,364. (*The President.*) I believe you are in the Indian Staff Corps?—Yes.

23,365. Where has your experience of plague chiefly been acquired?—I have been on plague duty in Dharwar district since the 27th of June. I was through the epidemic in Hubli as Chief Sanitary Plague Superintendent. After that I went into the Hubli taluka for about three weeks. Since then I have been in Ron taluka, engaged in district work in the villages. The subject upon which I wish to give evidence is the disinfection of houses in Hubli when I was Chief Sanitary Plague Superintendent.

23,366. Do you hold a special appointment now?—I am the Special Plague Authority in the Ron taluka. As I say, I wish to give evidence on the disinfection of the houses in Hubli. I insisted upon a payment for the disinfection, and the results were exceedingly good. When the disinfection was gratuitous I found that a good many people were giving bribes to the disinfecting muccadums in order to write "Disinfected" over the door, and go away. I found that this was not having good results, so about the middle of August I instituted a fee for disinfection. The people had to pay Rs. 2 for disinfection,—for a certain number of perchloride of mercury powders. In addition to that, they were advised to take more powders, according to the size of the house. Since I started that I have had most excellent results. The notes I have prepared show that there was an immunity of 94·6 per cent. after the first disinfection.

23,367. (*Mr. Cumine.*) Did the people do the disinfection themselves?—No, my parties did it, but the people had to pay for it. Having to pay for it they took a very lively interest in disinfecting the houses.

23,368. Why? I suppose the reason they objected to disinfection before was that it injured their things, or that they had to remove their property,—was not that it?—It was not, to a certain extent. It is the way with these people. If they have to pay for a thing they think there is something in it. I am sure it had a very marked result; the disinfection was much more satisfactorily carried out.

23,369. What were your grounds for thinking that the disinfection was more satisfactorily carried out?—Because we did not have to disinfect the same house so frequently.

23,370. In what month did you start your system?—In August. I have only the office copy of my lists here. I have sent in lists of some 450 houses taken in the worst galis in the town.

23,371. When did you allow the inhabitants to return to the disinfected houses?—The same day.

23,372. What privilege did you offer a man in return for his paying for the disinfection of his house?—Absolutely none.

23,373. Did you not offer him the privilege of allowing him to return to his house immediately without staying in the segregation camp?—No. Everybody had to pay Rs. 2. The Municipality was bankrupt. They could not afford to go on with disinfection thoroughly, and I suggested that the people should pay the money.

23,374. If you did not allow them to return to their houses, instead of going to the segregation camp, how do you know that the disinfection was so efficient?—From my records.

23,375. But the people were not in the houses; they were out in the segregation camp?—If they were twice-inoculated they were allowed to go back to their houses that evening.

23,376. Would you not expect such people to be protected in any case?—They are in a great many cases.

23,377. (*The President.*) These were only the plague houses?—Yes.

23,378. How many houses were there in this town?—About 10,000.

23,379. From how many do you draw your deductions?—Four hundred and fifty houses.

23,380. (*Mr. Cumine.*) You allowed, you say, the twice-inoculated to return to their houses after they had paid for the disinfection of them?—Everybody was allowed to return to his house if he was twice inoculated, whether his house was disinfected gratuitously or not.

23,381. If a man was not twice inoculated, was he allowed to return to his house if he paid for his disinfection?—Not if we could find him.

23,382. What did you do with him?—We sent him off to the segregation camp until he was twice inoculated.

23,383. If he was not allowed to go into his house but kept in the segregation camp, how did you know his house was efficiently disinfected?—That is what I have deduced from my returns. I have a house here, No. 1,909. There were 13 people in that house, and nine died before it was disinfected. When the tenth died the house was disinfected, and no more cases occurred in it.

23,384. Where were the people?—The people were in the house while these nine people died. We subsequently discovered that they had buried them surreptitiously.

23,385. After the house was disinfected upon payment where were the people sent?—We sent them off to the segregation camp; they came back, and they are alive still.

23,386. It is from the fact that they were unaffected after their return from the segregation camp that you deduce the efficacy of the method?—Yes.

23,387. (*The President.*) Did you do anything else; did you open up the house?—I burnt the floors.

23,388. Did you open up the house and take away the roof?—Yes, we opened up the roof.

23,389. If the people were in the segregation camp, how could they attend to the disinfecting of their houses?—If no member of the family was inoculated we allowed one to come down to superintend the disinfection of the house. He was then sent back to the segregation camp.

23,390. Were the houses also thoroughly cleaned?—Yes.

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23,391. And thoroughly ventilated?—It was difficult to ventilate those houses which had mud roofs.

23,392. What did you do with them?—Knocked holes in the walls. We did not get so much sunshine on the floor as we would have done had the houses been tile roofed.

23,393. Air was admitted freely?—Yes.

23,394. (*Mr. Cumine.*) Did you apply perchloride of mercury to each floor on one day only, or on a succession of days?—Only on the day on which the house was disinfected.

23,395. Then the difference between the system in force before you instituted your cash-payment system, and the system afterwards, is that the people personally took an interest in the disinfection after they had paid for it and they did not before?—Yes; and they did not allow the disinfecting parties to shirk their work in any way.

23,396. Has your system convinced the people of the efficacy of disinfection?—Absolutely.

23,397. How do you satisfy yourself that the house owner who used to pay the disinfecting gang a rupee or so before for omitting to disinfect his house does not do so still?—He may give the disinfecting muccadums money, but he certainly has his house disinfected too.

23,398. How do you know that?—I inspect it personally. I had four European Inspectors of the Poona police, who gave me the very greatest assistance in my disinfection.

23,399. Had you not them in the former days when the people used to pay for escaping disinfection?—I had not European Inspectors until within a week of my introducing the system. I used to go round from one house to another. I could not visit 80 houses in one day. I went round as many as I posiibly could. I prosecuted one mucaddum for taking Rs. 4, and he got a month's imprisonment.

23,400. Have you any figures to show the efficacy of this system of disinfection of yours? I believe you have notes with reference to some of the worst galis, have you not?—I have taken seven or eight of the worst galis at Hubli, and from the date that I started this disinfection, I have kept a record, and have made a list of all the houses which are disinfected. This list I have made out according to galis, and have been able to see any houses which have been disinfected more than once, owing to deaths having occurred in the house. The result leads me to the conclusion that there is great benefit derived from disinfection, seeing that so few galis show houses which have been disinfected more than once. I might say that my system of disinfection is the kiln process, as well as a perchloride process.

23,401. What is the kiln process?—Burning the floors.

23,402. Did you dig them up?—No.

23,403. Did you lay grass or wood on them?—Cotton stalk wood.

23,404. (*The President.*) Do you also open up the house?—If necessary. If the house is a dark house, we knock a hole in the wall and order them to put in a window. To get a 3-feet window we have to make a 5-feet hole, because they plaster it up so as not to have a big window. In this list I specially mention the case of Yellapur gali.

23,405. (*Mr. Cumine.*) How many houses did you disinfect in that gali?—I have 39 on this list, and out of those only one had to be disinfected twice, after a period of 11 days. I was able to spend a day in Hubli on my way here, and I inquired in regard to this house, and they told me that the woman who died was 75 years old, and that she had been sick with a cough for some time, and that there had been no bubo when she died. They told me that she died of grief.

23,406. Was the reason of your disinfecting each of these houses the first time, that a plague case had occurred in it?—A plague death.

23,407. Did you disinfect immediately after the plague death?—As a rule it was the next day.

23,408. Will you tell us about another gali?—Here is Hirripet. There are 23 houses only, and not a single house had to be disinfected again. Then there is Kamaripet, in which there were 38 houses, and only one had been disinfected again.

23,409. Will you give us the whole list?—Yes, the list is as follows:—

	Houses once disinfected.	Houses twice disinfected.
Ganeshpet	37	3
Yellapur	39	1
Siddanapet	24	1
Mangalwarpet	14	1
Ghantikeri	12	1
Bhoosapet	21	0
Bawapur	45	2
Kawalpet	37	2
Dajibarpet	21	0
Kamaripet	38	1
Sidwarpet	27	3
Pinjarooni	18	1
Arklekatti	18	1
Mullabada	17	1
Hirripet	23	0
Ninganapet	12	0
Badigeroni	6	0
Simpigali	5	0
Delaioni	12	1
Dhoragali	3	0
Pudharigali	6	0
Hirepet	9	0

23,410. Has this convinced you of the efficacy of perchloride of mercury as a disinfectant?—Yes, and burning; perchloride of mercury certainly for the clothes. That is what I lay great stress upon; they must disinfect the clothes.

23,411. You said, I think, that the success of your treatment had convinced the people of the efficacy of your system of disinfection?—Most certainly.

23,412. In what way have they shown that they are convinced?—As soon as I can get the whole figures compiled, I could show you a number of houses which have been disinfected of the people's own free will, independently of having had a case brought to our notice.

23,413. Do people come forward and ask you to disinfect their houses?—At one time they did when plague was very bad. I cannot say why, but they did.

23,414. Do you give any special privilege to people who pay for the disinfection of their houses over those who do not pay for it?—Yes.

23,415. What is that?—Instead of taking the whole of the tiles off their roofs, they only have strips taken off.

23,416. Do you think that that may be the reason of the people paying for the disinfection, and not a belief in the efficacy of the disinfection?—No; I only did that because the supervisors were making money out of the people, consequently I had to be severe. The supervisors went to a man, and said, "If you will give me a rupee, I will give you a gratuitous pass for disinfection." To guard against this I had to be rather severe. If the people were too poor to pay, I housed them in a camp for the time their roofs were entirely removed.

23,417. In the case of the houses of which you have given us particulars in your table, the twice-inoculated people were, you say, allowed to return to their houses immediately after disinfection?—Yes, provided they were twice inoculated. If we found anybody who was not twice inoculated, we segregated him.

23,418. When was he allowed to return?—As soon as he was twice inoculated. We did not recognise once inoculation.

23,419. Was an uninoculated person allowed to return to his house after 10 day's segregation?—He was kept 21 days in segregation.

23,420. And after that?—He was allowed to come back to his house. There were cases, of course, in which once-inoculated people were never segregated. I know a case in which a house had to be disinfected for a death, and never had to be re-disinfected, and the people were only once inoculated.

23,421. You have experience in the villages now?—Yes.

23,422. Do you use perchloride of mercury as a disinfectant in the villages?—I have not done any disinfecting there. I am going to begin now.

23,423. You do not know whether the people in the villages have been converted to a belief in the efficacy of disinfecting houses with perchloride of mercury?—I cannot speak from personal experience, but I know what has happened.

23,424. What has happened?—That people have been very glad to do it, and have come and asked for it. The people in the villages which it had not been intended to disinfect have come and asked for it to be done. They are very anxious to have it.

23,425. Has any special privilege with regard to liberty to return to their houses been given to people in villages who pay for the disinfection of their houses?—As yet there is no privilege, but it is understood when a village is disinfected that, provided no more plague occurs, the people will be allowed to return to their houses.

23,426. Do you think it is that privilege which makes the people anxious to get their houses disinfected?—No.

23,427. (*The President.*) How do you know?—When they come up they know they will not be allowed to go into their houses for some time.

23,428. But they are allowed to go in if the houses are disinfected?—I was not intending to disinfect Ron; I was going to allow the people in without disinfection, but the people had asked me to disinfect the town. It is in consequence of that that I am now going back to disinfect this town of 6,000 inhabitants.

23,429. In these houses in the galis the patients and contacts were removed for a short time only; how long were they removed from the houses?—If they were twice inoculated, there were not removed at all, but the inoculated members were sent to segregation camp.

23,430. What kind of houses were in these galis?—A good many of them were very good houses with rooms from 18 to 20 feet. In Yellapur as a rule there are good houses.

23,431. What is a good house?—A good house would have a large room in the middle, about 14 feet high, in which they keep the cattle, then on one side they would have a plinth of, say, four feet in height and eight feet broad off which again there would be two or three rooms. That would be a good quarter.

23,432. And the other galis?—Ganeshpet is a small gali; they are mostly labourers working in railway workshops. They have bad houses, very small and covered with tiles. The third bad gali was Kamaripet. These people were all potters, and they have bad houses as a rule.

23,433. Consisting of several chambers or one chamber?—About three rooms.

23,434. You speak of making holes in the walls and also of removing tiles, did you do both?—We never made holes in the walls to the exclusion of taking the tiles off the roof. The latter was always done.

23,435. And the holes in the walls were only made in those cases where it seemed to be necessary?—Yes, but it was not meant as a temporary measure, but with a view to permanently improving the house.

23,436. Supposing you have removed the tiles from the house, how could a person who was ill remain in it?—Every sick person was sent to the hospital.

23,437. How did those who had been twice inoculated continue to remain in the house when the tiles were off?—In the times of the rains they used to go into somebody else's house, or if they liked to make their own arrangements they could stop in their houses.

23,438. In these houses you therefore reproduced the conditions of a hut in the camp, that is, you increased the amount of ventilation?—Yes.

23,439. That is a disturbing element in all the deductions you draw?—To a greater extent in houses which were gratuitously disinfected.

23,440. Supposing these people had not been inoculated, and had been sent to segregation or health camp, would you anticipate that much plague would occur among them? Have you experience of cases occurring in the segregation camp?—Certainly.

23,441. Does plague occur much among people in the segregation camp?—I have heard of five cases. If the huts are properly constructed and ventilated the plague stops. You get very much the same conditions by opening up the houses.

23,442. Your disinfection consisted in the employment of perchloride of mercury, of burning the floors, and then of whitewashing?—I did not whitewash.

23,443. Did not the people whitewash?—A great many of them did, but it was not compulsory. In Hubli town I did not compel anybody to whitewash, but they generally did it. Whitewashing is to be made compulsory in the villages. My experience of disinfection is based entirely on Hubli.

23,444. Where the disinfection by chemicals was also reinforced by disinfection by free admission of sunlight and fresh air, and burning of the floor?—Yes, we did everything we possibly could.

23,445. You have had some experience of inoculation?—Yes, I have been inoculated myself.

23,446. I mean outside of yourself?—Yes.

23,447. You have seen a good deal of inoculation in the natives of the town?—Yes.

23,448. What opinion have you formed?—That it is an exceedingly good thing. I think it has a good effect on immunity.

23,449. If you only allow into the houses those people who in your opinion are immune against plague, how can you form an opinion as to what you have done in the houses by way of preventing plague?—It has been impossible to keep only those twice inoculated in the houses.

23,450. Or even those once inoculated?—If I had had one quarter to look after myself I could have done it.

23,451. Does nobody go who has not been once inoculated?—Cases have occurred in which uninoculated people have escaped the notice of the plague authorities.

23,452. But the great majority were once or twice inoculated?—Yes.

23,453. And were, in your opinion, to some extent immune already?—Yes.

23,454. (*Mr. Cumine.*) What idea, do you think, is at the bottom of the natives' desire to get their houses disinfected now? Is it merely that they have to pay for it?—There is a spirit of rivalry and competition in the matter which must also be taken into account; they like to think they have paid more for it than other people. One man says, "Oh, you paid Rs. 5 for your "disinfection; look at my chit, I paid Rs. 7.8." Another one says he paid Rs. 14, and he is talked about all over the town. A native does not believe in the value of what he gets for nothing; he thinks more highly of things for which he has to pay.

23,455. (*Mr. Hewett.*) Were you at Hubli when the original census was taken?—I was not; I came down after the epidemic began.

23,456. When did you get to Hubli?—On the 27th of June.

23,457. Were you occupied in taking the census every week?—No.

23,458. Did you get reports from the supervisors?—No. That was the Chief Plague Superintendent's work.

23,459. You know nothing about the figures?—Nothing that I could tell you. I heard them in conversation.

23,460. You were there in the middle of September?—Yes.

23,461. What do you think was the number of uninoculated persons actually resident in the town of Hubli in the middle of September?—I should say about 5,000.

23,462. You do not think it went below 5,000?—No.

23,463. While you were at Hubli the people were not allowed to leave the place, or supposed not to leave the place, except on a pass, were they?—No.

23,464. And that pass was only given to inoculated people?—Only given to twice-inoculated people, ten days after inoculation.

23,465. Whether they went by road or by rail?—Yes.

23,466. But a good many persons went who did not get passes, did they not?—I never found any.

23,467. If they did not, how did it happen that so few uninoculated persons were left in the town?—Because,

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I take it, that the majority were inoculated and went away, reducing the population.

23,468. The initial population was 55,000, and it went down to something between 37,000 and 38,000; that is a difference of 18,000 people. If only 5,000 of these were uninoculated, a certain portion of the uninoculated persons must have left Hubli?—Yes.

23,469. And they must have gone without passes, must they not?—That would be very difficult to say.

23,470. Do you think that the people leaving Hubli took the plague into the Dharwar district in the neighbourhood of Hubli?—Yes, I do think so.

23,471. Do you think they were mainly inoculated or uninoculated persons?—I do not think that the twice-inoculated people would have taken the plague with them.

23,472. Dharwar district has been very badly infected, has it not, both in the neighbourhood of the town of Hubli and the neighbourhood of the town of Dharwar?—Yes.

23,473. Plague is now spread all over the district, is it not?—Yes.

23,474. In your taluka how many villages are there which are now infected?—I have two villages which are submitting returns, and I have two others infected, but they have ceased to submit returns.

23,475. Then practically it is at present extinct in your portion of the district?—Yes, I hope so.

23,476. Have you been employed at Dharwar and Hubli during the rains?—Yes. On the 1st of November I went out into the Hubli taluka; and there was rain on the 8th of November for four days.

23,477. How many villages are there in the Hubli takula?—I could not say.

23,478. Could you tell us the maximum number of infected villages you had to deal with in the Hubli taluka?—I had about 20 infected villages myself.

23,479. And were there other officers working in it besides?—Yes, there were others besides in the south of the taluka, which Mr. Westropp, the First Assistant Collector, was looking after.

23,480. Did you find it possible to look after the 20 villages yourself quite satisfactorily?—No, I do not think I did.

23,481. What number of villages do you think you could have looked after satisfactorily so as to prevent the people who are evacuated from perpetually going back to their villages, and so on, in those days when you had difficulties to deal with?—In those days the difficulty I had to deal with was the fact that I had no staff at my disposal.

23,482. Then we will say during the rains?—During the rains I could look after a tract of about 30 square miles.

23,483. Could you look after such a district which was full of infection?—I think so; a tract of about 13 villages, with the staff that I have at my disposal now.

23,484. Thirteen badly-infected villages?—Yes.

23,485. How often do you think you ought to visit a village which is infected?—I ought not to leave it for more than three or four days.

23,486. Has your experience led you to form an opinion as to whether turning the people out during the rains caused great hardship?—Yes, certainly; I think it causes exceedingly great hardship.

23,487. Do you think that the general exposure made them liable to disease?—Yes, I think it did. I can give you one case. There is a village called Umachgi in the Hubli taluka, which had plague on the 30th of September. There were cases being returned every day as plague. Owing to the rains on the black cotton soil they asked Mr. Westropp to allow them to go in. He said, "You may go into your village, but you must "go out as soon as the rain stops." They returned two cases the next day. They have been living in that village ever since, and they have had no more cases of plague up to date. That was in an infected site.

23,488. How do you account for that?—I cannot.

23,489. You did not disinfect the houses?—No, we could not disinfect. That is only a single instance.

23,490. What I mean is this: are the people more subject to disease when they are sent out into the fields during the rains?—They complained bitterly about it. They look most wretched, and in a few cases I gave them blankets. During the rains they were put to more hardship, but now they do not mind it in the least. A great many of them told me that they liked being out.

(Witness withdrew.)

Rao Bahadur R. R. Bhardi.

Rao Bahadur R. R. Bhardi called and examined.

23,491. (*The President.*) I believe you are District Deputy Collector at Dharwar?—Yes.

23,492. (*Mr. Cumine.*) I think your experience of plague has been amongst the villages in the Dharwar district, has it not?—Yes.

23,493. When did you take charge of your duties?—In March.

23,494. In what talukas have you had experience?—In Gadag taluka especially.

23,495. And what others?—I had charge of Navalgund and Ron taluka also for a time.

23,496. In spite of all efforts plague was imported?—Yes.

23,497. Was it brought by human agency, or by what agency?—By human agency; there is no doubt about that.

23,498. There were two distinct centres, I believe, one in the Ron taluka and one in the Gadag taluka?—Yes, but one of them, Aher in the Ron taluka did not actually become a centre of infection as infection was not carried from that village to any other village.

23,499. Where did the infection come from in each case?—It came from Hubli.

23,500. In the second centre I think plague raged for a long time without being discovered?—Yes.

23,501. The result was that a lot of people fled from the second centre?—Yes.

23,502. Did they carry infection to many villages in your district?—Yes.

23,503. They carried infection to some villages, and infection was carried from those villages to other villages?—Yes.

23,504. By the end of November what was the state of things?—By the end of November six villages in the Gadag táluka, five in the Ron táluka, 21 in the Navalgund táluka, five in the Mundargi Petta and one in the Nargund Petta were found to have been infected. Early in December the Navalgund and Ron tálukas and the Nargund Petta were placed in the charge of separate officers. I have been able to ascertain that in many of the villages infection was carried by human agency. I have not come to know as yet that rats carried infection from village to village in my charge, and I doubt whether they can carry it from village to village unless the villages are very close to each other as Jantli and Shirur in the Mundargi Petta. In some of the infected villages, however, rats were reported to have died, but I am inclined to believe that they caught infection which the human agency had brought.

23,505. How many villages in your charge are infected at this moment?—Altogether 39 villages, excluding the town of Gadag particularly, of which I have charge.

23,506. Do you find it possible to attend to so many infected villages? On an average how often can you visit each of the 39 villages?—It is not possible to attend to so many infected villages when I have to do plague duty in addition to criminal and revenue work. I find that I visited about half the number of infected villages. I visited one village more than three times, some three times, some two times, and the others only once. Many of the infected villages which I could not visit were visited by me before they were infected, and they happened to be infected when I was touring away from them in another direction. I have got the Mamlatdar, Mahalkari, the special Head Karkun, the district Abkari Inspector, and other circle officers to work under me.

23,507. According to your opinion, what is the best measure for suppressing the epidemic?—Evacuation.

23,508. Have you any instance where partial evacuation has been successful?—Yes, I have one instance.

23,509. As a rule, is it a failure if plague has "caught on" in a village?—Partial evacuation was successful in one case.

23,510. In most cases, if plague is established in a village, is partial evacuation sufficient to stop it?—No.

23,511. I suppose the chief thing to be desired is to keep the plague out of a village, is it not?—Yes.

23,512. Do you find that people will do this efficiently themselves? Are they alive to the importance of it?—I doubt whether they will do it themselves.

23,513. As regards the early detection of plague, what have you to say?—Early detection of plague is the chief thing to be aimed at. If it is detected before it has taken deep root into the village, even partial evacuation and disinfection of the infected houses are, in my opinion, sufficient to suppress it. But when it is detected after a considerable portion of the village is infected, complete evacuation is the best means of destroying the infection. But complete evacuation can be effected easily and without causing hardship to the people only when the weather is fair.

23,514. In the rainy season is great hardship caused to the people by evacuating them?—Yes, I think so.

23,515. Please give us an instance or two where complete evacuation stopped plague within a short time?—The following are instances:—

Chikhandigal.—Plague was detected on the 16th September, within four days after its appearance. The village was completely evacuated by the 19th November. Up to that date 180 cases occurred of which 93 proved fatal. Snbsequent to complete evacuation only seven cases occurred within fourteen days. The number of deaths did not exceed that number.

Shirur.—Plague was detected in this village on the 22nd August, ten days after its appearance. The village was almost completely evacuated before the 31st August. But owing to heavy fall of rain on the 23rd September, people living outside the village went back to their houses. They were turned out again on the 4th October. Between the 1st and 23rd September 1898, the period during which people lived outside the village, only 39 cases occurred. Between the 23rd September and the 4th October, the period during which people lived in the infected houses, as many as 59 cases occurred. After complete evacuation only 14 cases occurred within 11 days.

I may cite Kitigeri as an instance where plague having been discovered early complete evacuation stopped the epidemic within a few days. The population of the village is 614. Plague appeared on the 1st January and was discovered on the 6th. Six cases had occurred before discovery. The village was completely evacuated on the 7th. After evacuation only seven cases occurred within six days.

23,516. Have you, on the other hand, instances where cases continued to occur for a long time even after complete evacuation?—There are instances where cases continued to occur for a long time even after complete evacuation. For instance, Timmapur, Madagnur, Jantli, Dambal, Alur, Ramenhalli. In Timmapur the village was completely evacuated on the 6th December 1898, and the date of the last case is 16th January 1899. 56 cases occurred after evacuation, of which 40 occurred within 19 days. In Madagnur the village was completely evacuated on the 22nd December 1898. The total number of cases after evacuation is 32, of which 22 cases occurred within about a fortnight.

23,517. Other things being equal is evacuation less effective in stopping plague when it takes place in the rains?—It may, or it may not, be.

23,518. Can you give us any instances where the epidemic has raged in a village with peculiar severity, and if so what, in your opinion, was largely the cause of the excessive severity?—In some of the villages in the Gadag, Navalgund, and Ron talukas the epidemic raged very severely, and I think that the severity of the epidemic was due to a great extent to the insanitary condition of the houses in which the cases occurred and to personal uncleanliness of the inmates thereof. Generally the houses of the agriculturists in villages are very filthy, ill-ventilated, and dark. The argriculturists tether their cattle inside their houses and allow cow-dung and manure to accumlate in the backyards. They never keep the place where cattle are tethered clean. Owing to want of ventilation and light, the houses stink so badly that one who is accustomed to live in purer atmosphere can hardly bear the smell. Further, the villagers generally do not keep their clothes and bodies clean. Many of them do not understand how to protect themselves against contagion. For these reasons I think people living in villages suffer more than those living in towns when epidemic breaks out, if they do not evacuate their unhealthy quarters and expose their belongings to sun and air. The villages which suffered more than the others in the Gadag taluka, of which I now hold charge for plague purposes, are the following:—

Number.	Name of Village.	Population of 1891.
1	Hirehandigol	992
2	Lingdhal	1,469
3	Huilgol	1,866
4	Kadadi	1,443
5	Balaganur	1,933

In Hirehandigol cases which occurred and proved fatal between the 16th July and the 11th August 1898 were taken to be cholera cases. These cases numbered 118. As the examination of several of the patients by Dr. Barron on the 11th August showed that they were all suffering from plague, the village was promptly evacuated. The evacuation stopped the plague within about five weeks. Before evacuation 160 cases occurred including the 118 cases referred to above. After evacuation 108 cases occurred, of which 80 occurred within 17 days and 28 during the period of three weeks following the first 17 days. Out of 108 cases which occurred after evacuation, 84 proved fatal. People went back to their houses without permission about the 22nd November 1898, when it rained, but they were turned out again on the 1st December as ventilation operations were not complete. Openings were made in the roofs of all the houses for ventilation and some of the houses were disinfected with perchloride of mercury. The village was allowed to be re-occupied on the 10th January 1899, one month after the completion of the ventilation operations. A suspicious case of plague having recently occurred, the village was re-evacuated on the 25th January 1899.

The village of Lingdhal caught infection on the 22nd July 1898 from a woman from Hirehandigol. But the infection was not detected till 21st August. Within about 10 days after detection the village was completely evacuated. One hundred and seventeen cases occurred before complete evacuation, of which 85 proved fatal. After complete evacuation 318 cases occurred, of which 287 proved fatal. In no other village such a large number of cases occurred after evacuation. But out of 318 cases, 226 occurred between 3rd and 20th of September 1898. The fury of the disease during that period is attributable to heavy fall of rain during the early part of the period and subsequent cold and bleak weather. The majority of the people were inoculated by Dr. Foy when they were living in huts outside the village. More than 600 people were once inoculated and 400 twice inoculated. It may be noted that only 33 persons out of the once inoculated were attacked. But most of them succumbed. No cases occurred among the twice inoculated. The twice inoculated were allowed to re-occupy their houses after disinfection, but the once inoculated and the uninoculated occupied their houses without permission when it rained heavily on the 19th November 1898. Before orders were passed to turn out the uninoculated people again, whom I thought it necessary to keep in open air for some time longer, two imported cases of plague occurred on the 27th December 1898, and a third suspicious case of illness was brought to notice and the village was consequently re-evacuated under my orders on the 5th January. All the houses in the village were disinfected, and many of them were disinfected under the personal superintendence of the special Head Karkun, according to Mr. Stevens' kiln process of disinfection, slightly modified by Dr. Foy to suit the circumstances of the locality.

In the village of Huilgol, with a population of 1,866, both cholera and plague prevailed simultaneously. The first case of plague occurred on the 7th September.

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Infection was brought from Lingdhal. Between 7th and 18th September only eight cases of plague occurred. Between 19th and 22nd September there were no attacks nor deaths from plague. On the 24th September Dr. Foy examined 11 cases of sickness but was able to discover plague in only four cases. The other cases were found more or less suspicious and had all the symptoms of cholera. Complete evacuation could not be promptly effected owing to rain and inclement weather. By the 10th October 1898 a greater portion of the village was evacuated, and by the end of October it was completely evacuated. Only a small number of twice-inoculated people were allowed to stay in the village. But a large number of cases of plague occurred among the people residing in the village before complete evacuation. Up to the 10th October 170 cases occurred, and between 10th and 31st October 228 cases occurred. Altogether, 398 cases occurred before complete evacuation, out of which 350 proved fatal. After complete evacuation only 45 cases occurred within about a fortnight About 300 persons were inoculated by Dr. Foy about the 10th September, of whom 35 only were inoculated a second time. Forty-five attacks occurred among the inoculated, of which 31 proved fatal. The records do not show how many cases and deaths occurred among the twice inoculated. But I think that they were few if not *nil*.

Kadadi has a population of 1,443. This village was infected about the 22nd October. Cholera was also prevailing in this village when plague appeared. Evacuation commenced about the 1st November, but took more than three weeks owing to rain and inclement weather and obstinacy of the people. Up to the date of complete evacuation, *i.e.*, 23rd November, 244 cases occurred, of which 154 proved fatal. After evacuation 117 cases occurred, of which 103 proved fatal. The date of the last case is 28th December 1898, but the severity of the epidemic continued till the 15th December, inasmuch as 107 cases occurred out of 117 up to that date after evacuation.

Balaganur is a village of 1,933 inhabitants, and in it also cholera was prevailing when plague appeared. The village was infected about the 8th October 1898. Evacuation commenced about the 20th October, but the progress of it was retarded by rain and sowing operations. Many of the people who had vacated their houses re-occupied them when it rained heavily, and they were turned out again. The evacuation was completed on the 18th November 1898. Up to the date of complete evacuation 243 cases occurred, out of which 176 proved fatal. After evacuation 299 cases occurred within about a month, of which 222 proved fatal. This large number of cases after evacuation was due to people remaining for a long time in the infected houses, and to some of the groups of their huts being close to the infected village site, and thirdly, to their not having kept plague patients at a sufficient distance from the healthy. The necessary improvements were made by the 10th December 1898. Up to that date about 275 cases occurred after complete evacuation. After that date only 24 cases occurred up to the 22nd December 1898. For about a month after that no cases occurred. Lately one case is reported to have occurred on the 22nd January 1899, and inquiry has been instituted as to how the person attacked caught infection.

23,519. I think you have some figures to give us with regard to inoculation?—Yes. I have got some figures from a statement prepared by the village officers. These show that 433 persons were twice inoculated in Lingdhal. Dr. Foy's statement shows 443. I do not know which total is correct. 678 persons were inoculated, of whom 433 were inoculated twice, according to the statement of the special Head Karkun.

23,520. How many of those twice inoculated were attacked?—None; but one of them is reported to have died of plague about the end of December, having caught infection in another infected village.

23,521. Out of those who were once inoculated, how many were attacked?—33.

23,522 And how many of them died of plague?—30.

23,523. When were the inoculations performed?—The inoculations commenced on August 21, and continued till October 14, 1898. The second inoculations commenced on August 30, and continued till October 14.

23,524. When was the epidemic at its highest in Lingdhal?—In September. Between the 3rd and 20th September it was very furious. I find that a good many were inoculated for the first time on the 7th, 8th, 27th, and 28th September, and for the second time on the 17th, 27th, and 28th September, and 14th October.

23,525. Was any census of all the people in the village taken at any time to find out who were in the village and who had left the village after the epidemic began?—It was taken immediately after evacuation. When we detected plague I issued orders to the Mamlatdar to have a census taken in all the larger villages and have a health organisation system introduced, that is, house-to-house inspection. In this particular village a census was taken.

23,526. When was it taken?—It was taken before the 5th September.

23,527. Has any superior officer of Government tested the census?—The special Head Karkun and Circle Inspector.

23,528. Have they tested it?—On inquiry made of the special Head Karkun lately, I learn that he did not test the census in each house, but that the census was taken by the schoolmaster, with the assistance of the village officers, and that whenever he had to segregate a patient and to see other inmates of the hut and those of the neighbouring huts, with a view to detect plague, he used to refer to the census register for their names. I learn that the Circle Inspector began to visit the village in October, but I have not been able to ascertain whether he tested the census or not. The number of people living in the village shortly after the plague was detected was 1,372.

23,529. Did not a large number of those run away afterwards before the epidemic ceased?—Not a large number, I think; some went away.

23,530. What became of the uninoculated?—As many as 339 of them died.

23,531. Can you give the number of the uninoculated?—There were 694 uninoculated.

23,532. How do you arrive at that figure?—The total population is 1,372. 678 persons were inoculated, and I deduct that.

23,533. Has any superior Government officer tested the figures returned to you by the village officers with regard to the number of inoculated people attacked with plague?—The statement shows that the village officers furnished the figures to the special Head Karkun, and he communicated the figures to me. On inquiry made of the special Head Karkun lately, I learn that he used to test from time to time the registration of deaths of inoculated persons in the village, and that the figures furnished are accurate.

23,534. Has anybody gone round to every house in the village after the epidemic was over and ascertained how many of the inoculated, and how many of the uninoculated, are alive?—The village officers have a register showing the names of persons who suffered from plague, and the names of persons who died; I believe the special Head Karkun has furnished the figures taken from that register to me, but I do not know whether the houses were visited or not. I rely upon the figures furnished to me.

23,535. (*The President.*) You have several instances of villages where the cases continued to occur a long time after evacuation?—Yes.

23,536. How do you explain that failure of complete evacuation?—Because the infection which they carried from the village through their having remained a very long time in that infected area was strong. Another reason was that the weather was cold and bleak, and, of course, they had to remain in huts which were not quite waterproof.

23,537. The huts which they occupied at night were very much closed up?—Yes, and they were also not waterproof; they were leaking.

23,538. Do you think that would produce plague?—If the people were predisposed to it, then they might get fever.

23,539. Within what period of time after the evacuation did the majority at these cases occur?—The majority occurred within 17 days. After that the disease declined.

23,540. Quickly?—Yes.

23,541. You mention in your précis two villages, Dambal and Alur, and you say the epidemic took a longer time to subside than in other villages, but you think that this fact cannot be taken to establish the efficacy of evacuation. Why do you say that?—I mean the contacts and patients were not segregated.

23,542. Those segregated were allowed to come in contact with others?—Yes, I presume that the contacts and the patients were not segregated from the healthy, and the healthy suffered.

23,543. You did not therefore carry out evacuation properly?—The villages were in the charge of the Mahalkari who was acting under my orders. My impression is that the segregation of the patients and contacts was not strictly carried out, and that people visited evacuated houses stealthily. But the fact of people having remained in the infected villages for a longer time before evacuation, and the cold and bleak weather which prevailed at the time of evacuation, seem also to have contributed to the steadiness of the disease.

23,544. With reference to your cases of inoculation was there any selection whatever of the persons inoculated, or did they volunteer?—We inoculated those who offered themselves. At the beginning, of course, many did not know the advantages of inoculation, and they were unwilling, but I was there and Dr. Foy was there, and we explained the advantages of inoculation, and we induced some of the people to get themselves inoculated. Subsequently when the inoculations were done I was not present, Dr. Foy was alone there, and I believe that the people offered themselves for inoculation knowing its advantages.

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23,545. As a rule these people who voluntarily came forward would be the more intelligent people I suppose?—I think they are all equally intelligent. They are generally illiterate people,

23,546. There is no particular prominent intelligence?—No; I do not think so.

23,547. And the houses were all the same?—Yes.

(Witness withdrew.)

Captain A. F. C. Colomb, I.S.C., called and examined.

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23,548. (*The President.*) You are a member of the Indian Staff Corps?—Yes.

23,549. How long has your plague experience extended?—From the 13th of November 1898.

23,550. (*Mr. Hewett*) Have you been employed in the Dharwar district ever since then?—I was in the Dharwar district the whole time.

23,551. Employed in the villages?—In village work altogether.

23,552. In what taluka?—In the Hubli taluka from the 13th November to the 13th December. I was then transferred to the charge of the Dharwar taluka.

23,553. Have you formed from your observations any opinion as to how the Hubli taluka and the Dharwar taluka respectively were infected?—It happened before my time, so I could hardly do so. Looking back to the village registers before the staff was properly organised it seemed to me that the records were not trustworthy. I tried to trace plague back, and I found that it was not worth doing.

23,554. Do you think the people from Dharwar town infected the Dharwar taluka?—I think it is undoubted in some cases. The Patel at Gadag (infected, 26-10-98) told me that many people from Dharwar came there and died of plague, but that was before my time. There were no villages infected by Dharwar since I took over the charge.

23,555. You had no work to do in Dharwar town?—No.

23,556. Have you paid attention to the possibility of infection by means of rats?—Yes, I have gone into that as closely as I could.

23,557. Have you found any instances in which rats began to die in a village without the possibility of the village having become infected by human beings, or by clothes brought by a human being?—No, not in any of the villages in my charge. In all cases there has been someone from the village, more often than not a member of the household where a mortality of rats occurred, who had been to an infected part.

23,558. Can you give specific instances of persons who had been to an infected place, and in whose houses the mortality among rats subsequently began?—I can. I take Halé Tegur as an instance. Halé Tegur is on the Belgaum district boundary, up in the north-west corner of the taluka. The village of Tegur, just about one and a half miles away, had plague. A Mahar of Halé Tegur used to go and work in the Kulkarni's house in Tegur and the Patel and the Kulkarni made no objection. A mortality among rats occurred in the Mahar's house in Halé Tegur. This village had to be evacuated.

23,559. Have you any other case?—I can give a case which is not in my précis, that of Mandihal (infected, 21-2-99). The first case of undoubted plague was that of a woman. Her huband was connected with police duties, and used to travel about somewhere near Dharwar, I could not discover where. A mortality among rats occurred in that house. The plague case was said to have been on the 10th, but the date had not been exactly recorded. A dead rat was found in the same village on the 10th, that was certain.

23,560. (*The President.*) I notice you use the term "rat-fall" in your précis; would one dead rat come under that category?—There may have been one dead rat in a house, but two or three is more general.

23,561. (*Mr. Hewett.*) When you found two or three dead rats in one house did it generally happen that other dead rats were found in the village, or not?—Almost invariably.

23,562. Does the rat mortality ever become very large?—That I cannot tell because the village is evacuated before we would be able to see it.

23,563. How long after the rats begin to die, does local infection among human beings begin to occur?—I worked it out at about three days.

23,564. From how many villages are your figures taken?—22.

23,565. What is the maximum period between the two events?—10 days according to the register, but the dates in it are not trustworthy; infection in this case occurred before my time.

23,566. What is the maximum you have seen yourself?—The maximum which I can be pretty certain about is six days. These rat mortalities are taken from reports by my native assistants who have to report them, and of course they may be wrong.

23,567. And the minimum?—The minimum is put down as one day.

23,568. May we take it that it is always possible that a rat mortality may not have been observed at first?—That is quite possible.

23,569. So that the intervals may be greater than your facts would give?—Yes.

23,570. Have you had some villages which have been infected without there being mortality among rats?—Yes, there have been cases.

23,571. Are such cases rare in your experience?—There are one or two cases noted. At Kabenur there was no mortality among rats before the plague appeared because plague was brought into the village by an imported case with plague on at the time. It was called an indigenous case in the report, because the man was a member of the village.

23,572. This would be in accordance with your idea that the origin of plague in a particular village is importation from outside by human beings?—Absolutely.

23,573. Were there any other villages where you had no mortality among rats?—Yes, Kotbagi, and Kallur.

23,574. In each of these villages was the first case of plague detected that of a person who resided in the village, but who got infected outside?—It was.

23,575. Have you noticed anything with regard to the virulence of the attack in these three villages compared with any other village in which rat mortality did precede local cases?—Decidedly; the infection was less general.

23,576. What inference would you draw from that?—That if the rats had had time, they would have made the infection more general, and the percentage of deaths would have been much higher,

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23,577. Your inference is, that having been able to put your finger on the place where the plague first occurred, your measures for stamping it out were more effective?—Yes, because infection had not been disseminated.

23,578. In addition to these three villages in which there was clearly no mortality among rats, there were a certain number of villages in which you were not able to definitely ascertain that there had been mortality among rats?—Yes, I have noted five cases.

23,579. In those villages was the mortality severe among human beings?—It was very light.

23,580. In those cases, do you think it possible that you also got hold of the first case?—I think I must have.

23,581. But you cannot say whether they were cases of local persons who got infection elsewhere?—I cannot say for certain, as I did not inquire into the origin of the disease here, but I have very little doubt that infection was from outside.

23,582. Can you say whether prompt evacuation has had a good effect upon the virulence of plague?—Yes, it has.

23,583. Can you give us figures to show that?—I have cases of eight villages which were evacuated before the first case occurred; that means that there had been a mortality among rats, and the village began to go out before the first case occurred. The average death-rate in those villages was 1·10 per cent. up to the 10th day after complete evacuation.

23,584. At your examination of those villages, were you able to satisfy yourself that there had been no plague cases before you took the people into camp?—Yes, in most cases, from the birth and death registers. There are five villages in which it commenced one day after the first case, and the average number of attacks up to the 10th day after evacuation was complete was 1·11 per cent. There were four villages in which it commenced two days after the first case had occurred, and the average attacks up to the 10th day was 1·84. I omit Kallur, because I feel sure that here the disease was cholera and not plague.

23,585. Will you specify the total population of each of the following groups:—1stly, the villages which were evacuated before there was any case; 2ndly, villages which were evacuated one day after the first case; and 3rdly, villages evacuated two days after the first case?—The total populations are as follows:—

1st. Villages commencing evacuation before the first case occurred - -	14,868
2nd. Villages commencing evacuation one day after the first case had occurred - - -	5,194
3rd. Villages commencing evacuation two days after the first case had occurred - - - -	3,448

23,586. Can you furnish any figures which show a higher incidence of attack among places in which evacuation was not prompt?—From my own experience I give three villages in the Hubli taluka, Unkal, Shirguppi, and Yerguppi.

Unkal.—Infected 11th September 1898:—
Evacuation commenced about 1st November 1898.
Evacuation completed 19th November 1898.
There were 487 attacks up to the completion of evacuation, and subsequently only one attack up to 11th December 1898.

Shirguppi.—Infected 14th October 1898:—
Evacuation commenced about 1st November 1898.
Evacuation completed 28th November 1898.
There were 130 attacks up to the completion of evacuation, and subsequently only 10 up to 10th December 1898.

Yerguppi.—Infected 28th September 1898:—
Evacuation commenced about 1st November 1898.
Evacuation completed 29th November 1898.
There were 456 attacks up to 29th November 1898, and three between that date and 10th December 1898.

23,587. Will you give the population of each village?—Unkal, 3,915; Shirguppi, 1,919; Yerguppi, 1,702.

23,588. Have you had any villages in which the mortality among rats occurred without subsequent deaths from plague among human beings?—Yes. Seven villages have been noted as having been immune through timely evacuation, although there had been mortality among rats. There are, in addition, some 20 villages in the taluka which were evacuated by reason of high mortality, or their being in the neighbourhood of infected areas which have, so far, escaped infection.

23,589. What does your experience enable you to say as to the advantage or disadvantage of evacuating villages?—I believe that rapid evacuation is everything, and I think that the mortality from plague would be less if, instead of waiting till the disease was present, whole districts were evacuated at once on plague appearing anywhere near them.

23,590. Is your experience in dealing with plague in this part of India entirely confined to the period of cold weather?—Yes.

23,591. Do you think that when the monsoon is on it would be practicable to evacuate a large number of villages?—Not on black cotton soil. It plague occurs again during the rains, I would camp the people on the roofs of flat-roofed houses. It is no hardship to evacuate villages during the rains in the hilly part of the Dharwar taluka. Here the people have built houses to meet such a contingency.

23,592. Do you think that taking the people out into the black cotton soil during the rains would cause them hardship?—It would cause them terrible hardships; I do not think it could be done at all.

23,593. Can you give us instances in which infection was carried through human beings?—I have already referred to Halé Tegur. Further instances are:—

Tegur.—Infected on 26th November 1898, by what was reported as an indigenous case. The first sufferers were two women who kept a sort of travellers' rest-house. They may have become infected through contact with their guests, but to make assurance doubly sure, one of them went to Gadag which was infected, and contracted plague within eight days of her return. The second woman nursed and slept in the same bed with the first. While these two were ill with plague, a woman from another house joins them and sits with them repairing baskets, and brings back with her some cakes, presumably from the infected house, of which she and her son partake; next day the son has plague.

Aminbhavi.—A long series of deaths from plague occur in the hut of the Timapur Kulkarni living in Aminbhavi. The two surviving members of the family are placed in a contact hut from which they abscond about the 26th January to Navalgund, where one of them promptly dies of plague, the disease having declared itself *en route*.

Tarkor (Tadkod).—Infected 24th January 1898, by what was reported as an indigenous case. The patient was a woman. Her brother was ill with plague at Khanapur, a neighbouring village, and she went to nurse him, remaining with him for two days before his death. She returned to Tarkor, and was attacked with plague within ten days of her return. The day after seizure her son was also attacked.

Kallapur.—Infected on 24th January 1899, by what was reported as an indigenous case. Patient was a woman, who on 17th January 1899, went to, and passed the night at, Alnavar, an infected village. There she bought some clothing which she was wearing when she was taken ill. Evidence was not procurable as to whose house in Alnavar she stopped in.

Jirigivad.—Infected 1st February 1899, by what was reported as an indigenous case. The first case was a trader who was in the habit of visiting all markets and infected and uninfected villages alike. The infection cannot be attributed to any certain village.

Kotur.—Reported as infected on the 4th February 1889 by several simultaneous indigenous cases. This village is a somewhat remarkable case of double infection almost simultaneously. Among the low castes at one end of the village, a house was pointed out as being the first house in the village to be evacuated. It appears that there had been a rat-fall there, and that the owner had locked the house and left hurriedly. His son had died at Navalur of plague about four months ago, and he had revisited Navalur within 10 days of vacating his house. Presumably, he brought back from Navalur some articles from his late son's house through which infection reached the rats. At the other end of the village, among the high castes, a woman died on the 30th January; she was a yarn

maker, and travelled from village to village selling the fruits of her industry. Her death was reported as fever, but there was a rumour of a rat-fall in her house on the 27th of January. A case of undoubted plague occurred in the same house on the 5th February.

These few cases may, of course, have been mere coincidences, but as inquiries into each fresh outbreak result invariably in the discovery of facts which point to human agency being alone responsible for the spread of the disease, one cannot but think that rats, monkeys, and squirrels have been accorded rather hasty condemnation. In any case the above cases serve to show the utter want of precaution, among the villagers themselves, to check the epidemic.

23,594. Is it your opinion that human agency is, in the majority of cases, the cause of infection?—It is my opinion that it is the cause of infection in all cases.

23,595. Your experience is that infection is never taken a great distance by any other agency?—That is my opinion.

23,596. Do rats move much from place to place?—As far as I can make out they never move out of the village. I have asked the question a hundred times, but I can get no single record of the rats ever leaving a village.

23,597. It has sometimes been asserted that the rats move off when there is plague; you do not think they leave the village?—No, they may run to one end, but I think they run back again.

23,598. Do you think that when a village becomes infected the rat tribe is exterminated there during the outbreak?—I think it must be. I saw a curious case the other day when I to went to dig up one of those houses to get earth from the floor to send to Bombay. There was one particular spot in the corner of a room where three people had slept who had been attacked by plague one after another. The house had been ventilated but not properly—a little light had got in. The only part of the floor which has been attacked by rats was the very place where these particular cases had occurred. Whether they had been trying to escape or not I do not know.

23,599. Are field rats ever infected?—I have never seen it myself. I give examples in my précis.

23,600. Those examples are not your own?—No.

23,601. Do you place any reliance upon them?—I think it is possible that field rats might be infected, but I do not think they spread the infection, Some native officials have told me that field rats have been infected.

23,602. Are field rats ever found inside villages?—Never inside the houses.

23,603. You do not find house rats in the camps?—Not a single example.

23,604. Have you noticed the infection among any other animals?—I have not noticed it myself, but I have had reports of monkeys.

23,605. Which kind of monkey?—The brown monkey.

23,606. Do you think that there is any reason why a brown monkey should take the infection more than the grey one?—Because the brown monkey lives about houses; he moves about the roofs, and if he can get a chance he will go inside to steal food, which the grey one will not do, being somewhat timid.

23,607. In connexion with infection by human agency, have you had any experience which would show you that particular classes have been to a large extent responsible for taking the plague from place to place?—As far as my experience goes it is the trading classes who are permitted under the "Spence Committee rule"* to travel. I cannot say that they have carried it more than other people, but in my experience they have been the people who have largely taken the disease from place to place.

23,608. Can you give us any figures with regard to them?—Referring to the likelihood of plague being spread by small traders, the following figures showing the employments of persons attacked at Gadag in December 1898, are interesting. There were in all 11 cases, of which five were traders, two goldsmiths, three females, and one was a labourer. The total number of traders in the town is about 100 in a population of between 4,000 and 5,000. It will be seen that seven of the 11 cases occurred in the travelling classes.

23,609. Do you think that it is possible by any system of passes to prevent free inter-communication from place to place?—I think it may be done in time. Free inter-communication may cease when the village officers understand its danger, and when villagers who can be absolutely proved to have introduced plague into their own village are punished.

23,610. I suppose you would mainly rely upon the people themselves, who would be desirous of excluding such persons?—Yes, we must depend upon them because we have not the staff to undertake the supervision. All uninfected villages are warned against the admission of strangers or persons from infected areas.

23,611. There is no Government agency to undertake the duty?—Not on a sufficient scale. The ward system is in force, the ward men being, as far as possible, Government servants. Nevertheless, thoroughly effective supervision is impossible.

23,612. Have you investigated in any way the question of the manner in which rats take plague about in a village?—I give an instance of the village of Kotur which was infected on the 30th of January 1899. Where plague attacks a village for the first time, I think that, given a sufficient interval for the rats to infect one another, the whole village site will finally become infected. In this district evacuation has usually been so rapid that instances of general infection of a village area are not to be had. But one interesting example of partial infection of this sort has just come to my notice. The village concerned is Kotur which has been referred to above. Here plague appeared probably about the 27th of January, but owing to concealment of the disease no steps towards a general evacuation were taken until the 7th of February 1899. Below is a rough plan of the village:—

Rough Plan of the Village of Kotur
Scale - About 1 inch = 100 yards

The houses marked black are those in which plague cases occurred, those outlined in black are those in which a rat-fall occurred. The dates given are those on which the patients died, having been taken ill probably on an average of four days earlier. The group of houses in black to the west are all inhabited by low-caste families, and those to the east all by high-caste families. In the western group house, it was the first in which a rat-fall occurred, exact date not known, while in the house just north of house B a rat-fall

* Bombay Government Notification, No. 5391/3495, dated 5-10-97) not published with the Commissioner's Proceedings.

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occurred on the 27th anuary. In house B there was a rat-fall on the 4th February, but it was at once vacated. The peculiarity here is that the deaths occurred all on the same day in the western group, and the majority on the same day in the eastern group; the dates of seizure were not kept by the village officers, but they may be assumed to have been about four days earlier in each case. For so many houses to have become infected simultaneously other than human agency must have assisted in spreading the disease. The inhabitants of the western group are of the shepherd class, and have absolutely no dealings with the Lingayats of the eastern group. There would appear to be little doubt that rats were the agency for the spread of infection. In the map the direction of the wind has been omitted; it was due east, that is, running straight up the page, while infection travelled north and south.

23,613. Where was the infection?—There was a double infection.

23,614. On which sides of the village?—On the east and west sides. In the western part of the village infection was spread purely by rats. Infection originated through importation in the house marked A, and here mortality among rats occurred first of all, though there were no cases, because the house was at once evacuated; but deaths were simultaneous among the other houses in that part of the village, showing the infection had been evenly diffused and evidently carried by rats.

23,615. How far was the house infected upon the west side from the nearest house infected on the east side?—About 160 yards.

23,616. This interval did not get infected at all?—It remained uninfected up to evacuation.

23,617. You then evacuated the village?—Yes, on the 7th of February.

23,618. Was there no personal communication between the inhabitants of the village?—It is very possible, as in the western infected area they are all of the same caste—shepherds.

23,619. Then is it absolutely conclusive that the rats did move on the western side of the village?—No, but I deduce that they did from the similarity of dates of deaths in these houses. I do not think that these people could have collected together so as to have become infected all on the same date.

23,620. How many houses were infected on the same date?—The date of attack was not registered, but only the date of deaths; but the attacks were probably about the same period. The deaths were all on the same day in six houses.

23,621. At the eastern end of the village how many houses were attacked?—There were five houses attacked.

23,622. Did the deaths take place simultaneously in those houses?—Not quite simultaneously—they were on the 4th, 5th, and 6th February, and one attack which was not returned, but which must have been plague, was on the 30th January, giving two attacks in the same house within an interval of five days.

23,623. Would it not have been possible for this plague case on the 30th January to have given infection to the other people?—Of course, it is possible, but I do not think it is likely, because the people in the eastern group of houses were of high caste and they had no relations with the low castes on the western side.

23,624. In the particular side with which we are dealing now you have deaths on the 4th, 5th, and 6th. Could not the case which took place on the 30th have given the infection to all those people on the 4th, 5th, and 6th?—Yes, that is very likely; that might have happened. But to show that rats were spreading infection, there is the house marked B, in which rats were found dead and the people evacuating at once did not get plague.

23,625. Did they leave before these deaths had taken place?—I was there on the 7th and they were gone then. I could not say the exact day they left.

23,626. You have said that house rats do not exist in the camps. Have you been able to observe the manner in which infection is carried about in camps?—I give the example of Kotbagi to which the plan below relates:—

At the first glance this would look like local infection also, but as a matter of fact, the disease was communicated by human agency alone. The disease first appeared in the house nearest the stream which furnished the water supply of the group of huts. This case was reported as a seizure and death on the 21st January, but one of the contacts confessed that the patient had been ill for three or four days before the 21st, and that the inmates of the houses infected on the 22nd, 25th, and 27th, when going to draw water, used to come into the hut and sit with the patient. In this respect the women were the greatest culprits and it was their custom to remain with the plague patient for half an hour or more at a time. It is generally the case that in a group of villagers' huts those in which plague has occurred are close together, generally 5 to 10 yards and seldom more than 30. But for all that the locality is not infected as, more often than not, there are uninfected huts between those in which plague has appeared. Infection is spread by the people themselves, especially by the women, who cannot resist the temptation of paying a visit to the invalid just to see whether he has plague or not. There was a hut at Aminbhavi in which a plague case had occurred; the hut was emptied of all property, the patient and contacts removed, and the roof pulled off. This had no sooner been done than a relation of the patient promptly annexed the dismantled hut and used it as a cattle-shed, while he turned an unoccupied corner into a cook-house for himself.

To cite another example. A certain roomy hut at Aminbhavi stood quite alone in a field some 200 yards distant from the rest of the group. In this hut lived seven members of a family, of whom five died of plague. Two women, connections of the family, visited the patients, contracted the plague, and carried it to other groups of huts, and another relative carried the disease to another village. The original hut was vacated on the first case occurring, but the contacts did not again move when the disease re-appeared among them. As between villages, so between huts or groups of huts, the spread of infection can always be traced to human agency; and among camps it would appear that there is no such thing as infection of locality, mainly because there are, as a rule, no rats to carry infection, or because the field rats are not so liable to the disease as the house variety. Sun-light and free circulation of air are also factors to be reckoned with. It is not easy to obtain cases of the continuance of the disease in camps being due to persons secretly re-visiting the infected village site for, unless such persons are caught *flagrante delicto* they will not confess. Personally, I have only come across one case of the sort, where a villager at Shirguppi returned secretly to his house one evening, a few days after the village had been evacuated, and was found there with plague next day; he stated that he was not ill when he re-entered his house, but the truth of his assertion is doubtful.

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23,627. Do you know whether these people got pneumonic or bubonic plague?—Bubonic.

23,628. Are you of opinion that plague can only be checked by isolating infected areas?—Yes.

23,629. How would you propose to do that?—That is a question that I have hardly thought out yet. If the villagers can be brought to understand that it is to their advantage to keep to their own villages and are punished for leaving, I think it can be managed. There is punishment for the village officers, but I do not think the mufassil plague rules lay down any punishment for the individual who introduces plague into his own village. I think such persons should be punished heavily, and that a few examples would make a great difference.

(Witness withdrew.)

Mr. J. L. Lushington called and examined.

Mr. J. L. Lushington.

23,630. (*The President.*) I believe you are in the Revenue Survey Department?—I used to be, but I am now Superintendent of Land Records and Agriculture.

23,631. You have been at Bijapur?—Yes.

23,632. (*Mr Cumine.*) You have had experience in two districts, Bijapur and Dharwar?—Yes.

23,633. Whilst you were in the Bijapur District the only place which got infected was Kaladgi, I believe?—Yes.

23,634. Where is the infection supposed to have come from?—From Sholapur.

23,635. Will you describe what was done in Kaladgi?—Towards the end of February the death-rate of the town of Kaladgi having increased, Assistant-Surgeon Mr. Redkar was sent down to find out the cause; he said there was plague, and I myself at once proceeded to Kaladgi. The place was at once evacuated, plague cases were taken to a hospital, and contacts to a contact hospital. A field census was made, and Circle Inspectors went round to find out cases every day in the fields. Disinfection was first made in every plague-stricken house and each house adjoining, and when that was finished the remaining houses (left out) in the street were disinfected and whitewashed. To the best of my recollection we did two whole streets and three or four separated houses. The rest of the town was then dry-brushed down and the houses cleaned. There were only about 26 cases in all, of which from eight to 10 took place after evacuation. Evacuation lasted until the beginning of May, when the people returned to their houses, and after return there were no cases; besides this, plague did not spread to other villages. All the roads of Bagalkot were patrolled to prevent people from Kaladgi entering secretly, Bagalkot being the large town of the district.

23,636. This was an instance of the successful stamping out of the plague?—Yes.

23,637. At its first appearance in a district?—Yes.

23,638. When were you transferred to the Dharwar district?—2nd December 1898.

23,639. What part of the district came under your charge for plague purposes?—East Navalgund and the west of Ron taluka.

23,640. How many infected villages have you under you at the present moment?—About seven or eight.

23,641. What is the area covered by them?—The two talukas join each other. The area is about 20 miles by 20 miles, or rather more.

23,642. What is your means of keeping a watch upon the mortality in the uninfected villages?—A weekly statement of deaths is sent in from the uninfected villages, and also a statement of sicknesses, which goes in weekly.

23,643. To whom?—To the Mamlatdar.

23,644. How many days after the close of the week does it reach you?—It ought to reach me the next day if I am in the same taluka. There is an extra day's delay if I am in the next taluka.

23,645. It ought to reach you on the day after the close of the week that it refers to?—It is sent in on Saturday; it would get to the Mamlatdar on the Sunday, and I ought to get it on the Monday evening.

23,646. To what extent are the people alive to the danger of admitting possibly infected people into their villages? Can they be relied upon to protect themselves against the entering into their villages of infected people?—No, they will let in relatives wherever they can.

23,647. When a village has become infected what do you think is the best way to treat it?—By prompt evacuation.

23,648. Is evacuation practicable without great hardship to the people during the rains?—In black cotton soil it would be a great hardship, but possibly in some of the western talukas, where they are more accustomed to rain, and have usually huts of their own on the borders of the jungle, it would not be such a hardship.

23,649. In what sort of soil does the plague rage most fiercely according to your observation?—In black soil.

23,650. Can you give any explanation of that?—Even now, in digging for foundations, 14 or 15 inches down the soil is found sufficiently moist to be able to make up a kind of clay balls.

23,651. So that you think it is the moisture that is the cause?—Black soil is a retentive soil, very clayey; I think that is the cause.

23,652. Have you noticed that villages along the banks of a river are more liable to infection than others?—Not necessarily, I think.

23,653. Do you find you can look thoroughly after the villages committed to your care? On an average, how many times can you visit each village?—I have to look after all uninfected villages as well. I, myself, go about once in 20 days to each village.

23,654. Do the people understand the danger of revisiting their evacuated houses? Will they boycott anyone who does it?—No, certainly not.

23,655. When a village gets infected, whom do you place there to look after it?—Whether it is infected or uninfected, it is under a Circle Inspector. He has a group of villages, some of which are uninfected and some infected.

23,656. Who remains on the spot to look after it?—Nobody except the village officers, but the Circle Inspector ought to be there directly he hears of a case, which will probably be the next day. I myself have given strict orders that they are to remain there for a day or two if plague is at all bad instead of going on to another village.

23,657. From what place did the infection of your villages come?—A tremendous number were infected before I got there. I started with 20 villages. My first weekly return in Navalgund showed 260 attacks in the 20 villages.

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23,658. Before you left Bijapur, had the epidemic appeared in Ilkal?—No. It did not get to Ilkal for three or four months afterwards. I took it over a free district, and left it a free district.

23,659. Do you know where it came from to Ilkal?—No.

23,660. If no European remains at hand, will the people of an evacuated village obtain for themselves the full benefit of evacuation by isolating the sick persons and preventing anybody from reimporting infection from the evacuated site?—I think a European is necessary.

23,661. Have you found disinfection with perchloride of mercury efficient in destroying the germ?—I have only had experience in one place, at Kaladgi, in Bijapur, and in that one case it was successful. Since that I have had disinfection going on in my villages and two of my small towns, but the people have not re-entered yet.

23,662. Is there any other form of disinfection which you have found useful?—I am in great favour of dryness and ventilation; not a scrap of water should be in a house. My plan of ventilation has been that a strip taken off the roof in every room—the theory is a 4-foot strip, but when rooms are narrow it has been less. The chief thing, however, is the 4-foot square hole in the wall. The sun and wind gets into that, especially the latter, and creates a thorough drying draught through the hole in the wall and out of the strip in the roof.

23,663. Have you any reason to believe that that is practically efficient in killing the germs?—I have no proof of it yet.

23,664. (*The President.*) You have one case of chemical disinfection with a satisfactory result?—Yes.

23,665. What did you do besides disinfection?—We evacuated the town, and disinfected the streets in which the plague occurred. After that we brushed down and whitewashed the rest of the houses. We only disinfected certain streets in which plague had occurred.

23,666. Did you open the roofs?—In a very small way. The doors, of course, were opened out and the roofs slightly ventilated.

23,667. How long were the houses kept unoccupied?—For two months.

23,668. What was the largest village you have managed to get evacuated?—A village of 9,000 inhabitants.

23,669. Was that successful?—It had been commenced by the person whom I succeeded; it was not thoroughly evacuated. When I came down I finished it off.

23,670. You had the whole of that village out?—Yes; they are out now.

23,671. Do you think it is feasible to evacuate a larger village than that?—Yes, with a little time. The last people in a village are the most difficult to evacuate and I got all those out.

23,672. How long did you take to get out this village with 9,000 inhabitants?—It was partially done before I got there, and so I cannot say.

23,673. How long does it take to evacuate a village?—About three days per 1,000.

23,674. You mean in fine weather?—Yes.

23,675. (*Mr. Cumine.*) Are there any complaints of theft of property in evacuated houses?—They have tried to burn houses. There is no property in the villages. I have everything taken out—cotton, grain, bags, everything. There is a little loose grain left sometimes.

23,676. Have there been complaints of theft of rupees?—I have not heard of any thefts, but they have tried to burn the houses.

23,677. Have any villagers asked for disinfection?—The villagers of Nalawadi, Navalgund taluke, requested that their village might be disinfected, or that I would sell them perchloride of mercury to disinfect the village themselves. As the disinfecting inspectors were working elsewhere I was unable to accede to their first request, and I did not comply with their second as I did not like to hand over so much poison to the people.

(Witness withdrew.)

(Adjourned till to-morrow.)

At The Collector's Office, Belgaum.

SIXTIETH DAY.

Friday, 3rd March 1899.

PRESENT:

PROF. T. R. FRASER, M.D., L.LD., F.R.S. (*President*).

Mr. J. P. HEWETT. | Mr. A. CUMINE.

Mr. C. J. HALLIFAX (*Secretary*).

Mr. Foy, M.B.
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Mr. FOY, M.B., called and examined.

23,678. (*The President.*) You are a Bachelor of Medicine and Master of Surgery of Aberdeen University?—Yes.

23,679. In what place have you been occupied in plague work?—In Bombay, Karachi, and in Gadag Betigeri.

23,680. You have some information with reference to the value of inoculation during a plague epidemic I think?—Yes, with reference to a place not getting so depopulated; that is the point I wish to mention. The town of Gadag Betigeri in the Dharwar district consisted according to the census of 1891 of 23,000 inhabitants. This fell by the 11th of February 1899, about four months after the epidemic, to only 12,400. Gadag which is separated from Betigeri by about a mile of open land was the first part where plague broke out. Prior to the outbreak of plague about 2,500 of the inhabitants of Gadag (which nearly equally with Betigeri shares the number of inhabitants) were once inoculated, and 365 of these twice, and more would have been inoculated had not the supply of serum failed them. With the appearance of plague there was a further rush for inoculation and a large number were inoculated before the epidemic had taken a very strong hold, and to prevent having themselves evacuated from their homes. A month after the epidemic had appeared the numbers of the inoculated were doubled, nearly

3,250 having undergone single and 1,720 double inoculation, and even these numbers would have been greater but for the short supply of lymph at that time. The epidemic began to spread in Betigeri about two months after its start in Gadag, although sporadic cases had already occurred there, the number of those who were once inoculated in Gadag Betigeri now being 6,179 and those twice inoculated 5,686. By this time the disease had got to its height at Gadag. The residents of this part of the town being mainly agriculturists had moved to huts erected on their fields, partly voluntarily and partly as a result of plague measures, (those who moved voluntarily included some of the inoculated), so that Gadag on the whole presented a very empty aspect. Many of the shops were closed, the Marwaris all left the town, the agriculturists were encamped on their fields, and the tradesmen and bankers had left for other uninfected places. Betigeri on the other hand learned by the example of Gadag. The people in it were mostly of the weaver class and although their work has suffered in the general panic it has not been entirely stopped. The people of Betigeri made a great rush for inoculation and thus avoided the general evacuation of the town by getting all the inmates of their houses inoculated. The consequence is that the main street of Betigeri has already presented a busy aspect, the shops remaining open, and some amount of trade is carried on. That the greater part of this good result may be put down to the feeling of safety produced by inoculation is shown by the fact that of the 12,426 occupants of Gadag Betigeri on 11th February 1899 as many as 11,703 were inoculated, 10,994 being twice inoculated.

23,681. Can you go further, and give us any facts which will show whether in addition to the checking of panic the spread of the epidemic was appreciably affected?—I am sorry I cannot give any opinion with regard to that point.

23,682. You have had a good deal of experience of hospital treatment?—Yes.

23,683. Were you in charge of some hospital?—Yes, of the Plague Hospital at Gadag.

23,684. Had you any opportunities of seeing whether the friends who attended the patients in the hospital became affected?—Of all the friends only three were affected.

23,685. Whether they were inoculated, or not inoculated?—Quite so. We had three cases amongst those who were attending on their friends. These three cases were all uninoculated.

23,686. Out of how many?—We had 334 admissions, and each patient had a friend if they liked. I suppose about 50 may not have had any friends with them. To 291 admissions we have a record of 117 friends, but all the inoculated friends were not recorded.

23,687. Had every patient one friend only, or more?—One only.

23,688. They were therefore a smaller number than the patients?—Yes, smaller; and there were three attacks among them.

23,689. In reference to the hospital servants did they become infected?—We had no attacks among them.

23,690. Were they inoculated?—The majority of them were, but not all.

23,691. Can you distinguish the numbers of those who were and those who were not?—There were six out of 23 who were uninoculated, one was once and 16 were twice inoculated.

23,692. How many out of the total admissions died within 24 hours after admission?—Sixty out of 334 died within 24 hours of admission into the hospital.

23,693. Have you any facts as regards the incubation period from your experience with the segregated contacts?—I have worked this out by adding the number of days after admission to the contact camp that the contact took the plague, and adding that to the number of days before their admission that their friend in the house had been attacked, that is to say I add together the number of days during which the contact was exposed to infection to the number of days within which he developed plague after his admission to the contact camp. I got the following figures:—

1 day	1 case.
2 days	2 cases.
3 days	1 case.
4 days	5 cases.
5 days	2 cases.
6 days	3 cases.
7 days	5 cases.
8 days	4 cases.
9 days	1 case.
10 days	1 case.
11 days	2 cases.
12 days	2 cases.
13 days	1 case.
16 days	1 case.
17 days	1 case.
18 days	2 cases.

If the time after admission to the contact camp which the patients took to develop plague in the above is taken, I get the following figures:—

1 day	1 case.
2 days	6 cases.
3 days	5 cases.
4 days	6 cases.
5 days	10 cases.
6 days	2 cases.
7 days	2 cases.
8 days	1 case.
9 days	1 case.
10 days	0 case.

This method alone does not eliminate the fact that the patient may have been exposed to infection and undergoing incubation some days before admission to the contact camp, whereas the other method quoted is open to the objection that the patient may have received infection from other persons than his friend. On the whole, however, it is unlikely that he should get infection from others than those in whose company he is constantly found.

23,694. What have you to say about the reduction of mortality under treatment in the inoculated, as contrasted with the uninoculated persons?—In the uninoculated 29 per cent., that is 54 out of 184 recovered. Among the once inoculated we had 17 out of 32 recovered, giving a percentage of 53. There were six per cent. still under treatment when these figures were taken.

23,695. Still in a precarious condition?—They were still under treatment in hospital; they had not recovered. Among the twice inoculated 34 out of 75 recovered, being a percentage of 45. 21 cases were still under treatment, making a percentage of 28.

23,696. What have you to say with regard to the clinical aspect of plague among the inoculated? Is there any difference in the case of the inoculated patient as contrasted with the non-inoculated patient?—Yes. The symptoms are of a milder type. Symptoms such as delirium, moreover, are not usually met with in the inoculated. Delirium occurred in 99 out of 184 uninoculated cases, nearly 54 per cent.; while among 107 inoculated cases only 24 had delirium, or 22 per cent. Of the 75 twice inoculated only 17 had delirium, while of the 32 once inoculated, only 7 suffered with it.

23,697. What have you to say with regard to the almost complete absence of evil effects of inoculation which you mention in your précis?—Practically, that evil effects have been almost absent. There are a few evil effects of inoculation, of which I have some notes here.

23,698. What were the chief evil effects?—In a few cases prolonged fever.

23,699. Of how many days?—About 10 days after inoculation.

23,700. A continued fever for 10 days?—Yes, a low fever of 100 or 101. In some there was hyper-pyrexia, and in some vomiting.

23,701. With regard to the hyper-pyrexia, that was not long continued?—No.

23,702. What do you mean by hyper-pyrexia?—Over 103 or 105.

23,703. Hyper-pyrexia, as you well know, is a very serious condition. At what temperature do you term it hyper-pyrexia?—The general experience has been that anything over 103 is not what usually occurs after inoculation.

23,704. Is it an abnormal height? I should not term it hyper-pyrexia, because that is very serious. In how many cases did you have a temperature above 103?—Two.

23,705. In those two cases what was the maximum temperature?—105.

23,706. How long did it last?—About the usual time, two days. Of course, ordinarily, pyrexia is not termed hyper-pyrexia till it reaches 107.

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23,707. What other evil effects did you notice?—Severe pains in the muscles and joints in two cases, very severe pains, and they had to lay up for three or four days. There was severe purging in two cases; and at the time of the operation fainting has been observed more in men than in women.

23,708. Have you encountered plague in the same person more than once?—One case.

23,709. How often?—Two attacks.

23,710. Would you give us a few facts about that one case?—This case had come from an infected house in the town. This woman, a Brahman, and the wife of a local pleader, first came to the hospital from an infected house in the town. She then had a small bubo in the right femoral region. A relative of hers had died in the house, and another was brought into the hospital. All were twice inoculated.

23,711. Were these relations living in the same house?—Yes. Her symptoms were of the mildest, the fever having subsided before admission to hospital, and never rising above normal again, except the afternoon of the second day, and she left the hospital in five days, the bubo subsiding entirely under local treatment. She then continued well for eight days, when she was re-admitted to the hospital with a bubo in the right axilla.

23,712. Have you a clinical chart of this case?—Yes. I have the admission and re-admission chart. The original chart is not complete as the patient had some fever prior to admission. The highest temperature recorded each time was 103.

23,713. Did the patient remain in the hospital in the interval between these two attacks?—No, she did not; she went away to the fields.

23,714. How long was she in the fields?—It was only eight days.

23,715. Before she left she was continuously exposed to plague, I suppose?—She was in the hospital in a separate hut practically.

23,716. Does it occur to you this might be a relapse? You would not express an opinion either one way or another?—I would say it was a second attack.

23,717. A re-infection?—Yes, a re-infection from outside.

23,718. What is your idea of the duration of the immunity produced by inoculation?—With reference to those attacked, the duration of immunity is 50 days.

23,719. After inoculation you think they are no longer safe after the period of 50 days has expired?—Yes.

23,720. Why?—Because they have come into the hospital with plague after that time.

23,721. Can you give us some special case?—There were 31 cases that came to hospital between 10 and 50 days after inoculation; 50 between 51 and 60 days; 21 between 60 and 90 days; 5 between 91 and a 120 days; 2 between a 120 and a 150; and 1 between a 151 and 180. 180 days was the maximum. There were 10 after single inoculation, and 5 after double inoculation in the first 10 days.

23,722. You observe what this may show: that the immunity may only last for 10 days, and that those who acquired plague after 10 days, were not in a position to be infected till that time; the immunity in all these cases above 10 days being lost in the 10 days. If it shows anything it shows that 10 days is the period of preservation?—That is why I ask you to take those attacked and those not attacked. If you take 100 cases out of 10,000, there are 9,900 unattacked the whole six months and longer.

23,723. Then you cannot say anything as to the period of immunity?—I can say it is over six months in the vast majority of cases.

23,724. What proportion of the people stayed in Gadag Betigeri, and what proportion went away?—The total number that have stayed is about 12,500, in the two places combined, out of a population of 23,000.

23,725. What is the total number of cases, and the total number of deaths reported among those who remained?—About 280 deaths out of 400 cases.

23,726. Have you any idea of the number of inoculated persons out of the 12,500 who remained?—About 12,000.

23,727. Practically the whole of them?—Yes. The following table gives details:—

TABLE of ATTACKS and DEATHS, with the Population on the same Date at different Stages of the EPIDEMIC of PLAGUE, comparing the Figures for the two Divisions of the Town of GADAG-BETIGERI. The greater part of Betigeri being inoculated comparatively sooner than Gadag led to its being less completely emptied.

Month and Date.	Description of Figures.	Gadag.				Betigeri.				Totals of Both.				Remarks.
		Uninoculated.	Once.	Twice.	Total.	Uninoculated.	Once.	Twice.	Total.	Uninoculated.	Once.	Twice.	Grand Totals.	
		About	About	About	About	About	About	About	About	About	About *	About *	About	
To 15 Oct. 1898.	Population	10,900	1,785	315	13,000	12,600	350	50	13,000	23,500	2,135	365	26,000	Cannot be given exactly. * These figures were taken from inoculation registers. Some few of the numbers given may have left the town. The remaining figures are only approximate. The figures for Gadag and Betigeri separately are unrecorded; approximate ones only are given.
	Attacks	12	—	—	12	—	—	—	—	12	—	—	12	
	Deaths	7	—	—	7	—	—	—	—	7	—	—	7	
From 15 Oct. to 18 Nov. 1898.	Population	About 8,643	About 2,000	About 1,516	About 12,169	About 12,160	About 560	About 180	About 12,900	20,813	2,560	1,696	25,069	
	Attacks	36	2	0	38	0	0	0	0	36	2	0	38	
	Deaths	29	—	—	—	1	0	0	1	30	0	0	30	
From 18 Nov. to 16 Dec. 1898.	Population	2,640	1,812	3,887	8,339	9,132	1,796	1,299	12,227	11,772	3,608	5,186	20,566	
	Attacks	64	7	11	82	6	0	0	6	70	7	11	88	
	Deaths	46	2	0	48	6	0	0	6	52	2	0	54	
From 16 Dec. 1898 to 13 Jan. 1899.	Population	500	381	4,306	5,187	2,135	1,524	5,148	8,807	2,635	1,905	9,454	13,994	
	Attacks	79	7	29	115	48	9	8	65	127	16	37	180	
	Deaths	74	4	11	89	34	3	5	39	105	7	16	128	
From 13 Jan. to 11 Feb. 1899.	Population	287	245	4,723	5,255	436	464	6,271	7,171	723	709	10,994	12,426	
	Attacks	13	2	23	35	29	8	36	65	42	10	56	100	
	Deaths	10	1	4	15	31	4	15	50	41	5	30	76	

23,728. Did you do anything else besides inoculating the people?—We evacuated the houses where plague occurred, and the neighbouring houses.

23,729. What disinfection did you do?—We disinfected the place.

23,730. Any neighbouring places?—No.

23,731. How did you treat contacts?—Those who come from the infected house are the contacts; we cleared them into the contact camp.

23,732. Have you anything to say about the occurrence of plague in inoculated and uninoculated pregnant women?—I have had six cases. Of those four were inoculated and two uninoculated. The four inoculated all recovered, and the two uninoculated died.

23,733. What was the stage of the pregnancy in those cases?—The uninoculated were three and five months, and the inoculated were from seven to nine months. One of the inoculated went out of hospital without miscarrying. The others miscarried.

23,734. Is it a more serious matter for a woman to miscarry within three or four months, as contrasted with from six to eight months? Apart from plague altogether, which is the more serious?—The later period in pregnant women attacked with plague, the earlier in non-plague cases.

23,735. (*Mr. Hewett.*) Did you keep a record of the average temperature of the 7,300 persons whom you inoculated?—No.

23,736. With regard to the three persons infected while attending on friends in hospital, can you give in each case how many days after arrival in hospital they were infected?—In one case I remember clearly it was the fourth day, another case took ill the next day, and another the same day.

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23,757. Did any of the cases occur under such circumstances that they could not have brought the infection from outside?—To my mind they must have all brought it from outside.

23,738. On account of the number of days within which they caught it?—Yes.

23,739. Have you had any experience of the kiln process of disinfection?—Yes.

23,740. Where did you try it?—In the village of Lingdhal, and very nearly the same method in Karachi.

23,741. How many houses did you try it in?—It was tried in almost every house that was disinfected in Lingdhal, and I disinfected 228 houses (some of which were large compounds, and large houses containing from 10 to 40 rooms each) at Karachi.

23,742. Did the residents of the houses do it themselves, or was there some establishment for disinfecting?—The supervisor that went out there did it at Lingdhal, and the houses at Karachi were done under my personal supervision.

23,743. After it had been done, did any case of plague occur in any house which had been disinfected in this manner?—As the people were all in the fields at Lingdhal it has not been tried there, and as I was transferred from Karachi immediately after the disinfection was finished, I have no idea how the present epidemic is affecting the houses disinfected under my supervision.

23,744. Then you have had no idea of its nature?—No.

(Witness withdrew.)

Major T. H. Hardy, I.S.C. recalled and further examined.

Major T. H. Hardy, I.S.C.

23,745. (*The President.*) You have been on plague work at Dharwar, and you have been before us on a previous occasion?—Yes, I have been examined before.

23,746. I believe you wish to say something in addition to what you have already told us?—I said I could talk about the measures adopted for the re-admission of the inhabitants of Dharwar who were out in the huts. Dharwar was partially evacuated, and about 18,000 people went away in one week at the commencement of October. They feared segregation and inoculation, and gradually by the end of November 1898, the population was reduced to 9,800 odd. They were allowed to come back provided they were inoculated.

23,747. Was that the only requirement?—That was the only requirement at first. They gradually came back at the rate of about 500 or 600 a week, provided they were doubly inoculated. A notification to that effect was posted on every door, and if any persons went into their houses without this requirement being fulfilled they were prosecuted. I had to prosecute one person, and it resulted in a conviction. Each man who came back was given a pass (produced) signed by me; and the number of persons inhabiting the house was entered on it. I have a book containing a list of all vacated houses, and I insert how long they were vacated, how long unroofed, and the date upon which the house was re-occupied, together with any other remarks, such as disinfection, &c.

23,748. What was the minimun period that a house had remained unoccupied before these people came back?—The minimum was about a month. In the interval the house had been unroofed. At first, up to the middle of December, if a house had been unroofed and vacated for a month the people were allowed to come in without disinfection, but after that every empty house, whether unroofed or not, was disinfected with percholride of mercury.

23,749. Therefore also the majority were disinfected with chemical substances?—Yes.

23,750. I think you wish to tell us the general result of this re-occupation?—The general result of re-occupation is that there have been only two cases of plague as the people came back. These were traced to importation, one from Gadag, and the other from Navalur, a village 3 miles distant.

23,751. Previously infected houses, therefore, no longer conveyed infection to inoculated persons?—They have not done so, so far.

23,752. But these houses were probably themselves no longer capable of giving infection?—I do not think so.

23,753. What have you to say about the measures adopted after the abolition of inoculation?—The people petitioned with regard to the loss of trade, so the Collector asked me to frame rules for the re-admission of the people from the huts around. I had a census taken of all the people as far as I could get them. It took from the middle of December until the end of January to get. 8,148 people were found in the huts. I went round myself occasionally with the Hospital Assistant to find out where the plague was. We found out one or two cases, and we burned their huts. Latterly, however, there has been no plague there, and the people were allowed to come in provided they were disinfected in their huts.

23,754. They were allowed to return to their houses in Dharwar?—Yes.

23,755. Was the result different?—They were uninoculated. They did not suffer. Nothing has happened.

23,756. It shows nothing either for or against inoculation?—It shows that evacuation has done good in that case.

23,757. Have you anything additional to say about the disinfection of houses which has not already been described?—No, I have nothing more to say. We have rules now about the people coming in. Of course we can only account so far for about half the original population at Dharwar; about 19,000 people are unaccounted for. I have here the last census (produced) showing the state of Dharwar town.

23,758. The population is only about 20,000 at present?—Yes.

23,759. As contrasted with a normal population of what?—In June last the population was over 38,000.

23,760. (*Mr. Hewett.*) Where do you think that the remaining 19,000 persons have gone to?—They have scattered over the country; they are not in huts round Dharwar. I think a great many of them have died. In one set of huts I visited about a fortnight ago, on inquiries from the people I found that out of an area of

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about four acres there were from 40 to 50 huts, and I was informed that in every hut there had been cases of plague, with one or two deaths at least in each.

23,761. These were the huts of persons who had segregated themselves voluntarily?—Those who had voluntarily evacuated.

23,762. At what distance from Dharwar town?—These huts were about a mile from Dharwar, on the main road between Hubli and Kalghati.

23,763. At the present moment, could these 19,000 people come back without being inoculated?—That has been stopped. I have the orders here (produced) which are at present in force.

23,764. For how long have they been in force?—It has not yet been sent round the districts: I did not like to circulate it until I had the people in from the huts around.

23,765. Upon what terms are you going to re-admit the people?—I have had a notification published by the Collector of all the plague-infected places, and we have a system of guarantors for arrivals coming into force. Uninoculated persons coming from an infected district who cannot get a guarantor will be sent to the health camp. Those who are inoculated will be allowed to go at once.

23,766. Whether a person comes from an uninfected place or from an infected place, he will be allowed to occupy his house if he has a guarantor?—Yes. But his clothing is disinfected, should it be dirty, or if the volunteer at the railway station thinks he is likely to carry infection, his clothing is disinfected.

23,767. In your opinion what proportion of the Dharwar population that left the town occupied huts in the neighbourhood of the town?—One-third.

23,768. How many people would that leave to scatter themselves about the country?—About 19,000. I do not know how many of them are dead.

23,769. Do you consider that those 19,000 persons did scatter plague throughout the Dharwar district?—Yes, certainly. I do not say that all the 19,000 persons scattered the disease, but some of them must have done so.

23,770. In all probability they were very badly infected?—Yes; I know that a great many infected people went to the villages round about, and did scatter plague.

23,771. Do you keep the keys of the locks on the vacated houses?—No; we paste up a notice that the people are not to go into a particular house. We paste the notice over the lintel of the doorway, so that it cannot be opened without breaking the notification. The case I brought before the Magistrate had a very salutary effect, the people come up readily and asked for re-admission.

23,772. Do you now think, at Dharwar, that, provided that a house has been properly disinfected, and that the person returning to it is not likely to be himself incubating plague, or to be carrying the infection in, it is quite safe for him to come back to his house?—I think the results show that.

23,773. That is to say the authorities at Dharwar now attach more importance to disinfection than they did?—Yes; I think they do. It has been disinfection combined with evacuation of the houses. All the houses have been unroofed as well.

23,774. Your treatment of the houses, whether by unroofing them, or by opening them out and disinfecting them, has, in your opinion, destroyed the local contagion?—Undoubtedly; I do not say whether it is due to disinfection or the emptying up of the houses. The combination of the two has done it, I should think.

23,775. (*Mr. Cumine.*) To what extent have the people been now converted to believe in disinfection?—I do not know about belief in disinfection.

23,776. I am referring to chemical disinfection?—My own opinion is that the people had a lot of trouble owing to being kept out of their houses. When they heard they could come back without inoculation provided their houses were disinfected, they would have all come back in one day if they could. According to the rule, however, they were not allowed to come back until their houses were disinfected. I had any number of petitions from people asking me to disinfect their houses. I do not know whether they believe in disinfection or not. I should think it is probably the other way, but that they accepted disinfection as a means of re-admission to their houses.

23,777. (*The President.*) Have you had any experience of disinfection without chemical disinfectants, such as opening of houses?—Yes.

23,778. Can you give the result with regard to the destruction of the virus?—In Kelgeri, about 3 miles away from Dharwar, I had all the houses opened, the roofs taken off, and the doors taken off. Besides that I have had all the ground in the rooms dug up to the depth of about 3 inches and thrown out into the sun. The people themselves willingly came forward and lime-washed their houses. They have all gone back now, with no return of plague.

23,779. How many people or houses does that instance deal with?—One village of about 1,000 inhabitants.

23,780. You did not use mercuric chloride?—No; nothing.

23,781. Are these inoculated persons?—There were eight families incoculated in the village, and they have been occupying their houses all the time.

23,782. The majority were uninoculated?—Yes.

23,783. In the case of Dharwar, had you, before this large exodus took place, made any provision for the people who wished to leave the town?—There was only one health camp in the town capable of holding 1,000 people.

23,784. Therefore, of course there was no actual measure in operation which would prevent their scattering themselves?—The only measure was that the Goverment of India allowed them to get bamboos at about a quarter price to build huts with outside the town. There was a depôt at the Dharwar station for that purpose.

(Witness withdrew.)

Major R. H. Forman, R.A.M.C.

Major R. H. FORMAN, R.A.M.C., called and examined.

23,785. (*The President.*) I believe you are in the Royal Army Medical Corps?—Yes.

23,786. I understand that you have volunteered to bring before us some cases in which inoculation appears to have produced good effects?—Yes.

23,787. Will you kindly state what you wish to say?—There were two groups of cases that I inoculated when plague first broke out in Belgaum with whom I have been able to keep in touch since. The two groups were amongst my own private servants in my own bungalow, and my hospital attendants with their wives and children in the Station Hospital at Belgaum. Of my private servants there were in all, including their wives and children, 28 people inoculated. There have been no cases of plague, and no deaths up to date. There were three uninoculated. One was a child of nine years of age, whose father refused to allow it to be inoculated. It died of plague 12 days after the other people were inoculated. I diagnosed the case myself as plague.

23,788. How long after?—The child got plague and died 12 days after the servants were inoculated. They were inoculated in December 1897.

23,789. The child was not ill at the time?—No, it was perfectly well. The other two cases that were not inoculated were not so distinctly under my own observation. One was a sweeper employed in the Cantonment as a Municipal sweeper, the husband of my sweeper woman, and sleeping in my compound. He, I am told, died of plague some months afterwards. He was not inoculated. The other was my bhisti. He threw himself into a well. I was informed that he had buboes and fever, and ran away to escape segregation. Of the 28 inoculated none died of plague, and of three uninoculated two are said to have died of plague, and one undoubtedly died of plague.

23,790. Were these people living in the compound?—Yes, except the bhisti.

23,791. How was the infection conveyed to the people; how did they become infected?—I do not

know. Plague was raging in the bazar close by. The bungalow was within a quarter of a mile of the Sadr Bazar.

23,792. Were there any rats?—There were rats about the bungalow, but I did not see any dead rats. All around the bungalow there were cases of plague and deaths.

23,793. Where were the servants living?—In my compound. I disinfected the houses myself.

23,794. When; at the time of inoculation, or before or after?—I do not know whether it was before or after. On further consideration, I am sure the houses were disinfected both before and after.

23,795. So that in addition to inoculation there was thorough disinfection?—Yes, they were all slushed down with perchloride of mercury, and the roof taken off.

23,796. Presumably the originally infective cause had been removed by disinfection?—Possibly so. I may mention that amongst my servants I found there was one woman who was inoculated who had died in a village, but it was said not to be of plague. I know that the woman many months before was the subject of malarial fever. She had an enlarged spleen and an enlarged liver. I know that from my own observation. I hesitated whether I should inoculate her at the time I think the statement that she did not have plague is true.

23,797. What other instances have you?—My hospital servants. These are under my own supervision. There were altogether 87 inoculated, of which 79 were continually resident in the place. Eight were there during the day only, and went home at night.

23,798. What was the total number of hospital attendants?—Ninety. Eighty-seven of these were inoculated. Since then of the inoculated (87) there has been one death, a daughter of one of the men, but she did not die of plague. She died of rheumatic fever and endocarditis. Of the three who were not inoculated, two were not inoculated because they were pregnant, at least, one had recently been delivered, and the other was pregnant. The third was a boy sixteen years of age, whose father refused to let him be inoculated. One of the uninoculated women has not had plague, she is alive now. The boy whose father refused to let him be inoculated died of plague on the 22nd February 1898, less than two months after the other people had been inoculated. The other woman died on the 24th of February, 1898. She also died of plague. She had been in attendance upon the boy.

23,799. Therefore two of these three died?—Yes. There were only three uninoculated, and two of them died. The houses were then evacuated for ten days, and disinfected. The people went back to them, and there has been no more plague in them since.

23,800. These houses, however, were occupied by 87 inoculated and three uninoculated people?—No. Eight of the people that were inoculated did not live in the houses. Seventy-nine of the inoculated, and the three uninoculated were always resident in the place.

23,801. When did these plague cases occur, nearly at the same time, or at different times?—One death took place on the 22nd of February 1898, and the other on the 24th of February 1898, two months after the other people were inoculated. The inoculation date was the 28th and 29th of December 1897.

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23,802. (*Mr. Hewett.*) Referring to the first group (28 cases), did the disinfection of your vacant houses take place before the child that died of plague took plague?—Yes, they had been disinfected before, several times, I think, but once, I am certain.

23,803. How many other residents were there in the particular house in which the child died of plague?—Three, father, mother, and the child. The child died in the segregation camp. The father and mother were allowed to go with it, but returned immediately after its death, and lived among the rest of the servants.

23,804. Among the people that you inoculated were there any persons over the age of 50 or 60?—Yes, two. One in the 28 group and one in the 87 group. The former was the father of my cook, an old crippled man; he is alive and well now. The latter was the mother of one of my hospital attendants. She left eight months ago, and died in Madras some six months ago. The cause of death is unknown to me, but her son states it was not from plague. I understand there was no plague in Madras six or eight months ago.

23,805. In the group of 28, how many under the age of three did you inoculate?—There was one of two years of age, and two of four years of age. Of course the ages are approximate.

23,806. There were three under five?—There were three under five years of age.

23,807. The old woman was the only person among the inoculated in the group of 87 who has died?—That is the only death among the inoculated.

23,808. How many of the inoculated persons were under five?—I have not got the ages; I do not know, six at least.

23,809. Were any rejected because they were too old or too young?—No, none were rejected. We gave graduated doses according to age, sex, &c. My observations are summarised as follows:—

1st Group.—28 inoculated; 3 not inoculated.

Of the inoculated, one woman died in her own village several months afterwards. The cause of death is unknown, and said not to be plague, and the woman was a confirmed invalid. The others all well now, and have not suffered from plague.

Of the uninoculated, all died, one 12 days afterwards of undoubted plague, the other two of what was said to be plague. One of the latter did not live in the compound.

2nd Group.—87 inoculated; 3 uninoculated.

Of the inoculated, one died some months thereafter of rheumatic fever and endocarditis, and one woman died in Madras six months ago, but not of plague. The others are all well, and have not suffered from plague.

Of the uninoculated, two died within two months, both being undoubted cases of plague.

Thus in a total of 115 inoculated cases, there were no attacks, and in six uninoculated cases there were five attacks and five deaths, though two of them were only hearsay cases of plague.

(Witness withdrew.)

Miss A. M. Corthorn, M.B., re-called and further examined.

Miss A. M. Corthorn, M.B.

23,810. (*The President.*) You are prepared to give us some further information as to the results of 37,000 inoculations which you have done?—Yes, they are the inoculations of Gadag. Up to the 18th February there were 20,859 people inoculated, of whom 17,997 were inoculated for the second time; that gives a total number of inoculations of 38,856. It is a little over 39,000 now.

23,811. Have you come to any conclusion with reference to these inoculated persons as to the interval which occurs between inoculation and plague?—I have worked that out for the Dharwar results, but not completely for the Gadag results; that is in process of construction.

23,812. Have you any information with regard to the dosage as influencing immunity?—I always give a larger dose than the prescribed dose, one-fifth more. When I came away from Dharwar there were 4,003 people who had been inoculated at the Civil Hospital, and 8,552 whom I had inoculated. I had inoculated more than twice as many as were inoculated at the Civil Hospital; 96 of the cases which I inoculated developed plague. A good many of those really came out because they fell within the incubation time, taking that as only 48 hours. Of the 4,003 who were inoculated at the Civil Hospital 78 got plague. Admitting all the cases, there were 96 cases amongst my 8,552 and 78 among the 4,003. In Gadag, out of the

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38,000 inoculations, I have done, roughly speaking, three-fourths, and Dr. Foy the remainder; 86 cases, so far, of the people whom I have inoculated have taken plague, and 66 of those whom Dr. Foy has inoculated. The only difference has been that I give larger doses than the other people who have been working with me. The plague incidence has been considerably smaller among the people I have inoculated than amongst those who have received the smaller doses.

23,813. Is there any relationship you wish to draw attention to between dosage and sex?—Women bear quite as strong doses as men; in fact, men react rather more than women do, that is, the higher temperatures have been observed in men.

23,814. I understand your practice is to make no difference between the two?—That is so.

23,815. Now, with regard to age?—Children get much larger doses in proportion to adults. I gave one-fifth of the dose to children under two, and got no reaction at all, and I found the same thing happened over and over again. I have no figures, but that is my general impression. When the child is brought for a second inoculation I say, "Was it bad the last time?" and the parents say, "No, not at all, he did not seem to feel it." So I have got into the way of giving a child of one year old 1 c.c. of the 2½ c.c. prophylactic, that is two-fifths of the standard dose, and a child of 12 has the standard dose.

23,816. Have you seen any case where the temperature did not rise?—Yes. I have not taken the temperatures in Gadag, but you cannot take temperatures when you are doing nearly 1,000 inoculations a day. The temperatures were only taken of those few cases at Hubli, regarding which I have submitted notes. (*See* Question No. 1,778.)

23,817. There is an enormous number of other cases where the temperature could not be taken?—Yes. I can only state the general impression that I have received by asking people at the time if they were bad at the first inoculation. I have made a practice of that. I am also working out a table, which is not yet complete, in regard to the interval between the first and second inoculation. I think that bears somewhat on the severity of the first reaction. I take it that a person who has had a very bad reaction will not be in a hurry to present himself for a second inoculation.

23,818. You have met with instances where there was no particular rise of temperature?—Yes.

23,819. Do you know whether any of those persons became infected?—There were no cases of plague among those people, but they were inoculated when plague was going down. They were inoculated in the Southern Mahratta Railway Lines in Hubli, and although there had been cases there before there were not cases after that time amongst the uninoculated. There were only a few uninoculated, but the plague had gone down very much. Therefore, the inference that those people who did not develop a temperature were still protected cannot be drawn.

23,820. In the case of second inoculations, did you have the same temperature reaction or a different one?—The general result was that the temperature was about the same. In a few cases I thought it was higher, that those who got it lower first got it higher afterwards; but when I worked out the figures it seemed about the same. Having a slight reaction the first time did not mean that they would have a big reaction the second time, and if they had a big reaction in the first place it did not follow that they would have a weak reaction in the second.

23,821. You did not have, in the second inoculation, cases where there was almost no rise?—No, not one.

23,822. It was exceptional to get only a low temperature?—Yes.

23,823. Were those people who were inoculated twice and who did not have a large rise of temperature equally protected?—I cannot say. I think the time at which the inoculations were done prevents one drawing any inference. I know they did not get plague, because I was able to follow them up; but plague was going down in that particular quarter of the town, and the inoculation may have had nothing to do with protecting them.

23,824. Have you done any third inoculations?—I have done some.

23,825. What was the temperature?—I have not been able to follow that up. They are people who come up to me in crowds. A good many people have asked to be inoculated a third time.

23,826. What is your opinion of the relative value of single and double inoculations?—In Dharwar it worked out largely in favour of double inoculations. We had a difference of 30 per cent. in the reduction of morbidity for the doubly inoculated, and about 18 per cent. in the reduction of mortality.

23,827. Have you got any further evidence on that point? Your case remains as it was before?—That is so. In Gadag, at present, it is not working out so largely in favour of the doubly inoculated as it did in Dharwar. The number of once inoculated present at any time as compared to the twice inoculated is so small that the comparison is a less valuable one than if they were present in about equal proportions. These charts* illustrate that, perhaps; they show the relative proportions of the three classes resident in the town, and the plague incidence among them.

23,828. You mean the three classes of once inoculated, twice inoculated, and uninoculated?—Yes. The black line represents the uninoculated, the blue line the once inoculated, and the red line the twice inoculated. The uprights give the proportion of cases in the corresponding column.

23,829. You have formed no opinion of the relation between the degree of temperature rise and the efficacy of the inoculation?—I have no figures on the point, but I have seen nothing to lead me to think those who have not reacted to the prophylactic are more apt to take the disease.

23,830. You are not in a position to say that, even independently of temperature rise, the efficacy may be the same, being rather the result of the dose, or the quantity, than of so-called reaction?—No.

23,831. (*Mr. Hewett.*) With reference to the statement prepared from the weekly reports which you have put in in your previous evidence, can you give us figures for Gadag which correspond with those given for Dharwar?—Yes; I have worked them out rather on different lines. I have tables prepared, as follows:—

STATEMENT from the WEEKLY CENSUS of the NUMBERS of UNINOCULATED, ONCE INOCULATED, and TWICE INOCULATED present in GADAG TOWN.

Week ending	Uninoculated.	Once Inoculated.	Twice Inoculated.	Total Population.
Nov. 18, 1898	20,813	2,580	1,696	25,069
„ 25, „	17,659 (75·5 %)	2,916 (12·9 %)	2,799 (11·9 %)	23,374
Dec. 2, „	15,473 (68·4 %)	3,459 (15·3 %)	3,660 (16·2 %)	22,592
„ 9, „	13,835 (63·9 %)	3,546 (16·3 %)	4,275 (19·7 %)	21,656
„ 16, „	11,772 (57·7 %)	3,608 (17·5 %)	5,186 (25·2 %)	20,566
„ 23, „	8,726 (45·66 %)	3,676 (18·5 %)	6,706 (35·84 %)	19,108
„ 30, „	6,145 (37·27 %)	3,168 (19·2 %)	7,173 (45·53 %)	16,486
Jan. 6, 1899	3,381 (24·7 %)	2,360 (16·27 %)	8,757 (59·63 %)	14,498
„ 13, „	2,635 (18·83 %)	1,905 (13·68 %)	9,454 (67·49 %)	13,994
„ 20, „	1,766 (13·13 %)	1,292 (9·6 %)	10,390 (77·27 %)	13,448
„ 27, „	1,135 (8·61 %)	1,071 (8·12 %)	10,977 (83·27 %)	13,183
Feb. 3, „	907 (7 %)	774 (6·5 %)	11,149 (86·5 %)	12,830
„ 10, „	723 (5·86 %)	709 (5·62 %)	10,994 (88·52 %)	12,426
„ 17, „	732 (5·97 %)	585 (4·77 %)	10,940 (89·26 %)	12,257

* Not printed with the Proceedings of the Commission.

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STATEMENT from WEEKLY CENSUS of PLAGUE INCIDENCE among the UNINOCULATED, ONCE INOCULATED, and TWICE INOCULATED in GADAG TOWN.

	Uninoculated.		Once Inoculated.		Twice Inoculated.		Total.	
	Cases.	Deaths.	Cases.	Deaths.	Cases.	Deaths.	Cases.	Deaths.
Previously reported	4	4	0	0	0	0	4	4
Week ending—								
Oct. 14, 1898	1	1	0	0	0	0	1	1
„ 21, „	3	0	0	0	0	0	3	0
„ 28, „	4	3	0	0	0	0	4	3
Nov. 4, „	3	1	0	0	0	0	3	1
„ 11, „	7	2	0	0	0	0	7	2
„ 18, „	7	5	0	0	0	0	7	5
„ 25, „	11	9	0	0	0	0	11	9
Dec. 2, „	14 (87·5 %)	13 (92·85 %)	2 (12·5 %)	1 (7·14 %)	0	0	16	14
„ 9, „	14 (93·37 %)	7 (87·5 %)	1 (6·6 %)	1 (12·5 %)	0	0	15	8
„ 16, „	28 (87·5 %)	14 (73·3 %)	3 (9·37 %)	1 (6·6 %)	1 (3·13 %)	0	32	15
„ 23, „	22 (84·6 %)	15 (83·3 %)	2 (7·69 %)	1 (5·5 %)	2 (7·69 %)	2 (11 %)	26	18
„ 30, „	36 (72 %)	27 (79·4 %)	6 (12 %)	3 (8·82 %)	8 (16 %)	4 (11·76 %)	50	34
Jan. 6, 1899	40 (71·42 %)	35 (85·38 %)	3 (5·36 %)	1 (2·44 %)	13 (23·22 %)	5 (12·2 %)	56	41
„ 13, „	28 (66·6 %)	23 (96·6 %)	4 (9·52 %)	2 (6·6 %)	1 (23·8 %)	5 (16·6 %)	42	30
„ 20, „	9 (39·13 %)	10 (58·8 %)	3 (13 %)	2 (11·7 %)	11 (43·47 %)	5 (39·4 %)	23	17
„ 27, „	14 (51·99 %)	13 (68·4 %)	3 (11·1 %)	2 (10·52 %)	10 (37 %)	4 (21 %)	27	19
Feb. 3, „	12 (38·7 %)	8 (44·4 %)	1 (3·22 %)	0	18 (58 %)	7 (46·6 %)	31	15
„ 10, „	9 (29 %)	8 (53·3 %)	0	0	22 (70·97 %)	7 (46·6 %)	31	15
„ 17, „	3 (12 %)	6 (42·85 %)	1 (4 %)	0	21 (84 %)	8 (57·14 %)	25	14

23,832. Was a census taken of Gadag, or have you taken the assumed population from the census of 1891?—A special census was taken in November or December 1897.

23,833. Who took the census, a European or a native?—It was taken under the direct supervision of Mr. Vincent, who was in close personal touch with almost all the people in the town. He threw himself very heartily into the work. He was the Chief Plague Superintendent, and he superintended the whole of the census; he did not go round to the different houses, but he did really know the people in Gadag and Betigeri.

23,834. The population was found to be 25,069?—Yes.

23,835. Subsequently the weekly population recorded in this statement declines week by week; can you tell us how it has been calculated?—Yes. Every Saturday morning the Superintendents of the districts went round. They kept the books of the number of people in each house, and they went to each house and saw how many of those people were present in the town at that time, and examined their inoculation checks, or passes, and only those were put down as doubly inoculated who showed a double inoculation certificate; and only those were put down as singly inoculated who showed a single inoculation check. That they did look at these certificates is indirectly proved in this way, that after the weekly census was taken people came up to have any slight errors in their certificates, which they themselves had not noticed, rectified. There is always a difficulty in getting a native woman to mention her husband's name. That is a very great difficulty in registration, that the wrong husband's name is registered. Each week these mistakes would be discovered by the Supervisor in going round, and the people would come up to me in the afternoon to get those errors rectified. The division into those three classes is based only upon that fact. I have never worked out any results upon the register as a whole, but on the actual numbers that were stated in this weekly census to be present in the town.

23,836. Previously by the native Superintendents?—Yes, and submitted to the Plague Superintendent.

23,837. With regard to the deaths among the uninoculated, and the once and twice inoculated, who decided in each instance the class in which the deceased person should be recorded?—The doctor in charge of the hospital, Dr. Foy. And I, myself, went to see all these cases in hospital. I went every day to look at all the cases that came in, and cross-questioned them as to whether they had been inoculated or not inoculated.

23,838. Whom did you cross-examine?—The patients, and the attendants who came with them.

23,839. They had one attendant as a rule?—Yes. Then if one could not get the information, one would go to the Supervisors of the district, and find out from them how they were entered in the book. The Supervisors get to know the people pretty well, because they go round every week and examine the checks and certificates in this way.

23,840. Were the people's addresses, as well as their names, entered in your register?—Yes.

23,841. And you have no doubt the registers are accurately prepared?—I am sure, every now and then, a slight error did come in. We had the errors of the husbands' names, for instance.

23,842. Who supervised the clerk who made the record?—I watched him the whole time myself; he was my clerk.

23,843. You were doing a lot of inoculation at the time, was it not rather difficult to combine those two duties?—Yes; it rather tired me when I did it. I had a man standing at one door to see that nobody went out without being registered. I stood at one table, and had a clerk by me who gave what I call my inoculation check to the persons inoculated. He took the fee and gave the check, which had the serial number on it. The patient who had just been inoculated walked up to the other table which was near to me, where the registering clerk sat, and his name was registered, and the name and the date written on the check. If a person did not come up with the right number he was at once stopped, because the clerk had missed one number out. I kept well on the alert, and was looking at the book every few minutes to see if he was ticking the people off in the order in which they came.

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23,844. Was the same care taken in registering people who were inoculated at Dharwar?—Yes.

23,845. Were you present when Capt. Leumann made his inoculations?—He did no inoculations there. At Dharwar I took exactly the same precautions; it was at Dharwar that I instituted that system.

23,846. Did you have the same system from the beginning of your work at Dharwar?—Yes, exactly.

23,847. You did a certain number of inoculations at Hubli?—Yes.

23,848. Were the Hubli registers as accurate as those at Gadag?—I had nothing to do with the registers at Hubli, but I do not think they were done so well. It was because of those shortcomings that I adopted my procedure at Dharwar.

23,849. Did you observe the inoculations carried out at Hubli by other persons?—Yes.

23,850. Did you observe Capt. Leumann actually inoculating?—Yes, I helped him.

23,851. How many persons were inoculated in the course of a day?—I could not tell you. I believe he did 700 in one day at Hubli.

23,852. At the time he was inoculating at that rate, were the registers accurately kept?—I could not tell you; I was not with him when he did those inoculations. I thought his system did allow room for some mistakes occurring, and that is why I adopted my system. I adopted some of his methods, but by always keeping the number of the person inoculated in my own hands, I think I sufficiently kept the person in touch with the certificate. A person who was inoculated twice got his certificate at the end. When he came up to me for the second time for inoculation, he presented his inoculation check, and the clerk who was by me asked him his name; he did not read out the name on the check, but the person had to give his name, and if the name did not correspond with the name on the ticket he was told he had brought the wrong ticket, and was sent back to get his own ticket.

23,853. (*Mr. Cumine.*) Why was the census taken in November or December 1897?—Because plague was expected.

23,854. Who is it that goes round every Saturday with the census book in his hand to inspect the people?—The supervisors and their clerks.

23,855. Do you mean a supervisor goes with a clerk, or do they go independently?—No; one clerk goes with each supervisor.

23,856. How many supervisors are there?—I cannot tell you; I have not done that part of the work at present. I forget the exact number of wards into which the town is divided. It is divided into six sub-divisions, but I am not sure what the number of wards is. I think each sub-division contains something like five or six wards, which would make about 24 wards altogether.

23,857. Would there be 24 supervisors, probably?—Yes, each supervisor has an average of 150 houses.

23,858. Assuming there are 25 supervisors, and a population of 25,000 people, each supervisor would have to see 1,000 people each Saturday?—That would have been so at the early part. Since a weekly census has been taken, we have never had 23,000 people there; it was 20,000, and on November the 18th, the first time the sub-division was made into three classes, it has been rapidly declining until now it is only 12,000.

23,859. Did any European officer test the work done by these supervisors on Saturday?—Yes. Mr. Vincent himself had to go round one week to one ward, and another week to another ward. He was generally out in that part of the town in which they were taking the census.

23,860. Do you mean taking the original census?—No, the weekly census. I do not say he went down with each one in particular, but he was generally supervising and seeing that the work was done correctly; they were done under his personal supervision the whole time.

23,861. Do you know whether he was testing it in the sense of taking the census register in his hand and making the people stand up in front of him?—I never saw Mr. Vincent do that, but I am quite sure, from what I know of his work, that he would have done it at several houses. He would not go to every house and do it, but that would be his way of testing it. He would go into certain houses and call the people out, and see whether the work had been done correctly in that house.

23,862. That is what you think he would have done?—Yes.

23,863. With regard to those people who died in hospital, you said that Dr. Foy was the authority to determine whether they should be registered as inoculated or uninoculated. Who was the authority that decided this with regard to the people who died in the town without going to hospital?—Dr. Foy, if he was called in; otherwise, the Hospital Assistant, whose work it was to see those cases. He saw the cases and sent in the returns, and when he was in any difficulty he referred to Dr. Foy as the Chief Medical Officer there. It was not a very difficult question to decide, because it was a question of getting a certificate from them. They were not to allow any uninoculated person to pass, because the information was not only wanted for my returns, but to decide whether the people were to go into contact camp or not. Directly a case occurred, there was an overhauling of the certificates by the officer who found the case, in order to decide which of the people were to be sent to the contact camp, because the doubly-inoculated people were not sent to contact camp.

23,864. Were the once-inoculated people sent to contact camp?—Yes, but the people were very anxious to bring up their certificates, and, therefore, no case would be likely to be missed.

23,865. I see that it was to the advantage of the twice-inoculated people to produce their certificates. But if the once-inoculated people were sent to a contact camp, what motive was there for them to produce theirs?—They do not hide certificates. Imitation has so very much to do with determining what a human being will do. When some brought out their double certificates, the others brought out their single certificates. The whole of the certificates are kept together, whether single or double, and the head of the house, when questioned by the supervisor, brings you out a roll of certificates—single and double certificates together.

23,866. How is this within your knowledge?—Because they are brought to me again and again. I have been into the houses to ask the people whether they are inoculated, and they produce these rolls of certificates in the way I have mentioned. If you go round to any town where great stress is laid on inoculation, you will find that is what they do. These people keep their certificates rolled up or mounted on bits of cardboard, and they are all tied up in a handkerchief, and the head of the family keeps them.

23,867. Have you been in the house of a once-inoculated person, after he had died, and seen the way in which the certificates are kept?—Yes.

23,868. Have you been in many?—Not many, because it was not my work; but I know the habits of the people. It is their habit to keep their certificates in that way.

23,869. I do not think you told us what the motive was for the friends of a deceased once-inoculated person to produce his certificate on the day of his death. What advantage is it to them?—One thing is this. They say, "you compel us to be inoculated, and here "is an inoculated person who has died."

23,870. What advantage is it to them?—There is no advantage, but it is a human propensity. They think that you have told them that a certain result will not follow a certain course, and when the result happens they bring you the evidence to show how it has happened. I think that is a very natural human motive.

23,871. Have people not to pay a fee for getting themselves inoculated?—Yes.

23,872. Therefore is there not a motive on the part of the brother of a deceased once-inoculated man to retain the deceased's certificate in order that he (the brother) may not have to pay a fee for getting himself inoculated?—Yes, but that is where the system of registration picks those people out. These people find every time they bring up a wrong ticket that they are sent back, and they find it is no use bringing up

another person's certificate. That is what Captain Brownrigg, who has lately been appointed our Chief Plague Superintendent, has remarked. He said that in Hubli he did find people presenting other people's certificates, but never in Gadag. Where you have a system of registration, the people are being continually reminded that they cannot cheat, or that if they do they will be found out. When their certificate is presented they are asked their name. They do not know the name on the certificate because very few of them can read, and they give their own name.

Miss A. M. Corthorn, M.B.
3 March 1899.

23,873. If A's brother has been inoculated, the name of A's brother will be on the certificate. Does not A know his own brother's name?—Yes, but he has to report his brother's name as the person who has died. The name of every member of the family is down in the supervisor's book. Therefore as A would have to give the name of his brother, who had died, as being inoculated, he cannot choose that name for himself.

23,874. Will you tell us whether your register of inoculated persons is in Mahratti or in English?—It is in English.

(Witness withdrew.)

(Adjourned till Monday, 6th March, at Sholapur.)

At The Railway Station, Sholapur.

SIXTY-FIRST DAY.

Monday, 6th March 1899.

PRESENT:

PROF. T. R. FRASER, M.D., LL.D., F.R.S. (*President*).

Mr. J. P. HEWETT. | Mr. A. CUMINE.

Mr. C. J. HALLIFAX (*Secretary*).

Mr. W. M. MUAT, M.B., called and examined.

Mr. W. M. Muat, M.B.
6 March 1899.

23,875. (*The President.*) You are a Bachelor of Medicine?—Yes, and Master of Surgery, Glasgow.

23,876. You are in an official position here?—Yes, temporarily on plague duty; special work.

23,877. You have been inspecting health camps?—Yes.

23,878. And also supervising contacts?—Yes.

23,879. Since the re-occupation of the city of Sholapur in April 1898 you have had some experience as regards the inspection of corpses, I think?—I have all the time up to the present; it is still going on.

23,880. Have you encountered any difficulties?—I do not think I have ever really; perhaps once or twice they may have said some slight thing, but I have had no serious difficulty at all.

23,881. To what extent do you inspect corpses?—Every corpse; every person who dies in the city.

23,882. Male and female corpses?—Yes, Muhammadans and Hindus alike.

23,883. Are there no pardah nashin women here?—I have not had any trouble at all. I have seen every corpse that has died for quite a year, I should think, in the city.

23,884. And no objection was raised at all?—No serious objection. There was only one case, and it was not a serious objection at all. They simply said that they wanted a female doctor.

23,885. What does your examination of the corpse consist of?—I examine the eyes and tongue, the situation of the lymphatic glands and the general appearance of the face.

23,886. You examine the groins, for example?—Yes, the groins and the axillæ.

23,887. Have you been here all the time of plague at Sholapur?—I came here in the first or second week of December 1897.

23,888. When did plague commence in Sholapur?—I came here just at the height of the epidemic. The epidemic started on the 23rd of September 1897.

23,889. Did you find when you came that the measures to eradicate the plague were already in operation?—Yes, they were.

23,890. What were the measures?—The removal of plague cases to the Plague Hospital and the segregation of the families in which the cases had occurred; then there was evacuation of the city; and then, after the city was evacuated, the houses were all disinfected with chemicals, and thrown open, holes were made in the roofs and walls, and all filth was removed from the houses and streets, and alleys and courts, and burnt. The city had a thorough clean up, I should say.

23,891. Were all these measures adopted simultaneously?—Of course the disinfection took place when the city was evacuated.

23,892. But the other measures?—The first three measures were concurrent, and then the disinfection and the burning of dirty rags took place. The city was evacuated, and the filth removed.

23,893. I understand first that the evacuation was only in parts?—Yes, it was at first only partial. The epidemic started in a small suburb of the city, the Sadr Bazar, just about a mile and a half from the city. It is situated between the European quarter and the city. The plague started in the European quarter, amongst the servants of the Europeans who were living in camp. Then it was carried by the servants, I presume, down to the small Sadr Bazar. One man was found suffering from plague in a temple in the Sadr Bazar, one of the original household that was attacked. He was found in the temple sick of plague.

23,894. At first you evacuated that part in which cases occurred only?—That was evacuated first. The city was commenced to be evacuated in October 1897, and it was completely evacuated by the end of December 1897.

23,895. Before any evacuation took place, was there any segregating of the affected and their contacts at the very commencement?—I do not know really, as I was not here when it started.

23,896. When you were here evacuation was proceeding, in the first place, in affected areas?—The original infected area had been evacuated when I came here.

23,897. Had any subsequently infected area been evacuated?—When I came here they were just commencing to evacuate the real city.

Mr. W. M. Muat, M.B.

6 March 1899.

23,898. Do you know why they did not content themselves in evacuating a limited area?—The plague had commenced in the real city, in Sholapur.

23,899. When plague appeared in the true city, they started to evacuate the whole city?—Yes.

23,900. And that took from October until when?—Until the end of December, from the 26th of October till the 31st of December.

23,901. That is rather slow progress, is it not?—It is a rather large city, 63,000 people, to make arrangements for people going out into camp; you cannot do it all in a week.

23,902. You had 60,000 people to deal with, had you?—No, I should say about half the population, 30,000 or so.

23,903. And to evacuate 30,000 people, you think you would require about two months?—I should think so, quite. You did not know that the houses that the people fled from were evacuated or not; and you had to classify all the houses in order to find out whether they had really left. You had to go over the whole city to see whether they had left or not, so that you were dealing with a larger population than 30,000.

23,904. What became of the people who had disappeared?—They had gone into the villages and the surrounding places at once.

23,905. At what stage of the epidemic was the great voluntary exodus, or flight, as it were?—I could not say it was, but I suppose it was about a month.

23,906. Before you came, or after?—It was still going on when I came; the bulk had gone out.

23,907. The bulk had gone out before you came?—Yes.

23,908. I suppose you did not know where they went to?—They went to the surrounding villages and fields, and they disappeared. They could not go out by train, for instance. Tickets were refused, without a special pass.

23,909. You do not know when the voluntary evacuation really commenced?—No, I could not say definitely; it had commenced before I came.

23,910. Why did the people run away?—I thought it was simply on account of the fear of segregation, and of going to the Plague Hospital, and also the disinfection of their houses, and that sort of thing. It was not so much the fear of the disease, I think, from what I have seen.

23,911. You think the exodus did not occur till after segregation had been put into operation?—The people knew all that took place in Bombay, and other cities. They knew that they would be segregated, and their houses disinfected, and that they would be put to all that trouble in that way. They knew, of course, what was going on.

23,912. Have you any knowledge as to whether by this flight, plague was imported into any of the neighbouring villages?—Yes, distinctly.

23,913. Cases did not come within your personal knowledge, but you know of them?—I have had nothing to do with the villages, but I have heard that that did take place.

23,914. How many people did you get into the health camps?—I did not know the population of the health camps at that time.

23,915. Have you any idea; was it 5,000, or 20,000?—There were three main camps, and it is rather difficult to say.

23,916. Were the evacuated people allowed to return to the town on any conditions?—The city was completely evacuated at the end of December, and in March, special well-known people were allowed to go back, people who had been known to have been living in the fields, or in the camp, healthy people.

23,917. Not before?—No, not till March 1898, I think.

23,918. In the meantime they were not allowed to go to the city at all?—They were not supposed to, of course.

23,919. They may have gone contrary to regulations?—Yes, they may have gone contrary to orders, but I do not know.

23,920. Not merely to follow their occupations during the day?—They were working at their occupations outside in the camp, the weavers, and others.

23,921. You managed to transfer the weaving and other apparatus?—Yes, we transferred it.

23,922. And all the ordinary handicrafts were carried on outside, in the camps?—Yes; I have seen them making wheels, and also blacksmiths working there.

23,923. After this evacuation, how did you disinfect the houses; what did you do?—They had special pumps to scatter disinfectants all through the houses, to wash the houses and sprinkle them thoroughly with disinfectants. All rags and dirty clothes were taken out. Anything of value was disinfected, but the other things were burnt.

23,924. You disinfected with chemical disinfectants?—Yes. Of course in the case of the lower houses, the poorer houses, the roofs were taken off; that is in single storey houses.

23,925. You let in light and air as far as you could?—Yes.

23,926. What did you do with regard to the better houses?—They were treated with chemicals, and after that they were limewashed. The better houses were only disinfected with chemicals.

23,927. Had they a sufficient number of ventilating apertures?—Yes; they were respectable houses with good windows, and so on.

23,928. Do you know whether this evacuation did any good, as far as the extinction of the plague among the evacuated was concerned?—Yes, I should think so, most distinctly.

23,929. On what grounds do you think so?—The moment the city was evacuated, plague disappeared.

23,930. In the camps?—It disappeared altogether, if you refer to the statistics.

23,931. I think you said the evacuation was completed in December?—Yes.

23,932. Could you tell us, generally, how many cases had occurred before this evacuation was completed?—The following list shows the weekly number of attacks and deaths in Sholapur City:—

Week ending			Cases.	Deaths.
1st October	1897		13	9
8th "	"		6	4
15th "	"		26	8
22nd "	"		50	45
29th "	"		116	90
5th November	"		146	117
12th "	"		143	118
19th "	"		221	182
26th "	"		363	297
3rd December	"		502	377
10th	"		501	436
17th	"		300	246
24th "	"		134	149
31st "	"		53	36
7th January	1898		26	27
14th "	"		9	9
21st "	"		7	11
28th "	"		5	8
4th February	"		1	2
11th "	"		—	7
18th "	"		1	4
25th "	"		5	3
4th March	"		—	2

23,933. The number of cases in the town, then, immediately became greatly reduced in a very striking way after the evacuation was completed at the end of December?—Yes, it was a very striking fact.

23,934. Do you know how many cases occurred in the segregation camp?—Yes, I can give you the number if you care to have them.

23,935. Can you give them, week by week, or every 10 days, as they occurred?—There were several segregation camps. The first segregation camp was formed in the fields round the Plague Hospital. There was a special Plague Hospital built. There were 1,421 contacts admitted into this camp. Out of that number there were 101 cases of people who developed plague. 1,320 people were discharged.

23,936. Can you give me the numbers in the evacuation camps?—These are the health camps. I could not tell the number of inhabitants in the health camps.

23,937. Can you tell me the number of cases that occurred among the people who were evacuated from the city?—No, I could not give you that.

23,938. Have you not got any statistics?—No.

23,939. Have you no means of finding out how many people got plague, under your own observation? Were they not reported?—Yes, they might be worked out.

23,940. Can you evolve that information from this table?—That is how the cases occurred. The dates of the plague cases and deaths that occurred among the evacuated population are as follows:—

Week ending	Cases.	Deaths.
7th January 1898	10	9
14th „ „	9	9
21st „ „	7	11
28th „ „	5	8
4th February „	1	2
11th „ „	0	7
18th „ „	1	4
25th „ „	4	2
4th March „	0	4
Totals	37	56

23,941. The impression is that the evacuation had very quickly reduced the number of cases?—In my opinion that is so.

23,942. The figures bear that out?—Yes; and, from what I saw, I should think that is so.

23,943. When you allowed the people to return to the town, did you have any recrudescence, as it were, of plague?—No, none whatever.

23,944. Has it remained free from plague since their return?—Yes, it has.

23,945. There have been no further cases up to the present?—There have been a few scattered imported cases through the town occasionally since.

23,946. By what means did you prevent these imported cases from starting a new epidemic? — The contacts of these cases were immediately segregated, and the house was treated in the former way. The house was evacuated, and re-disinfected, and left unoccupied for a certain time.

23,947. Everyone was removed from that house?—Yes; and then the house was disinfected, and left unoccupied for about a month or six weeks.

23,948. How would you explain the success with which you have now prevented an importation of plague into this large city, from lighting up an epidemic, as contrasted with the failure of your measures when the plague originally occurred? What is the difference in your methods now, and how do you explain why you have succeeded now when you did not succeed before?—The city is in a different condition to what it was then; it is very much cleaner, for instance.

23,949. Anything further?—Probably the very susceptible people have taken plague and died; and the cases have been immediately detected, and steps taken.

23,950. Is not that probably alone enough, that you are able to get sufficiently early information now, where you could not get it previously? Is not that the material point in your success now?—It might be half the point. I consider that it is principally because most of the susceptible persons have been removed.

23,951. And what are the other points?—The city is cleaner, and the people are not so susceptible; having gone through an epidemic, the susceptible people have been removed, and have had plague, and all that.

23,952. What is the chief provision in making segregation useful for preventing an epidemic when plague is imported into a town?—My opinion is that we must catch the first one or two cases.

23,953. Have you any knowledge as to rats having been instrumental in propagating plague?—No; I have not. On two occasions since the epidemic I have been asked to go and see dead rats. The people get very excited, of course, and I went and saw the rats. The first case I went down to see was that of a young rat and a mouse, lying together. That created a great scare, but I took it that cats had caught these two and killed them. However, the people were very excited. Since then I have seen two dead rats in a house, but there was nothing further. The house was disinfected, and so on.

23,954. There have been no great number of deaths among rats in this epidemic to your knowledge?—No; not to my knowledge.

23,955. How do you think the plague is extended from one person to another?—I think by contact—dirty infected clothes. That is all I have had experience of.

23,956. Have you had any instances of clothes having conveyed infection?—Yes.

23,957. Will you detail any of those instances?—I could not give a specific instance, but I know of several cases where clothes had been stolen from the Plague Hospital by temporary attendants, had been concealed, and cases occurred from those clothes. I thought they had occurred from those clothes.

23,958. The people who obtained these clothes suffered from plague?—Yes; they took plague.

23,959. What was the general characteristic of the houses in which most cases of plague occurred? Were they the best houses, or the worst?—Cases occurred amongst the better and the poorer classes equally, in my opinion.

23,960. In what kind of houses did the better classes live? Can you give any description of the kind of house?—They are very good houses of course. In comparison the houses in which the better class people live here are really very good houses, I should say.

23,961. And the poorer class houses?—They are generally four mud walls, with a roof of thatch, or sometimes tiles.

23,962. An equal number you think occurred among the rich and the poor?—Yes.

23,963. You have no experience of plague in the out-villages?—No: none at all.

23,964. (*Mr. Hewett.*) I understand you to mean that the possession of a good house did not secure a person against immunity from plague?—No.

23,965. The disease broke out first of all in the servants' quarters of the Europeans?—Yes.

23,966. It then went to the Sadr Bazar?—Yes.

23,967. How do the houses in the Sadr Bazar compare generally with the houses in the rest of the town?—There are also good houses there of course, but there are also dirty houses and huts.

23,968. Are they pretty good there, or are they below the average?—There are not so many rich people in the Sadr Bazar; they are mostly working people.

23,969. You say 1,500 cases were reported here in the five weeks ending December 31st. I suppose you cannot estimate what the number of the people in the town was during any one week of that period?—No, it would only be guess work.

23,970. But is it the case that after the end of November, the population of the town was rapidly diminishing?—Yes, distinctly so.

23,971. Do you think it is possible that the epidemic reached its height in the middle of December, and then went down of its own accord?—I do not know, I could not say that. I thought it was the evacuation that did it—the people coming out of the houses where the infection was.

23,972. When the 501 cases took place in the week ending 10th of December, was not a great portion of the town evacuated by that time?—Yes, I suppose about three-quarters of the houses had been inspected, and the people turned out.

23,973. When you examine a female corpse, do you examine the groin?—I do always.

23,974. There is no objection taken to your doing so?—No, not at all.

23,975. Can you tell us the number of imported cases that have taken place since the people have returned to their houses?—Ten. One in April 1898, one in June 1898, one in August 1898, one in September 1898, two in October 1898, three in November (two of them on the same day, and from the same place); and one in February 1899.

23,976. (*Mr. Cumine.*) You said I think that segregation of contacts is useful, if applied to the first two or three cases detected in a town?—Yes, I think so.

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23,977. Does the following passage in your précis of evidence still represent your views on the subject of segregation:—"Segregation to be of any avail must "ensure that the first two or three cases are detected: "after that the means defeats its own end in that the "inhabitants immediately take fright. Notwithstanding the means taken to prevent it, the inhabitants "of this city immediately fled to the surrounding "villages, carrying the infection with them. The small "number of plague cases occurring in the contacts "segregated, did not, I think, counter-balance this "evil."?—I am still of that opinion.

23,978. Did you notice that there was anything in the nature of the floors of a room that had any effect on the number of people attacked in that room? Were they stone floors, or wooden floors, or earthen floors, that were most liable to get infected with plague?—Most of the houses in the city have earth floors; a very great majority.

23,979. You did not notice that one kind of floor was more liable to give infection than another?—No. You see so many earth floors, that your attention would not be drawn to that.

23,980. (*The President.*) As you use both words, segregation and evacuation, might I ask you if you mean exactly the same by both words?—No.

23,981. Will you define what you mean by segregation?—By segregation I mean contacts being put in a special camp.

23,982. You mean the removal of the patients and the contacts only, not the surrounding inhabitants?—Yes.

23,983. By evacuation what do you mean?—General evacuation, the people leaving the city.

23,984. Of the whole city, or a large area?—Of a large area.

23,985. Therefore, while you think the segregation and the removal of the patients and of the contacts in the same house is useful, still you consider that it is only so where you have very early information of first cases?—Yes.

23,986. On the other hand, you think that evacuation is always useful?—Yes, always useful.

23,987. (*Mr. Hewett.*) Do I understand that you think that evacuation is useful to the place from which you remove the people? Do you think Sholapur town for instance would benefit by evacuation?—Yes.

23,988. But might not the places outside somewhat suffer from it?—It just depends on the way it is done, I take it.

23,989. You say that people ran away from the town carrying infection to the neighbouring villages. Do you think that the evacuation of the city led to that result?—It did, but that is not the compulsory evacuation by the authorities. That came about by the people running away themselves, in order to try and escape segregation.

23,990. In order to make the evacuation of a big town effective, is it not desirable that you should be able to get hold of all the people who leave the town, and keep them under observation?—Yes.

23,991. Is it possible?—No, not in a town of 63,000 inhabitants.

23,992. (*The President.*) In regard to this question of segregation, if you got early information of one or two imported cases, such as I understand you had originally, and if you segregate these people (the actual patients and the contacts) even although there were a panic, if they were the only cases, would there be any harm in other people running out of the town because of the panic?—No, I do not think so, if the people were apart from the commencement, and had no contact with the cases.

23,993. But I understood you to say that there would be no benefit unless you got information of imported cases and took away the patients and the contacts? You now get information of the imported cases, and are able to segregate them and the contacts?—We get all information up to now, I should say.

23,994. You do that without panic now?—Yes.

23,995. There has been no exodus?—Not because of these specific cases.

23,996. How do you account for that?—I suppose the people are not so much afraid now, and the measures are not so very irritating to them.

23,997. What measures?—Segregation and evacuation and disinfection.

23,998. Have you made any variation or change in the measures?—No.

23,999. The measures remain the same?—Yes.

24,000. But the people are not so frightened of them now?—No.

24,001. As they become accustomed to them, they learn what they mean?—Yes, they see the benefit of it to themselves.

24,002. (*Mr. Hewett.*) Supposing that you had an outbreak again in this town, do you think that the people would run away?—I do not think they would, unless it became very bad. If it did, I suppose they would.

24,003. They did not run away when the epidemic first broke out?—No, but when it started in the Sadr Bazar, the people in the main city took fright and ran away.

24,004. Have you noticed that the Mangs in this town have remained relatively free from plague?—Yes, I could take you to a little patch on the borders of the town which was not evacuated. There were very few cases, and the people lived there all the time.

24,005. Why were they permitted to live there?—Because they would have fled, and gone out, and struck work.

24,006. Were they not used to work within the town?—Yes.

24,007. That was the object of keeping them there?—Yes, that was the object of keeping them there.

24,008. Do you attribute their comparative immunity to the fact that they did not mix much socially with the other members of the town?—I am not prepared to answer the question. I mentioned the fact of the few cases that occurred in the unevacuated Mangawada, to show that plague cases were not more frequent amongst the poor and dirty classes than amongst the rich and better classes.

(Witness withdrew.)

Lieut. M. E. L. Bruce, I.S.C.

Lieut. M. E. L. Bruce, I.S.C., called and examined.

24,009. (*The President.*) I believe you are in the Staff Corps?—Yes.

24,010. And you have been on special work at Sholapur?—Yes.

24,011. Since what time?—15th November 1897.

24,012. (*Mr. Hewett.*) At that time how many villages were affected in the Sholapur district?—I was working in the city from the 15th of November to the 26th of December. That was the date it was turned out. There were about three villages which were known to be infected during that time.

24,013. You were employed in the city up to the end of December?—Up to the 26th December, and then the village inspection was done by me to find out how many villages were infected. I went out on 26th January. We then knew of only about three or four villages, but by the 4th of February we knew of 50.

24,014. I should like to ask you one question about the town first; can you give an estimate as to the number of persons there were in the town in the week ending 10th December, when there were 501 cases of indigenous plague?—I cannot give you the exact figures, but I should think there would be from 25,000 to 30,000 people in the town.

24,015. By the beginning of February you found 50 villages infected?—Yes.

24,016. Were you able to trace the infection in the case of many of these villages?—The infection could be traced in almost all with very little trouble. I found out that people had come from Sholapur and were living in the fields and some in houses.

24,017. Since February 1898, when there were 50 villages infected, what was the maximum number of villages you had infected at one time?—There were 50 villages infected at that time all round Sholapur, and there were three or four villages in the Madha taluka.

24,018. Have you had more than 50 villages infected at one time?—No.

24,019. At the present time how many villages are infected?—At the present time there are about seven villages infected, though they are not certain whether it has not stopped.

24,020. Otherwise you have no plague in the district?—In two villages we have got plague, I have got reports coming in every day.

24,021. You have two villages with plague and seven which are suspected?—Seven are under inspection.

24,022. During the time when plague was at its worst what was the maximum number of European officers employed in the villages?—There were three European officers.

24,023. I suppose some of these villages are at considerable distances from one another?—The place was divided into wards. The furthermost villages are about 15 or 17 miles from here.

24,024. During that time what was the maximum number of villages under your personal control?—Fifty villages and ten infected villages, I think.

24,025. Did you find it possible to visit each of these villages once a day?—No.

24,026. How often could you visit them?—I did not go in for visiting villages a great number of times. I camped at one village, and did the whole of the work in that village, and stopped plague in it if possible at once, and then went on to the next.

24,027. The people remained in the fields after the village had been evacuated?—Yes, the people remained in the fields.

24,028. And you stayed there until plague was practically got under?—No; I did the work necessary to stop plague in the course of two or three days; I then went on to another village and worked at that for two or three days.

24,029. You did not stay in these villages to see if the people obeyed instructions with regard to their not going back to their houses?—No.

24,030. Do you think that more European officers than were employed in Sholapur are required to carry out work in the villages effectively?—Yes, most distinctly more were required. I was entirely alone in the work. I had a Plague Officer under me, and I had a certain number of sepoys who helped me.

24,031. Were you employed in the villages during the monsoon?—I was out all through the rains, from July to the present time, practically.

24,032. Do you think that making the people evacuate a village site during the rains causes hardship, or not?—I think it causes them a certain amount of discomfort, but it is quite worth it. The people do not object to it, to anything like the extent one would suppose.

24,033. You think that during the rains in the black cotton soil and in the exposure to which they are subjected, the people are not made more liable to other disease by being evacuated at that time?—I noticed absolutely no amount of higher mortality from other diseases. The people were wonderfully well considering everything.

24,034. When you have got the people segregated in their own fields, and a case of plague occurs among them, what measures do you take?—I make them commence in the early morning, and pull down the original hut in which the case occurs. Next I make them put up a small hut for the sick man, a three-cornered hut so that nobody else can get into it. I send them off to have their clothes washed, and late in the evening when they have done that they build a new hut for themselves. I make them keep out as many things as possible, only allowing them to take in their cooking materials and the clothes they actually require.

24,035. Who does that if you do not happen to be in the particular village in which the case has occurred?—The village officers have instructions in writing and they are told to carry them out. As a rule I think they do the work fairly well.

24,036. Do you use any disinfectants in the villages now?—No.

24,037. Why not?—Because as far as the houses are concerned we have several cases of villages which have shown that it does not appear to be necessary; they have been re-occupied without any further recrudescence. With regard to clothes and other things, washing and sunning them have proved quite sufficient.

24,038. In the case of these villages which have been re-occupied without disinfection, how long did you keep the people out?—In some cases over three months.

24,039. What was the minimum time for which you kept them out?—Three months.

24,040. You think that after people have been kept out of a village site for three months, it is practically safe to let them back again without taking any measures to disinfect beyond improving ventilation and allowing sunlight to enter?—Ventilation and emptying the houses.

24,041. Do you think that plague is taken from village to village mainly by human agency or any other way?—Mainly by human agency.

24,042. How do you think it is carried about in a village?—By rats.

24,043. Have you noticed any instance in which rats brought plague from one village to another?—No.

24,044. In your experience what is the course of infection in a village?—First of all a case occurs in one house, and then I have always thought that rats take it to other houses. Of course, people communicate it themselves.

24,045. Do you think that the importation of the disease from outside is in the majority of cases followed by mortality among rats before the residents of the place get infected?—I think it is.

24,046. Have you any instances of villages in which there has been no mortality among rats?—No. It may have occurred, but I do not know of it. It has not been observed.

24,047. Have you had any village in which no mortality has been reported to you amongst rats?—No.

24,048. That is to say you have had rats dying in every village?—Yes.

24,049. Have you noticed that any other animals are subject to plague?—No.

24,050. Have you had any instance in which plague has been imported from one village to another simply by means of clothing without any person having come from an infected to an uninfected area?—No, I have no experience of that.

24,051. Have you noticed that the Mahars and other outcast people have escaped infection when a village has been infected?—Very frequently, even though not evacuated.

24,052. In how many villages have you noticed this?—In four or five, I should think.

24,053. Would you infer from that that the reason for their escape is that they do not mix with other people?—That, and the fact that they do not keep corn in their houses which might attract vermin.

24,054. (*The President.*) What sort of lives do these Mahars lead?—They are always working. They are practically village servants. Of course, they are very poor. Some of them in the big villages own a certain amount, but as a rule they own nothing. They are fed by the village.

24,055. Their life is a very out-of-door one, is it?—Yes, very.

24,056. More so than the other members of the community?—Much more.

24,057. That is the great characteristic?—Yes.

24,058. Have you got any example of an evacuated village in the rainy season where the people seemed to be not much discomforted by being out?—Three villages were out through very rainy weather. I was out with them all through the time, visiting them, and going round to see what their complaints were. Of course, they were put to some discomfort undoubtedly, but they managed all right, and their complaints were

Lieut. M. E. L. Bruce, I.S.C.

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very few. They certainly preferred to be out to living in the village.

24,059. How did they show their preference?—I had one case where they tried to raise an objection, I offered to let them go back to the village, but they refused.

24,060. Have you any examples of the speedy termination of an epidemic in a village by the complete evacuation of that village?—Yes.

24,061. Could you give us a few examples briefly?—Plague commenced in Korali village in the Sholapur taluka about the 21st of March last, and I got news of it on the 27th. Six families were infected then. The village was vacated on the 29th of March, and plague ceased on the 2nd of April. In all nine families were infected.

24,062. Can you distinguish between before and after?—Yes. There was only six families attacked on the 27th. Between that and the 2nd of April three more families were attacked.

24,063. What is the normal population of this village?—About 800.

24,064. Have you other examples of a similar description?—Nothing so good. Patri village was a rather worse example, because plague only showed itself in the form of buboes in one family; we had about four cases of fever immediately afterwards, and about two deaths, and then it stopped. I do not think it is a sufficiently good case.

24,065. Do you know which of the villages evacuated had the largest population?—Mole village, with about 4,500 inhabitants.

24,066. Is that the maximum?—Yes.

24,067. Do you think it is quite convenient to evacuate a village of that size?—No, I think it caused plague to spread to about 10 other villages.

24,068. Because of what?—Because the people simply ran away, and there was nothing to stop them.

24,069. You had no cordon?—No.

24,070. You could not well manage so large a village?—I could not manage it at all; it was quite impossible. I was by myself.

24,071. (*Mr. Hewett.*) When you say you could not manage it yourself, I suppose you mean that the people were determined not to stay in the fields?—The people had lands in other villages. Naturally there was a great number of well-to-do people in such a large population, and they simply went off to other villages. If the other villages could have been depended upon to keep them out, it would have been all right; also, had we been able to make examples of them. In any case it is very difficult.

24,072. Would you draw a distinction between the smaller and the larger villages? In the smaller villages you mainly had agriculturists who are tied down to the village, and do not want to run away to other places?—Yes.

24,073. And, consequently, with a small agricultural community you would find it much easier to take the people out of the village site and keep them in the fields?—Much easier; they would be more inclined to obey in any case.

24,074. But your difficulties are increased when you meet with a population that is accustomed to travelling and has interests outside the village site?—Exactly.

(Witness withdrew.)

(Adjourned till Tuesday, 7th March, at Ahmednagar.)

At The City Magistrate's Office, Ahmednagar.

SIXTY-SECOND DAY.

Tuesday, 7th March, 1899.

PRESENT:

PROF. T. R. FRASER, M.D., LL.D., F.R.S. (*President*).

Mr. J. P. HEWETT. | Mr. A. CUMINE.

Mr. C. J. HALLIFAX (*Secretary*).

Mr. C. Hudson, I.C.S.

7 March 1899.

Mr. C. HUDSON, I.C.S., called and examined.

24,075. (*The President.*) You are the Collector here?—Yes.

24,076. And you have had considerable plague experience in Ahmednagar?—Yes.

24,077. (*Mr. Cumine.*) When did you come to Ahmednagar?—In the end of December, 1897, practically the 1st of January, 1898.

24,078. When did the plague begin in the Ahmednagar district?—I cannot tell you that off-hand, but about the end of October.

24,079. What was the first place affected, do you remember?—I do not know that. Captain Walton will be able to tell you that.

24,080. Before you came to Ahmednagar had you had experience of plague in any other district?—Yes; in the Thana district, Salsette taluka.

24,081. For how long?—From February, 1897, to December, 1897.

24,082. Have you noticed whether infection amongst rats always intervenes between the infecting imported case, and the first local case?—No; I have not always noticed that.

24,083. Do you know of any instance where clothes have certainly carried infection?—Yes; I know an instance in this district, in Rajapur.

24,084. Can you, with certainty, exclude the possibility of human agency, the possibility that the person who brought, or went to fetch the clothes, was the agency that communicated the infection, and not the clothes?—In this case it was a Marwari family attacked, and the infection was given to the Mahars. There was no plague in the Maharwada. The Mahars got the plague, and I think it is almost certain that it was conveyed in that way.

24,085. It is certain that the Mahars got the clothes?—Yes; that is quite certain.

24,086. Is it certain that the clothes were from an infected person?—Yes.

24,087. Could you describe the circumstances to us?—No; I cannot say any more than that.

24,088. Is the case within your own knowledge, or has it been reported to you?—It has been reported fully from this district, in an official report to Government.

24,089. How is it, that, in your opinion, plague is carried from village to village, by what agency—human

agency or rats, or what?—I think nearly always by human agency. In one case they said rats had done it, but it was only a little bit of village gossip, I think.

24,090. How do you think it is carried from house to house within the limits of a village?—I do not know. It does certainly seem to go from house to house, but in this city of Ahmednagar there were no deaths of rats observed like there were observed in Bandra. In Bandra I should say rats certainly did carry it about.

24,091. Did you see any rats going from one house to another?—No. (Note by witness on correcting proof of his evidence:—Since giving my evidence I have had two opportunities of observing the spread of infection; in the Maharwada of the city, and in a village called Sonevadi; in neither case could it be said to have spread from house to house as at Chichondi, where no dead rats were observed.)

24,092. Have you any experience of the kiln system of disinfection?—No; I should think it would be a very dangerous system to adopt generally, judging by experience in Savedi here, where there were three workmen engaged in digging up the floor of a room, and two of them got plague. Of course no precautions were observed, but still they got it.

24,093. Have you any instances where animals, other than rats, such as cats, pigs, and squirrels, certainly died of plague?—No.

24,094. Is there any instance within your knowledge where plague has recrudesced, that is to say, where a village which was infected has had a long interval of freedom, and plague has appeared in it again without a re-importation of infection from without?—No.

24,095. Have you any instance where a village has become infected this year, which was not infected last year, but which is next to a village that was infected last year?—Yes; in the town of Bassein itself, in the Thana district.

24,096. What happened there?—The plague began there in December, and it was supposed to have been stamped out in June.

24,097. I was asking you about a case where plague has appeared in a village this year which was not affected last year, but which was next to a village that was affected last year?—In the case of Savedi, Varanghusi, Chichondi Patel, and a village in the Khandesh district recently at Kanaswadi.

24,098. This last is not within your knowledge?—No.

24,099. What happened in those cases which are within your knowledge?—Savedi is next door to the city of Ahmednagar, and there was no plague there last year, and it broke out this year.

24,100. How long was the interval between the ceasing of plague in the Ahmednagar City and the appearance of it in Savedi?—About six months.

24,101. Have you noticed whether there was any heavy infant mortality in Savedi during those six months?—No, I have not noticed it. I do not think there was.

24,102. What is the next instance?—In Varanghusi a similar thing occurred. Pendshet, a small neighbouring village in the Ghauts, was infected last year, and, about six months after, Varanghusi became infected.

24,103. Have you been unable to trace any infection of Varanghusi this year from without?—When I wrote my précis of evidence it was suspected that it may have been imported from a village in the Thana District, and it is possible it was so imported. I am inclined to think myself it was so imported.

24,104. The third case?—In Chichondi Patel, 13 miles from here. There was plague in a village called Athvad previously, and Chichondi Patel got infected this year, and the source has not been traced.

24,105. About Chichondi Patel have careful inquiries been made to find out whether it did not come into it this year from without?—Yes, very careful inquiries have been made. I myself and others inquired on the spot, and we could only find out that the Chamars first got infected, but whether they were infected by Marwaris or not has not been found out; it is only known that the Chamar families first got plague.

24,106. Had any of them been away, or had any outsider come to them?—That has not been found out.

24,107. Have you examined the death-rate during the interval of six months to see whether it was suspicious?—Yes.

24,108. Was it suspicious?—No. That is how the 20th of October was fixed as the probable date of the outbreak; by examining the births and deaths register.

24,109. Then the outbreak had been going on for some time this year in Chichondi Patel before it was discovered?—Yes, from about the 20th of October till about the 30th of November or the 1st of December.

24,110. Have you noticed whether cases that come direct from Bombay into this district readily infect this district, or whether the cases that most readily infect this district are cases that have come from some half-way house, such as Lanauli?—No, I have not noticed that.

24,111. Is there any other interesting fact that you wish to tell us about?—Yes, with regard to inoculation.

24,112. What have you to say about that?—I remember reading Professor Gajjar's evidence, and he seemed to think that inoculation was prejudicial to the uninoculated; that it made them get plague in some way or other. My experience is quite opposed to that, because about half the camp was inoculated and half not, and nobody suffered in any way; none of the uninoculated got plague, or anything like that, from the inoculated.

24,113. At that time there was no plague in the camp?—I was camped near infected villages, but there was no plague actually in the camp.

24,114. (*The President.*) Have you encountered any cases in which plague has occurred among the inoculated?—Yes, I know of two cases, but I have not seen them.

24,115. Can you tell me if the type with regard to the virulence of plague has varied at any times of the epidemic?—No, I do not think so.

24,116. It has been the same throughout?—Yes.

24,117. Have there been any cases which have been so mild that the diagnosis was difficult?—Not that I personally know of. I know of mild cases escaping from camps, and getting let through without being discovered to be plague cases at all. I know of one instance of that kind where infection was carried by a mild case of that description.

24,118. The symptoms were so slightly pronounced that it was not recognised as a plague case?—Yes.

24,119. By whom would that recognition be made?—By a Hospital Assistant.

24,120. Have you had much experience of disinfection?—Yes.

24,121. What is your opinion as to the value of disinfection?—I think it is a very good thing for the workpeople who are engaged on the work.

24,122. Who are putting the houses in order, or what?—In the Deccan districts, where they have to open up mud roofs.

24,123. Do you mean that the houses should be disinfected before the workmen go in to do that?—Yes.

24,124. Do you include in that exposure to light and air, or merely chemical disinfection?—I think it would do, if the workmen had disinfectants to wash themselves with.

24,125. I am speaking rather of the habitations?—I am referring to the safety of it for opening up, simply. I think opening up the houses is the best thing of all.

24,126. You put that kind of disinfection first because it comes to be a disinfection before disinfection by chemical agencies. Is that the result of your experience?—Yes, but I could not give any explicit reasons.

24,127. That is the impression which is produced in your mind?—Yes.

24,128. How long do you consider, before a house became safe, that this non-chemical method of disinfection should be in operation in that house? Would it be a matter of a few days or weeks?—I think it would be weeks.

(Witness withdrew.)

Capt. W. C. Walton, I.S.C.

7 March 1899.

Captain W. C. WALTON, I.S.C., called and examined.

24,129. (*The President.*) You are in the Indian Staff Corps?—Yes.

24,130. What is your official position with regard to plague?—I am Magistrate of the Cantonment, and I am in charge of all plague operations in the cantonment.

24,131. (*Mr. Hewett.*) The cantonment was the first part affected here, was it not?—I have been looking back in the old correspondence, and I find there was a case in Bhimgar on the 9th of March 1897, which is a village joining on to the Sadr Bazar.

24,132. Was that an isolated case?—Yes, and apparently imported.

24,133. Do you know where the patient came from?—It was brought by a Mali from Poona. His wife had died there of the disease.

24,134. Did any people get the infection from this man?—I do not know where they got the infection.

24,135. At that time did you hear of any more plague cases?—No, at least I cannot trace any. I did not come here till December. On looking up the correspondence, I find that there was a case.

24,136. When was the first case in the cantonment? On the 27th of September, 1897.

24,137. Was this man a private servant?—He was a bullock driver of the Roman Catholic Chaplain.

24,138. Had he any connexion with Bhimgar?—He went out to buy grass for the bullock. It was a rainy day, and he got wet, and whether he went to Bhimgar, or whether he went among the gawlis in the Sadr Bazar, is not known.

24,139. Did any other servants of the master of this man get plague?—Four of them died in hospital with plague, the bullock driver, the cook, the daughter of the cook, and also the cook's son.

24,140. Who were the next people affected after these servants?—The gawlis were the next.

24,141. Was there much intercourse between the gawlis and the Kunbi who was first affected?—He might have gone to buy grass from the gawlis. Of course they keep cows, and that is the only way I can connect them.

24,142. Would it be likely that there would be social communication between them?—Yes.

24,143. I understand that the outbreak among the gawlis took place on the 3rd of October, 1897?—Yes, that is the first case heard of.

24,144. And after that you had a search of the Sadr Bazar on the 8th of October?—Yes, a search was made.

24,145. What orders were then given to the residents of the Sadr Bazar?—They were ordered to vacate their houses within 24 hours and take up their residence in a camp.

24,146. What was the population of the Sadr Bazar?—About 3,000.

24,147. And how many of them took up their residence in the camp within 24 hours?—1,600.

24,148. What happened to the remaining 1,400?—They scattered all over the country, and I do not know what happened to them.

24,149. Then after the people occupied the camp, did many cases of plague occur among them?—A good many cases, yes. In a short time it went up to 66 cases.

24,150. Up to Christmas were how many there?—There were 75 cases. By the 28th of November there had been 66 cases and 49 deaths, and the people and their clothes were then all disinfected.

24,151. By Christmas 1897 there had been nine more cases?—Yes.

24,152. How do you account for the occurrence of cases in the camp?—I do not account for it, but the Principal Medical Officer of the Poona district came here, and he accounted for it by the fact that the clothes had not been disinfected.

24,153. You mean before the 28th of November?—Yes.

24,154. But after the disinfection on the 28th of November, there were other cases?—There were a few dropping cases.

24,155. How do you account for the cases having occurred?—I cannot account for them all. I know one particular case I accounted for was that of a man who had strict orders that he was not to go to the city which was infected at that time, and he went to the city, and slept there.

24,156. Do you think that other inmates of the camp were doing the same thing?—Very few; we had policemen on the roads.

24,157. Did you have any cases of plague in the camp after Christmas day, 1897?—No, that is the last case in the camp. There were other cases in the cantonment.

24,158. In other parts of the cantonment?—Yes.

24,159. In what parts of the cantonment were there cases after Christmas, 1897?—There were cases in the Kotla, in the Regimental Bazar, and, I think, amongst the Native troops.

24,160. Were the cases very numerous there?—The Kotla is a walled enclosure inhabited by Muhammadans, and I got information one morning that there was a case of plague there. I rode down, and found two dead bodies, and everybody cleared out; and about four or five of them died in the surrounding country to my knowledge. They were found by various Mamlatdars, and brought in.

24,161. These people who left the Kotla were under no supervision after they left?—They cleared out before we knew anything about it.

24,162. Were there cases in any other parts of the cantonment?—There was one case in the syces' lines of the Royal Artillery.

24,163. Only one case?—Yes.

24,164. Did you find it necessary to evacuate any other portion of the cantonment?—The Regimental Bazar was evacuated, but they did not run away; we caught them all.

24,165. How many cases were there before and after evacuation respectively?—On the 9th December a case occurred in the Regimental Bazar, in the Native Infantry.

24,166. How many cases occurred in the Regimental Bazar before and after evacuation respectively?—Only one.

24,167. That case occurred on the 9th of December; when did you evacuate the place?—The same day.

24,168. And you had no further case?—No.

24,169. When you evacuated the Sadr Bazar, and the other places that were evacuated, did you do anything to the houses?—They were unroofed and washed with perchloride of mercury and afterwards whitewashed.

24,170. After what date was that?—It was commenced as soon as the Sadr Bazar was vacated, on the 8th of October.

24,171. Did you have any cases among the people who went back to whitewash or cleanse their houses?—All the work was done by a gang of coolies. The people did not do their own houses. The whitewashing was done whilst the roofs were off; that was all done by a gang of coolies. I have not any certain information about it, but I believe one or two coolies at first took plague.

24,172. Was disinfection by means of perchloride of mercury carried out in every house of the Sadr Bazar, the Regimental Bazar, and the Kotla?—I cannot answer for the Sadr Bazar. I think they were all done. All the infected houses were done, but not the uninfected houses.

24,173. Did you do any uninfected houses in the Regimental Bazar?—One on each side of the infected houses.

24,174. Did you unroof only the infected houses or all the houses?—In the Sadr Bazar they were all unroofed, and the houses in the syces' lines and the Artillery were all unroofed, but I did not unroof all the houses in the Regimental Bazar.

24,175. When did you permit the people to go back to re-roof their houses?—On the 25th of January orders were issued that they might go in by day for re-roofing purposes.

24,176. When did you permit them to re-occupy the bazar?—On the 19th of March, 1898.

24,177. When were they permitted to re-occupy the Regimental Bazar?—At the same time, I think.

24,178. And the Kotla?—Yes, all of them.

24,179. Since they were permitted to re-occupy these places, have there been any cases of plague in the cantonment?—There have been two cases this year.

24,180. Where did they occur?—One was rather a strange case in the Royal Artillery syces' lines of a woman who had just borne a child, and she was taken off to the hospital. They said she had plague, and she died.

24,181. When was that?—Three months ago.

24,182. Had she come from outside?—As far as I could understand there was no reason why she should have plague.

24,183. The second case?—The second case was in a paddock in the Remount Depôt.

24,184. Did the patient come from outside?—It is rather a long story. A boy aged about 15 years died in this paddock. About six men with their families were looking after the horses in the open paddock, and they were living in six bamboo huts absolutely segregated, so to speak. The boy died there, and the Hospital Assistant saw him and said he had no signs of plague—that he died of relapsing fever. No particular precautions were therefore taken. A fortnight afterwards a baby died in these same six huts, and the Hospital Assistant again said there were no signs of plague whatever; the baby had died of ordinary fever. Then a third baby died three days afterwards, so I went down to see what they were dying from. The men themselves said they were located on an old graveyard, which was very unhealthy. I burnt down all those bamboo huts and sent them into another paddock. The mother of the baby had fever, and she recovered next day, and that made me think there was no plague. About three days after that another baby got fever, and then I sent them all to segregation camp, and disinfected their clothes. The baby died in camp. They were practically segregated in the second paddock which I sent them to, but I did not disinfect their clothes. After sending them to the segregation camp, I disinfected all their clothes. They have been out now about a week, and they have not had any fever.

24,185. Who decided that any one of these cases was a case of plague?—The Hospital Assistant who saw the baby that died said there were no buboes and no signs of plague, and the senior Medical Officer, Colonel Lane, saw the baby, and told me that it had buboes, and that it was plague: that is the last baby that died.

24,186. Did he make a report to you that this was plague?—After it died he did; it died in the segregation camp.

24,187. Who decided that the case of the woman who died in the Royal Artillery syces' lines was a case of plague?—The Civil Surgeon, Major Willis.

24,188. You were not able in that case either to trace any communication from outside by friends or relations of the woman, or to ascertain whether it was brought by clothes or anything of that sort?—The people had a right to go to the city or to any other villages. The woman was just going to bear a child, and it is not likely that she had been travelling about the country.

24,189. Was there plague in the city at that time?—There were a few dropping cases.

24,190. Has the death-rate in the cantonment during the year 1898 been above the normal?—No, much below.

24,191. Have you carried out any subsidiary measures for the improvement of the sanitation of the cantonment?—Yes. While they were out in camp I went and took up all the private latrines, and destroyed them, and disinfected them with perchloride of mercury, and then when they came back they were not allowed to have any private latrines, except very special ones. Also, in rebuilding their houses, they had to build on a particular pattern, allowing for lots of ventilation, big doors, and a good height to the roofs, &c.

24,192. You also condemned a certain number of houses?—Yes, we condemned some, and made one or two new roads right through them in some places.

24,193. (*The President.*) The result of your evacuation was successful on the whole, was it not?—Very successful directly the clothes were disinfected, but not before; the same as with the case in the Remount Depôt paddock.

24,194. What do you think is the essential requirement in order to obtain success in evacuation?—I think the great thing is not to allow the ground to be soiled by people evacuating all over the ground like they do in these places. You see people making a latrine of the road in most of these Indian cities. I think it is a very important thing that that should be stopped, and it can be stopped, I know.

24,195. In the evacuated camp, you mean?—No, in the bazars.

24,196. I am talking about evacuation. For instance, on removing people into camp, I thought you said that was successful in your experience?—It was successful after their clothes had been disinfected.

24,197. And in order to obtain the highest success by evacuation, what are the chief requirements to stop the epidemic? Under what circumstances would you be most likely to succeed in stopping the epidemic when you have adopted evacuation?—If you disinfect the people's clothes, and always make them sleep in camp.

24,198. The evacuation must be complete, and they should be segregated?—Yes, allowing them to go to business by day.

24,199. Is evacuation more likely to be successful if it is carried out just after the epidemic has commenced, or in the later stages of it?—It should be carried out at once.

24,200. Therefore, one of the greatest requirements for ensuring success is what?—Evacuation at once.

24,201. That I should think would imply early information?—Yes.

24,202. After you have disinfected, cleaned, and ventilated, have you found that the people who returned did not again acquire plague?—No, they did not.

24,203. You spoke of the structural changes which you have made; did these include windows where they were required?—Yes, two windows in each room, and a ventilator in the roof.

24,204. In what spirit have the people taken these changes?—Very well indeed.

24,205. Do you think they appreciate any advantages from these changes?—I do. They block their windows when they can, but I always unblock them. They like the ventilators. I always say to them, "You "were more healthy in camp than you were anywhere "else, and if you block up your windows, you will be "put into camp again, and you will get more air "there." They get more air in camp than they do in their houses.

24,206. But do they quite appreciate the advantages of the alterations?—They prefer a stuffy house, I think, but they do not grumble about it.

24,207. Do you think you could teach them the advantages of not blocking up their windows?—I am sure of it; they all see that they are more healthy here.

24,208. You said they blocked their windows, but when you tell them not to do it, do they still persist in it?—There is no doubt they do; they have to be opened up every morning.

24,209. By others than the inhabitants?—Yes, but they do not take it in a bad spirit at all. I think they appreciate the advantage of it, although they do not appreciate the comfort of it.

24,210. In regard to the whitewashing, I think you said that some coolies became affected when they were whitewashing?—When they were unroofing.

24,211. Previously to this unroofing, had any disinfection been carried out in these houses?—I cannot tell I was not here then. Every time when I have unroofed a house I have always disinfected first, and I have never had coolies affected then.

24,212. Those were cases which came under your observation, in which the unroofing was preceded by chemical disinfection, and in those cases there was no plague among the workmen?—Never. I have had coolies lifting plague patients in their arms, and showing the greatest pluck, and I have never had any plague amongst them.

(Witness withdrew.)

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KHAN SAHIB BYRAMJI SORABJI CARDMASTER called and examined.

24,213. (*The President.*) I believe you are Chairman of the Ahmednagar Municipality, and member of the Ahmednagar City Plague Committee?—Yes.

24,214. Has your plague experience extended throughout the whole time of the several epidemics?—Yes.

24,215. In what years had you plague in Ahmednagar?—We had some imported cases in 1896. Then we had also imported and indigenous cases in 1897 and 1898.

24,216. With regard to 1896, was there any epidemic following the importation?—No.

24,217. How do you think you escaped the epidemic at that time?—We had precautions at the railway station. We only had imported cases. There was no centre of plague in the surroundings. As soon as we detected any arrivals from Bombay or Poona, or from an infected place, we kept an eye upon them and had them inspected, and if we found anything wrong we drafted them to the hospital. There was another reason. That year was perhaps the driest year in Ahmednagar. There was a famine and scarcity of water. There was no centre of plague in the vicinity.

24,218. One of the points you have stated in your précis is that each imported case was kept under observation?—Yes.

24,219. So that you obtained immediate information?—As soon as an arrival from Bombay or Poona came into the station we registered his name, and a report was sent to the Municipality. As soon as we received the report we kept an eye over that man and watched him.

24,220. What did you do with him if plague occurred?—If he showed symptoms he was at once taken to the Plague Hospital and treated there, and the contacts and the family were also segregated.

24,221. And the house?—The house was disinfected.

24,222. In what way?—In 1896 perchloride of mercury disinfection was not prevalent. The house was whitewashed, and certain parts of the roof were cut open. The floor was also whitewashed—the outside and inside of the house and floors were whitewashed.

24,223. You did not employ any powerful chemical disinfectant?—No, not in 1896.

24,224. On the other hand you did open the roof?—Yes.

24,225. Could you tell me how many imported cases you had in this way to deal with in 1896?—There were 26 cases.

24,226. What was the population of Ahmednagar at that time?—36,000—perhaps more at that time—about 38,000.

24,227. In 1897 what was the history of the importation of plague?—So far as I have gathered, I think the importation came from the cantonment. In the last week in September we heard of cases occurring in the cantonment. We heard that a certain reverend gentleman had come from Bombay, and that one of his servants was attacked with plague. At that time the cantonment authorities had no separate hospital of their own, so that the patient was brought to Sidhibagh Hospital. For a month or so there was nothing in the city, but in the last week of October there was one case of plague—a Muhammadan, who had been to the cantonment. He was a native of Ahmednagar, but he had been going to the Sadr Bazar in the cantonment.

24,228. He took plague?—Yes, he had plague in the city. He was found in the city. As it proceeded we found that a great many people had escaped from the cantonment, and had sheltered in the city limits. We inquired into the history of the case, and it was established that the people had plague from the cantonment limits. The locality first affected was the nearest to the cantonment limits with only a wall intervening. There is a big open space, and most of the people who had dealings with the cantonment people lived in that locality, where plague was traced in Ahmednagar at the beginning.

24,229. I understand you did not get very early information of these cases—not so early as in the instance of the imported cases from the railway station?—No. In 1897 we had what we call the death-certificates system here. As soon as there was any death from any cause whatever a Hospital Assistant went there and certified the cause of death. The body was then taken out to the ghats for burial or cremation, so that there was no possibility of escaping or concealment. If there was a death, the death would be at once reported, and the cause of it. I should think that the first case detected so was very probably the first case of plague.

24,230. You got the first case by the death register?—Yes.

24,231. But these cases may have remained living and affected with plague in their houses for a considerable number of days before you found them out?—That was so with one Muhammadan family, and also with one barber's son. The boy was ill for about five or six days, but we only learned of plague there after the death of the boy.

24,232. During these five or six days is it possible that he may have disseminated plague by persons seeing him, or otherwise?—We made inquiries about it. Perhaps it was possible that the plague could be disseminated, because in the neighbourhood of it—the very next house—there was plague.

24,233. Subsequently?—Yes.

24,234. What was the further history of the 1897 epidemic?—We actually found plague travelling in a line always from house to house and from ward to ward. When we found plague broke out in ward No. 5, where the barber's son was, we found it was actually disseminating itself. We found numbers of cases there. We evacuated that ward first by blocks and then the ward. As we went on evacuating, the parts evacuated remained safe, and plague was actually found in those parts which were not evacuated.

24,235. In the contiguous part?—Yes.

24,236. And so it extended?—Yes.

24,237. What was the date of the first infection of 1897?—29th October.

24,238. Can you tell us what was the total number of cases of plague in that epidemic?—There were in all 407 cases.

24,239. What was the mortality?—Out of that number 92 recovered and 315 died—the period ranging from the 29th October 1897 to March 21st, 1898.

24,240. By the 21st March had the plague subsided?—Yes, it had.

24,241. In 1896 was there any special mortality among rats?—No.

24,242. In 1897 was there any special mortality?—No.

24,243. Will you describe the measures you adopted in the 1897 epidemic: you have already gone so far as to say that you evacuated?—Yes. When we found a house infected the first step taken was to carry the contacts to the hospital and the neighbouring people to the segregation camp. They were kept there for 10 days. If there was a second case in the same family 10 days were counted from the second date upon which the last case occurred. Soon after the patients and the contacts were removed, the house was disinfected with perchloride of mercury and the roof opened up. As a rule, we cut a hole three feet by two feet in the roof. If there were windows in the wall they were opened up, and the house was left open for a month or so, and nobody was allowed to enter it.

24,244. That refers to a house in which a plague case occurred?—Yes.

24,245. You also spoke of the neighbouring houses being emptied: what did you do with those houses?—In the beginning we started blocks—that is, a certain number of houses was taken on each side. The Civil Surgeon was generally present, and he would decide that certain houses on each side should be vacated and disinfected. The neighbouring houses were also disinfected, and opened up as long as plague had not assumed an epidemical form. But once the plague had assumed the epidemical form, then the people were asked to leave in numbers, in lots, and the houses were all opened up and the windows were opened. All the houses were, so to say, disinfected and whitewashed.

24,246. You adopted the same treatment as you would when disinfecting a plague house?—Yes.

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24,247. You found that your first measure did not succeed in checking the progress of the plague to neighbouring houses, or to other parts of the town, did you not?—Yes, because when we found cases were occurring, although the neighbouring houses were evacuated, we thought that the block system would not do, and therefore we went to the ward system of evacuation.

24,248. Very small evacuation was insufficient; therefore, you tried a larger evacuation?—Yes.

24,249. What was the success attained by the larger evacuation?—As soon as a ward was evacuated, we found the mortality in that particular ward subsiding at once.

24,250. You mean among the people who were in camp?—They were asked to go and live in the fields. The Municipality did not give them any encampment. The open fields were the place where the people used to go to.

24,251. What is the history of the epidemic in the fields?—We have not sufficient data as to the field mortality. Still, from the reports I received, I found that the mortality was comparatively very little.

24,252. Trifling relatively to the condition of the city?—Yes.

24,253. To what extent had you to proceed in evacuating by wards?—We went on evacuating, ward by ward. We first evacuated Ward No. 5, then No. 4, and then No. 3. We evacuated Ward No. 8 last. I found until the last ward was evacuated, plague was appearing in those very places where people were actually found. We went on evacuating ward by ward. A certain number of people were allowed to stay in these wards who were disabled and blind, and could not go out. However, as we went on evacuating, we thought that we would keep the other parts safe—those parts which were not evacuated, and which had not got plague at the time when plague broke out in the wards here which were evacuated. Although we took special care to see the people who were asked to leave one ward did not go to other wards, still plague was found in those very wards which were not evacuated and which had not plague in the beginning.

24,254. Indeed, the evacuation even on the more extensive scale of ward evacuation was not sufficient to prevent the plague extending throughout the city to parts not evacuated?—That is so.

24,255. Did you evacuate the whole city?—Yes, we evacuated the whole city in the middle of March. We had to keep a certain number, who were blind and too ill to be removed, but they represented a very small number.

24,256. Did you follow out the same treatment in the evacuated houses as you have already described?—Yes.

24,257. What was the history of the epidemic among the evacuated after the entire evacuation had been effected?—Among the evacuated, those who had gone out to fields, the mortality was comparatively very small, and the general health was good.

24,258. At what season of the year was it—the dry season?—Ward No. 5 was evacuated in January, and by the middle of March, the whole of the city was completely evacuated.

24,259. You were still in the dry season?—Yes. We allowed the people to enter in the middle of April.

24,260. They commenced to return then?—Yes. The wards first evacuated were allowed the opportunity of taking in men, and the whole city was again full. A good many people had left, but all the wards had an opportunity of taking in people by the end of May.

24,261. You had no inconvenience from the wet season during this operation?—No, we had not. We had a very sharp shower of rain about the end of February, 1898, and that, so to say, put a good many people to trouble and inconvenience. There was also a good deal of cough, and the mortality was not at all high.

24,262. You mean the mortality from other causes than plague?—Yes.

24,263. You think it was low?—Yes, it was low; but even with that shower and the cough and all that, it was not at all high—not so high as one would have expected it in the city.

24,264. Did you notice if plague commenced in any parts of the town which, in their sanitary or any other condition were different from other parts?—I noted that the part where the plague first broke out was the part which is most unhealthy; where there is no drainage system.

24,265. But this part also had this condition—that it was nearest to the infected cantonment, had it not?—Yes. The people had begun to leave the other parts of the city, and there was no possibility of discovering the particular item of insanitariness. There are other parts of the city equally bad in point of sanitation, I admit, but still we had not an opportunity of gauging that condition on account of people beginning to leave of their own accord.

24,266. Where did they go to?—Mostly all the people camped in the fields about a distance of two or three miles from the city.

24,267. They were not specially under control in the fields, were they?—They were put under control by the Collector's staff. The Akbari Inspectors were put on. One reverend gentleman, Mr. Smith, was also put in charge of the outskirts.

24,268. Do you think that any of these people went further than the camps—went into the neighbouring villages or towns?—Yes, they did; not after plague had assumed an epidemical form, but at the time it had broken out.

24,269. Why did they run away in that way?—I suppose on account of the experience they had had in the cantonment and Sadr Bazar.

24,270. What point in that experience caused them to run away?—They came to know that in the Sadr Bazar people were being asked to go out and leave their houses.

24,271. Do you not think it was rather the plague measures than the plague itself that they feared?—I should think so.

24,272. Within your knowledge are there any instances of plague having been imported into a place previously non-infected by this running away of the people? When the people ran away from Nagar did those people who went into other villages or towns infect any of those villages or towns?—Not so far as I know. I do not remember, but there were cases in the fields.

24,273. You have described among your measures the making of openings in the houses?—Yes.

24,274. Why did you find that desirable?—Openings were made so that the rays of the sun could penetrate into the rooms, which were generally very dark.

24,275. What sort of ventilation is there, generally speaking, in the houses?—There are very few openings for ventilation in mud houses. If there are any they are very small. They are not suited to bringing in the rays of the sun direct. The best remedy was to have openings made in the roof.

24,276. Now we will come to the epidemic of 1898. What were the chief features of the commencement of this epidemic?—The chief features noticed about the 1898 epidemic were that rats were found, while they were not found in the 1897 epidemic. The Sisters of the S.P.G. Mission complained that they found a good many rats dying. We found that two girls were suffering from plague.

24,277. Were these the first cases which came to your knowledge?—Yes, in 1898.

24,278. Therefore, before these cases occurred there had been an observable mortality among rats?—We did not observe any mortality among rats in the city, nor outside. It is only that feature, the S.P.G. catching plague, that brought to our mind the mortality of rats in that direction.

24,279. And that mortality occurred before the first cases in that part of the town?—It is not within the town; it is just outside the town—2 miles or 1½ miles out of the town.

24,280. Is it within the Municipal boundary?—It is not strictly within the Municipal boundaries—it is a little further up.

24,281. Will you describe the introduction of plague into the Municipal boundaries?—This year we found some cases in the Maharwada. That is outside the city gate. We have not yet ascertained how that part of the city came to be infected this year. What we have found out is that the surrounding villages were first infected this year. That is one feature of this year

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which was rather peculiar. Before the city could be infected, most of the surrounding villages were found to be infected, and we were taking care not to allow these people to come in. The part that was infected last month in Nagar is the part called Maharwada. It is suspected that those people must have mixed with other villages, or that people from other villages that were turned out must have mixed with the people here. During the last three or four days we are marking one particular feature, namely, that the fields which had plague last year are now showing cases this year too. Though the city was free for a number of days—for a number of months even—cases cropped up in the fields; and it is not understood how the fields could be infected, but there was no infection in the city.

24,282. But there was infection in the surrounding villages?—Yes, there was infection in the surrounding villages.

24,283. Those are the chief features?—Yes.

24,284. Will you kindly describe what you did in this second epidemic—what were your measures—were they the same?—Yes, they were the same as on the previous occasion.

24,285. When did you effect evacuation during the second epidemic?—We only evacuated blocks. We have not turned out the wards yet. We thought, as it was simply beginning, we would try the system of evacuation by blocks; and we have so far succeeded in keeping in check plague in one part of the city, called Nepti Gate.

24,286. Has it been restricted to that part?—We found one case and one death in one part of the city in October, 1898. We evacuated about 40 houses, and turned out about 300 people. We were successful as regards that arrangement. Plague did not advance.

24,287. In that part of the city?—Yes.

24,288. What about the other parts of the city?—The other parts of the city also remained safe.

24,289. What has been the number of cases in this second epidemic?—Up to date we have had 37 cases.

24,290. Have these occurred in the city chiefly, or outside?—The number in the city is comparatively very small.

24,291. How many, do you know?—About five or six.

24,292. The 1898 epidemic has been quite unimportant?—It has, because we are safe.

24,293. At present you have nothing to cause anxiety here?—No.

24,294. These cases chiefly occurred in the encampments?—Yes, in the fields.

24,295. Where the people have voluntarily gone out?—Most of the people had left last year, and had built huts in the fields. They have not yet returned.

24,296. The people in the fields, I suppose, might quite well communicate with neighbouring villages?—They may, or the people in the neighbouring villages may communicate with them.

24,297. I presume that is including infected villages?—Yes.

24,298. So that there is no mystery about the origin of these cases in the fields?—I think it is rather a mystery.

24,299. What is the mystery?—The origin of it.

24,300. Do you mean the origin of any particular individual case?—From inquiries, we have learnt that though it is quite possible and probable that there may have been communication, still the people themselves say that they have not moved out nor have they allowed people to mix with them. People who are known to have come from infected villages are watched, and are not allowed to mix with other people.

24,301. How complete is the watching; is it impossible or even very difficult for them to go beyond the boundary that is watched?—It is difficult, but it is not impossible.

24,302. Have you formed any special views with regard to the methods of communication of plague?—I should think particularly by contacts.

24,303. Do you think that the communication is chiefly by human agency or by some other means?—Principally by human agency.

24,304. Have you any instance in which clothes, apart from men and women, have been the channel or the means of conveyance of plague?—I have not had any special opportunity of noting that. I have had reports like that, but I have no special knowledge of persons secreting clothes or concealing clothes and getting plague.

24,305. You had reports, but it did not come to your own observation?—No.

24,306. The official report would be very useful?—I have not traced that report out.

24,307. It is only an impression, therefore, in your mind that it may be so?—Yes. That is my impression from the reports I have received from people.

24,308. Do you think that rats have a share in the communication of plague?—Only from what I have heard and seen with regard to the S.P.G. Mission.

24,309. Only from that instance?—Yes.

24,310. Do you know if other animals besides rats have had plague?—I saw cats.

24,311. What do you think are the features which are most conducive to the spread of infection? You draw attention to floors, want of ventilation, and general insanitary conditions, do you not?—Floors of houses have much to do with the communication of plague. Most of the people here sleep on floors and move bare-footed in their houses, and therefore run the risk of infection. Roofs of houses here contain so many crevices and holes that they form good harbouring haunts for plague germs in spite of disinfection with perchloride of mercury. Want of ventilation is very healthy for the thriving of plague bacilli. Houses here are so constructed that they have very little ventilation, and even that is closed during the night. There are certain houses which even require a light in the daytime. Weak health and bad feeding also encourage plague. This was verified by the numerous attacks that took place amongst the Sali, Kosti, and Kongadi people. These were the people who had suffered most in the famine, and no sooner was famine over than plague overtook them in their half-famished state. Milch-cattle stables, unless they are built upon sanitary principles, with outlets for the proper discharge of urinary matter and dung, also prove very flourishing to plague, and, with this in view, the then Civil Surgeon, Dr. Stevenson, advised the Municipality to drive out all cattle and provide for them suitable stables outside the city.

24,312. With regard to the various steps you have adopted, which do you think is the most effective?—Evacuation.

24,313. You have a very clear opinion as to that?—I am quite positive.

24,314. I suppose it is a natural inference that the more complete the evacuation the more effective it is as a measure?—Yes, and much more if it is undertaken in the first stages of plague.

24,315. Would you express any opinion as to the value of disinfection in itself—of habitations, I mean?—I am rather sceptical about it.

24,316. On what grounds?—On the grounds of the construction of the houses in the mufassil towns. There are so many crevices and so many things to look after. I do not think disinfection with perchloride of mercury can thoroughly reach those crevices and holes which are found in numbers in the houses. However satisfactorily it may appear to be done it will not be perfect.

24,317. Have you any instance in which the value of perchloride of mercury has been tested and shown to be inefficient?—I cannot mention any specific instance.

24,318. You believe in the efficacy of the influence of sun and air?—Yes.

24,319. But you have no case in which you have established the insufficiency of chemical disinfection?—No.

24,320. Have you any knowledge as to the possibility of food substances conveying plague?—No.

24,321. None at all?—No.

24,322. You have had, I think, to entirely disinfect and clean this city twice?—Yes.

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24,323. What has been the general result of that upon the health of the city apart from plague?—I have found cholera disappear, and noted, particularly last year, that cholera had entirely disappeared. Generally speaking, we have cholera in July and August every year, and we have a mortality of about 50. I noted the complete disappearance of cholera last year.

24,324. Have you anything to say with regard to the water supply? What does the water supply of Nagar consist of?—Old aqueducts. Water is brought into the city under masonry galleries to a certain distance, and under a new scheme it is brought in in iron pipes.

24,325. Even since water has been brought here in that way cholera has been fairly prevalent until last year?—Yes.

24,326. Have you any statistics to show the mortality during the last two years as contrasted with any previous periods?—Yes.

24,327. Does that bear out your contention as to the increased healthiness of the city within the last two years apart from plague?—Last year, just at the time plague was raging—say from November to March—we did not get any complete statistics because the people were out in the fields. What was shown in the mortality statement last year was rather vague and I should not like to trust that.

24,328. This year?—This year we found mortality in certain weeks rising, and in certain weeks falling, but contrasted with previous years it was almost an average.

24,329. Then there is nothing actually to prove that there is increased healthiness, but only that cholera has diminished?—Yes.

24,330. I think you noticed something in connection with rats in this city after plague had appeared?—After the disinfection and evacuation of the city, a disappearance of the rats was noticed in 1898.

24,331. There are now almost no rats—they have been reduced greatly in numbers?—Yes.

24,332. I think you have already stated that in 1897 a mortality among rats did not precede plague?—Yes. In 1897 we did not mark the mortality among rats, and in 1898, so far as this city is concerned, there has been no complaint as to rats being found. There is only the S.P.G. Mission case where rats have been found.

24,333. Is there much overcrowding in the localities of this town within the houses?—In certain wards there is overcrowding.

24,334. Was there any observable relationship between the incidence of plague in these wards as contrasted with other wards?—In Ward No. 6, which is the most overcrowded part of the city, the mortality during plague time was high; but that ward has been inhabited by people who are more intelligent than the people in the other wards. The people left, however, as soon as they found that the other wards would have to suffer.

24,335. (*Mr. Hewett.*) You say that plague occurred in 1898 in the fields in which it took place in 1897. Do you mean that it occurred in the same hut or in the same collection of huts?—I am not sure whether the huts were dismantled last year and reconstructed this year, but the cases occurred in the same area.

24,336. Do you know whether the huts in which plague occurred last year were always disinfected?—I am not sure.

24,337. Might it be possible that the infection may have remained there?—It is possible.

(Witness withdrew.)

Mr. Mohanlal Hiralal called and examined.

Mr. Mohanlal Hiralal.

24,338. (*The President.*) I understand that you are a member of the Ahmednagar City Plague Committee?—Yes.

24,339. (*Mr. Cumine.*) You are a volunteer witness, are you not?—I was asked by the Collector to give evidence.

24,340. You are not an official?—No.

24,341. You have heard what Mr. Cardmaster told us?—Yes.

24,342. Have you any interesting facts to bring before us in addition to those he has given us?—In the year 1896, when the first cases occurred here, no steps were taken to disinfect the houses. The infection having come in the summer months it did not affect this town at all. There were six or seven cases, and we did not adopt any steps in 1896, because it was during summer. The plague did not remain here at all. The inmates of the same houses and the neighbouring houses were not affected. In the summer climate even importations had no effect here. It was in the winter season when plague came in 1897. It was about the end of January that the whole town was evacuated. Mr. Cardmaster said it was about the end of March that the whole town was evacuated, but I think it was about the end of January. For nearly three months the inhabitants were out. The opening of the houses to the influence of the sun had a very good effect. I do not believe in the process of disinfection with perchloride of mercury. I know of my own knowledge that it was not fully carried out according to the directions of the Civil Surgeon or the medical men. The houses are so peculiarly constructed that every portion of them cannot be satisfactorily disinfected, and the workmen did not do the work satisfactorily. Still, the opening of the houses, and leaving every room open to the rays of the sun had the desired effect. I am inclined to think that the sun did the work of disinfection more than the perchloride of mercury. That is my impression about it. This year we have simply had sporadic attacks—cases here and there.

24,343. Why did not these sporadic cases spread plague?—In my idea it is owing to the effect of last year's disinfection, namely, the houses having been opened up to the sun. The cases came to our knowledge very early, and steps were taken to remove those people. The infection could not travel.

(Witness withdrew.)

Mr. Edulji Rustomji, called and examined.

Mr. Edulji Rustomji.

24,344. (*The President.*) You are Vice-President of the Ahmednagar Municipality?—Yes.

24,345. And Chairman of the Plague Committee?—Yes.

24,346. (*Mr. Cumine.*) You go over pretty much the same ground as Mr. Cardmaster, do not you?—Yes, very nearly the same, except that in my précis of evidence I have noted some special facts to mention.

24,347. Do not go over the same ground, but kindly mention any special things that he did not touch upon?—I have in my précis mentioned that infection came first from Bombay, subsequently from Poona, and lastly a case from Kirki. I have given all the details, and described how the first case, an imported case, came into the city of Ahmednagar. One of the Padamsalis, by name Ramaya, came from Bombay. I have stated all about that in my précis. I have also stated in my précis that the climate was peculiarly chill and cold about the time that plague was raging in Ahmednagar, and also that at the same time there was a scarcity of water. The weather and the atmosphere, although chill, was dry. I have also described, at considerable length, how the city was first, block by block, evacuated as it became infected, and subsequently by wards, as has been described by Mr. Cardmaster. I might specially mention that while the system of opening up the houses was being carried out, it came to my notice that there were certain houses in this very town which required candle-light to get a vision in, even during the day, and those houses were the most typical instances which I have, where air and light had never got to them since they were constructed. These houses, such as those I have described in the Cloth Market, were constructed ages ago. These houses were specially treated in regard to ventilation and light, the owners were made to provide windows and roof ventilation, and light from the roof

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was also insisted upon. Another thing that Mr. Cardmaster has not stated, is, that while we were disinfecting and opening up the houses for the benefit of the people, we had a very valuable agency, and perhaps a very honourable one too, in having our head officials—the Executive Engineer, the Civil Surgeon, the Assistant Collectors, and the District Superintendent of Police:—all these had divided the city wards amongst themselves, and they were in their divisions personally and zealously superintending the operations of disinfecting and opening up the houses. These officials were subsequently aided, in order to see that each house and each room was thoroughly disinfected, by European soldiers, and native soldiers were afterwards brought in. Of course, by their presence, I am thoroughly convinced, on inquiry, that there was not a room left in any house of the town, which was not thoroughly done up. I have also noticed in my précis that the Municipality had to do this at no ordinary inconvenience as regards its finances.

24,348. What is the result of the thorough cleaning-up that the city got?—It has reduced the ordinary mortality of the town as compared with the previous five years.

24,349. Have you noticed whether people who sleep on the ground are more liable to catch plague than those who sleep on beds?—Yes, that is my experience of it. I found that several of my own servants, and other people whom I have known, and also on inquiry, have had reported to me that most of those people who were actually sleeping on the ground got attacked with plague and died.

24,350. Have you noticed whether people are more liable to catch plague if they sleep on one floor of a house than on another?—I think so. That is my experience, but it is not based upon a very large number of observations, but I know in one or two cases that people who slept on the higher floors did not get plague, although they were contacts.

24,351. Is there, in your experience, a particular danger in natives going into infected houses without shoes on?—Yes, there have been two instances I have particularly noticed in the case of people going in to do repairs to the S.P.G. Mission outhouses.

24,352. Will you tell us what you think are the best methods of keeping plague out of a district?—Observation camps, disinfection stations, medical examination stations, and a system of medical surveillance are, I think, excellent modes of keeping plague out of any station.

24,353. And immediate evacuation of infected places?—Yes.

24,354. (*Mr. Hewett.*) Have you Voluntary Committees at present to keep under observation people who arrive from outside?—We have a Committee appointed to visit the railway station, one member of the Committee going every day in the week and assisting the Hospital Assistant to examine the arrivals at the station.

24,355. If people come from outside now do you know where they go to live?—Yes, we keep a register at the railway station, and keep the village officers, and the officers of the talukas, the Mamlatdars, informed of people going to their talukas, having arrived by rail.

24,356. Is that what you mean by medical surveillance?—The people that come from infected places are asked to go for medical examination to the City Hospital for 10 days after their arrival.

24,357. That holds good in the city?—Yes. We generally find out if any people are actually suffering. I think there was a case so found, of people examined at the hospital—that there was a case of plague amongst them.

24,358. (*The President.*) Do you remember what was the total number who were in the camps outside the city, at any one time?—Nearly the whole of the city was turned out, and I should think that over 20,000 must have been living in the camps outside.

24,359. Then there are 16,000 not accounted for?—What I think had become of them, was that they had gone to other places. I know some of the Marwaris had gone to Marwar, and some had gone to Gujarat, and some had gone to other places, but I do not know whether the whole of the 16,000 had gone in that way.

24,360. You described the measures adopted in evacuated houses. In some cases you made structural alterations that were required?—Yes.

24,361. Can you tell me whether the people have made use of those structural alterations that you introduced?—We made them provide permanent windows, and openings in the roofs, but in several cases I found they had closed them up, by hanging clothes and pardahs over them. We then asked them to open them up again. It appears that most of the people who live in those houses do not like very free ventilation, and somehow or other, I found in some cases they had closed up the windows that I had provided—only temporarily—they had not permanently closed them up; they could be opened at will.

24,362. Do you think that these permanent improvements will be taken advantage of by the people in time?—I think that the better class of people have been using these windows, and that only the poorer classes tried to shut them up by hanging clothes and curtains and pardahs, or something like that, over them. I think, however, that they will gradually come to appreciate the value of light and ventilation. Of course, in some cases in which the houses were very dark, I have seen that the people kept them open.

24,363. Will you kindly tell me what is your opinion as to the most successful method of dealing with plague epidemics?—From what I have seen from the beginning to the end of plague in Ahmednagar, I am fully convinced that evacuation of infected houses and opening them up to sunlight and ventilation and whitewashing and otherwise thoroughly cleaning them up is the best remedy to escape wholesale ravages by plague; that plague sticks most and does greater mischief in dark, ill-ventilated, dirty houses and localities, and can be stamped out of a sanitary house much easier and quicker; and that by people living in the open, they not only escape attack of plague but get better and stronger as regards their ordinary and general health and constitution. I have heard several people say they have been feeling stronger and better after their stay out in the open in huts and sheds.

(Witness withdrew.)

Major C. F. Willis, I.M.S.

Major C. F. Willis, I.M.S., called and examined.

24,364. (*The President.*) You are in the Indian Medical Service?—Yes.

24,365. And are a Doctor of Medicine, and a Member of the Royal College of Physicians?—Yes.

24,366. What is your official position here?—Civil Surgeon.

24,367. (*Mr. Hewett.*) Can you give us some general idea of the course that plague had pursued in the villages outside Ahmednagar?—I can only tell you with regard to the latter part of the year. I was not here during the first; I was up at the front then. It commenced this time at Chichondi, a village 13 miles away from here.

24,368. When was that?—I cannot tell you the exact date; it was run by the village authorities. It was about October or November.

24,369. Did it extend from there?—It did not extend at all from there. The people evacuated into their fields, and the plague did not extend. There was no plague in the villages near.

24,370. What was the next place affected?—Savedi, about two miles from here, and about half a mile beyond the Mission, where plague first appeared. There they had a lot of cases. The cases had been returned as usual as anything, until I went there and found two or three cases of plague. The usual measures were then taken, the village was evacuated, and the people turned out into the fields, and that village has been thoroughly cleared out, and no spread of plague, I think, took place from there.

24,371. Have any of the other villages in the neighbourhood of the town been affected?—Towards Poona, there was another village called Khedgaum, which had

plague there. That was about the third village. They had several cases, and from there I think it spread to other villages, to another village called Sonawadi, a village about three or four miles further on.

24,372. Do you know the maximum number of villages attacked at any one time in the district?—I think the maximum would be five. I do not think, even then, they all had cases at the same time, for the first two or three villages have been free from plague now for several weeks.

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24,373. Then there has been no serious outbreak?—No; only one or two cases occur now and then.

(Witness withdrew.)

(Adjourned till Wednesday, 8th March, at Nasik.)

At The Collector's Office, Nasik.

SIXTY-THIRD DAY.

Wednesday, 8th March 1899.

PRESENT:

PROF. T. R. FRASER, M.D., LL.D., F.R.S. (*President*).

Mr. J. P. HEWETT. | Mr. A. CUMINE.

Mr. C. J. HALLIFAX (*Secretary*).

Mr. R. A. L. MOORE, I.C.S., called and examined.

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24,374. (*The President.*) I believe you are in the Indian Civil Service, and Collector of Nasik?—Yes.

24,375. And you occupied a similar position in Surat?—Yes, I did.

24,376. Where you had an opportunity of seeing two epidemics?—I did; at the beginning of 1897, and from the middle of 1897 up to May 1898.

24,377. Were these the only two epidemics that occurred in Surat?—Yes; in fact, I saw all the plague that occurred in Surat.

24,378. The first epidemic, I think, commenced in February?—It did.

24,379. And terminated in May?—Yes; May 1897.

24,380. Can you tell us what part of your Collectorate was chiefly affected in the first epidemic?—The chief place affected was Rander. As a matter of fact, putting aside a few imported cases, plague began at Rander. That is a town on the other side of the Tapti, opposite Surat, with a population of about 10,000 people. It spread from there, and subsequently, as far as I can remember, in the month of March it began at Bulsar. It chiefly prevailed at those two places during the first epidemic.

24,381. Can you account for its introduction into Rander?—I made inquiries, and, as far as I can gather, it arrived in this way. There was a vakil who lived at Rander whose wife had a brother at a Gaikwari village called Wariav. The brother came from Bombay to Wariav, and died of plague. While he was ill, this lady, the wife of the vakil, went on a visit of condolence to her brother. She returned to Rander, and herself developed plague and died. From that the disease spread throughout the town. That was the result, from inquiries that were made.

24,382. How long previously to its becoming indigenous did this importation occur?—As far as I remember, a fortnight. I ought to say that when this imported case occurred I was out in the district. I received, first, an anonymous petition and subsequently a letter from the Hospital Assistant at Rander, stating that he believed that there was plague in the town, and that it was being concealed. I immediately came in, and very shortly after my arrival plague became very evident.

24,383. You did not, therefore, get very early information about this importation?—No, not for several days after it occurred. I was in the district, and there was no plague at the time.

24,384. The information was not early enough to allow you to deal with the first case?—It was not dealt with at all.

24,385. Previously to its becoming indigenous was there any marked mortality amongst rats or other animals?—None that I was aware of.

24,386. If there had been, you would have heard of it?—Yes, if there had been any marked mortality.

24,387. After plague became indigenous, how did you treat it?—At the beginning we acted in this way. We had a field set apart, and in this field we put up a Municipal Plague Hospital, and we also allowed any castes who wished to do so to put up their own hospitals. Some castes did put up their own hospitals. Anybody taken with plague was removed to the Municipal or Caste Hospital, as the case might be, with two attendants. At first we started an arrangement of having the women all in one special building, but that was not subsequently carried out, as it was found impossible to separate the sexes owing to the fact that the attendant on a woman would be a male, and *vice versâ*. The husband would come with the wife, and the wife would come with the husband. Then we had the plague house vacated, and all the tiles removed and all the doors and windows opened and barred by bamboos. The place was then thoroughly doused with a solution of perchloride of mercury, one in 1,000, out of a fire engine. At the beginning I also had all the floors dug up, they were chiefly earth floors, to a depth of about six inches and taken outside the town and burnt. That was done to the plague house, and also to the houses on each side. If that was not found sufficient we proceeded to turn out whole localities and treat them exactly in the same way, except that we did not dig up the floors.

24,388. What was the proof of its not being efficient?—That in spite of the house being cleared out, perhaps in a day or two, the next house, or the next house but one, would develop plague, and so on down the street. When I found plague going down a street, I turned the whole street out.

24,389. Was that more efficient?—Yes, I think so.

24,390. That would seem to imply that plague still extended?—Yes, undoubtedly it did still extend.

24,391. In the first place the extension appeared to be by contiguity?—Yes. Possibly, I am not conveying a right impression. The plague did not spread actually to the next house, but it would spread from one house

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to three or four houses down, where there were relations or friends living of the people from a plague infected house. The plague does not begin in No. 1 house, and then go to No. 2, and No. 3, and No. 4. It will begin, say, in No. 1, and then go to No. 4, where the people in No. 1 have some friends or relations.

24,392. Therefore, it may extend across a street?—Yes, or to another street altogether.

24,393. Do you think the method of conveyance is by human agency?—Yes, by persons, and also by concealment of infected kit, clothes, and such like. People, when they are ordered to bring out kit and to have it disinfected, or, in a case of clothes belonging to the patient, to have them burnt, often conceal a part, and it is smuggled off by a friend or neighbour, and the friend or neighbour by so doing takes plague to his own house. I have found that out.

24,394. That house being at a considerable distance?—Yes, it may be.

24,395. Have you any cases in which it has been at a considerable distance?—Yes.

24,396. And you think it was carried in the clothes?—It was in property carried from an infected house.

24,397. Where it was not probable that it was carried by human agency?—Where it was not probable.

24,398. But, of course, you could not altogether eliminate that?—Yes, that is my impression after a couple of years' experience.

24,399. In a general way, was a large proportion of the population affected?—Yes, a very large proportion, certainly.

24,400. Was the mortality considerable?—The mortality was very considerable. I cannot tell you exactly what it was, but I should say it was four-fifths; 80 per cent. of those attacked.

24,401. Perhaps, in a similarly general way, you could give the proportion of those attacked to the population?—As I have had three epidemics to deal with, I get confused in my figures, and I should not like to say.

24,402. What have you to tell us with regard to Bulsar?—With regard to Bulsar, I may say that I had to supervise plague matters generally, and visit places, but the actual work was done by Captain Dyson, as he was then, the Deputy Sanitary Commissioner. The arrangements which I have described with regard to Rander were very similar to those carried out at Bulsar.

24,403. That is to say, the extreme measure was partial evacuation and not complete evacuation?—At first, and then, subsequently, streets were evacuated. The whole town was not evacuated.

24,404. Now we come to the second epidemic, which I think also involved three towns?—Yes, Rander, Bulsar, and Surat.

24,405. Which was the first of these three places affected in the second epidemic?—The first place was Surat in the second epidemic.

24,406. Do you know how plague was introduced into Surat?—Yes; I believe it was introduced by people coming from Bombay, especially people of the Ghanchi caste.

24,407. Who are the Ghanchis?—The Ghanchis are oil pressers.

24,408. Had they fled from Bombay?—They came from Bombay.

24,409. Because plague was in Bombay?—Yes, because plague was in Bombay. I should mention that Surat has a large business connection with Bombay. There are a large number of low castes in Surat who go for service to Bombay; for instance, a large number of people came from the caste called Surtis; they go as syces, cooks, and butlers to Bombay. They became infected in Bombay, and then came back and spread plague in Surat.

24,410. At about what time was the second epidemic?—The second epidemic in Surat began in August 1897—the mortality began to rise then. I am speaking from memory but I think I am right. I left Surat in May, 1898.

24,411. Was the course very much what you have described in the case of Rander?—There was a slight epidemic in the beginning of 1897 in Surat, I suppose about 150 cases. That was at the same time that plague was going on at Rander, and was introduced from Rander. It did not spread much during the first epidemic in Surat. We were very rigid in our measures. We burnt down some of the plague houses altogether. When plague first started in Surat we had houses pulled down and burnt. The plague did not spread much at that time, and then it stopped altogether.

24,412. In addition to partial evacuation, you burnt down some of the houses?—Yes.

24,413. It was a small epidemic?—Yes, about 150 cases.

24,414. Do you remember when it ceased?—I think it ceased in Surat in May 1897.

24,415. Between May 1897 and August 1897 the town of Surat was absolutely clear?—Yes, there might have been a few dropping cases perhaps; as a matter of fact I think there was one case, which occurred in July.

24,416. Was there any case which you could not trace to importation?—No, I think there was one imported case in July, to the best of my recollection, between May 1897 and August 1897 in Surat town; but there was no indigenous plague. Then, in August, the mortality began to rise rapidly.

24,417. Will you describe what measures you adopted in Surat after August?—Plague began in the middle of the rains, and, consequently, we were in a great difficulty. We could not go in for evacuation to any great extent, because we had nowhere to put the people. We did, however, turn out any plague cases we had, and removed them to the Municipal hospital, which we started near the railway station, at a place called the Parakh Dharamsala. We made the same arrangements as before. We allowed two attendants to attend upon each sick person, and we disinfected the houses, but we did not go in for segregating the contacts, because we had nowhere to put them. The people had to turn out of their houses on account of the disinfection. Some of them spread through the town, and some of them were provided with quarters in a building which was put at our disposal by a native gentleman, the Bakshi Sahib of Surat—the leading Musalman gentleman in the place. Then, aftera bit, the different castes asked me to be allowed to start their own hospitals, to which I agreed on condition that they should pay all the expenses, and allow the hospital to be under the supervision of the Civil Surgeon. They were to have their own Medical Officers, but to be under the supervision of the Civil Surgeon. Each religion did start its own hospital The Hindus had one, the Parsees had another, the Shiah Musalmans had another, and the Sunni Musalmans a fourth. Finding that there was great concealment, I started house-to-house search parties. Each party consisted of one or two European and some native gentlemen and a party of police. We went round and searched all the suspected quarters. I am bound to say that, as far as the search went, it did more harm than good, because it was almost impossible to conceal from the people where we were going to. The consequence was that they removed patients before we came, and thereby infected fresh houses. Plague went on, and the mortality continued to rise until November, and then Mr. Wingate, who was then the Plague Commissioner, arrived. He said that, in his opinion, we ought to start the ward system in Surat, which we did immediately; that is to say, the town was divided into nine wards. At the head of each ward there was put a European ward officer. They were all officials, except one, Mr. Page—the Bank Agent—who very kindly volunteered. Then, as far as possible, I gave each Ward Superintendent a European assistant, also a Government official, and a lot of honorary assistants in the shape of native gentlemen, who volunteered for the work. In this matter of volunteering I ought to say that the Parsees and Muhammadans were especially good; the Hindus were less ready to help. Under each Ward Superintendent there was a special staff put. We had for each ward one or two fire engines for disinfecting, with a gang of coolies under a Muccadum. We also had a number of police, and we had a staff of sweepers. The Ward Superintendent and his Personal Assistants were supposed to recognise plague as a rule. If they had any doubt they either sent for the Civil Surgeon or the Assistant Surgeon, who was put especially on that duty, and he decided whether that case was one of plague or not. If the case was decided to be plague, the plague patient was removed either to the Municipal Hospital, which we started in what was really the Civil Hospital, with an extension in the High

School—the Civil Hospital being removed into a part of the Jail—or else the patient, if he chose, was taken to his own caste hospital. In case of concealment, by way of punishment, people who wished to be taken to their own caste hospitals were not taken there. Supposing a Hindu concealed a case, and said he wished to go to his own caste hospital, by way of punishment he was taken to the general hospital. We had also a segregation camp for contacts; that is to say, all persons who were living in the house of the patient were taken to the segregation camp which was outside the city, and put under a pensioned Sergeant-Major of Gunners.

24,418. Was that during the rainy season?—It was in November, after the rains had stopped, and after Mr. Wingate's arrival. The segregation camps consisted of tatti huts built by the Municipality, and food was provided for all the people in the segregation camp, and to the poor, also, blankets and warm clothes were provided out of a charitable fund which I got up, and to which the native gentry of Bombay and Bhawnagar, especially, subscribed freely. They were kept in a segregation camp for 10 days, and if they developed no plague they were allowed to go; but they were not allowed to return to their houses in the city without a written order by myself.

24,419. They were allowed to go away?—To go where they pleased. That was after 10 days' segregation. We also found that it was not sufficient to turn out one or two houses when plague occurred, so we turned out whole streets, and then, after a bit, we turned out several streets, whole quarters in fact, and we camped the people down in what we called health camps all round the city. We had four Municipal health camps under official supervision, and there were a large number of small camps which were constructed by the people of their own accord. With regard to these voluntary camps, I had to close them subsequently, as I thought they were sources of spreading infection. There was no supervision over them, and the people used to go into the town and visit plague-infected relations, and come back again and generally spread infection. I therefore had to close these camps subsequently. In the four large health camps the people who had any means had to build their own hut, but people who had no means were provided with matting by the Municipality. In the case of widows and such like, huts were built for them. In each camp we had a staff of police and a staff of sweepers to keep the latrines in order, and we had a Sanitary Officer over the camp; we had Superintendents who were either Eurasians or Parsees. They also had a staff of Karkuns to keep the register of arrivals and give passes to people to go in and out of the camp. They were allowed to go out in the daytime, but they had to come back at night to sleep.

24,420. What is the maximum number of people you had in these camps at one time?—Of all sorts, both in the Municipal and voluntary camps, we had 35,000 at one time.

24,421. Inhabitants of Surat?—Yes, inhabitants of the city.

24,422. Had you many cases of plague in the Government health camp?—We had in one camp especially, which was occupied by the people I mentioned, the Ghanchis, and also by the low caste of people called Golas, rice-pickers. We had to move the site of that camp twice and to burn down the huts and burn the ground. This was partly due to the fact that these castes were excessively filthy in their habits, and partly to the fact that they were allowed to go to the town during the daytime to work for their living, but in reality they used to hire themselves out as plague corpse bearers.

24,423. Was there any provision to prevent overcrowding of the Government huts; was there any limit to the number of people in one hut?—Yes, I think I am right in saying that the maximum number allowed in one hut was five. If the family consisted of more than five, they were given another hut just adjoining.

24,424. You spoke of 35,000 being camped outside the city?—Yes, at one time; that is in round numbers.

24,425. What was the total population of the city?—110,000. We had about one-third of the population camped out.

24,426. And a considerable number remained in other wards?—Practically, the remaining two-thirds. We took great precautions against people running away from Surat.

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24,427. Were those precautions successful?—Yes, in preventing plague spreading out of the Surat district, but not in preventing people going out of the city and into the villages. We could not stop that, but we did prevent plague from spreading from the Surat district. That I am prepared to say.

24,428. Do you know the total number of cases during the second epidemic?—In round figures I should think it was about 2,500 or 3,000.

24,429. Do you know the mortality?—The mortality was about the same, 75 to 80 per cent.

24,430. Did this measure which you adopted again, consisting of only partial evacuation of the city, do much good in your opinion, or not?—I think it lessened the number of cases.

24,431. But it did not stop the progress of plague?—It distinctly did not stop plague, because plague went on from August 1897 to May 1898.

24,432. You began to adopt extreme measures in October 1897, and plague continued till May 1898?—Yes. It had stopped entirely before I left Surat. I did not go until it stopped. I left Surat on the 25th May. Plague stopped early in May.

24,433. In the case of the whole of the houses which had been occupied by plague patients, was disinfection quickly carried out?—Yes, certainly. It was generally carried out the same day, or, at any rate, the next day. We had the fire-engines filled with the mixture of perchloride of mercury continually going round the streets.

24,434. Was it soon after that that the houses were opened up so as to admit air and light?—Yes; the houses were opened up immediately, and they were swept out too. What we did was this. We opened up the house, first swept it out, and then doused it with this mixture.

24,435. What did the opening up consist of?—In taking up nearly all the tiles, throwing open the doors and windows, and barring them with bamboos. In the case of people being recalcitrant, who would not open their doors and windows, we took the doors off their hinges.

24,436. After you commenced evacuating large areas, what treatment was adopted in the houses which were evacuated where there had not been plague cases?—Practically the same.

24,437. Disinfection and opening up?—Yes; and another additional step was taken, namely, the official organisation whitewashed all the houses in which plague had occurred after they had been disinfected, and a general order was given to the populace that, in the case of houses not actually infected with plague, the owners should whitewash them. Nobody was allowed to return without whitewashing their houses.

24,438. Have you any towns in your district where complete evacuation was carried out?—We had no towns; we had villages.

24,439. Can you remember any examples of the complete evacuation of villages; do you remember the general results sufficiently to be able to tell us whether complete evacuation was more promptly successful than the partial evacuation which you have described?—There was one village near Surat, called Umra, which was evacuated, and a place called Adajan which was evacuated.

24,440. Let us take these two places. What result did you obtain by that complete evacuation?—I remember the case of Adajan more particularly as I paid several visits there. I was afraid it would be a stepping stone between Rander and Surat. I cannot say that evacuation completely stopped plague, as dropping cases went on afterwards.

24,441. How do you account for these dropping cases?—Some of the people were infected before they left the village; others, or the same people, took out infected kit, which had belonged to people who had taken plague in the village, and then they developed plague when they got outside. Their friends and neighbours went and visited them, and if one of them died, his caste people came and sat round him and lamented, and accompanied the corpse to the burning pyre.

24,442. There were a few dropping cases?—Yes, cases went on.

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24,443. You spoke of people having gone out who were already infected; these would develop plague in the next few days after evacuation?—I cannot say that. It went on. It is not as if cases occurred within ten days and then stopped. It went on and on.

24,444. You found there was a great deal of inter-communication?—Certainly. When people are camped out it is impossible to stop them.

24,445. Inter-communication with the infected places—with Surat itself?—Yes; people used to go into Bandar from Adajan, which is two miles away.

24,446. Have you had any experience of inoculation?—None at Surat, because there was none. At least, I remember two cases. The result of one was rather unfortunate.

24,447. Had you any experience of inoculation at Bulsar?—Yes. That was during the second epidemic there. There was a very keen and intelligent Parsee Assistant-Surgeon, Dr. Dadachanji. There were two distinct epidemics at Bulsar, the first of which, as I said, was run by Captain Dyson, and the second was run by the Assistant-Surgeon, Dr. Dadachanji. For a long time the latter managed to keep plague out of Bulsar. With my permission he put on very strict quarantine and for a very long time kept plague out. But at last a man sneaked away from Bulsar to go to a marriage party at Bilimora. He came back and developed plague, and started the plague in Bulsar after three months safety through quarantine. When it was finally started, Dr. Dadachunji asked to be allowed to start inoculation, and I said if he could get the consent of the people, so that there would not be any ructions, I had no objection, and consequently he did get some people, who had been instructed by Professor Haffkine, to come up and perform the inoculations. As far as my experience of inoculation at Bulsar goes, it comes to this. I went down and made examinations, and I found the case mortality was about 25 per cent, but plague went on; that is to say, inoculation apparently saved a number of people from dying, but it did not prevent plague from spreading. That is the general impression I got there from what I saw of inoculation.

24,448. What is your general opinion as to the value of disinfection by chemical agencies?—I think it is not worth the money spent on it.

24,449. Upon what grounds do you express that view?—I consider evacuation and leaving the houses thoroughly open for a month equally effective. Unless disinfection is done under skilled supervision, I consider it so imperfect as not to be effective.

24,450. Have you any data to give us which would illustrate what you say?—I can give you the data of the present outbreak in Nasik—the plague which is going on now. We have had no disinfection throughout the district. In the villages, the plan I have gone on is this. I have sent round circular orders to all the Mamlatdars to have weekly reports from the village officers with regard to the mortality of the village. If the mortality rises, or two or three deaths occur in a house, they immediately go and examine the village with the Hospital Assistant, and if they find a case of plague they report it to me. I then send an evacuation order, and the village is turned out at once; the houses are all left open, and if it is a plague house, the doors are taken off and barred with bamboos. The people are not allowed to return until I give a written permit. If they do return they are prosecuted. I do not give a written permit for a month after the last death or recovery from plague. I found that system worked quite as well as disinfection. With regard to what I have said about the imperfectness of disinfection, I speak from personal experience. When I was in Surat, it was my daily work to ride round the city and examine the houses which had been disinfected. I frequently found that a room had been left undisinfected, or, perhaps, the walls had been done and the floor left untouched, or the ceiling had not been disinfected, and suchlike omissions; parts of the house had been soaked, while the rest was comparatively dry. I also found mistakes in the mixing of the solution.

24,451. Have you got any instance of a place in which plague occurred after it had been disinfected by chemical agencies?—I had one or two cases occurring both in Bulsar and Surat, but I cannot give you the names. A few cases of that sort did occur. It is my general recollection.

24,452. Cases in which houses had not been opened up, but only disinfected?—I will not say that—I think both. They had been both disinfected and opened up.

24,453. Then it seems to tell against both?—Yes, it does. The idea of disinfecting without opening up was not adopted at all in Surat. Dr. Dyson when in Bulsar, as he reported to me, did adopt that arrangement. That is to say, he turned the people out for 24 hours, thoroughly disinfected the house, and put them back again. I have a very strong impression that in those cases plague recurred in some of the houses in which the people were put back.

24,454. Have you got any cases in which houses were thoroughly opened up, and in which no disinfection by chemical agencies had been adopted, in which plague had occurred?—No, I have not noticed any instance of that sort.

24,455. What is the period of time required for disinfection by the free access of air and light?—I have always gone on the principle of at least one month; the more the better, but at least one month.

24,456. What, in your opinion, are the most effective measures in dealing with an epidemic?—In my opinion evacuation, and thorough ventilation of all infected houses for a month, kills plague.

24,457. Do you still agree with the whole of the last paragraph in your précis:—"The towns have been saved "by the introduction of the ward system into them all, "and the villages have been dealt with as in Surat by "prompt and entire evacuation with ventilation (but not "disinfection) of houses. I have discarded disinfection, "because of its expense and uncertainty unless executed "under skilled supervision. In my opinion evacuation "and thorough ventilation of all infected houses for a "month kills plague"?—I stated there that "the towns "have been saved by the introduction of the ward "system into them all." That was true when this was written—as a matter of fact apparently the towns had been saved, but unfortunately, since writing this, plague has broken out in Nasik, Igatpuri, and Sinnar.

24,458. What was your reason for discarding disinfection?—I have discarded disinfection because of its expense and uncertainty unless executed under skilled supervision.

24,459. In connection with your plague measures in Surat, did you undertake any operations to improve sanitation?—We closed several thousand pit privies with fresh earth, allowing the householders to construct open privies over them. These pit privies were in the ground floor of the houses either inside or just outside the house wall. They were cleared at irregular intervals; in some instances they had not been cleared within the memory of the present owner, and were very often in the immediate proximity of a well from which water was taken for drinking. An attempt was also made to clear away a crowded rookery inhabited by Golas (rice pounders) by offering them other sites (free of charge) as well as a certain amount of free building material. The attempt failed, as the Golas refused to move, and the Municipality were too poor to apply the Land Acquisition Act.

24,460. (*Mr. Hewett.*) How many villages were infected?—There a great number of villages infected, especially in Chornsi taluka, round Surat. There were two parts of the district in which a large number of villages were infected. There was the Chornsi taluka round the city of Surat, and part of the Bulsar taluka—that is the south of the Bulsar taluka round the town of Bulsar. That was undoubtedly due to traffic between the villages and the two towns respectively.

24,461. Have you any idea of the number of people who left Surat?—Yes, I made a calculation and I estimated that about 10,000 people had gone away. We had a pass system.

24,462. Those who left went away on passes?—Yes, on passes. A few may have gone before, but the great majority went away on passes.

24,463. You do not think many got away without passes?—No, I do not think so.

24,464. How many villages are infected in the Nasik district?—At present about a dozen including three Municipal towns.

24,465. Have there been a number of villages infected in the district at any time?—From the beginning of this epidemic I suppose there have been 25 towns and villages infected altogether.

(Witness withdrew.)

Lieut. A. N. DAVIDSON, I.S.C., called and examined.

Lieut. A. N. Davidson, I.S.C.

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24,466. (*The President.*) I believe you are occupied with plague work in Nasik?—Yes.

24,467. (*Mr. Hewett.*) How long have you been employed here?—Since the 13th December 1897—roughly, 14 months.

24,468. What is the population of Nasik?—At the beginning it was 24,000 or 25,000.

24,469. When you came here in December, 1897, was there plague in the town?—Yes.

24,470. To what extent?—It averaged eight cases a day. The greatest number that I remember in any one day was 13 cases and deaths.

24,471. Can you give the total number of attacks and mortality in the town up to date?—In the first epidemic there were 501 attacks and 384 deaths, and in the second epidemic 182 attacks and 149 deaths up to date.

24,472. How long did the first epidemic in which there were 501 cases last?—It commenced in October. There had been imported cases in the town previously.

24,473. When did the indigenous cases begin?—In October 1897, plague was declared to be indigenous.

24,474. When did the first epidemic terminate?—The last three cases were in March, 1898.

24,475. In dealing with that epidemic, did you make the people evacuate infected blocks?—Yes.

24,476. How much did you evacuate when you got a case of plague?—When we had a few cases in any block we evacuated the block.

24,477. When you evacuated a block, did you put the people into the segregation camp or did you allow them to go where they liked?—A camp was made and they were told they might occupy it. I should think not 10 families occupied it; they went where they wished.

24,478. Did any proportion of the people leave the town altogether?—A very small proportion. I was employed on plague work in the fields for some time.

24,479. Having got the people out of their houses, did you take measures with respect to the infected locality?—Every house was disinfected with perchloride of mercury.

24,480. How long after the disinfection were the inhabitants allowed to come back to the town?—Two or three months, in my experience.

24,481. Did you allow them all to come back?—The people who were turned out first were Musalmans and Kalals. They were out for some time. They would not come in when they were first allowed to do so, because their clothes had to be disinfected.

24,482. You insisted on the disinfection of the clothes on going out and coming in?—Not on going out; on returning, we did.

24,483. You allowed some of them to return within three months, I believe?—Yes.

24,484. After the close of the epidemic in March, did you have any dropping cases?—There were two cases. There was one case in the Collector's compound and one in the Abkari Inspector's compound. They were friends—one a syce and the other an indoor servant.

24,485. How were they infected?—I do not know.

24,486. Did these cases occur in houses in which there had been already cases of plague?—No; there had been no previous cases. The last case in March occurred in a house in which there had been no previous case.

24,487. Did these cases you refer to occur in March?—Towards the end of March.

24,488. After the end of March, when you considered that the epidemic was over, what interval was there before you had another case of plague?—I think we had an imported case in August or September from Trimbak.

24,489. Is that a town in this district?—Yes, about 18 miles from here.

24,490. What did you do when you got an imported case?—We sent it to the hospital, together with the contacts.

24,491. Did you do any disinfection?—There was no disinfection this year. We opened the doors and windows and took off the roofs and barricaded the doors on the ground floor.

24,492. How long did it take for plague to become indigenous the second time?—We had an occasional imported case until 14th January 1899. Towards the end of January it became indigenous. I received information about two weeks previously that plague was being concealed in a portion of the city which did not belong to me, and I reported that to Mr. Menzies.

24,493. Do you remember whether the population of the city, in the interval between August and January, was above or below normal?—About normal, I think.

24,494. The second epidemic is going on at present?—Yes.

24,495. Have you been proceeding on the same principle of evacuating blocks?—No evacuation has been carried out at all. The people have been advised to leave the city.

24,496. What action do you take when you find a case in a particular house?—We have sent the case to the Plague Hospital and we have all the contacts removed.

24,497. You have done nothing to the house?—Only opened it up.

24,498. Since you have adopted this plan of only opening up the house and not disinfecting it, have you allowed any people to return to the houses so treated?—No.

24,499. So that you have had no opportunity of finding out whether it is an effective way of disinfecting houses?—No. People have been working in the house by day and cleaning the house, and no case has occurred among them. None of the people turned out of their houses have gone back to live in them.

24,500. Even though you had gangs working in the house, would it necessarily follow that the house was free from infection?—No.

24,501. I presume you took special precautions with regard to such gangs?—These were private people turned in by the owners of the houses to clean the houses.

24,502. There have been no bad results to persons so employed?—None yet.

24,503. How long have these gangs of coolies been doing this work?—I have only seen it done in one house.

24,504. How long ago was that?—With regard to the case imported from Ozar, the people were removed from their house; after about six weeks they applied for permission to re-occupy it. I gave them permission, but the house-owner and his family did not go to live in the house. He turned in coolies to clean the house. The house was being repaired when first evacuated, and the work which had been discontinued there was again proceeded with.

24,505. In this particular instance the case that occurred was imported, so that it is quite possible that the house was never infected, is it not?—It is quite possible.

24,506. Have you formed any opinion as to how the second infection took place?—I am of opinion that it was caused by the Kalals who go to the Ghoti Bazar, a badly infected village. I think they brought it from there. They are rice dealers, and deal largely in rice.

24,507. How far off is Ghoti?—About 20 miles, near Igatpuri.

24,508. Have you observed any cases of plague occurring during the second epidemic in houses in which it occurred in the first epidemic?—No, but I know as a fact that five have occurred.

24,509. In those five cases that have occurred, did they occur without the possibility of infection having come from outside?—I do not think it occurred from plague lying dormant in the house.

24,510. Has the great majority of cases which occurred during the second epidemic occurred in houses in which there were not cases in the first epidemic?—Out of 200 cases only five occurred in the houses which had been previously infected.

24,511. Have cases which occurred in the second epidemic occurred mainly in the same quarters as those in which they occurred in the first epidemic, or in

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different quarters?—In the same quarters. It is not my part of the city, but I know it is in the same quarter. It has not spread so rapidly into the town.

24,512. Have you noticed any mortality among rats?—No. I do not suppose I have seen 10 dead rats since I have been here.

24,513. Was there any mortality among rats before you came?—I heard of it.

24,514. You heard that there were dead rats in the first epidemic?—Yes, I heard of it. When I have gone round the city I have always looked for rats, and when I saw a dead one I made inquiries.

24,515. What system have you now got for preventing imported cases coming in without being detected?—The people have been warned, since we have started work in June, that they must report every case of arrival from infected places within six hours, and to the best of my belief we get the majority of these. When these cases are reported I see the new arrivals daily for 10 days. Out of, roughly, 8,000 people that passed through my hands from infected and uninfected places I have only had three imported cases of plague.

24,516. You mean you have seen 8,000 of these arrivals?—I have not seen 8,000, because out of that 8,000 5,000 or 6,000 would be from uninfected places. But out of 2,000 or 3,000 arrivals from infected places I have only had three imported cases of plague.

24,517. Do you find that the people are willing to help you to find cases of plague?—Yes.

24,518. Do you attribute that at all to the fact that they have already been previously harried by plague?—Yes. They are desirous of keeping people from the infected part of the city from going into the part we still believe to be uninfected.

24,519. Do you think they assist you more than they would in a town in which there had been no outbreak of plague?—Yes. I think they recognise that it is to their own good to keep it away from their houses.

24,520. Do you find that ordinary cases of death and sickness are reported here now?—Yes.

24,521. Do they report sickness?—Yes. I get reports of such things as children teething. I am speaking now about my part of the city. My opinions are not the same with regard to the Musalmans' end of the city.

24,522. (*The President.*) What is the quality of the houses in the quarters which you have spoken of as being chiefly infected?—The majority of the houses are dirty and dark.

24,523. That is in contrast with the general condition of the houses in this place?—I think the houses where plague chiefly is are especially dirty.

24,524. I suppose they are all pretty dark in the town?—The majority of the houses are dark, and there is no ventilation.

24,525. Was there any overcrowding, do you think, in these houses?—In some of them I think there was.

(Witness withdrew.)

Mr. G. W. Hatch, I.C.S.

Mr. G. W. Hatch, I.C.S., called and examined.

24,526. (*The President.*) You are the Assistant Collector of the Eastern Division of Khandesh?—Yes.

24,527. (*Mr. Cumine.*) I think your experience of plague is confined to the eastern part of Khandesh?—Yes, to the Jalgaon and Bhusawal talukas.

24,528. When did indigenous plague first appear in the Khandesh district?—In November 1897, in Jalgaon.

24,529. Is it known for certain where the infection came from?—There are two stories. One is that it came from Ghoti in the Nasik district, and the other that it came from Sholapur. In both cases Marwaris suffering from plague were supposed to have come and died at Jalgaon.

24,530. It came by human agency?—Yes.

24,531. After the discovery of the first suspected case, did you go and stay at Jalgaon, and watch all the deaths?—Yes, I went and stayed there and watched the deaths.

24,532. Did you mark down on a map* against the houses where they had occurred the deaths that had been occurring throughout the month of November?—Yes. All the deaths in November were marked on the map.

24,533. What is the special lesson that the map seems to teach? Does it seem to show that the noting of the localities where deaths are occurring is even more important than the noting their mere number, for purposes of seeing whether plague is in a town?—The map speaks for itself. There was an excessive mortality during the month of November, and 28 deaths occurred in *this* circle (*pointing on the map*). The average number of deaths for the month in previous years being 33, in November 1897 61 deaths occurred, and the excess (28) occurred in *this* small circle (*pointing on the map*).

24,534. (*The President.*) Plague deaths?—We did not know. By looking at the map one can see whether the deaths occurred in one locality. The probability is that they were due to plague.

24,535. (*Mr. Cumine.*) How many more villages became gradually infected in Khandesh?—Those I know about were Bholana and Pimpralla.

24,536. What was the total number in the Jalgaon taluka?—The total number was three in the Jalgaon taluka, and seven in the Jamner taluka.

24,537. Did the infection come from Jalgaon in those cases?—Yes.

24,538. There was one village became infected in the Amlaner taluka, I think?—Yes, and one in the Chalisgaon taluka.

24,539. Is it known where the infection came from in those two cases?—No, it is not known.

24,540. In what month did the 1897-98 epidemic cease in Khandesh?—In April 1898.

24,541. Did plague appear again this fair season in Khandesh?—Yes, it broke out in Kanaswadi, village within two miles of one of last year's plague-stricken villages—Bholana.

24,542. Will you give us an account of the outbreak in Kanaswadi?—Kanaswadi is a village of 269 inhabitants, almost entirely Kolis. It is distant about two miles from Bholana, and has little intercourse with other villages besides Bholana. The last case of plague at Bholana occurred on 18th March 1898. In November 1898, indigenous plague occurred at Kanaswadi. At that time there was no plague in Khandesh. There is no history of any case having been imported into Kanaswadi from outside the district, and I cannot consider that such a thing would have been possible. Examination of the death register at Kanaswadi, coupled with enquiry among the people, showed that the first death from plague had probably occurred on 1st October, and the second on the 2nd November. There were no deaths in the village between those dates. There was a story of a girl who had come from Bholana in August or September and who suffered from fever and a swollen face, but who recovered. Enquiries at Bholana showed that the village had been free from plague for the last five months. The accompanying extract from the death register shows this fact clearly enough.

Extract from Bholana Death Register
(from April to December 1898.)

Date.	Person.	Age.	Cause of Death assigned.
April 3rd	A Mahar woman,	25 years	Syphilis.
April 4th	A Dhangar child	1 year	——
June 25th	A Koli child	1 day	Bowel complaint.
July 10th	A carpenter	42 years	Paralysis.
July 12th	A Koli child	7 months	Bowel complaint.
August 2nd	A Koli youth	15 years	Bowel complaint.
September 23rd	A Kunali child	1½ month	Accident.
September 25th	A Koli child	1 year	Bowel complaint.
October 26th	A Vaidu child	1 year	Measles.
November 3rd	A Koli child	18 days	Bowel complaint.
December 4th	A Koli child	3 years	Bowel complaint.
December 6th	A Mahar girl	8 years	Fever.

* Not published with the Commission's proceedings.

† In the same house I discovered the girl with buboes on the 8th or 9th December.

However, on the 6th December, a girl had died at Bholana from fever, and in the same house on the 9th or 10th December I found another girl suffering from fever with buboes in the groin. The girl and her family were segregated; and she got well. No other person in Bholana fell ill during December. I took the census of the whole village several times in order to satisfy myself on this point. Nor has there been any outbreak at Bholana since. To my mind the only explanation of the outbreak at Kanaswadi is that the plague germ was imported from Bholana, that it existed at Bholana, throughout the rains, in a weakened form, and that it became virulent again when it got a fresh habitat at Kanaswadi.

24,543. I believe plague spread, this fair season, to several other villages in the Khandesh district. Do you remember how many, approximately?—I fancy some six or seven. It has come in fresh from Hyderabad.

24,544. Will you put in the figures with reference to the outbreaks with which you are best acquainted, viz., those at Jalgaon, Bholana, Pimpralla, Kanaswadi, and Gozora?—The table is as follows:—

Village.	Population.	Date of First Case.	Date of Last Case.	Total.	
				Cases.	Deaths.
Jalgaon - -	15,000	23 Nov. 1897	28 Feb. 1898 -	115*	102*
Bholana -	800	22 Jan. 1898	18 March 1898	30	23
Pimpralla -	3,100	27 Jan. 1898	19 March 1898	26	22
Kanaswadi -	269	1 Oct. 1898	8 Dec. 1898 -	25	21
Gozora - -	950	30 Nov. 1898	1 Feb. 1899 -	24	22

* To these figures should be added about 40 cases and deaths that occurred during October, November, and 1st week of December, *i.e.*, before there was any organization for discovering and recording cases.

24,545. With regard to evacuation, what are the lessons you think it teaches?—I have noticed three points: the first was, that partial evacuation is useless to stop the spread of plague unless it follows on the early detection of the first imported case. There have been several examples of that in the villages I have seen. In Jalgaon the evacuation of the quarter in which an excessive number of deaths had occurred was found to be useless. In the next few days cases occurred just outside the evacuated area, although we had left a considerable margin. At Bholana, when the village was evacuated, two families, in which were women about to be confined, were allowed to remain in the village. Their houses were distant from the infected quarter. Within the next ten days a case of plague occurred in each family. At Gozora a man died suddenly from fever on the 30th November. A fortnight later I heard that this man was a visitor from Kanaswadi (an infected village). I went to Gozora, disinfected the house in which the man died, and segregated the one person living in it. Fifteen days later, that is on 31st December, a death occurred in the next house; for the next five days a death occurred each day in neighbouring houses. On 7th January I went to Gozora and found a case of plague, Inquiries showed that the six deaths, from 31st December to 5th January, were all probably due to plague. It is noteworthy, that exactly one month elapsed between the death of the imported case and the first indigenous case. Between 30th November and 31st December only one death—of an old man from asthma—occurred in the village. On the other hand, I have seen many cases in which prompt evacuation of a few houses following on the discovery of the imported case has prevented the plague catching hold of the village. In all these cases the contacts were segregated and the houses disinfected within 48 hours of the death of the imported case.

24,546. I believe you have some charts which you wish to put in?—Yes.*

24,547. In the town of Jalgaon, I believe, plague was bad in November, and the town was evacuated at the beginning of December. When did the attacks finally cease?—The last case was on the 28th of February.

24,548. So that the plague took about three months to stop after evacuation?—Yes. We had only six cases throughout February.

24,549. You found that evacuation decreased the mortality?—Yes, it brought it down at once, as the charts* show.

24,550. (*The President.* Do the examples which you have mentioned of total evacuation apply to villages into which plague had only recently been imported?—In Jalgaon plague had been in the village for quite a month before evacuation.

24,551. Could you show us, from the Jalgaon chart,* what the effect of evacuation was?—There were two evacuations. We, at first, left in one-sixth of the town, but turned it out a month later. We left in one detached portion of the town. It did not seem necessary to touch it, as it was right away from the other part. Afterwards, however, we found deaths began to occur in the unevacuated portion, and we turned that out also.

24,552. (*Mr. Cumine.*) What is the population of Jalgaon?—15,000 roughly.

24,553. Is there a large trading community there?—Yes, it is the centre of the cotton trade.

24,554. When you turned all the people out, did you institute a system of passes, and put policemen upon the roads to attempt to prevent the people from leaving Jalgaon without permission?—Yes, that was done.

24,555. Was that successful?—I do not think it was; the people got away as much as they liked.

24,556. Did they carry infection to the other villages in their flight?—They carried it to the Jamner taluka. The place (Jalgaon) was too big to deal with properly; one could not keep an eye on 15,000 people in the fields camped all over the place.

24,557. What do you think is the size of a village you can evacuate so as to keep control upon the people, and stop plague within a few days, and prevent the people running away and infecting other districts?—I should say a village of not more than 2,000—a village where you can take a roll call every day and see that all the inhabitants are there.

24,558. Was taking a roll call found to be specially useful?—I have found it completely effective. Nobody ever ran away. In these other three villages of which I have charts*, Bholana, Kanaswadi and Gozora, I used to go round and take the roll call every morning; I never found a single man missing.

24,559. Were there any merchants or shopkeepers in those villages where the roll call system was successful?—There were a few, just one or two families.

24,560. Do you think, in the black soil of Khandesh, people could be turned out in the heavy rains without causing great hardship to them?—No, I do not think they could.

24,561. One chart* you have put in refers to the village of Bholana. How was it that plague did not stop immediately in that village?—In the case of Bholana the cases that occurred after the people had been out in the fields for a week were all traceable, either to contact with patients in the hospital, or to residence in the village subsequent to the general evacuation. There were two families allowed to stay in the village, and there were people who attended on their friends in the hospital. They were the only people who got plague there after the first week. At Kanaswadi there was not a single case after evacuation.

24,562. Will you describe the way in which you prevented the spread, to other places, of plague from infected villages?—In all the villages, except Jalgaon, a house to house census was taken at the time of evacuation. Subsequently, a daily roll call was called of all the people camped in the fields. No cordon or police guards were required. People knew their roll call would be taken, and they did not attempt to bolt. The result was most satisfactory. Neither Bholana, Pimpralla, Kanaswadi or Gozora became a fresh centre of infection. Bholana and Pimpralla were infected from Jalgaon. I have already given an account of the way in which Kanaswadi became infected. Gozora was infected from Kanaswadi before it became known that plague had broken out at the latter village.

24,563. Are there any particular points connected with the Jalgaon measures to which you wish to draw our attention?—Jalgaon is a large centre of the cotton

* *See* App. No. LXVII. (A) to (D) in this Volume.

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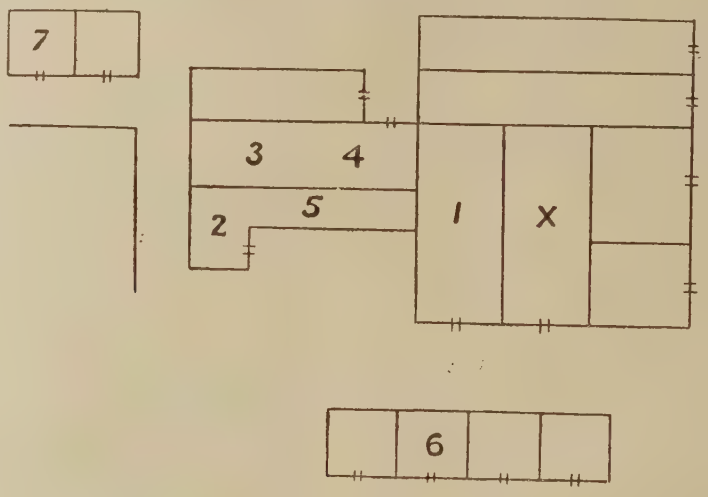
trade; it has a mill employing 1,000 hands and some 20 cotton cleaning and pressing factories. After the first panic on the discovery of the plague it was found possible to carry on the business of the town in spite of evacuation and plague restrictions. A bazar was set up in a field, and the cloth and grain merchants moved their goods and chattels out and opened their shops there and carried on their trade without much inconvenience. The mill put all their employés into camp at the first alarm of plague, with the result that not a single case occurred among them throughout the epidemic.

24,564. I think they had a special doctor for their employés, had they not?—Yes. One noticeable point about evacuation is that it costs nothing at all in these small villages. The people build their huts of karbi (*i.e.*, jowari straw).

24,565. Was there any striking example at Jalgaon of the way in which an influential native can get the people to go out if he leads the way?—Yes, we had several examples of that. It was all done by the influence of two or three native gentlemen.

24,566. When the Patel of the Kunbis went out, did not the whole population of Kunbis follow him?—Yes.

24,567. Do you put in a plan of the infected quarter of Gozora?—Yes, the plan of the infected quarter of Gozera, showing the order in which the deaths occurred, is as follows:—



× marks the house in which the visitor from Kanaswadi died suddenly of "fever" on 30th November 1898.

1 was a Koli girl, aged 5, who died on 31st December 1898.

2 was a Koli boy, aged 11, who died on 1st January 1899.

3 was a Sunar girl, aged 12, who died on 2nd January 1899.

4 was a Sunar woman, aged 41, who died on 3rd January 1899.

5 was a Koli, aged 40, who died on 4th January 1899.

6 was a Koli, aged 18, who died on 5th January 1899.

7 was a Koli girl that I found suffering from plague when I visited the village on 7th January.

24,568. You looked after some villages yourself, did you not?—Yes, Bholana, Gozora and Kanaswadi; I was there all the time.

24,569. What is the number of villages which, you think, one European can, personally, look after efficiently?—If he has to do all the superintendence himself he cannot possibly look after more than two or three, allowing two or three hours' work in each village.

24,570. (*The President.*) Have you adopted any system of disinfection in connection with your operations?—Yes. In all these villages, as soon as the evacuation was complete, the roofs of the houses were opened. In the small villages we took the roofs completely off. The houses were then disinfected with perchloride of mercury.

24,571. How long was it before the people were allowed to return to their houses?—We kept them out until the hot weather, about two months after the last case.

24,572. Have you any examples of any recurrence of plague in houses which had been so treated?—No, not after they were allowed to go back to the villages.

24,573. Are these villages now free from plague?—Yes, I have mentioned the case of Bholana. I found a case at Bholana this year, one case.

24,574. With the exception of that instance, they have all remained free?—With that exception, they have all remained free.

(Witness withdrew.)

(Adjourned till Monday, March 13th, at Bombay.)

At The Secretariat, Bombay.

Monday, 13th March 1899.

SIXTY-FOURTH DAY.

PRESENT:

PROF. T. R. FRASER, M.D., LL.D., F.R.S. (*President*).

Prof. A. E. WRIGHT, M.D.
Mr. J. P. HEWETT.
Mr. A. CUMINE.
Dr. M. A. RUFFER.

Mr. C. J. HALLIFAX (*Secretary*).

Mr. G. MULLANNAH, re-called and further examined.

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24,575. (*The President.*) You, I think, have some information to give us in regard to experiments made on cotton* ?—Yes.

24,576. Will you be good enough to detail them?—I received six samples of cotton on the 22nd of January 1899.

24,577. Where did they come from?—They came from Kopbal.

24,578. Was there anything connected with plague in regard to this cotton?—The samples were supposed to have been taken from houses where plague cases occurred.

24,579. It occurred in each house?—Yes.

24,580. How long, before the cotton was taken from each house, had there been plague in the house?—I do not know that. I took a little bit of cotton from each of these four samples, and inoculated four broth tubes. Next day there was growth of a number of organisms in each tube. I examined the slide preparations from each, and among these four tubes I found one which contained polar staining bacilli. That cotton was from a place called Sampur. On the 23rd of January, therefore, I inoculated the rabbit with a half c.c. of this broth, in which polar stained bacilli were found. The rabbit died on the 25th; it was found dead in the morning about six o'clock when the laboratory was opened. At the *post-mortem* examination, we found that the inguinal and the axillary glands were enlarged, and that the lungs were congested. In the spleen, and in the lung, there were a few polar stained bacilli found, but the gland was full of polar stained bacilli. These polar stained bacilli were of different shapes; some looked like plague, but others were narrower and longer. I have a slide preparation of it here. (*Slide produced and handed in*). I made cultures from gland, blood, spleen, and lungs, but from the gland I had pure growth of cocco bacilli (yellow). From the lungs I got two different kinds of organisms, both polar stained, but one was motile, and the other not. Both, when grown in broth, made the broth cloudy, so I concluded that these organisms were not plague. I then inoculated two more rabbits with these.

24,581. From the original broth?—No, with the bacilli which were obtained from the lung. The rabbit which was inoculated with the culture of cocco bacillus did not die.

24,582. (*Dr. Ruffer.*) How much did you inoculate?—Two c.c. Four c.c. of broth was mixed thoroughly with the growth on one agar tube, and, out of that, two c.c. were used. I inoculated two rabbits, Nos. 154 and 155, with non-motile, and motile polar staining bacilli respectively. No. 154 was inoculated on the 4th with 2 c.c., and died on the 17th. The glands were not enlarged, there was no congestion of the lungs, and there were no signs of plague; it only died of septicæmia, and the polar staining bacilli were found in the blood and spleen. Rabbit No. 155 was inoculated on the 4th, and this died on the 12th, of septicæmia. In these animals neither of them had the glands enlarged, nor congestion of the lungs, but the polar stained bacilli were found in both of them. The rabbit inoculated with the cotton was suspicious. There were *post-mortem* signs of plague, but I could not get a growth of bacilli which I could call plague bacilli. The bacilli I cultivated from it were not plague. Probably there were more organisms in the cotton than these bacilli, and perhaps there might have been plague bacilli as well; but I could not give any definite conclusion about that, as I did not get a culture of plague from it.

24,583. The *post-mortem* appearances, and the morphological appearances, were those of plague?—Yes.

24,584. But the further tests did not confirm you in your opinion?—No, not in my opinion.

24,585. So that you can draw no accurate conclusion?—No, personally, I cannot.

24,586. On the whole, therefore, you have not succeeded in finding the bacillus in cotton?—No.

24,587. Are these all your observations in regard to cotton?—Yes.

24,588. You also made some experiments in order to test the prophylactic value of Haffkine's prophylactic fluid?—Yes.†

24,589. Would you kindly narrate these experiments, and the results of them generally?—Thirty rabbits were inoculated with contaminated Haffkine's fluid; out of these, 20 recovered.

24,590. (*Prof. Wright.*) What does contaminated mean? Artificially contaminated?—The fluid, as it is received from Bombay, was examined, and proved to contain septic organisms.

24,591. (*The President.*) A fluid in which you found living organisms?—Yes.

24,592. You did 30 experiments with the so-called contaminated Haffkine's fluid?—Thirty-eight; and 30 out of these were subsequently inoculated with plague, from which 20 recovered.

24,593. You first injected this prophylactic, and then there was a certain interval; what was the interval?—The interval varied; sometimes it was one week, and sometimes it was two months. In one instance there was more than two month's interval.

24,594. You then administered the plague virus by sub-cutaneous injection?—Yes, generally, but a few cases had a loop-full of pure culture of plague rubbed into a scratch made.

24,595. What was the general result?—Twenty recovered and 10 died. Among the 10, two died accidentally, and eight died of plague; besides these, from the protective itself, eight died.

24,596. What was the smallest dose of the protective administered in those eight experiments?—To those which had prophylactic alone, the smallest dose ad-

* *See* App. No. LXVIII. in this Volume.

† *See* App. No. LXIX. in this Volume.

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ministered was 20 c.c. To those eight that died of plague the smallest dose administered was 1 c.c.

24,597. Twenty c.c., in a rabbit of average weight, appeared to kill the rabbit?—Yes, 20 c.c. in a rabbit of the weight of 1,500 grammes.

24,598. Twenty c.c. of protective in a rabbit invariably produced death?—No, in eight only of 38 cases.

24,599. Did you do any other experiments with the protective alone, except these eight? In all your experiments with contaminated protective fluid, did all the animals die?—Not all the animals; only those eight out of 38 cases.

24,600. The other 30 received both virus and protective. How many experiments were done with the protective alone?—Eight experiments were done with the prophylactic alone (if I am allowed to call them so). The eight animals, unfortunately, did not survive the effects of the prophylactic, to be further experimented upon with plague.

24,601. Therefore, all the experiments which you made with the protective alone, resulted in death?—All the 38 rabbits were inoculated with the contaminated protective fluid alone first; out of these, eight died from the effects of the protective. The remaining 30 survivors received subsequent inoculation of plague.

24,602. Now with regard to the re-sterilised fluid?—There were ten rabbits inoculated, and out of these, nine recovered after subsequent inoculation with plague. One died, and this one had a dose of one c.c. only.

24,603. What was the interval of time between the inoculation with Haffkine's fluid, and with the plague virus?—The rabbit, No. 140, for instance, had protective fluid on January the 19th, 1899, one c.c., one dose, and a second dose on January the 21st, 1899, two days afterwards, and it had plague on the 29th, eight days after the second dose.

24,604. Can you tell us what were the intervals of time between the inoculations?—From four days to about a week.

24,605. The interval between the last inoculation of Haffkine's fluid, and the administration of the plague virus, varied between what period of time?—It varied from four days to 14 days. None of the rabbits in this case died from the protective alone.

24,606. How many rabbits did you experiment with?—Ten.

24,607. In what doses?—In doses varying from one c.c. to 20 c.c.

24,608. Therefore, the 20 c.c. of the re-sterilised fluid did not produce death, whereas the 20 c.c. of the non-sterilised so-called contaminated fluid produced death?—Yes, in eight cases out of 38.

24,609. Can you tell us if the dose of virus which you administered was, so far as you were able to ascertain, near the minimum lethal dose, or much in excess of it?—One c.c. is about one and a half loop-full. Some rabbits recovered from one loop-full and some did not. Usually, with one c.c., every one used to die; that is the minimum lethal dose, I found.

24,610. What is the relation between one loop-full, and one c.c.?—One c.c. is equal to one and a half loop-full.

24,611. Therefore one half more than a loop-full was always fatal?—Yes.

24,612. With one loop-full they recovered sometimes?—Yes, but in these experiments, where only one loop-ful was used, the culture was rubbed into a scratch made, and one cannot say how much is really absorbed.

24,613. What experiments did you do with only one loop-full?—Experiments Nos. 72, 73, 74, 76, 77, and 79.

24,614. What was the interval of time between the inoculation of the protective and of the virus in these experiments; were they varying or all the same?—The same, about five days.

24,615. Five days in each case?—Yes.

24,616. And with an interval of five days between the two; what was the minimum quantity of Haffkine's fluid which appeared to save life?—Twenty c.c. was the only dose used in all these experiments.

24,617. Did you do any experiments on the same conditions with less than 20 c.c.?—No, not with one loop-full.

24,618. Twenty c.c. is, relatively speaking, a large quantity?—Yes.

24,619. Do you think very much less would have been successful?—I think so. There are three in the list. 153 with 12½ c.c. and 136 and 137 with 6¼ c.c. each.

24,620. Were there any serious symptoms in those experiments in which 20 c.c. were employed and the animals recovered? Did the animals suffer badly?—The symptoms were serious; the temperature was very high.

24,621. Was there any other serious symptom other than the temperature?—Yes, local symptoms, abscesses, and ulcers.

24,622. Was the animal very ill?—Yes.

24,623. Was it lethargic; was there any paralysis?—There was no paralysis.

24,624. Did it eat its food?—No, it became very weak and reduced in weight, and laid about.

24,625. Therefore 20 c.c. was a great deal too much in excess of the quantity required?—Yes, we found 2 c.c. protected in one case, No. 162, but the fluid was more or less pure, not nearly so contaminated as that of earlier date.

24,626. What experiment did you do with 2 c.c., where the conditions were the same?—Experiments No. 161, 162, 163, and 164.

24,627. What were the conditions of those experiments?—These rabbits were inoculated with 2 c.c. of more or less pure fluid, only one dose, and then after about nine days they were inoculated with one c.c. of plague, and all of them recovered.

24,628. Have you used a smaller quantity?—Yes, we used one c.c., and the animal died.

24,629. Anything between one c.c. and two c.c. is sufficient to prevent death?—I do not know; we have no experiments on this point.

24,630. Assuming that in a rabbit two c.c. is the quantity required, how much would be required for a man, also assuming that the effect is equal in both cases?—About 90 c.c., according to weight, if the power of different constitutions be not taken into consideration.

24,631. Have you any experiments that seem to indicate the duration of the protection?—We have here one experiment to show that the protection lasted for two months.

24,632. What were the conditions of that experiment?—Rabbit No. 71 was inoculated with 20 c.c. on the 19th of November 1898, and it was inoculated with plague on the 21st January 1899.

24,633. What was the interval?—Two months and two days.

24,634. What was the dose of virus?—One c.c.

24,635. The quantity of protective that you found necessary was ten times greater than when the interval was nine days?—Yes, but less fluid might have had the same duration of protection, but we have no experiments on this point.

24,636. Supposing 20 c.c. is required to produce protection for a period of two months in a rabbit, how much would be required for a man?—Ten times 90.

24,637. Have you any experiments that show that less than 20 c.c. would produce protection for less that two months?—No.

24,638. Have you any experiments, to show protection in one month?—No. We have not done any experiments to find out that a certain amount of fluid has a definite duration of protection.

24,639. Have you any experiments excepting the one you have referred to, which indicate the time for which any given quantity will produce protection? In this instance you have an experiment showing protection for two months. Have you any experiments which show protection for either a longer or a shorter time than two months?—There are many of shorter.

24,640. How much shorter; what interval of time?—Only a week or two.

24,641. They were only a week and two months?—The duration varies from five days to two months and few days. No. 111 had a duration of 24 days.

24,642. In what condition was the Haffkine's fluid when you first used to obtain it?—All contaminated.

24,643. Contaminated with what? — With living organisms.

24,644. Organisms of putrefaction?—Yes.

24,645. Did you find that any improvement took place latterly?—Yes.

24,646. In what respect?—Latterly, there were no organisms in some, and others were much purer.

24,647. Was it absolutely pure latterly? — Some bottles were absolutely pure.

24,648. Not all were absolutely pure?—No, not all.

24,649. What effects did you observe from this impure fluid?—Abscesses, ulcers, septic intoxication, and the reactionary fever.

24,650. Did you get death in any instance?—Yes, in eight cases out of thirty-eight, before referred to.

24,651. What kind of symptons?—Toxic.

24,652. Not suggestive of plague toxæmia?—No, septic toxæmia or intoxication.

24,653. What was suggestive in the reactionary fever?—Plague toxines.

24,654. I understand that you yourself, finding that you had living organisms, took some measures to get rid of them?—Yes, under the orders of the Plague Commissioner.

24,655. What did you do?—I heated the fluid for three consecutive days, for a quarter of an hour, at 60 degrees Centigrade.

24,656. What was the result of that treatment?—The fluid became sterile.

24,657. There were no longer any living organisms?—No.

24,658. Did you make experiments at the same time with this fluid alone?—Yes.

24,659. What kind of symptoms did you obtain with it?—We never had abscesses or ulcers, and always had a temperature lower than with the contaminated fluid.

24,660. A lower temperature and no local symptoms?—No local symptoms and no deaths too.

24,661. What was the maximum dose which you injected of the re-sterilised fluid?—Twenty c.c.

24,662. That, again, was a dose which, you have already stated, was generally fatal with the contaminated fluid?—Contaminated fluid was fatal in eight cases out of 38.

24,663. You found that it was not so toxic; what did you find in regard to it protective power?—It had a protective power.

24,664. In an equal or in a less degree?—In one case it was more powerful, because the same dose killed the rabbit in the case of unsterilised. In the sterilised, it saved the animal under equal conditions.

24,665. Was there any increase in protective power?—Certainly in one case, No. 122, compared with 128.

24,666. (*Dr. Ruffer.*) You say in Clause A of your Memorandum* that you inoculated with plague animals protected with re-sterilised Haffkine's fluid, and that nine recovered. Did you inoculate control animals of the same weight at the same time?—Yes.

24,667. What happened to the animals?—The controls died.

24,668. In each case?—Yes.

24,669. After how long;—After three or four days.

24,670. To re-sterilise the fluid, you say you heated it to sixty degrees Centigrade for a quarter of an hour for three consecutive days. How did you ascertain the temperature of the fluid when you heated it?—With a thermometer.

24,671. Where did you put the thermometer?—In the water surrounding the fluid.

24,672. How did you know that the inside of the bottle had reached 60 degrees? It takes some time for the heat to penetrate through the bottle to the fluid inside?—A few minutes were allowed for the heat to penetrate through the bottle to the fluid inside before we began to reckon the required time.

24,673. How long did you keep the bottle in the water?—For a quarter of an hour.

24,674. You think that sterilised it?—Yes. It did sterilise it.

24,675. Did you make any experiments to find out if it was sterilised?—Yes, by inoculating agar tubes with the fluid and no growth occurred in them.

24,676. How much fluid did you take?—Twenty or thirty drops.

24,677. In Class B of your Memorandum,* when did you look for contaminations? Did you look for them at the time you inoculated the fluid into the animals, or some time before?—In some cases it was done before, in others afterwards, whenever we wanted to examine any fluid, but, generally, at the time of inoculation, when the bottles were opened for inoculating purposes, the agar tubes were inoculated with the fluid.

24,678. You think that is a good method?—Yes.

24,679. You say in your remarks*:—"As received by "us originally, Haffkine's fluid was, without exception, "contaminated with organisms of putrefaction"?—Yes.

24,680. How many bottles of Haffkine's fluid did you get originally?—I cannot say the exact numbers off-hand, but it was more than 20 dozen.

24,681. Did you make a bacteriological examination of each one of them?—Of each one used in the experiments.

24,682. You say you did not examine every bottle. I want to know how, then, you can say that "without exception" they were all contaminated? — Generally one bottle from each brew was examined, other times two or more from each brew.

24,683. How can you say they were contaminated without exception, if you have not examined every one?—We examined one bottle from each brew.

24,684. You say it was contaminated, without exception, and I want to know how you can say that if you did not examine each bottle?—It means every on without exception, of those that were examined.

24,685. In your statement* you say "When employed "by inoculation in rabbits, animals which are pecu-"liarly resistent to ordinary septic processes." Can you tell me on what evidence you base that statement?—Rabbits are supposed to be immune from septic organisms.

24,686. Why?—It is a generally accepted opinion of scientific men. Our first series of experiments (control) prove it; besides, I have inoculated some rabbits, and have seen them immune; they do not die, they get abscesses, and recover afterwards.

24,687. From what organisms? — Staphylococcus pyogenes aureus and albus.

24,688. How much do you inoculate?—Twenty c.c. of pyogenic cocci in broth.

24,689. Did you inoculate the same quantities in other animals than rabbits?—No.

24,690. How can you say they are peculiarly resistant if you made no comparative experiment; on what do you base that statement?—I know, for certain, that if 20 c.c. of pyogenic cocci be inoculated into a rat, it is quite sure to die. So I did not think it is worth while to do such an experiment.

24,691. How much does a rat weigh?—I do not know.

24,692. A rat would weigh about 200 grammes at most, and a rabbit possibly ten times as much. What do you think reactionary fever is due to?—The toxines of plague.

24,693. Then you do not think it is due to plague itself?—No.

24,694. Did you ever find the plague bacillus in any animal inoculated with the prophylactic alone?—Yes, once.

24,695. Did you find plague bacillus in that animal?—Yes.

24,696. How did you ascertain that the animal died of plague?—From *post-mortem* appearances of plague, and the organisms isolated from that animal proved to

* *See* App. No. LXIX. in this Volume.

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be plague bacilli when further tests were applied to them.

24,697. How long after the inoculation did the animal die?—The rabbit, No. 96, was inoculated on the 14th December, and died on the 20th; it died six days after inoculation.

24,698. Were you experimenting with plague bacilli in your laboratory at that time? Were there any other experiments going on with plague in the laboratory during that week?—No, not at that time nor a week following.

24,699. There had been, surely. I see you inoculated an animal on the 1st of January, and also one on the 12th of December?—The 1st of January is more than two weeks from that date.

24,700. I see you inoculated five animals on the 12th of December?—Yes.

24,701. Was that animal anywhere near the other animals?—No, it was in a separate cage.

24,702. It was in the same room?—Yes.

24,703. Is it possible that it might have been infected from another animal?—No, it was in a separate cage. There were a good many rabbits, and others must have died of plague also.

24,704. A good many did die?—Only those inoculated with plague died of plague.

24,705. You state that one of these animals died of enteritis, in experiment No. 72?—Yes.

24,706. Did you find the plague bacillus in that animal?—No.

24,707. No. 73 died of tetanus; did you find any plague bacilli in that animal?—No.

24,708. Then these animals should be excluded?—Yes.

24,709. Then how did they die of enteritis and tetanus after being inoculated with plague?—The above experiments refer to those animals which had a loop-full of pure plague culture rubbed into the scratch made, and they had open sores.

24,710. How does an animal get tetanus unless you put it into it?—It can catch it naturally, from the cage or from the grass or dust in the cage, without the virus being put into it by somebody.

24,711. Where was the animal inoculated?—In the thigh.

24,712. Where did the symptoms begin?—In the leg.

24,713. That looks as if it came from inoculation?—Not at all, if it came from inoculation it would affect the other animals too.

24,714. It looks like it?—I do not see how it does. The other rabbits would have got tetanus as they were inoculated on the same day and at the same time.

24,715. What animals were inoculated on the same day?—Nos. 72, 73, 74, 76, 77, and 79.

24,716. No. 75 died of toxæmia. What do you mean by toxæmia in that case: what were the symptoms?—There were no organisms to be found in any organ; and we made cultures, and did not get any growth.

24,717. That is a negative thing; what were the positive symptoms you noticed?—Nothing special, except that it was inoculated with toxines of plague.

24,718. On what clinical symptoms did you base your diagnosis of toxæmia? If you make a diagnosis you must have based it on some phenomena observed; what were the phenomena?—The animal was inoculated with toxine, and there were no organisms. The cause of death was not found out. As the result of inoculation the animal became very ill, had a very high temperature, and then died.

24,719. (*Prof. Wright.*) You said one rabbit died of plague after inoculation with Haffkine's prophylactic?—Yes.

24,720. How many cages have you for rabbits in your laboratory?—About 26 or 27.

24,721. Do you think that a rabbit has died of plague in every one of those cages?—No.

24,722. Was it an absolutely new cage?—No, it was a burnt cage. If the animal dies in a cage we burn the cage, and afterwards the same cage is used for fresh rabbits. We have all iron cages, and we put them in the fire and heat them when required, and they are then used again.

24,723. The rabbit was put into a cage of that kind?—Yes.

24,724. I suppose it was in a cage on the floor beside other cages?—Yes; it was raised above the floor. We generally keep the cages on rows of bricks, and so they are raised above the floor.

24,725. Are there other rabbits also on that platform?—Yes, on the same level, but not on continuous platform.

24,726. Can the urine from one rabbit fall into another cage?—No.

24,727. Why not?—It would fall on the ground; it cannot get into another cage. All cages are apart from one another, separated with little spaces. The floor of one cage is not continuous with the other.

24,728. Have any rabbits died from accidental causes in your laboratory?—Yes, some that had open sores died of tetanus.

24,729. You have, every now and then, deaths among your animals in the laboratory, have not you—among the animals that you have not been experimenting upon?—There was only one rabbit that died in that way, and that died, as I have said before, of tetanus.

24,730. You have often other accidental deaths, have not you? It is a common thing in a laboratory to have accidental deaths among the rabbits that you keep in stock?—There are only two here; one died of enteritis and one of tetanus.

24,731. Is it not a common thing, in your opinion, for rabbits to die accidentally—the rabbits that have not been experimented upon?—We had only two cases of such deaths.

24,732. In what period?—Over a period of ten months.

24,733. What do you infer from the fact that that rabbit died; do you infer that there were plague bacilli in that particular bottle?—Yes.

24,734. Had you tested that particular bottle?—It was tested here in this way, by inoculation.

24,735. You did not test it microscopically?—Yes, I did.

24,736. Did you find any organisms?—No. Nothing besides cocci and a few slender bacilli.

24,737. You still believe that the bottle contained plague organisms?—Yes.

24,738. Why?—Because it is not always possible to find them with microscopical examination alone.

24,739. Have you any record of that particular bottle; in your list you have the entry "Contaminated"? Would you tell us what you found in that case?—The organisms isolated from that particular bottle were staphylococcus pyogenes aureus and albus and long slender, non-motile and non-pathogenic bacilli.

24,740. Those you found in that bottle?—Yes.

24,741. How did you find out it was plague? After that rabbit died did you get cultures?—Yes.

24,742. Were they pure cultures of plague?—Yes.

24,743. Were there no other organisms?—No.

24,744. How did you know it to be a pure culture of plague; did you inoculate other animals with it?—Yes.

24,745. Did you get a cultural test?—Yes, and I also inoculated pigeons and fowls.

24,746. Why?—Because it might have been mistaken for fowl cholera, and I inoculated the fowls to see whether fowls could be inoculated with this.

24,747. What control animals did you have for these various experiments?—Healthy rabbits.

24,748. Did you use a control every time you inoculated a series of animals?—Generally one control rabbit; but sometimes we had six.

24,749. Why are these controls not entered on your list?—It is not entered in this list, which was simply a list of particular experiments put together for the information of the Commission; it is entered in the usual records which I have with me now.

24,750. You say that every one of the controls died throughout the whole of your experiments?—Yes, except those inoculated with a loop-full; among those, two controls recovered.

24,751. Why are these controls not entered on your list; is it simply an accidental omission?—They are not entered.

24,752. Did you, in every case, find that heating to 60 degrees three times effectually sterilised the fluid?—Yes, in every case.

24,753. (*The President.*) You have one rabbit in your experiments in which the conditions produced were very suggestive of plague?—Yes.

24,754. Buboes were produced by the culture of the cotton?—Yes.

24,755. Where was the inoculation performed?—In the thigh.

24,756. In the right or left, or both?—We generally do it on the left.

24,757. Do you know where the buboes were in that case?—In both the groins, and in both axillæ.

24,758. Was there any time succession in the appearance of these buboes, or did they appear simultaneously?—I cannot say, as it is not possible even to feel them during life.

24,759. In regard to some questions you were asked in reference to re-sterilization, you have subjected your fluid to a temperature of 60 degrees for a quarter of an hour, three times?—Yes.

24,760. I understand the fluid was contained in a flask, which was plunged into water, the temperature of which you ascertained?—Haffkine's bottles were heated in a metallic vessel containing water.

24,761. How large was that vessel; what was the capacity of it?—Just large enough to hold about ten bottles of Haffkine's fluid.

24,762. What do you mean by the toxines of plague, as distinguished from plague itself?—One is the product of the other. When you say plague, it may mean bacilli; an animal dying from plague bacilli will be a source of infection.

24,763. Plague bacilli, plus toxines?—Yes.

24,764. They are both plague—the toxines as much as the bacilli?—Yes, but the one being a chemical substance can never be infectious, whereas the other, being living, is always infectious.

24,765. (*Prof. Wright.*) Did you choose the six bottles of prophylactic, sent to me, at Calcutta, for testing purposes?—Yes.

24,766. Were those fair samples of the bottles which you had?—Yes.

24,767. You have spoken of two kinds of bottles; you had one earlier series, which were found very contaminated, and one later series, which you found less contaminated?—Yes.

24,768. Were the bottles which you sent to Calcutta taken from the earlier series?—Yes.

24,769. Were all the bottles which you used in your experiments contaminated?—Yes.

24,770. What about the later series of bottles; did you get them after the Commission had left Hyderabad?—Yes, after the Commission had left.

24,771. Did you get a series of different brews then?—We received bottles of different brews on two occasions.

24,772. You found those a great deal less contaminated?—Yes.

24,773. Did you find any of them contaminated?—Yes, some; but less contaminated. One day I examined six bottles; three were contaminated, and three were not contaminated.

24,774. (*The President.*) Do you think that plague has varying degrees of infectiveness?—Yes.

24,775. In the statement of experiments handed in,* it is stated that "Plague is a disease which shows "itself in many forms. In its virulent forms it is "highly infectious, in its mild aspects it is hardly "infectious at all. To make inoculation universally "acceptable what is now wanted is that Haffkine's "fluid shall be made of uniform strength, so that with "a minimum dose the re-actionary fever shall be of "known intensity and protective power, and not under "any circumstances contagious." Have you any evidence in support of the statement that plague is not contagious in some form?—I think every form is contagious.

24,776. Who wrote this statement about plague?—Dr. Lawrie.

24,777. Do you adhere to it?—No, I do not adhere to that.

24,778. Why did you sign it?—It is mentioned, in brackets, that I do not agree with it.

24,779. That refers to a different point from what I have examined you upon. It is a question of contagiousness. I understand you do not adhere to it?—No, I do not adhere to it.

24,780. (*Prof. Wright.*) Did you do all these experiments yourself?—Yes.

24,781. Did Dr. Lawrie take any part in the experimental work?—No he simply ordered me to do it, and supervised it and verified all the results.

24,782. And then he wrote the report?—Yes.

24,783. (*The President.*) Look at experiments Nos. 166 to 173. Are these experiments in which 2 c.c. of the sterilized fluid were injected?—In half of them it was re-sterilized fluid, and in the other half more or less pure.

24,784. When was plague inoculated?—Eleven days afterwards.

24,785. What has been the general results of these experiments?—I do not know the results.

24,786. You have just heard by telegram that Nos. 166 to 173 are all alive?—Yes.

24,787. Therefore, in each case, the 2 c.c. has been successful?—Yes.

24,788. What was the dose of the virus?—One cubic centimetre.

24,789. Do you know of any control experiments made before this series?—Yes.

24,790. The telegram says that they are all dead?—Yes.

24,791. Then there is experiment No. 112. Kindly look at that. How does this differ from the series we have now been speaking about?—In the amount of the dose, and the period of time.

24,792. Is the dose larger or smaller?—Ten times larger.

24,793. The dose of what?—Of Haffkine's contaminated fluid.

24,794. Is there any other difference?—The time.

24,795. What is the difference in time?—No. 112 was inoculated with Haffkine's fluid on the 28th of December 1898.

24,796. It is the interval of time I want to know. What is the interval of time between the two?—Two months and 12 days.

24,797. What was the interval of time in the case of the other experiments?—11 days.

24,798. Was the dose of virus the same?—Yes.

24,799. The result, in experiment No. 112, is that the animal died, but that experiment is in no way comparable with the others in so far as the interval of time was, relatively, an enormous one?—That is so.

24,800. Let us take another experiment, No. 104. I see the doses are the same; was the interval of time the same as in No. 112?—The interval was two months and 24 days.

24,801. That animal died also?—(Note added by witness on correcting proof of his evidence:—It was alive before I left Hyderabad, and now I see it is still alive.)

24,802. It is not at all comparable; it only shows that 20 c.c. was insufficient when the time is so protracted?—That is so.

24,803. (*Dr. Ruffer.*) We found, in examining the proof of your evidence which you sent back, that you had added several things to your evidence?—Yes.

24,804. The Commission was of opinion that this could not be allowed, but that if you wished to make any additions now to your former evidence you could do so. Perhaps you will kindly state what additions you intend to make. I find that in Question No. 5221 I asked you, "You made a *post-mortem* examination, what did you find"? and your answer was, "Germs

* *See* App. No. LXIX. in this Volume.

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were found in the blood, spleen and glands." Do you wish to add anything to that?—*Post mortem* signs of plague. The glands were enlarged.

24,805. Anything else?—Hæmorrhagic. Plague germs were found in the blood, spleen, and glands. It died of septicæmia.

24,806. Is that all you wish to say?—Yes.

24,807. At Question No. 5231 I asked you, "Did you grow the microbes in gelatine"? and you answered, "No." At Question No. 5233 I said, "You have no knowledge of how it grows in gelatine"? and you replied, "Yes, I have." Then I asked, "How does it grow"? and you replied, "It does not liquefy the gelatine," and so on. The two things are contradictory?—I thought you asked whether I had grown plague gems in gelatine, so I stated accordingly. I have grown plague germs in gelatine in Berlin, but not in India. There is, therefore, nothing contradictory in these two things.

24,808. So that the second answer refers to the Hyderabad plague microbe. You say you have no knowledge that it grows in gelatine. "It" refers to the microbe you isolated in Hyderabad?—To your Question No. 5231, "Did you grow the microbes in gelatine"? my reply was, "No." To your Question No. 5233, "Do you know how the plague germs grow in gelatine"? I replied, "Yes, I have, but not here." Here I had not mentioned the name of the place (Berlin) where I did grow plague germs in gelatine.

24,809. But you do not know how the Hyderabad microbe grows in gelatine?—No. I have not cultivated plague germs isolated in Hyderabad on gelatine on account of heat.

24,810. Then at Question No. 5247 I ask you, "What "is the morphological difference between the bacillus "you found and these other bacilli, the bacillus of "fowl cholera, for instance?" and your reply was, "They resemble one another." Is there anything you wish to add to that?—There is little or no morphological difference under the microscope.

24,811. I said, "What is the morphological difference "between the bacillus found and these other bacilli, "the bacillus of fowl cholera, for instance?" and you answered, "They resemble one another"?—They resemble one another except in the involution forms. If you find peculiar characteristic involution forms of plague there is a difference.

24,812. I asked, "What is the morphological dif-"ference between the bacillus found and those other "bacilli, the bacillus of fowl cholera, for instance." Your answer was, "They resemble one another." Do you wish to add anything to that answer?—Yes. It differs in the involution forms. The plague has particular involution forms.

24,813. You are referring here to the bacillus which you found in the earth?—Yes, that has the same peculiar involution forms as the plague germs of Bombay.

24,814. In answer to my Question No. 5252, you simply say, "By Gram solution." Is there anything else you wish to add to that?—By means of Gram's solution, its characteristic growth in broth, and in broth and ghee, its invisible growth on potato, characteristic involution forms, its very, very feeble or non-motility, and the effects of inoculation on different animals.

24,815. Then, in Question No. 5260 I say, "How is this a 'control experiment' of No. 1"? Is there anything you wish to add to your answer?—The word "control" is really wrong; it is not scientific. These experiments were done only to the mere suggestions; they were controlled to the suggestions made by Dr. Lawrie and others as to the effects on rabbits of septic or other fluids.

24,816. At Question No. 5266 I ask you, "How can 'sterile pus' be described as a 'putrid substance'"? and your answer is, "Our object was simply to note "the effects of inoculation of pus directly from a liver "abscess." Do you wish to add anything to that?—Sterile pus was never described as a putrid substance. I received pus from a liver abscess, but I did not know if the pus contained organisms or not. I found afterwards that there were no organisms in it, so it was put as "sterile pus." The object of inoculation was only for various suggestions, whether you get septicæmia from these substances, and then to see the difference between the bacillus which produces septicæmia, and the bacillus of plague.

24,817. The same addition would do for Questions Nos. 5269 and 5270?—Yes. They were all "control experiments" in that sense.

24,818. Then, at Question No. 5283, I ask you, "Why is your bacillus not the pneumo-coccus"? and you answer, "There are great differences. First of all, the mor-"phology; the pneumo-coccus is a large bacillus. "One can see it at once." Do you wish to add anything to that?—The shape and size of the pneumo-coccus, its capsule, its growth in different media, and the effects of inoculation into different animals.

24,819. At the last I see I gave it up, and I said, "I give it up. I cannot find who is responsible for it." At the time you made no answer to the question?—I have already given the answer before, that I am not responsible for the statement, but I am only responsible for the experiments.

24,820. (*Prof. Wright.*) We have before us a letter from Colonel Lawrie with regard to the finding of plague in cotton.* Did you make the experiments upon which that letter was based?—It referred to the experiments I made.

24,821. Will you please answer my question in the form I put it; did you make the experiments upon which that letter was based? We have a letter from Colonel Lawrie stating that the plague bacillus was found in a sample of cotton sent in for examination by Mr. Stevens. Did you make the experiments upon which that statement was based?—Yes.

24,822. Did anybody else assist you in making these experiments?—No.

24,823. Colonel Lawrie's report is based exclusively upon your experience on this subject?—Yes, but he himself also examined the animals, caltures, and preparations.

24,824. I see you say that the samples were received on the 22nd instant; how many samples were there?—Six.

24,825. Then you say, "Cultures were made in broth from these samples on that day"?—Yes.

24,826. "On the 23rd the broth cultures became cloudy and contained growths"?—Yes.

24,827. Does that mean that you inoculated six broth tubes and all were cloudy?—No; I took only four out of the six samples. I inoculated four broth tubes. The next day we found organisms in all; but one tube contained polar staining bacilli.

24,828. You received six samples, and your experiments were carried out with four?—Yes, on that date.

24,829. In the case of each of these four samples you inoculated one tube of broth?—Yes.

24,830. How were these samples of cotton introduced into the broth; were they received in a bottle?—No; they were in parcels; in envelopes.

24,831. Then there were no precautions taken in shutting them up inside those envelopes?—These envelopes were shut up in a tin case.

24,832. What happened? Did you tear open the envelopes?—Yes, with bacteriological precautions.

24,833. Then you put a piece of cotton into a tube?—Yes.

24,834. And the next day you found all four tubes contained organisms; all were cloudy?—Yes.

24,835. Did you examine the contents of each of these tubes?—Yes.

24,836. And in one you found polar stained bacilli?—Yes.

24,837. You paid no attention to the other three samples?—No.

24,838. You proceeded to inoculate that one?—Yes.

24,839. What happened?—The rabbit had a temperature of 104° and afterwards 105°.

24,840. At 2 p.m. that day three rabbits were inoculated with ½, 1, and 2 c.c. respectively, of broth culture; is that so?—Yes.

24,841. You inoculated three rabbits?—Yes.

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* *See* App. No. LXVIII. in this Volume.

24,842. These rabbits were not new rabbits; they were rabbits which had been previously used, were they not?—Yes.

24,843. Why did you choose rabbits that had been used for previous experiments?—We had no other rabbits; we had no supply of rabbits.

24,844. Therefore, you had to take rabbits which had been previously used for experiments with Haffkine's prophylactic. It says here, that, at 2 o'clock on the 23rd instant, three rabbits, which had been inoculated with culture organisms found in Haffkine's fluid, were taken; those were inoculated?—Yes.

24,845. You did not use new rabbits, because you had no new rabbits?—The rabbits used were not inoculated with Haffkine's fluid itself, but with the organisms isolated from it, and which produced no effects.

24,846. Did one of these rabbits die?—Yes, No. 143.

24,847. It says here that these rabbits were inoculated with ½, 1, and 2 c.c.'s respectively?—Yes.

24,848. The temperature of the first rabbit rose to 105°?—Yes.

24,849. That was the rabbit that had ½ c.c.?—Yes.

24,850. What happened to the rabbits that had 1 c.c., and 2 c.c.?—There was no rise of temperature and they did not die.

24,851. There was no rise in the temperature?—No.

24,852. Supposing you have plague bacillus in a culture, and you give one rabbit ½ c.c., another 1 c.c., and another 2 c.c., which rabbit would you expect to get a high temperature, the one that received the small dose of plague or the one which received the large dose of plague?—One expects higher temperature in an animal which had a larger dose. But the facts here were not so.

24,853. When you made a *post mortem* of the rabbit that had the small dose, you came to the conclusion that it had died of plague?—*Post mortem* appearances were like plague. The glands were enlarged, and there was congestion of the lungs.

24,854. Taking the glands first, have you not constantly found that when you inoculate rabbits with any septic micro-organisms they get enlarged glands?—Only one gland, near the seat of inoculation; I never found all the glands of the body enlarged.

24,855. How large were these glands all over the body?—They were six or eight times the normal size.

24,856. How large altogether, the size of a lentil?—Yes.

24,857. Would that, of itself, have led you to suspect that the rabbit died of plague?—No, there was the condition of the lungs.

24,858. What did you find in the lungs?—There was congestion of the lungs.

24,859. Have you found congestion of the lungs only in plague?—In other diseases too.

24,860. Did the *post mortem* give you any reason to suppose that this was plague?—The organisms resembled plague.

24,861. That is another matter. Apart from microscopic observation, would you have been able to tell that that rabbit died of plague?—Those two characteristic signs were very suggestive of it.

24,862. Those seemed so strong that you think the rabbit died of plague?—Yes, I thought so.

24,863. You have never seen those independently?—I have never seen all these signs together.

24,864. Then you examined under the microscope and found bacteria which you took to be plague?—Yes, they looked like plague bacilli.

24,865. What did you stain them with?—With gentian violet.

24,866. You found bi-polar staining?—Yes.

24,867. Did you come to the conclusion that they were plague bacilli?—I wanted to get a culture of those organisms, and then to see whether they behave like plague or respond to similar tests.

24,868. Did you communicate the fact that you had found this to Dr. Lawrie?—I told him I had found bacilli like plague.

24,869. And did Dr. Lawrie say that they were plague bacilli?—Yes.

24,870. Did he inform the Government that plague bacilli were found?—I think so.

24,871. Were you a party to that; are you responsible for it?—I am not responsible for it, and I was not a party to it.

24,872. Did you authorise Dr. Lawrie to state that?—Dr. Lawrie made the statement on his own authority. I asked him to wait for a few days in order that I, myself, might be sure whether it was plague or not.

24,873. You were not willing to say straight off that those bacilli were plague?—No.

24,874. When you further investigated the matter, did these bacteria turn out to be plague bacilli?—No, not in my opinion, but in Dr. Lawrie's opinion they were.

24,875. You were satisfied that they were not plague bacilli?—Yes, the organisms isolated were not plague.

24,876. You think the report was really based upon insufficient evidence?—Yes, I think so, but Dr. Lawrie does not.

24,877. And your opinion, now, is that there was no plague in the cotton?—I cannot give any definite opinion about that. I could not get any plague germs separated.

24,878. Inasmuch as only the rabbit that got the least of this germ died, do you think it is probable that there was plague in this cotton?—I think it is probable that it contained plague germs.

24,879. You said a minute ago that the germs you found were not plague?—I said the germs isolated were not plague. But it is also possible that there might have been plague germs which I could not isolate on account of rapid growth of other organisms.

24,880. There may be plague germs anywhere, but have you any reason to suppose there were plague germs there?—In that particular case *post mortem* appearances were very suggestive of them.

24,881. You had no reason, from microscopical examination, to conclude these were plague germs then?—No; I inoculated two more rabbits with these organisms which I separated, but they did not produce the same symptoms, *i.e*, the enlargement of the glands and congestion of the lungs. Hence it shows that the organisms which produced these changes were not isolated.

24,882. Your conclusion is, that you did not get plague bacilli out of those rabbits?—Yes, from that rabbit.

24,883. You do not think there is any experimental evidence to show that cotton does carry infection?—There is no experimental evidence.

24,884. Do you not think that, if the bacteria which you grew had been plague, you might have expected to find the other two rabbits dead which received larger quantities?—Yes, but in a broth tube you generally get clumps of bacteria clinging on to one place, and it is possible also that you might get all the bacilli of one kind in half a cubic centimètre. You will always find in a broth tube that the organisms are not distributed evenly.

24,885. Where are the bacteria distributed in a broth tube, at the top or bottom?—The bottom generally.

24,886. It was the first rabbit that got the stuff from the top which died, while the one that got the stuff from the bottom survived. If it had been well mixed up they would all have had an equal quantity of plague?—Even if you shook the tube well up, I do not think it would be possible to distribute all the organisms evenly, unless the shaking up is kept up for some time.

24,887. Was the control experiment made with ordinary cotton?—Yes.

24,888. How was the control experiment made, was that piece of cotton put into broth?—Yes.

24,889. Did you make a plate cultivation from it?—No.

24,890. How are you able to say that there were no such organisms present?—I examined slide preparations and there were none of those bacilli.

24,891. Do you not think there might have been, but that they were over-grown by the other contaminations?

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—It is possible that it might contain polar staining bacilli other than plague.

24,892. (*The President.*) The Commission wishes to ask you two questions with regard to your experiments with cotton. The first is, did you inform Dr. Lawrie that you had not been able to find the plague bacillus in a sample of cotton examined by you?—Yes.

24,893. The second is, when the statement had been published in the press that you had isolated the plague bacillus from raw cotton, did you contradict it verbally or in writing?—Verbally.

24,894. To whom did you contradict it?—Dr. Lawrie. I gave my opinion to him but he differed from it, and still maintains his original opinion.

(Witness withdrew.)

Capt. C. A. Johnston, I.M.S.

Captain C. A. JOHNSTON, I.M.S., recalled, and further examined.

24,895. (*The President.*) We want to ask you some further questions, especially with reference to the statement* you have last sent in?—Certainly.

24,896. (*Prof. Wright.*) You made these experiments, reported by you in this statement with regard to the sterility of Professor Haffkine's prophylactic?—Yes.

24,897. Are these experiments the same as you did when the Commission was at Hyderabad?—Yes, in continuation of the experiments that were done about the 20th of December.

24,898. They refer to the examination of seven bottles of prophylactic chosen at random out of the stock in the Hyderabad laboratory?—Yes.

24,899. I think, when the Commission was at Hyderabad, you brought up six of these seven tubes, showing obvious contamination?—Yes, and the seventh was doubtful.

24,900. Did the seventh show a slight growth?—Yes, a very slight growth of staphylo-cocci.

24,901. Did you proceed with the examination of the organisms which grew in those tubes?—I separated all the cultures and tried to isolate them as much as I could. I examined them microscopically, and also passed the different bacilli isolated through rabbits.

24,902. Were any two of the bottles you examined bottles belonging to the same brew? No; I do not think any of them were of the same brew, they were all different brews.

24,903. Your experiments did not enable you to judge whether micro-organisms were present in the prophylactic before it was bottled or whether they were introduced in the course of bottling?—I could not say how the contamination did take place, but I suspect that it took place during the process of bottling.

24,904. I presume you could get evidence of that if you examined two different bottles of the same brew, and found micro-organisms in them?—Yes.

24,905. Have you done that?—Yes, I have done it, but not for the Commission.

24,906. Would you tell us about the experiments which you made?—From one brew two different bottles were taken, and two different sets of micro-organisms were found, one bottle contains only cocci, the other bacilli as well as cocci. They were nocuous, but they were not violently pathogenic so as to produce death, except in one case in a rabbit.

24,907. As there were different micro-organisms in different bottles, do you draw the inference that the bacteria were not carried through, but that they are introduced into the bottle?—Yes.

24,908. With regard to the micro-organisms that you did isolate, were you able to recognise or name any of them?—No, but I think bacillus mesentericus fuscus and vulgatus was one.

24,909. Did you investigate the micro-organisms which you found with regard to their pathogenic power upon animals?—I did. I passed every one of them into a rabbit, and in some cases through birds, fowls, and pigeons.

24,910. When you inoculated the rabbits with these micro-organisms after you had isolated them, did you inoculate them from pure cultures?—Yes.

24,911. Did you use any large quantities when you inoculated them?—No, not very large quantities.

24,912. Could you give us an idea?—The growth as it is grown, finally separated out in the agar tube, was scraped off and put into from two to four c.c. of broth, and that we inoculated.

24,913. The whole tube full?—As much as could be got off.

24,914. The bacteria inoculated would be enormously more numerous than the bacteria that could be inoculated along with the prophylactic?—Yes. They were not virulently pathogenic organisms, except in one case in which the rabbit was killed. 5338 was the number of the brew that was virulently pathogenic. The sub-culture showed bacilli irregular in size and form, some were thin and others were thick. They were not pure. It was not all one kind of bacillus. I think there was more than one form of bacillus in the growth when I inoculated.

24,915. In your inoculation experiments with these contaminated micro-organisms it happened that one rabbit died?—Yes.

24,916. That rabbit had been inoculated with a mixed cultivation?—Yes; I think it was a mixed cultivation with mixed bacilli.

24,917. You made some experiments with Haffkine's prophylactic fluid after the fluid had been submitted to a process of re-sterilization?—I did not make any myself personally, but I suggested that they ought to be made. I could not be present myself, because I was in camp for the whole week. Before I went out I asked them to do it. They were carried out, and I used to go there two or three times a week to see how they were getting on. As far as I could judge, there was a distinct difference between inoculating rabbits with the re-sterilized Haffkine's prophylactic fluid and inoculating them with the ordinary fluid received from Bombay.

24,918. You mean that the rabbits which were inoculated with the re-sterilised fluid had much less reaction?—Yes, distinctly so, in most cases.

24,919. Were these rabbits afterwards tested to see whether the protection given by the sterilized fluid was as good as that given by the contaminated fluid?—I should say it was even better.

24,920. I suppose you anticipate that in man the symptoms would be less severe?—Distinctly less severe.

24,921. Have you made any experiments by inoculating men with the re-sterilized fluid?—No, I have not had the chance of doing that. I should think the results would be very much better, from what I have seen of the inoculation fluid.

24,922. (*Mr. Hewett.*) Did you have anything to do with the experiments made regarding the existence of the plague bacillus in cotton?—I had nothing to do with them as regards finding the bacillus, but I was asked to see some of the organisms got from it. I suggested that broth should be used to see if there were any germs, plague or otherwise, in the cotton.

24,923. Did you see the results?—I saw the result of the case, which was quoted as one of plague.

24,924. What is your opinion?—I do not think it was plague myself. I said that, before it could be stated to be plague, we wanted more corroborative evidence.

24,925. To whom did you say that?—To Dr. Mullanah. I do not think Dr. Lawrie was present when I said that.

24,926. (*The President.*) I understand your general view is that it is better to have the prophylactic fluid re-sterilized?—Yes, if it is contaminated I think it is very much better to have it re-sterilized. It does not seem to produce any deleterious effect on the fluid itself from the preventive inoculation point of view.

* *See* App. No. LXX. in this Volume.

24,927. In so far as the specimens which came under your own observations are concerned, would it be better if the fluid were re-sterilized?—Yes, but recently I have examined two or three bottles which were received this month or last month, and I find that there are no growths at all in them; they show no contamination.

24,928. (*Prof. Wright.*) Did you find that in every case the process of heating the prophylactic to a temperature of 60° C. three times was effectual in sterilizing it?—Yes.

24,929. Were any considerable number of re-sterilized bottles examined?—About ten.

24,930. Do you know whether there were any spore-bearing organisms?—In the ones I did there seemed to be spore-bearing organisms in one brew.

24,931. Even those were effectively sterilized?—Yes. I think the spore-bearing organism is the bacillus mesentericus.

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24,932. (*The President.*) Is there anything else which you desire to bring to the notice of the Commission?—May I draw attention to one question which I asked myself when I saw the paper about the case which died of tetanus. I said at the time, that it was very odd to get a death from tetanus from inoculation for plague. I should like to say that these loops-full were not done by hypodermic injection at all: they were scratch inoculations. The fur was taken off the animals, the surface was abraded, and it was rubbed on. I advised them to exclude that altogether.

24,933. In that case the whole loop-full was not introduced into the animal?—No, it was not done by hypodermic injection at all.

(Witness withdrew.)

Dr. Dinshah Pestanji Ghadiali called and examined.

Dr. Dinshah Pestanji Ghadiali.

24,934. (*The President.*) What are your medical qualifications?—I am a graduate of the Independent Medical College, Illinois.

24,935. Will you put in the statement which you have sent to us as your evidence before the Commission?—It is as follows:—

I have the honour to set forward my views regarding the plague and shall appear before the Commission with pleasure if so necessary.

I hold that the plague is not an outburst owing its cause to a particular microbe or microbes only, but is the effect of an unhealthy condition of various elements; that the elements concerned in the present case are bad water, foul gases, and want of light; the unhealthiness of the soil also comes under the same category; that bad food (traceable to poverty) is also an aggravator of the disease, distinctly proved by the poor succumbing in large numbers; the same result indicates the necessity of proper well-lighted and ventilated habitations for the low classes of people; that the unsatisfactory drainage of the city is one of the great causes of the evil, owing to the large quantities of foul gases emitted and the saturation of the soil by the leakage of faulty pipes, &c.; the soil of Bombay, Surat and other places indicates these defects very clearly; that the injudicious opening up of such drains results in the disease breaking out in otherwise unaffected localities, as proved by the primary ravages at Modi Bay; that the mice serve only as a semaphore of the presence of foul gases and not as the spreader of the evil; that the increased havoc now distinctly indicates that the plague is merely a child of other causes more powerful than the simple so-called microbe; that the systems of so-called disinfection by various fluids do more harm than good; they merely substitute one foul odour for another; that the best way the process of real disinfection can be carried on is by means of oxygen gas; it will improve the atmospheric conditions as well as serve as an exhilarator; that the reason of the mortality being greater among the youthful and the growing is traceable to the prevalence of social vices, *e.g.*, self-pollution; this cause may be said to require paramount attention; that the disease requires attention not as a plague, but as the precursor or indicator of unhealthy conditions of the elements before named; that the segregation of people to open places without comfort or protection from the weather is just as harmful as the dreaded fever itself, for, in people unaccustomed to such rigour, it is liable to produce rheumatism, bronchial inflammations and so on; that the only means to fight the disease are good and thorough sanitation and the elevation of the mental tone of the people by cheerful measures; that the inoculation by M. Haffkine's serum had better be avoided; it is fully poisoning a person to ward off the effects of poison; also the theory of its action is based upon unknown principles; no statistics have been put forward to indicate clearly the total killing of the so-called germs and their non-resuscitation in the human body.

(Witness withdrew.)

Mr. P. B. Dantra, called and examined.

Mr. P. B. Dantra.

(Evidence translated.)

24,936. (*The President.*) What is your occupation?—I am an estate broker.

24,937. Have you had any direct connection with plague operations?—I am a member of one of the Voluntary Plague Committees, of C. Ward.

24,938. You have volunteered to give us evidence?—Yes.

24,939. What opinion have you arrived at as to the origin of plague?—I think it is caused by the drains and night-soil.

24,940. The drains and night-soil have been in their present condition long before the plague came here, have they not?—There were complaints about the drain since it was made.

24,941. When was the drain made?—In 1881.

24,942. How do you explain the fact that plague did not commence sooner after 1881 than 1896?—The work of constructing the main drain was taken in hand in 1881. The population also began to increase, and the sullage also began to accumulate in the drain.

24,943. What is it that proceeds from this accumulation which causes plague?—The sullage and night-soil accumulating in the drain. In 1896 there was no rain for the last two months of the monsoon, and the night-soil and other sullage accumulating in the drain gave rise to the plague.

24,944. Do you mean that the accumulation polluted the air?—I am not a professional man, and I am not able to answer that question. I request you to read my précis of evidence.

24,945. I have read it. You have made some proposal as to the best measures to adopt; what do you think is the best measure?—Instead of the night-soil getting into the drain, it should be taken a long distance out to sea and thrown there, as is done in Hong Kong.

24,946. Do you believe in the efficacy of bonfires?—The bad effluvia coming from the gutters may perhaps be removed by the burning of bonfires.

24,947. It is the effluvia of the sewage which, in your opinion, is the cause of plague; is that so?—The exhalations coming from the gutters are the cause of plague, and the rats coming out of the gutters are another cause of plague.

24,948. The bonfires are intended to destroy the plague in the air?—Yes.

24,949. Are these effluvia being continually given off, or only at certain times?—The effluvium continues coming out always.

Mr P. B. Dantra.

13 Mar. 1899.

24,950. Do you propose that bonfires should always be burning?—The burning of bonfires at one place or at several places at one time will not be sufficient.

24,951. Then it requires continuous bonfires?—Wherever it is found that the atmosphere of a certain district is contaminated, the bonfires should be continued there for some time every evening.

24,952. Suppose the effluvia are bad in the daytime, should there be no bonfires to destroy the plague then?—In the morning and afternoon there are the rays of the sun; in the evening there are no rays of the sun, and bonfires should be burnt in the evening, as I do in my own street.

(Witness withdrew.)

(Adjourned till to-morrow.)

At The Secretariat, Bombay.

SIXTY-FIFTH DAY.

Tuesday, 14th March 1899.

PRESENT:

PROF. T. R. FRASER, M.D., LL.D., F.R.S. (*President*).

Mr. J. P. HEWETT. | Dr. M. A. RUFFER.
Mr. A. CUMINE.

Mr. C. J. HALLIFAX (*Secretary*).

Col. D. Robertson, I.S.C.

14 Mar. 1899.

Colonel D. ROBERTSON, I.S.C., recalled and further examined.

24,953. (*Mr. Hewett.*) Since you were before us last, you have had a considerable outbreak of plague in the Mysore city?—Yes, an outbreak commenced very shortly after, and is still continuing.

24,954. What is the population of the city of Mysore?—Seventy thousand.

24,955. Has the epidemic been severe there?—Yes, it has been severe; the total mortality, I think, had reached 57 or 58 per diem.

24,956. What system has the Darbar of the Mysore State adopted as regards the people sick of plague in Mysore town?—The system adopted in the Mysore city has been throughout the voluntary system, as opposed to the forcible system which we tried and worked so unsuccessfully in the Bangalore Civil and Military Station. The system, briefly, is the adoption of all the safeguards, or the measures that were considered necessary under the forcible system, with the exception that everything is left to the people themselves, that is to say, the acceptance of these measures is left to the people.

24,957. Do the people report cases of sickness?—The reporting is surprisingly accurate in Mysore as regards the death-rate; I am not sure about sickness; but every case of death is reported, and before interment or burning takes place a certificate is obtained from the Ward Officer.

24,958. And have you means of ascertaining that the reports of deaths are practically accurate?—I have personally satisfied myself that the reports of death are accurate, for the reason that they are prepared independently by two branches of the city administration, and they do not always agree. There is a variation of one or two, which is, in itself, a sign of honest attempt to prepare a correct total. I think there is no question about it that the reporting of deaths is accurate.

24,959. When a case of plague is reported in the Mysore city, how is the patient treated?—The house would be at once marked as infected, and the people would be urged to take the sufferer to the hospital and to vacate the house as soon as possible, in order that it might be disinfected.

24,960. Do you know whether, in the majority of cases, the people prefer to take the patient to hospital, or to treat him in his own house?—In the majority of cases they treat him in their own houses. I may mention that the State has provided men with English qualifications who go about the city and afford medical relief gratis wherever it is acceptable, but in the majority of cases the people prefer to treat the patient in their own houses

24,961. When the relatives of the patient elect that he should be treated in his own house, what is done with the other inhabitants of the house?—They are allowed to remain there.

24,962. And permitted to have free access to the patient?—Yes.

24,963. Is any attempt made to isolate the patient in one particular portion of the house?—I believe not. At one time they professed to isolate the patient, and, indeed, to turn out the people in the infected house, but after a time it was found that the attempt was hopelessly impracticable, and it was dropped, and practically now the people are left to themselves.

24,964. Between the interval of the reporting of a case of plague and the death or recovery of a patient in his own house, is any attempt made to disinfect the house?—Yes, unless the patient is practically *in extremis*, they would move him out, and disinfect the house, and put the man and his surroundings back again.

24,965. Is there any subsequent system of daily disinfection of the house?—No.

24,966. Does the patient as a rule in Mysore sleep on the floor or on a bed?—The invariable custom in Mysore, I think, is for the people to sleep on the floor.

24,967. When you gave us evidence as regards Bangalore, you told us that the attitude of the people had been anything but helpful. Do you consider that the attitude of the people in Mysore has been helpful?—I think the attitude of the people in Mysore has decidedly been helpful. This effect may partly have been contributed to by the manner in which the Seringapatam town riots were dealt with by the Darbar. The forcible measures adopted by the Darbar undoubtedly tended towards a more pacific attitude on the part of the people. I may mention that the Darbar was very apprehensive of the advent of plague in Mysore, so much so that they requested that volunteers might always be kept ready to turn out at any moment should occasion be required. Mysore containing a considerable Muhammadan population, the Darbar feared outbursts of turbulence amongst them, but up to the present time I am glad to say that these apprehensions have been entirely falsified.

24,968. Have you had many instances in which dead bodies have been thrown out in the Mysore city, as at Bangalore and in the Civil and Military Station?—I have not heard of one.

24,969. Then may it be assumed that whatever the other effects of the measure adopted in Mysore city are, it has been more successful in detecting cases of plague in the houses in which they have occurred?—Certainly.

24,970. Have you been able to observe how the system adopted in Mysore has affected the spread of plague in particular portions of the town?—One very marked feature of the outbreak in Mysore has been the slow manner in which it marched within the town; that is to say, it stuck to one particular district or muhalla of the town, and it did not, as it did in Bangalore Civil and Military Station, jump from one part to the other. The movement has been very slow.

24,971. To what do you attribute that?—I think the cause may fairly be attributed to the fact that the people do not bolt from apprehensions of segregation, but they remain in their houses. There is no terrifying cause, or influence, which would induce them to bolt from one part to the other.

24,972. Have you been able to compare the manner in which the country-side in the neighbourhood of Mysore city has been effected by plague, as compared with the country-side in the neighbourhood of Bangalore city and Bangalore Civil and Military Station?—On this point, my information is necessarily derived from the Darbar, but the authorities—and I believe their reports are fairly accurate—assure me that the country round the Mysore city is very little infected as compared with the country around Bangalore.

24,973. I understand you to say that in both cases the reports come from Darbar?—In both cases, certainly.

24,974. So that there would be the same element of error?—Yes, certainly.

24,975. Do you think that the fact that the neighbourhood of Mysore has been less infected than the neighbourhood of Bangalore could be accounted for by the fact that the villagers now know about plague, and keep strangers out of their villages?—Yes, I think that may have something to do with it, and it is also possible that people may have camped out more than they could in the time of the Bangalore epidemic, when we had almost continuous rain.

24,976. But you would not exclude the possibility of benefit having resulted from fewer people having left Mysore?—I think that is the main cause.

24,977. Could you give us an estimate of the number of people that left Mysore?—I believe it is impossible to do that, because no census was taken of Mysore at the commencement of the plague, and the ward registration of the inhabitants was very defective. They do not know themselves, but looking at the town as I have frequently seen it of late, it did not strike me that it had emptied to any appreciable extent.

24,978. Was there any panic, or any serious disturbance of the ordinary business of the people in Mysore?—There was no panic and no disturbance whatever.

24,979. Was there any disturbance of business in Bangalore city?—Most decidedly. The food supplies ran out almost entirely, so much so that grain was almost selling at famine rates, and we had, for the Civil and Military Station at all events, to improvise an agency for importing grain and firewood, and foodstuffs for the people.

24,980. Are you of opinion that it is not possible, at the present moment, to pronounce authoritatively on the relative merits of the systems adopted in Bangalore and Mysore respectively?—That is so, but at the same time I think there can be no question that the evidence, so far as it is available, points unmistakably in favour of the Mysore voluntary system.

24,981. Would you, as the time goes on, send us reports as to the progress?—Certainly. I may mention that Mr. Cadell is my Assistant, and he has various figures to give, and anything we can supply we will.

24,982. (*Dr. Ruffer.*) Are the sanitary conditions in Mysore the same as in Bangalore?—If anything, the sanitary conditions in Mysore are worse than in Bangalore.

24,983. You mean that the people are more crowded together?—I think the town stands lower, it is more waterlogged, and it is an older city.

24,984. Is there more overcrowding than there is in Bangalore?—I should think that it is about the same; there is very little to choose in that respect; but, as regards the appearance of the town, and the position, I think I should say that Mysore was distinctly more insanitary.

24,985. Are they the same class of people?—Yes, practically.

24,986. Will you tell us how many cases have been treated in their houses, and how many in the hospital?—I did not come to Bombay prepared to give evidence, and I have no figures. Mr. Cadell has them.

24,987. Can you tell me whether the mortality is greater amongst the people who have been treated in their own houses, or amongst the people in hospitals?—That I cannot tell. If Mr. Cadell has not got these figures, it will be quite easy to get them.

24,988. You say that the houses were disinfected as soon as a patient was reported?—That I believe is the order.

24,989. Is the house disinfected again after death?—I am not sure about that.

24,990. Do you know how the house is disinfected?—Yes, I watched it being done. That is perhaps a rather weak point in the Mysore work; I do not think the disinfection is under sufficiently competent supervision. This, however, is a detail in working.

24,991. Who is in charge of the disinfection?—They have a European of no particular status.

24,992. A medical man?—No.

24,993. (*Mr. Cumine.*) Do you find that leaving a patient in his house leads to many of the other members of the family catching the disease?—I asked them in Mysore particularly to record facts about this, because I recognised that it was one of the most important points; they have done so to some extent, and Mr. Cadell will produce the figures.

24,994. (*The President.*) When did the plague commence in Mysore?—I do not know the exact date of the first case, but I think it was in November or December. Mr. Cadell has the figures of when the cases began.

24,995. From the commencement, the system which you have just described has been in operation?—Yes.

24,996. The so-called voluntary system?—Yes.

24,997. During this time, has plague to your knowledge steadily increased in Mysore?—No, it has fluctuated. It increased up to a certain point and now the tendency is a downward one. There is no marked rapid fall, but the last telegrams I got stated that the total mortality was 26 per diem.

24,998. You cannot distinguish the plague cases?—Yes, but I believe in a town like that, that the only safeguard is the total mortality.

24,999. What do you estimate the plague deaths at?—In these figures that are given me, the total mortality being, say 26, the plague deaths would be about 19 per diem, the normal mortality being about six or seven.

25,000. We had the opportunity of seeing the houses when we were in Bangalore; are the houses in Mysore about the same?—Yes.

25,001. There are a good many one-chambered houses?—Yes, very rarely more than one storey; but they are easier to disinfect. I do not know whether you remember that in Bangalore there are many flat roofs, in Mysore they are nearly all gable roofs and tiles.

25,002. How does that facilitate the disinfection?—Because you can take off the roofs.

25,003. In disinfection you include the removal of tiles?—Yes, and letting in the sun and air.

25,004. Air and sun disinfection, as well as chemical disinfection?—Yes.

25,005. I do not quite understand how this disinfection is effected in one-roomed houses when the patient is in the house?—I mean one storey houses: the houses have small rooms, and as a rule they are one-storeyed.

25,006. The floor, I suppose, is of the same nature as in Bangalore—mud?—Yes, mostly mud with cow-dung.

25,007. The houses are usually quite dark?—Yes, certainly.

25,008. You, I understand, are not in a position as yet to give any definite opinion as to the success of this system?—Not definitely, except intermediately that everything points at present to a favourable conclusion.

25,009. It has gone on since October or November?—Not so early as that. I think the first cases were in December. All the first cases were imported, but I think the first indigenous cases were in December.

25,010. On the whole it is on the decline?—Yes, I think there is a downward tendency.

(Witness withdrew.)

Mr. P. R. Cadell, I.C.S.
14 Mar. 1899.

Mr. P. R. CADELL, I.C.S., recalled and further examined.

25,011. (*The President.*) We wish some further information from you with regard to Bangalore City?—Yes.

25,012. (*Mr. Hewett.*) You have some figures with regard to the Civil and Military Station which will supplement Colonel Robertson's evidence at Bangalore Will you kindly put them in?—Yes.

25,013. First of all, will you state what they are?—Firstly, the progress of disease by wards in Bangalore and Mysore.

STATEMENT I.

BANGALORE CITY.

NUMBER of DEATHS from all Causes by WARDS from 12th August 1898 to 28th February 1899.

Ward Number.	Population.	Total Mortality.	Percentage to Population.	Ratio per Mille per Annum.
Ward I.	20,663	1,545	7·5	138
Ward II.	27,095	2,670	9·9	182
Ward III.	11,843	1,090	9·2	169
Ward IV.	20,684	1,793	8·7	160
Total	80,285	7,098	8·8	163

BANGALORE, CIVIL AND MILITARY STATION.

PROGRESS of the DISEASE by WARDS from 15th September to 28th February 1899.

Circle Number.	Population.	Plague Attacks.	Percentage to Population.	Plague Deaths.	Percentage to Population.	Total Mortality.	Percentage to Population.	Ratio per Mille per Annum.
I.	4,774	49	1	37	·8	60	1·3	27
II.	16,196	610	3·7	512	3·1	1,102	6·8	148
III.	5,514	392	7·1	295	5·3	426	7·7	168
IV.	2,174	87	4	64	2·9	87	4	87
V.	5,413	356	6·5	295	5·4	411	7·6	166
VI.	11,233	525	4·6	416	3·7	655	5·8	126
VII.	10,916	177	1·6	144	1·3	309	2·8	61
VIII.	10,912	510	4·7	439	4	695	6·4	139
IX.	9,844	257	2·6	215	2·2	430	4·4	96
X.	3,841	158	4·1	146	3·8	201	5·2	113
XI.	2,264	100	4·4	99	4·4	165	7·3	159
XII.	4,562	267	5·9	225	4·9	563	12·3	268
XIII.	8,318	382	4·6	340	4·1	925	11·1	242
XIV.	4,120	48	1·1	25	·5	55	1·3	27
Outside limits and unknown.	—	82	—	46	—	46	—	—
Total	100,081	4,060	4	3,298	3·3	6,130	6·1	133

MYSORE CITY.

PROGRESS of the DISEASE by WARDS from 1st October 1898 to 28th February 1898.

Ward Number.	Population.	Plague Attacks.	Percentage to Population.	Plague Deaths.	Percentage to Population.	Total Mortality.	Percentage to Population.	Ratio per Mille per Annum.
1	33,251	1,356	4·1	1,074	3·2	2,155	6·5	156
2	31,187	470	1·5	387	1·2	977	3·1	75·2
3	9,610	104	1·1	53	·6	76	·8	19·
Total	74,048	1,930	2·6	1,519	2	3,208	4·3	104

NOTE.—No death from any cause is recorded from Ward III. from 1st October to 20th January, which suggests some inaccuracy and is being inquired into.

The deaths in Ward III., which was only constituted on 18th January, have, up to that time, been recorded under Wards I. and II.

25,014. Have you returns of deaths among races?—Yes.

25,015. Will you put them in, all three—for Bangalore City, the Civil and Military Station, and Mysore?—Yes.

STATEMENT II.

BANGALORE CITY.

NUMBER of DEATHS from all Causes by RACES from 12th August 1898 to 28th February 1899.

	Population.	Total Mortality.	Percentage to Population.	Ratio per Mille per Annum.
Hindus and others	67,388	5,280	7·8	145
Muhammadans	10,472	1,671	15·9	294
Native Christians	2,425	147	6·1	112
Total	80,285	7,098	8·8	163

BANGALORE, CIVIL AND MILITARY STATION.

MORTALITY by RACES from 15th September to 28th February 1899.

——	Population.	Plague Attacks.	Percentage to Population.	Plague Deaths.	Percentage to Population.	Total Mortality.	Percentage to Population.	Ratio per Mille per Annum.
Europeans - -	4,985	18	·3	12	·2	37	·74	16
Eurasians - - -	2,649	47	1·7	35	1·3	77	2·9	63
Native Christians -	10,268	414	4·4	320	3·1	517	5	109
Muhammadans - -	23,892	871	3 6	789	3·3	1,881	7·9	172
Hindus and others -	58,287	2,650	4·5	2,142	3·7	3,611	6·2	135
Total - -	100,081	4,000	4	3,298	3·3	6,130	6·1	133

MYSORE CITY.

SPREAD of the DISEASE by RACES up to 28th February 1899, from 1st October 1898.

——	Population.	Plague Attacks.	Percentage to Population.	Plague Deaths.	Percentage to Population.	Total Mortality.	Percentage to Population.	Ratio per Mille per Annum.
Hindus and others -	58,741	1,345	2·3	1,026	1·7	2,223	3·8	91
Muhammadans - -	15,307	585	3·8	493	3·2	985	6·4	155
Total - -	74,048	1,930	2·6	1,519	2	3,208	4·3	104

25,016. Is there any point you would like to call our attention to with regard to those three returns?—As regards the races in all three cities, the Muhammadans suffered much the most severely.

25,017. Are the Muhammadans in all those three towns in a depressed condition?—Yes, they are all poor—very much poorer than the Muhammadans elsewhere; and they are much more crowded, I think, than the Muhammadans elsewhere in their houses.

25,018. When you say the Muhammadans have suffered more than other classes, I suppose you are deducing your conclusion from the total mortality rather than from the number of recorded plague attacks?—From the total mortality.

25,019. On the ground that the difference between the normal mortality and the mortality during the plague outbreak is practically attributable to plague?—For Mysore City and Bangalore Civil and Military Station I have the plague figures as well as the total mortality. In the Civil and Military Station the Muhammadans, from recorded plague deaths, suffered less than the Hindus; but their total mortality was considerably higher. In Mysore, both from the recorded plague deaths and the total percentage of the population, it was higher than in the Hindus.

25,020. And Bangalore City?—In Bangalore City the record of deaths from plague was so inaccurate that I could not get the figures. I have got the total mortality.

25,021. Do the Muhammadans in those three towns live in more overcrowded houses than the other portions of the population?—No; I could not say that.

25,022. Have you any other reason which you can offer for their being attacked more than the Hindus?—Except that there was greater concealment in the Civil and Military Station among the Muhammadans, and where, in the end, the Hindus have been got to report the cases early, the Muhammadans never did it in the Civil and Military Station. A much smaller proportion of the Muhammadans, also, underwent inoculation.

25,023. You think the fact that they had plague cases and that no precautions were taken may have increased the plague mortality among them?—I think so.

25,024. The next statement you put in is one relating to the hospital figures?—Yes. The statements with regard to Bangalore City, Civil and Military Station, and Mysore City, are as follow:—

STATEMENT III.

HOSPITAL FIGURES: TOTAL ADMISSIONS.

Hospital.	Admissions.	Discharges.	Deaths.	Percentage of Mortality.	Remaining.
Bangalore City - -	2,131	709	1,422	66·7	—
Civil and Military Station:—					
North Camp - -	940	336	596	63·4	8
South Camp - -	768	306	462	60·2	—
Total - -	1,708	642	1,058	61·9	8
Mysore - - -	206	20	101	49	85

NOTE.—The hospital figures for Mysore are up to the 28th February, and for the Civil and Military Station up to the 3rd of March, 1899.

25,025. In the figures for the Plague Hospital of Mysore City, you show the total admissions to the hospital to have been 206; could you give us any estimate as to the number of persons suffering from plague who were treated in their own houses in Mysore?—No, not those who were treated separately from those who died without treatment.

25,026. You cannot give any approximate estimate?—No, I am afraid not, as treatment was voluntary. The total mortality in the houses was, of course, higher.

25,027. Then you have a statement of hospital figures as regards the inoculates?—Yes. It is as follows:—

STATEMENT IIIA.

HOSPITAL FIGURES for INOCULATES.

——	Admissions.	Deaths.	Discharges or Convalescent.	Percentage of Mortality.
Bangalore City -	57	31	26	54·4
Civil and Military Station:—				
North Camp	87	24	63	27·6
South Camp	41	12	29	29·3
Total -	128	36	92	28·1
Mysore - -	26	9	Not shown	34·6

NOTE.—The figures for the Civil and Military Station are up to the 3rd of March, and those for Mysore up to the 28th of February, 1899. The figures for the Military Hospitals are contained in separate statements relating to the Military.

Mr. P. R. Cadell, I.C.S.
14 Mar. 1899.

25,028. And you have a statement of the number of cases of plague which occurred amongst the contacts?—Yes, for all three. The proportion of contacts in Mysore, where the contacts were all voluntary, was higher than in the Civil and Military Station.

STATEMENT IIIB.

ADMISSIONS to CONTACT CAMP.

—	Admitted.	Developed Plague.	Percentage developing Plague.	Died from other Causes.
Bangalore City -	3,960	238	6	—
Civil and Military Station:—				
North Camp	1,527	50	3·3	3
South Camp	887	25	2·8	—
Total -	2,414	75	3·1	3
Mysore City -	452	27	6	—

NOTE.—In the earlier stage of the epidemic, contacts in the Civil and Military Station were allowed to go out to their work. Many absconded, and the above numbers were, therefore, not all detained for ten days.

25,029. The camp was reserved for contacts in Mysore; it was not a health camp?—No, entirely a contact camp.

25,030. Have you any information to give us as to how long after the contacts came into camp they developed plague?—I have figures for the Civil and Military Station only. They are:—

Number developing plague—

On first day after arrival in camp -	26
,, second ,, ,, -	16
,, third ,, ,, -	5
,, fourth ,, ,, -	5
,, fifth ,, ,, -	8
,, sixth ,, ,, -	1
,, seventh ,, ,, -	3
,, eighth ,, ,, -	1
,, ninth ,, ,, -	7
,, tenth ,, ,, -	3
	75

25,031. Have you got the number of houses in which more than one seizure has been recorded?—It is as follows in Mysore:—

NUMBER of HOUSES from which more than ONE SEIZURE has been recorded.

In 76 houses there were	2	attacks.
,, 26 ,, ,,	3	,,
,, 4 ,, ,,	4	,,
,, 6 ,, ,,	5	,,
,, 4 ,, ,,	6	,,
,, 2 ,, ,,	7	,,
,, 1 ,, ,,	10	,,
119		

In Bangalore City there was more than one seizure in 297 houses, but the exact number in each is not known.

25,032. In all the other houses in Mysore in which plague cases occurred, was there only one attack?—There could have been only one attack.

25,033. Do these figures refer only to cases definitely found to be due to plague?—Yes.

25,034. Have you a statement with regard to the number of inoculations performed in Bangalore City, Bangalore Civil and Military Station, and Mysore?—Yes, as follows:—

STATEMENT IV.

BANGALORE CITY.

SHOWING NUMBER of INOCULATIONS performed from 2nd September 1898 to 28th February 1899.

—	Number of Inoculations.	Remarks.
Week ending 8th September 1898	81	
,, 15th ,, ,,	241	
,, 22nd ,, ,,	328	
,, 29th ,, ,,	4,212	
,, 6th October ,,	4,384	
,, 13th ,, ,,	1,280	
,, 20th ,, ,,	1,108	
,, 27th ,, ,,	1,285	
,, 4th November ,,	2,667	
,, 11th ,, ,,	1,385	
,, 18th ,, ,,	2,134	
,, 25th ,, ,,	1,630	
,, 2nd December ,,	1,486	
,, 9th ,, ,,	1,506	
,, 16th ,, ,,	575	
,, 23rd ,, ,,	1,191	
,, 30th ,, ,,	976	
,, 6th January 1899	1,032	
,, 13th ,, ,,	677	
,, 20th ,, ,,	560	
,, 27th ,, ,,	1,134	
,, 3rd February ,,	836	
,, 10th ,, ,,	673	
,, 17th ,, ,,	601	
,, 24th ,, ,,	559	
From 25th February to 28th February 1899 - - -	332	
Total - - -	32,871*	

* Of these 1,350 were second inoculations.

CIVIL AND MILITARY STATION.

INOCULATION PERFORMED.

Week ending.	By Civil Officers.	By Military Officers.	
		First Inoculation.	Second Inoculation.
Up to 23rd September	1,075	—	—
,, 30th ,,	694	—	—
,, 7th October -	732	—	—
,, 14th ,, -	952	970	—
,, 21st ,, -	2,720	875	—
,, 28th ,, -	6,205	2,497	—
,, 4th November	4,558	4,784	—
,, 11th ,, -	1,068	1,899	—
,, 18th ,, -	951	355	66
,, 25th ,, -	1,636	354	1,403
,, 2nd December	2,124	51	994
,, 9th ,, -	2,266	6	2,568
,, 16th ,, -	1,127	24	508
,, 23rd ,, -	756	—	100
,, 30th ,, -	555	—	—
,, 6th January -	686	—	—
,, 13th ,, -	741	—	—
,, 20th ,, -	500	—	—
,, 27th ,, -	582	—	—
,, 3rd February	398	—	—
,, 10th ,, -	322	—	—
,, 17th ,, -	223	—	—
,, 24th ,, -	220	—	—
,, 3rd March -	168	—	—
	31,259*	11,815	5,639

* Of these, 30,296 were first inoculations.
,, 963 were second ,,

Total first inoculations -	42,111
Total second ,, -	6,602
Number of inoculates reported attacked -	376
,, ,, ,, died	262

MYSORE CITY.

1.—INOCULATIONS PERFORMED.

—	First Inoculation.	Second Inoculation.
From commencement to 27th January - - -	28,685*	65
Week ending 3rd February -	476	14
„ 10th „ -	201	2
„ 17th „ -	222	3
„ 24th „ -	82	5
Total - -	29,666	89

* Of these—

3,745 were done in October.
6,926 „ „ November.
15,251 „ „ December.

25,922

2.—NUMBER of INOCULATED PERSONS ATTACKED.

—	October.	November.	December.	January.	February.	Total.
Seizures - -	—	1	3	53	61	118
Deaths - -	—	1	2	38	43	84

—	Number of Inoculates attacked.	Number verified by Comparison with Certificates.	Percentage.	Deaths.	Percentage.	Number verified by Comparison with Certificates.	Percentage.
Removed to Hospital	26	—	—	9	34·6	—	—
Remained outside -	92	—	—	75	83·5	—	—
Total - -	118	98	83	84	71·2	62	73·8

25,035. You have endeavoured to get the information asked for from Col. Robertson with regard to the incidence of plague among the inoculated and uninoculated?—Yes.

25,036. Have you got that only for the Civil and Military Station?—We have only that accurately for the Civil and Military Station.

25,037. You put in other figures?—I have put them here for what they are worth, but for Mysore city they are inaccurate so far as regards the examination of bodies, and I would not vouch for them. The statement attached for the Civil and Military Station shows that of the cases actually seen and pronounced to be plague, 10·8 of the seizures and 9·4 of the deaths were among inoculated persons, against 85·1, or 85·5 respectively among the uninoculated; this is considerably lower than the proportion of the inoculated to the uninoculated population:—

STATEMENT No. V.

CIVIL AND MILITARY STATION.

PROPORTION of INOCULATED PERSONS ATTACKED to TOTAL NUMBER of ATTACKS and DEATHS.

Number of Plague Cases seen by Government Medical Officers and found to be or not to be inoculated.								Number of Cases seen by Hakims and pronounced to be plague.								Number of Bodies buried without Permit, and counted as Plague.
No. of Cases seen by Govern- Medical Officers.		No. of these found to be Inoculated.		No. of these found not to be Inoculated.		No. doubtful or Unknown.		No. of Cases seen by Hakims and pronounced Plague.		No. of these found to be Inoculated.		No. of these found not to be Inoculated.		No. of these Doubtful or Unknown.		
Attacks.	Deaths.	Attacks.	Deaths.	Attacks.	Deaths.	Attacks.	Deaths.	Attacks.	Deaths.	Attacks.	Deaths.	Attacks.	Deaths.	Attacks.	Deaths.	
3,473	2,772	374	260	2,961	2,374	138	138	9	9	2	2	3	3	4	4	516

The remaining persons who died between 15th September and 28th February, were either seen by Medical Officers and certified not to have died of plague, or were seen by Hakims and Baids and pronounced not to have died of plague, or were buried without certificate, before the certificate rules came into force.

—	Number seen and pronounced to be Plague.	Percentage inoculated.	Percentage not inoculated.	Percentage unknown.
Attacks -	3,482	10·8	85·1	4·1
Deaths	2,781	9·4	85·5	5·1

25,038. Could you tell us how the fact of a person being inoculated or uninoculated was ascertained for the purpose of these returns?—Inquiries were always made in the household, and if possible a certificate was recovered.

25,039. Do you think that there is any margin of error by reason of any disposition of the people to retain the certificate?—I think there is a slight margin of error.

25,040. But you would not attach any material importance to it?—No. Inquiry was made on that point, and the medical officer said that they only found rare instances in which there was suspected transference of the certificates.

25,041. Can you tell us when the first indigenous case of plague took place in Mysore?—It was on the 26th November.

25,042. Do you know whether there was in Mysore any mortality among rats before cases began to occur?—I do not know.

25,043. Is Mysore city dirtier or cleaner than the town of Bangalore?—It is dirtier and generally more unhealthy. Amongst those who know them well, there is only one opinion, that Mysore is ordinarily a far dirtier and more unhealthy town than Bangalore. Col. Benson says the difference between the healths of the jails in each place is striking. It is true that this year there has been singularly little malarial fever in Mysore jail: but in general the town is extremely malarial, lying in a hollow, with no fall for its sewage. Mr. Architrao, Civil Surgeon of Mysore, writes, "The town is incom- "parably more filthy than the pettah of Bangalore. "In places like old Agrahar the houses are inde- "scribably damp for the greater part of the year."

25,044. Has past experience shown that epidemics of cholera attacked both places alike?—As regards liabilities to epidemics, both Bangalore, so far at least as the Civil and Military Station is concerned, and Mysore have been subject to visits of cholera, though in Mysore there has been no epidemic since 1891. Formerly it was more liable to cholera.

25,045. At the time of the year when plague broke out in Bangalore there was much more difficulty in dealing with the disease than at the time it broke out in Mysore, was there not?—Yes. In respect to climatic conditions it cannot be doubted that the plague attacked Bangalore

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at the more unfortunate period of the year. Both in October and November there was heavy rain, and this not only hampered all active operations carried out by the officials, but drove the people into their houses, just at the beginning of the cold weather. In Mysore, the disease has spread at a much drier season; the weather has become, for the Province, warm, and the season of the year is approaching in which the plague has, I think, usually dropped in Bombay.

25,046. When was this evil effect of climate most apparent as regards the epidemic in the Civil and Military Station?—From about the 11th to the 20th November in the Civil and Military Station.

25,047. Then it would not have been operating at the time that plague was at its worst in Bangalore city?—I could not say about Bangalore city from personal knowledge. There was rain in October, too. I cannot say what the exact effect was on the plague operations.

25,048. Do you think between the 22nd October and the 4th November, from what you have heard, that the operations in Bangalore city were seriously prejudiced by the rains at that time?—Yes, I was told so. I put in a statement of the rainfall, as follows:—

STATEMENT of RAINFALL in BANGALORE.

Day.	August.	September.	October.	November.	December.
1	—	0·10	—	—	0·40
2	—	—	—	—	0·01
3	0·14	0·36	0·16	—	—
4	0·01	0·07	0·10	—	—
5	—	2·29	—	0·51	—
6	0·11	2·57	—	0·66	—
7	0·06	0·13	0·05	1·75	—
8	—	2·45	—	1·50	—
9	—	0·82	—	—	—
10	—	0·02	—	0·01	—
11	—	0·01	0·17	0·25	—
12	—	0·11	—	0·03	—
13	0·15	0·02	—	0·21	—
14	0·07	0·03	—	0·40	—
15	0·37	—	0·17	0·01	—
16	—	—	0·01	0·01	—
17	—	—	2·59	—	—
18	—	—	—	—	—
19	—	—	0·02	—	—
20	—	0·37	—	—	—
21	—	0·13	—	—	—
22	—	—	—	—	—
23	—	0·06	—	—	—
24	—	—	0·08	—	—
25	—	1·41	—	—	—
26	0·50	0·66	0·10	0·12	0·04
27	0·37	0·64	0·13	0·03	—
28	0·04	0·06	—	0·02	—
29	0·08	0·01	—	—	—
30	—	—	—	0·18	—
31	0·26	—	—	—	—
Mean	2·16	11·93	3·58	5·69	0·45

No rain in January.

25,049. When the outbreak took place in Mysore, to what extent had inoculation been resorted to there?—By the end of November in Mysore 10,600 people had been inoculated, and by the end of December 26,000 people—over one-third of the population—had been inoculated.

25,050. And in Bangalore city and in Bangalore Civil and Military Station the people were not inoculated until plague was among them, were they?—No. At the end of October in the Civil and Military Station, by which time the plague had got very bad, there were only 12,000 persons inoculated: at the end of November 20,000 were inoculated. By that time the disease had begun to decrease so that less than one-fourth of the non-military population was inoculated before the disease turned.

25,051. You say in the précis of your evidence: "It "may be taken, I think, that Bangalore, both the "Civil and Military Station and city, underwent an "abnormally severe attack." What do you mean by that, precisely?—That there were a very large number of cases. I do not go into the causes of it at all, as I cannot say whether one type was more severe than the other.

25,052. You do not mean to say that the conditions of the epidemic were different?—No, I say that the results were different.

25,053. Will you kindly tell us the state of plague at present, first of all in Mysore and secondly in Bangalore Civil and Military Station?—In the Civil and Military Station it is practically extinct. Last week we had only five indigenous cases and the week before only four. They are all sporadic cases. With the exception of these sporadic cases plague is extinct.

25,054. Can you give us the total mortality of Bangalore city, the Civil and Military Station, and of Mysore city respectively up to date, from the beginning of the outbreaks?—In Bangalore city there have been 2,652 recorded deaths from plague. The total mortality has been 7,115, and taking an estimated daily death rate of seven from the normal causes, the excess over that normal is 6,016. In the Civil and Military Station there have been 3,306 recorded deaths from plague, a total mortality of 6,192, and taking the normal average of 9 per diem the excess over the normal since the beginning of the outbreak has been 4,779. In Mysore city there have been 1,592 deaths from plague. The total mortality has been 3,301. Taking the normal from other causes of 7 per diem, the excess over the normal is 2,202. So, roughly, it may be taken that plague mortality in Bangalore city was 6,000, in the Civil and Military Station it was 4,800, and in Mysore 2,200 up to date.

25,055. I believe you have prepared charts which show in respect to Bangalore city and Bangalore Civil and Military Station the normal mortality, the total mortality, and the plague mortality during both epidemics. And one for Mysore city shows the total recorded mortality and the total plague mortality?—Yes.*

25,056. Perhaps you will complete these two charts as to Mysore and Bangalore Civil and Military Station and send them to us by the end of April?—Yes.*

25,057. I believe you were at Bangalore Civil and Military Station when the disease was there in epidemic form?—Yes.

25,058. Did you observe any difference in the attitude of the people in the Bangalore Civil and Military Station and Mysore?—The difference has been very striking in the way the Mysore people have freely reported the cases. As far as we know they have not tried to remove the sick from the town or from the houses, and they have reported very freely indeed. In Bangalore Civil and Military Station they did not report until the very end and only after great compulsion. They also removed the sick to a very considerable extent.

25,059. Has the result been that you found plague at a much earlier stage in Mysore?—I believe so.

25,060. When you found a patient suffering from plague in Mysore, I understand that he was given the option of remaining in his house or going to the hospital?—Yes.

25,061. Were the patients urged to go to hospital as a rule?—They have been advised to go.

25,062. But the result has been that only a comparatively small number of the sick have gone?—Yes, comparatively few.

25,063. Supposing you found a plague patient sick in his house, what did you do with the people who were in the house with him?—If they agreed to allow the patient to remain in the house, nothing was done to the other people.

25,064. What was done to the house?—Nothing was done.

25,065. No disinfection?—No. The rule was that a house was not to be disinfected until the patient either died or recovered. As a matter of fact Col. Benson tells me that the houses have generally been disinfected excepting the room in which the patient has been living.

25,066. (*The President.*) At the commencement or afterwards?—Immediately.

25,067. (*Mr. Hewett.*) The room in which the patient was suffering was left alone till the patient either recovered or died?—Yes.

* *See* App. No. LXXI. (i) to (iv) in this Volume. The charts have not been completed beyond the beginning of March.

25,068. The whole of the family were permitted to visit him, I suppose, where he was lying?—Yes.

25,069. Does the patient as a rule lie on the floor or on a bed?—In Mysore, on the floor.

25,070. Have you observed whether the plague has moved more rapidly about different parts of Mysore city than it did about the Civil and Military Station at Bangalore?—It moved very much more slowly in Mysore.

25,071. To what do you attribute that?—To the people not being disturbed and not moving. I do not think there has been any other cause, unless the disease can be said to have been, of itself, more virulent in Bangalore.

25,072. Has there been any panic in Mysore?—No, none.

25,073. And the ordinary business has been conducted throughout the epidemic?—Yes, throughout the epidemic.

25,074. Has there been any exodus of the people to outlying parts of the country?—A large number have gone—about 20,000—but not nearly to the same extent as in Bangalore city.

25,075. Have you had any opportunity of ascertaining whether the neighbourhood of Mysore has been infected as much as Bangalore?—It has been far less affected. In Bangalore the whole of the surrounding tálukas were highly infected.

25,076. Do you think that the fact that the neighbourhood of Mysore has not been so badly infected may to a certain extent be accounted for by the villagers knowing more about the measures?—Yes, and in being more careful in keeping strangers out.

25,077. Do you think that this is the only circumstance which has affected the extent of the infection near Mysore?—There is the fact of their camping out. In Mysore, they certainly did learn that lesson thoroughly. They camped out very early on the appearance of the disease. I think something must be assigned to the fact, that far fewer sick people were taken out than in the case of Bangalore.

25,078. Have you observed anything else with reference to the relative treatment in hospital in Bangalore, and in the Civil and Military Station and Mysore city?—There are so many still remaining under treatment in Mysore hospital that I do not think it is safe to draw any deductions. The figures are so far more satisfactory in Mysore. In Mysore the mortality has been 49 per cent. so far; whereas in the Civil and Military Station the mortality was 61 per cent., and in Bangalore 66 per cent.; but, of course, the numbers in the Mysore hospital have been smaller.

25,079. Have you any reason for thinking that the voluntary system, as adopted in Mysore, will, in the long run, lead to a greater extension of plague in Mysore city?—I think it is inevitable it will spread over the whole city. It is spreading. It is attacking quarters which have hitherto been free. I think it is inevitable that it must go all over the town.

25,080. Do you think that it is likely that it will be more severe than if the system adopted in Bangalore city and Civil and Military Station were adopted?—I do not think there will be as high a mortality as in Bangalore.

25,081. I am speaking of the city?—Yes.

25,082. I understand you to say that apart from the city itself, the result of the voluntary system will be, to a certain extent, that the neighbourhood will suffer less severely than it did at Bangalore?—Yes, so far as we can say at present.

25,083. That would rather tend to show that the disease is less likely to extend from a place in which the voluntary system is at work?—Yes.

25,084. Do you think that the disinfection which has been carried out at Mysore has been effectively carried out?—I do not think so. There has been very little European supervision in Mysore—only one or two subordinates.

25,085. Do you think that disinfection under a system, such as you have had in Mysore, might have been properly carried out if you had had proper supervision?—I do not see how it could be. According to the rule, the people may object to their houses being disinfected until the patient has died or recovered. It gives infection a very long time to spread.

25,086. Why do the people object to a house being disinfected before the patient has died?—It is the Darbar's rule that the house need not be disinfected until then. Even if the house was disinfected, with the exception of the room in which the patient was lying, there would obviously be danger of fresh infection to the house as long as the patient remained ill in it.

25,087. Have the people of Mysore displayed any objection to disinfection?—I believe they did at first, but to a very small extent.

25,088. Has the effect of this system been that people have been in better heart during the epidemic?—I think so. I saw a very marked difference in the demeanour of the people in Mysore than in Bangalore.

25,089. Has this voluntary system caused greater or less expense than the one adopted in the Civil and Military Station?—Far less expense. The system of the Civil and Military Station was extremely expensive, whereas in Mysore, there was very little expense indeed.

25,090. In addition to the other evils which existed in Bangalore Civil and Military Station, was there any difficulty owing to the staff running away?—The sweepers in the early part of the epidemic ran away—they struck.

25,091. Did you find any difficulty of that sort in Mysore?—No, but that could have had nothing to do with the system, except as to the results—that the extent of the disease and the high mortality might make the sweepers strike. The sweepers themselves would not be influenced by the plague measures.

25,092. Were they influenced particularly by fear of death?—Yes, and by the desire to earn higher wages—reasons which would cause an ordinary strike.

25,093. Do you think that it would be possible to combine the evacuation of infected quarters with the voluntary system?—I think so. The officers of Mysore say it would have been impossible in Mysore without disturbing the people unduly. I think it would be possible in a second epidemic, and I think in most places it would be possible to persuade the people in a first outbreak.

25,094. Would you explain exactly what you mean by a combination of the two—how would you combine the systems?—By combination I mean telling the people that they must leave an infected quarter, and providing huts for them, and saying they must go there; and as far as the sick are concerned, taking their sick with them, and having no segregation amongst them of any contacts, or even of the sick, except what they carried out themselves—simply making the people leave the infected quarter, and go to another place; otherwise leaving them entirely at liberty, though, if necessary, forbidding them to go to adjoining villages.

25,095. That is to say, under such a system there would be no compulsion as regards going to hospital, and no compulsion as regards being placed in the contact camp?—That is so.

25,096. (*Dr. Ruffer.*) How did you disinfect the houses in Mysore?—The actual work in Mysore was entirely under the Darbar—the State officials. I cannot talk of the actual work.

25,097. How was it done?—The disinfection was done on the same system as it was done in Bangalore.

25,098. Did you provide rules for disinfection?—Yes; the Mysore State had rules.

25,099. Can you put those in?—Yes, they are as follows:—

"10. *Disinfection.*—When it is provided in these rules, or in any others, which are or may be in force in the cities of Bangalore and Mysore that disinfection may be employed, disinfection as follows, as often as seems to him necessary, may be ordered by any plague authority, unless where otherwise provided:—

"*Of the Person.*—In a bathing tub, with Jeyes' fluid or carbolic acid, strength, 1 in 100.

"*Of Travellers' Bundles.*—(1) If specially ordered by the President, or any officer deputed for the purpose, to be opened out and steamed for 20 minutes by steam pressure of 20 lbs. and temperature 250° or (2) otherwise, to be treated as below.

"*Of Clothes, Silks, &c.*—To be exposed first one side then the other, for eight hours in a strong sun.

"*Other Cloths, &c.*—In perchloride of mercury, 1 in 1,000.

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"*Of an infected House.*—If its value is Rs.100 or less, and there is no danger of the fire spreading, it may be burnt. Otherwise, if a plague authority so directs, it may be unroofed and treated as provided in the next paragraph, after removal of all surrounding obstructions (walls, huts, sheds, pandals, &c.), to ensure a complete perflation of air.

"*Of Houses in an infected Area.*—Each house shall be opened to light and air in its dark rooms. Infected things of small value, such as bedding, cots, &c., may be burnt. Everything else, as well as the entire interior of the house, including the floor, shall be sprayed or sprinkled with perchloride of mercury. The floor of the rooms used by the sick person shall, if of earth, be dug out to a depth of two inches, and removed to a spot well away from habitations, and thoroughly burnt. Rubbish shall be burnt or carted away. Then the exterior and interior of the house shall be limewashed, chloride of lime being mixed with the limewash.

"*Of other Places and Spaces in such an Area.*—As any plague authority may direct."

25,100. What do you mean by saying that the cases in Bangalore now are sporadic?—I mean scattered and unconnected with each other. The five indigenous cases we had last week occurred in five different quarters of the town. There was not any particularly infected locality, and the week before, similarly, six cases were found in five different quarters.

25,101. Did you have corpse inspecti on in Mysore?—Yes, there has been corpse inspection.

25,102. Has every dead body been seen?—Yes, after the 22nd January.

25,103. Are you inspecting all the bodies now?—I believe so, in Mysore and in the Civil and Military Station they are now inspecting all the bodies.

25,104. Do you think there was much difficulty in examining the bodies of women?—I cannot say to what extent it was carried out in Mysore, whether it was strictly carried out or not. In the Civil and Military Station, all the bodies of women have been examined by females.

25,105. Do you know whether there have been any disturbances owing to corpse inspection?—No disturbance.

25,106. Did you give any special advantages to inoculated people?—Not in Mysore, because there were no special advantages to give to anybody, as there were no restrictions.

25,107. Have you any knowledge of inoculated people leaving the place?—They undoubtedly did so in large numbers in Bangalore.

25,108. I am speaking of Mysore?—In Mysore, no. I think a certain number would get inoculated because they know inoculated people have greater privileges on the railway.

25,109. You think that people get inoculated to go away?—Yes, a certain number.

25,110. That was the case in Bangalore was it not?—Yes.

25,111. A great many inoculated people did leave Bangalore, did they not?—Yes. In Bangalore city, out of 28,000 inoculates 12,000 are stated to have left.

25,112. Will you turn to Statement No. IV. about Mysore city? When you say the number of inoculates attacked, I suppose you mean the number you know of?—Yes.

25,113. There may have been more than that number attacked?—I do not think there is any reason to suppose there may have been many more, because inquiry was always made in the case of death.

25,114. There may have been many more attacked than were registered, might there not?—Not many, in Mysore they got the reports of the attacks, and they would always inquire.

25,115. You think the reports were accurate?—Yes, the people had no reason for misleading in Mysore.

25,116. You say "number of inoculates, 118, number "verified by comparison certificates, 98"?—Yes.

25,117. That means, that in 20 cases you could not find the certificate?—Either the certificate could not be found or the entry could not be found in the register.

25,118. So that there is a possibility of 17 per cent. of people having died who were not inoculated, or that the certificates had disappeared, or had been kept by the people themselves?—Yes, or that the inoculation had been done in all cases, and that the register had not been properly kept.

25,119. So that there is a possibility of an error of 17 per cent.?—Yes.

25,120. (*The President.*) With regard to the Statement No. I. referring to Mysore city, how do you account for the very large number of cases in Ward No. 1 relatively to other wards?—I think the return is unreliable, because the deaths in the third ward were not separately entered till the 13th January. Therefore, I think a great number of them may have been entered under the first ward.

25,121. The figures are not altogether reliable?—No, but the disease has been worse in the first ward. It started there.

25,122. It was worse there?—It has been actually worst there. I do not think the ward was worse than the other parts of the town from a sanitary point of view.

25,123. It only commenced there?—Yes, it got a footing there.

25,124. You have already expressed the opinion that Mysore is, on the whole, a dirtier place than Bangalore?—Yes.

25,125. Is there any difference with regard to overcrowding or the character of the houses?—The back streets are generally narrow and dirty in Mysore. I cannot speak of the overcrowding.

25,126. There is a larger population, I suppose, in a smaller space?—Yes.

25,127. Overcrowding is not only in the houses but in the area, I suppose?—In the area. I do not know about overcrowding in the houses. The area is smaller.

25,128. I see that you have made some reference to cholera having practically disappeared from Mysore since 1891?—Yes.

25,129. Can you tell us what kind of water supply there is in Mysore?—Cholera has disappeared since the new water supply was introduced.

25,130. Why do you think disinfection is quite useless under this voluntary system?—Because there is no certainty of doing it in time, and it can very likely only be done several days after the infection has had a start.

25,131. Therefore, if the voluntary system has any advantage, it is independent of any advantage gained by disinfection?—That is so.

25,132. You do not think it quite fair to compare the conditions in Bangalore so far as plague is concerned, with the conditions in Mysore, at present?—I think it would be dangerous to draw too strict a comparison between the two places.

25,133. Experience may, for anything you know, show that it would become much worse in Mysore than it has been even in Bangalore?—Yes, it might; but it does not seem likely to do so.

25,134. What was the great difference in the system pursued in Bangalore compared with the system followed in Mysore?—In Bangalore there was compulsory segregation—the sick person was always taken off to the hospital at once, and the persons living in the same house were removed to the segregation camp for ten days. In Mysore, no person has been removed to hospital except of their own consent, or with that of their relatives, and taking people to the segregation camp has been entirely voluntary. No one has been obliged to go there who did not wish to go.

25,135. Therefore, Mysore had not the advantages of so-called evacuation?—No, there was no evacuation in Mysore.

25,136. Therefore, there is no comparison to be made between Mysore and Bangalore with regard to evacuation in one and its absence in the other?—With regard to evacuation in large areas, in neither; but in Bangalore Civil and Military Station there was evacuation in some small areas, but that was subsidiary to other plague measures.

25,137. But judging between the native towns in both cases?—There has been no evacuation on a large scale in either.

25,138. I think you wish to put in some further tables which you have prepared, regarding the military population, and to make a statement with regard to the number of uninoculated people?—Yes, the statements are as follows:—

Mr. P. R. Cadell, I.C.S.

14 Mar. 1899.

2ND MADRAS LANCERS.

——	Fighting Men.		Followers and Families.		Plague Attacks.											
					Fighting Men.						Followers and Families.					
					Among uninoculated.		Among once inoculated.		Among twice inoculated.		Among uninoculated.		Among once inoculated.		Among twice inoculated.	
	Number once inoculated.	Number twice inoculated.	Number once inoculated.	Number twice inoculated.	Seizures.	Deaths.	Seizures.	Deaths.	Seizures.	Deaths.	Seizures.	Deaths.	Seizures.	Deaths.	Seizures.	Deaths.
Week ending 14th October 1898 -	61	—	283	—	—	—	—	—	—	—	—	—	—	—	—	—
„ 21st „ -	186	—	718	—	1	—	—	—	—	—	—	—	—	—	—	—
„ 28th „ -	360	—	816	—	2	2	—	—	—	—	—	—	—	—	—	—
„ 4th November 1898 -	492	—	1,085	—	—	—	3	2	—	—	2	1	—	—	—	—
„ 11th „ -	541	—	1,112	—	—	—	5	4	—	—	5	4	6	4	—	—
„ 18th „ -	553	—	1,298	—	—	—	1	—	—	—	—	—	6	4	—	—
„ 25th „ -	251	303	863	435	—	—	—	—	—	—	—	—	—	—	—	—
„ 2nd December 1898 -	181	373	805	493	—	—	2	1	—	—	—	—	1	1	—	—
„ 9th „ -	127	427	708	590	—	—	—	—	—	—	—	—	1	—	—	—
„ 16th „ -	67	487	653	645	—	—	1	—	—	—	—	—	1	1	1	1
											Afterwards free.				—	—
Total - -	—	—	—	—	3	2	12	7	—	—	7	5	15	10	1	1

——	Fighting Men.	Followers and Families.
Total strength - -	554	1,298

——	Seizures.	Deaths.
Total among uninoculated - -	10	7
„ once inoculated -	27	17
„ twice „ - -	1	1
Total - -	38	25

"Q. O." SAPPERS and MINERS.

——	Fighting Men.		Followers and Families.		Plague Attacks.											
					Fighting Men.						Followers and Families.					
					Among uninoculated.		Among once inoculated.		Among twice inoculated.		Among uninoculated.		Among once inoculated.		Among twice inoculated.	
	Number once inoculated.	Number twice inoculated.	Number once inoculated.	Number twice inoculated.	Seizures.	Deaths.	Seizures.	Deaths.	Seizures.	Deaths	Seizures.	Deaths.	Seizures.	Deaths.	Seizures.	Deaths.
Week ending 14th October 1898 -	380	—	43	—	—	—	—	—	—	—	—	—	—	—	—	—
„ 21st „ -	382	—	43	—	—	—	—	—	—	—	—	—	—	—	—	—
„ 28th „ -	749	—	778	—	2	2	—	—	—	—	6	4	—	—	—	—
„ 4th November 1898 -	766	—	1,532	—	—	—	—	—	—	—	—	—	—	—	—	—
„ 11th „ -	767	—	1,584	—	—	—	2	—	—	—	—	—	2	2	—	—
„ 18th „ -	740	28	1,589	38	—	—	1	—	—	—	—	—	4	—	—	—
„ 25th „ -	738	30	1,560	67	—	—	5	3	—	—	—	—	1	—	—	—
„ 2nd December 1898 -	421	347	1,536	109	—	—	—	—	—	—	—	—	—	—	—	—
„ 9th „ -	348	421	1,345	306	—	—	1	1	—	—	—	—	2	1	—	—
Total - -	—	—	—	—	2	2	9	4	—	—	6	4	9	3	—	—
									Afterwards free.							

——	Fighting Men.	Followers.
Total strength -	769	1,651
Evacuated lines on -		
Cases after evacuation		

——	Seizures.	Deaths
Total among uninoculated - -	8	6
„ once inoculated - -	18	7
Total - -	26	13

Mr. P. R. Cadell, I.C.S.

14 Mar. 1899.

1st Madras Pioneers.

	Fighting Men.		Followers and Families.		Plague Attacks.											
					Fighting Men.						Followers and Families.					
					Among uninoculated.		Among once inoculated.		Among twice inoculated.		Among uninoculated.		Among once inoculated.		Among twice inoculated.	
	Number once inoculated.	Number twice inoculated.	Number once inoculated.	Number twice inoculated.	Seizures.	Deaths.	Seizures.	Deaths.	Seizures.	Deaths.	Seizures.	Deaths.	Seizures.	Deaths.	Seizures.	Deaths.
Week ending 14th October 1898	3	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
„ 21st „	251	—	1	—	—	—	—	—	—	—	—	—	—	—	—	—
„ 28th „	315	—	54	—	1	1	—	—	—	—	—	—	—	—	—	—
„ 4th November 1898	528	—	532	—	—	—	—	—	—	—	—	—	—	—	—	—
„ 11th „	772	—	785	—	—	—	—	—	—	—	—	—	—	—	—	—
„ 18th „	772	—	785	—	—	—	—	—	—	—	—	—	1	—	—	—
„ 25th „	554	221	759	26	—	—	—	—	—	—	—	—	—	—	—	—
„ 2nd December 1898	554	221	759	26	—	—	—	—	1	1	—	—	—	—	—	—
„ 9th „	348	447	736	50	—	—	—	—	1	1	—	—	—	—	—	—
„ 16th „	213	584	727	59	—	—	—	—	—	—	—	—	—	—	—	—
„ 23rd „	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
„ 30th „	—	—	—	—	—	—	—	—	—	—	—	—	—	—	1	1
„ 6th January 1899	—	—	—	—	—	—	—	—	—	—	—	—	3	—	—	—
„ 13th „	—	—	—	—	—	—	—	—	—	—	—	—	1	—	—	—
„ 20th „	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
Total	—	—	—	—	1	1	—	—	2	2	—	—	5	—	1	1

	Fighting Men.	Followers.
Strength	797	786

	Seizures.	Deaths.
Total among uninoculated	1	1
„ once inoculated	5	—
„ twice „	3	3
Total	9	4

17th Madras Infantry.

	Fighting Men.		Followers and Families.		Plague Attacks.											
					Fighting Men.						Followers and Families.					
					Among uninoculated.		Among once inoculated.		Among twice inoculated.		Among uninoculated.		Among once inoculated.		Among twice inoculated.	
	Number once inoculated.	Number once inoculated.	Number once inoculated.	Number once inoculated.	Seizures.	Deaths.	Seizures.	Deaths.	Seizures.	Deaths.	Seizures.	Deaths.	Seizures.	Deaths.	Seizures.	Deaths.
Week ending 14th October 1898	193	—	2	—	—	—	—	—	—	—	—	—	—	—	—	—
„ 21st „	193	—	10	—	—	—	—	—	—	—	—	—	—	—	—	—
„ 28th „	304	—	78	—	1	1	—	—	—	—	—	—	—	—	—	—
„ 4th November 1898	762	—	768	—	1	1	1	1	—	—	—	—	—	—	—	—
„ 11th „	774	—	932	—	—	—	1	1	—	—	—	—	—	—	—	—
„ 18th „	774	—	932	—	—	—	—	—	—	—	—	—	—	—	—	—
„ 25th „	591	185	787	145	—	—	3	3	—	—	—	—	2	1	—	—
„ 2nd December 1898	320	460	582	377	—	—	1	—	—	—	—	—	—	—	—	—
„ 9th „	142	638	406	553	—	—	—	—	—	—	—	—	1	1	—	—
„ 16th „	63	717	258	701	—	—	—	—	—	—	—	—	2	1	—	—
„ 23rd „	50	730	171	788	—	—	—	—	—	—	—	—	—	—	—	—
„ 30th „	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
„ 6th January 1899	—	—	—	—	—	—	1	—	—	—	—	—	—	—	—	—
„ 13th „	—	—	—	—	—	—	—	—	—	—	—	—	1	—	—	—
„ 20th „	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
„ 27th „	—	—	—	—	—	—	—	—	—	—	—	—	1	—	—	—
3rd February 1899	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
Total	—	—	—	—	2	2	7	5	—	—	—	—	7	3	—	—

	Fighting Men.	Followers.		Seizures.	Deaths.
Strength - -	780	959	Total among uninoculated - - -	2	2
			„ once inoculated - -	14	8
			„ twice „ -	—	—
			Total - -	16	10

FOLLOWERS and their FAMILIES of the COMMISSARIAT and TRANSPORT DEPARTMENT.

	Number once inoculated.	Number twice inoculated.	Plague Attacks. Among uninoculated.		Among once inoculated.		Among twice inoculated.	
			Seizures.	Deaths.	Seizures.	Deaths.	Seizures.	Deaths.
Week ending 14th October 1898	—	—	—	—	—	—	—	—
„ 21st „ - -	56	—	2	2	—	—	—	—
„ 28th „ -	418	—	—	—	—	—	—	—
„ 4th November 1898	739	—	—	—	—	—	—	—
„ 11th „ -	988	—	—	—	—	—	—	—
„ 18th „ -	1,088	—	—	—	—	—	—	—
„ 25th „ -	1,215	57	—	—	—	—	—	—
„ 2nd December 1898	1,215	57	1	—	1	—	—	—
„ 9th „ -	1,166	106	—	—	1	1	—	—
„ 16th „ -	1,146	126	2	1	5	3	—	—
„ 23rd „ -	—	—	—	—	1	1	—	—
„ 30th „	—	—	—	—	—	—	—	—
„ 6th January 1899	—	—	—	—	—	—	—	—
„ 13th „ -	—	—	—	—	1	1	—	—
„ 20th „ -	—	—	—	—	—	—	—	—
„ 27th „ -	—	—	—	—	—	—	—	—
„ 3rd February 1899	—	—	—	—	1	—	—	—
Total - -	—	—	5	3	10	6	—	—

Total Strength.		Seizures.	Deaths.
1,272	Total among uninoculated - -	5	3
	„ inoculated - -	10	6
	Total - -	15	9

FOLLOWERS and their FAMILIES of 4TH (Q. O.) HUSSARS.

	Number once inoculated.	Number twice inoculated.	Plague Attacks. Among uninoculated.		Among once inoculated.		Among twice inoculated.	
			Seizures.	Deaths.	Seizures.	Deaths.	Seizures.	Deaths.
Week ending 14th October 1898	—	—	—	—	—	—	—	—
„ 21st „ -	—	—	—	—	—	—	—	—
„ 28th „	—	—	8	8	*2	*1	—	—
„ 4th November 1898	948	—	2	2	10	9	—	—
„ 11th „ -	1,033	—	13	12	2	2	—	—
„ 18th „ -	1,053	—	8	8	2	2	—	—
„ 25th „ -	1,105	—	2	—	1	1	—	—
„ 2nd December 1898	1,105	—	—	—	—	—	—	—
„ 9th „ -	487	618	—	—	—	—	—	—
„ 16th „ -	—	—	Afterwards free.		—	—	—	—
Total - -	—	—	33	30	17	15	—	—

* This does not agree with the date on which inoculation is stated to have begun.

Evacuated Quarters on 17th November.

Seizures after Evacuation.		Seizures.	Deaths.
3	Total among uninoculated - -	33	30
	„ inoculated - - -	17	15
	Total - -	50	45

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FOLLOWERS and their FAMILIES of J BATTERY, ROYAL HORSE ARTILLERY.

—	Number once inoculated.	Number twice inoculated.	Plague Attacks.					
			Among uninoculated.		Among once inoculated.		Among twice inoculated.	
			Seizures.	Deaths.	Seizures.	Deaths.	Seizures.	Deaths.
Week ending 14th October 1898	—	—	—	—	—	—	—	—
,, 21st ,, -	—	—	—	—	—	—	—	—
,, 28th ,, -	—	—	—	—	—	—	—	—
,, 4th November 1898	505	—	—	—	—	—	—	—
,, 11th ,, -	505	—	—	—	—	—	—	—
,, 18th ,, -	505	—	—	—	1	1	—	—
,, 25th ,, -	505	—	—	—	1	1	—	—
,, 2nd December 1898	505	—	—	—	—	—	—	—
,, 9th ,, -	93	412	1	1	—	—	—	—
,, 16th ,, -	93	412	—	—	1	1	—	—
Total - -	—	—	1	1	3	3	—	—

Not evacuated.

—	Seizures.	Deaths.
Total among uninoculated - -	1	1
,, inoculated - - -	3	3
Total - -	4	4

FOLLOWERS and their FAMILIES of the 21ST FIELD BATTERY, ROYAL ARTILLERY.

—	Number once inoculated.	Number twice inoculated.	Plague Attacks.					
			Among uninoculated.		Among once inoculated.		Among twice inoculated.	
			Seizures.	Deaths.	Seizures.	Deaths.	Seizures.	Deaths.
Week ending 14th October 1898	—	—	—	—	—	—	—	—
,, 21st ,, -	—	—	—	—	—	—	—	—
,, 28th ,, -	—	—	—	—	—	—	—	—
,, 4th November 1898	485	—	—	—	—	—	—	—
,, 11th ,, -	595	—	—	—	—	—	—	—
,, 18th ,, -	595	—	1	1	4	4	—	—
,, 25th ,, -	595	—	1	1	—	—	—	—
,, 2nd December 1898	595	—	—	—	1	1	—	—
,, 9th ,, -	372	223	1	1	1	1	—	—
,, 16th ,, -	—	—	—	—	4	3	—	—
,, 23rd ,, -	—	—	—	—	1	—	—	—
Total - -	—	—	3	3	11	9	—	—

Evacuated on 12th December 1899.

Seizures After Evacuation.	—	Seizures.	Deaths.
3	Total among uninoculated - -	3	3
	,, inoculated - -	11	9
	Total - -	14	12

FOLLOWERS and their FAMILIES of 25TH FIELD BATTERY, ROYAL ARTILLERY.

—	Number once inoculated.	Number twice inoculated.	Plague Attacks.					
			Among uninoculated.		Among once inoculated.		Among twice nioculated.	
			Seizures.	Deaths.	Seizures.	Deaths.	Seizures.	Deaths.
Week ending 14th October 1898	—	—	—	—	—	—	—	—
„ 21st „ -	—	—	—	—	—	—	—	—
„ 28th „ -	459	—	—	—	—	—	—	—
„ 4th November 1898	459	—	—	—	1	1	—	—
„ 11th „ -	578	—	—	—	1	1	—	—
„ 18th „ -	578	—	—	—	1	1	—	—
„ 25th „ -	667	—	2	2	—	—	—	—
„ 2nd December 1898	667	—	—	—	2	2	—	—
„ 9th „ -	427	240	4	4	6	6	—	—
„ 16th „ -	—	—	—	—	1	—	—	—
					Afterwards free.			
Total - -	—	—	6	6	12	11	—	—

Evacuated on 12th December 1898.

Strength.	Seizures after Evacuation.	—	Seizures.	Deaths.
667	1	Total among uninoculated -	6	6
		„ inoculated - -	12	11
		Total - -	18	17

STATEMENT No. I.

TOTAL of NATIVE TROOPS and FOLLOWERS.

—	Fighting Men.		Followers and Families.		Plague Attacks.											
					Fighting Men.						Followers and Families.					
					Among uninoculated.		Among once inoculated.		Among twice inoculated.		Among uninoculated.		Among once inoculated.		Among twice inoculated.	
	Number once inoculated.	Number twice inoculated.	Number once inoculated.	Number twice inoculated.	Seizures.	Deaths.	Seizures.	Deaths.	Seizures.	Deaths.	Seizures.	Deaths.	Seizures.	Deaths.	Seizures.	Deaths.
Week ending 14th October 1898 -	637	—	328	—	—	—	—	—	—	—	—	—	—	—	—	—
„ 21st „ -	1,012	—	828	—	1	—	—	—	—	—	2	2	—	—	—	—
„ 28th „ -	1,728	—	2,603	—	6	6	—	—	—	—	14	12	2	1	—	—
„ 4th November 1898 -	2,548	—	6,553	—	1	1	4	3	—	—	4	3	11	10	—	—
„ 11th „ -	2,854	—	8,112	—	—	—	8	5	—	—	18	16	12	9	—	—
„ 18th „ -	2,839	28	8,403	38	—	—	2	—	—	—	9	9	20	13	—	—
„ 25th „ -	2,134	739	8,056	730	—	—	8	6	—	—	5	3	3	2	—	—
„ 2nd December 1898 -	1,476	1,401	7,769	1,062	—	—	3	1	1	1	1	—	5	4	—	—
„ 9th „ -	944	1,933	5,739	3,098	—	—	1	1	1	1	6	6	12	10	1	1
„ 16th „ -	691	2,209	5,508	3,330	—	—	1	—	—	—	2	1	14	9	—	—
„ 23rd „ -	678	2,222	5,421	3,417	—	—	—	—	—	—	—	—	2	1	1	1
„ 30th „ -	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
„ 6th January 1899 -	—	—	—	—	—	—	1	—	—	—	—	—	3	—	—	—
„ 13th „ -	—	—	—	—	—	—	—	—	—	—	—	—	3	1	—	—
„ 20th „ -	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
„ 27th „ -	—	—	—	—	—	—	—	—	—	—	—	—	1	—	—	—
„ 3rd February 1899 -	—	—	—	—	—	—	—	—	—	—	—	—	1	—	—	—
Total - -	—	—	—	—	8	7	28	16	2	2	61	52	89	60	2	2

—	Seizures.	Deaths.
Total among uninoculated - -	69	59
„ once inoculated -	117	76
„ twice „ - -	4	4
Total - -	190	139

Mr. P. R. Cadell, I.C.S.

14 Mar. 1899.

STATEMENT II.

Corps.	Strength.	Plague Seizures. Among uninoculated.	Among once inoculated.	Among twice inoculated.	Total.	Plague Deaths. Among uninoculated.	Among once inoculated.	Among twice inoculated.	Total.	Remarks.
A. Fighting Men.										
Q. O. Sappers and Miners	769	2	9	—	11	2	4	—	6	
2nd Madras Lancers	554	3	12	—	15	2	7	—	9	
1st Madras Pioneers	797	1	—	2	3	1	—	2	3	
17th Madras Infantry	780	2	7	—	9	2	5	—	7	
B. Followers and Families of Native Troops.										A. AND B.
Q. O. Sappers and Miners	1,651	6	9	—	15	4	3	—	7	Percentage of case mortality among uninoculated - 73·8
2nd Madras Lancers	1,298	7	15	1	23	5	10	1	16	„ „ once inoculated - 51·4
1st Madras Pioneers	786	—	5	1	6	—	—	1	1	„ „ twice inoculated 100·0
17th Madras Infantry	959	—	7	—	7	—	3	—	3	
Commissariat and Transport	1,272	5	10	—	15	3	6	—	9	Among Total 58·6
Total A. and B.	8,866	26	74	4	104	19	38	4	61	
C. Followers of British Troops.										C.
4th Q. O. Hussars	1,105	33	17	—	50	30	15	—	45	Percentage of case mortality among
J. Battery, R.H.A.	505	1	3	—	4	1	3	—	4	uninoculated - 93
21st F.B., R.A.	595	3	11	—	14	3	9	—	12	„ „ once inoculated - 88·4
25th F.B., R.A.	667	6	12	—	18	6	11	—	17	
Total C.	2,872	43	43	—	86	40	38	—	78	Among Total 90·7

In addition to the above, two European soldiers and a European child were attacked, of whom one died.
Two men of the Army Hospital Corps and one Recruit, 8th Madras Infantry, were attacked, of whom one died.
The above were all uninoculated.

	Attacked.	Died.	Percentage of Mortality.
The totals thus are among uninoculated	75	61	81·3
„ „ once inoculated	117	76	65
„ „ twice inoculated	4	4	100
Total	196	141	71·4

STATEMENT III.—NUMBER of DAYS AFTER INOCULATION that INOCULATED PERSONS were ATTACKED.

Number of Days after Inoculation	1	2	3	4	5	6	7	8	9	10	10 to 15.	16 to 20.	21 to 30.	31 to 40.	41 to 50.	51 to 60.	Over 60 Days.	Total.	Remarks.
Native troops	—	1	2	—	1	1	1	—	—	—	1	1	6	7	7	2	—	30	There is a discrepancy of two between the number of inoculates here shown as attacked, and the number shown in Statement II.
Families and followers of Native troops.	—	—	1	—	—	2	—	1	—	2	6	2	3	15	6	5	8	51	
Followers of British troops.	1	2	1	1	4	3	4	2	2	1	3	3	6	4	5	—	—	42	
Total	1	3	4	1	5	6	5	3	2	3	10	6	15	26	18	7	8	123	
	33																		

STATEMENT IV.—EVACUATION and SEGREGATION.

The following Corps segregated their contacts only:—

2nd Madras Lancers.
1st Madras Pioneers.
17th Madras Infantry.
Followers J. Battery, R.H.A.
Commissariat and Transport Followers.

The following evacuated their lines:—

—	Date.	Cases after Evacuation.
Q. O. Sappers and Miners - - -	16th November -	10*
Followers, 4th Q. O. Hussars - - -	17th November - -	3
Followers, 21st F.B., R.A. - -	12th December -	3†
„ 25th F.B., R.A. - - -	10th December - -	3

The numbers of the contacts segregated are not known. Only two contacts appear to have developed plague, but it is not certain whether this is the whole number.

* The Sappers were encamped on their Parade Ground, close to their old lines, and to infected quarters.
† Two of these were officers' syces who lived partly at the bungalows.

The exact number of the military population is not known, as there are always a certain number of unauthorised inhabitants of the lines. Taking it, however, at the figure of the Census of 1891, namely 15,133, the following figures may be taken:—

Total Population of the Station by Census of 1891.	Estimated Deaths from Plague to end of February.	Percentage of Mortality to Population.
100,081	4,779	4·77

Total Civil Population.	Estimated Deaths from Plague.	Percentage of Mortality to Population.
84,948	4,638	5·46

Total Military Population.	Deaths from Plague.	Percentage of Mortality to Population.
15,133	141	0·93

Exact figures are, however, obtainable with regard to the fighting men of the native regiments. Their strength during the epidemic of plague was (*vide* Statement II. of the papers submitted) 2,900 inoculated men, in addition to eight uninoculated men who were attacked by plague, or 2,908 in all. Of these 25 died from plague, or 0·86 per centum.

In estimating the probable number of deaths among the total population, a fairly high death rate from causes other than plague has been assumed, and no account has been taken of the considerable exodus which took place among the civil population, which, if it could be accurately estimated, would show the percentage to be even more favourable to the military population.

The Principal Medical Officer thinks — in the Resident's opinion, with justice — that, considering the fact that the native troops were living in the close neighbourhood of localities which suffered severely from plague, the extremely small mortality from plague among them is a strong testimony of the efficacy of the inoculations that were carried out among them at an early stage of the epidemic.

I have not entered the figures of the uninoculated in the statements, because it is impossible to arrive at them accurately. All the people in the lines were supposed to be inoculated, with the exception of old women and people in a delicate state of health, but it is obvious that a great number must have remained uninoculated, especially among the followers of the British troops. I think so, because a certain number of cases have occurred which have been recorded as being among uninoculated persons, when everyone was supposed to be inoculated. The figures regarding the strength of the fighting men of the native troops may be taken as absolutely accurate.

25,139. Can you give me an example of that?—One example is in J. Battery, R.H.A. In the week ending the 9th December there was one case among the uninoculated, although the whole strength of the battery was supposed to have been inoculated on the 4th November. Similarly, in the 21st Field Battery, there were three cases occurring among the uninoculated in the weeks ending the 18th and 25th November and the 9th December, while the whole strength of the battery was supposed to have been inoculated on the 11th November. Similarly, in the 25th Field Battery, there were six cases among the uninoculated, although the whole strength of the battery was supposed to have been inoculated.

25,140. You conclude from this that there were more non-inoculated people than was known?—I think so; they did not succeed in inoculating the whole number, except among the native fighting men, and possibly among the followers of the native regiments.

25,141. In all these cases in which you put deaths as having occurred among the inoculated people, I suppose the fact that these people had been inoculated was verified?—Yes; it was entered in the medical officer's book, with the date. I should like to put in a chart,* showing the number of cases among the inoculated and uninoculated, and the number of inoculated persons week by week. I wish to state, as an indication of the efficiency of the inoculation, that the total mortality from plague among the fighting men of the native troops was only 0·86 per cent., whereas among the civil population it was estimated at 5·46 per cent.

* *See* App. No. LXXII. in this Volume.

(Witness withdrew.)

Mr. E. L. Cappel, I.C.S., recalled and further examined.

Mr. E. L. Cappel, I.C.S.

25,142. (*Dr. Ruffer.*) You have handed in a statement concerning Hubli, giving the inoculated population, &c.?—Yes.

25,143. Comparing these figures with the figures* handed in by Captain Leumann we found the two did not agree. Can you give us some explanation of that fact?—The explanation of that is that Dr. Leumann, as he has stated in his evidence, found it necessary from time to time to correct the figures which were given to him by the Chief Superintendent and by the other officers concerned.

25,144. In what way did he correct them?—By further inquiry, and on several occasions the figures were rather materially corrected in that manner. Dr. Leumann has said that those corrections were communicated to me as Collector, but I have searched the files, and I do not find that such communications were made to me at all—at any rate, there is no record on the Collector's files at Dharwar. I only used the original figures, and he has put forward the corrected ones, and, therefore, those corrected figures will stand. There was another point brought forward in Dr. Leumann's evidence, which was that his figures and mine did not agree as to dates.

25,145. There is only a difference of three days, and probably that was the time that they took to reach you?—The reason was that Dr. Leumann's figures, and the figures of all the other officers concerned in plague duty, were required by me for the special purpose of a weekly report to Government, which had to leave my office, or at least to be made up, on Fridays. In order to get those figures in time, the officers reporting to me had to report on Tuesdays and that makes the three days difference.

* *See* App. No. XV. in Vol. I. and Question No. 3064.

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25,146. If you take your figures you will see you give an account of the plague as far as the week ending the 2nd of December, whereas Dr. Leumann only gives them to the 22nd of October. Must you not correct the other figures now, because we can only take Dr. Leumann's figures as far as the last date in his report?—There are no corrected figures. The figures that are given beyond that may be taken to be accurate. They are not corrected by anybody, and there is nothing to show that they require to be corrected.

25,147. But did not all the other figures require to be corrected up to the 22nd of October?—No, all the figures up to August were not challenged at all.

25,148. But from August?—From August Dr. Leumann made corrections occasionally.

25,149. I think he made corrections in all of them; is that not so?—Yes, but those corrections did not amount to anything very vital. There is a difference of about 3,000 in inoculated cases, which, considering that the total inoculations ran up to 40,000, will not vitiate any inferences to be drawn.

25,150. Then you have no further corrections to offer?—I have no further corrections to put forward.

25,151. Then we take Captain Leumann's figures, as far as they go, as being your own figures?—Yes. I put in a statement reconciling my figures with his, as follows:—

HUBLI.

Weeks ending.	Population for the Week.	Inoculated			Uninoculated.	Attacks in					Deaths in				
		Once.	Twice.	Total.		Once inoculated.	Twice inoculated.	Total inoculated.	Uninoculated.	Total.	Once inoculated.	Twice inoculated.	Total inoculated.	Uninoculated.	Total.
Up to June 17	47,427	2,323	531	2,854	44,573	Details of these corrected figures cannot be found.		1	123	124	—	1	1	106	107
„ „ 24	47,082	3,368	2,220	5,588	41,494			2	28	30	2	1	3	22	25
„ July 1	47,485	4,487	3,956	8,443	39,042			1	37	38	1	—	1	29	30
„ „ 8	46,537	5,057	5,420	10,477	36,020			4	52	56	3	3	6	55	61
„ „ 15	46,518	5,974	7,289	13,263	33,255			8	58	66	4	2	6	34	40
„ „ 22	45,240	6,565	8,959	15,524	29,716			13	100	113	1	6	7	82	89
„ „ 29	43,809	9,386	10,311	19,697	24,112			21	145	166	6	9	15	100	115
„ August 5	43,707	10,016	12,660	22,676	21,031			22	169	191	7	9	16	140	156
„ „ 12	42,768	11,339	15,845	27,184	15,584			37	317	354	5	14	19	272	291
„ „ 19	40,441	10,265	19,491	29,756	10,685			69	350	419	30	31	61	386	447
„ „ 26	39,400	9,671	23,362	33,033	6,367			66	446	512	21	20	41	371	412
„ September 2	38,210	7,569	26,547	34,116	4,094			32	393	425	8	20	28	328	356
„ „ 9	38,382	6,798	28,671	35,469	2,731			47	261	308	11	23	34	227	261
„ „ 16	38,408	6,381	30,911	37,292	1,116			55	153	208	7	40	47	138	185
„ „ 23	38,408	6,567	31,638	38,205	937			35	106	141	11	44	55	106	161
„ „ 30	39,315	6,820	31,872	38,692	603			28	76	104	4	16	20	58	78
„ October 7	39,500	5,258	33,963	39,221	279	4	11	15	41	56	4	10	14	40	54
„ „ 14	39,600	5,217	34,527	39,774	200	3	17	20	14	34	3	15	18	18	36
„ „ 21	39,953	5,286	34,763	40,049	200	—	6	6	11	17	—	6	6	11	17
„ „ 28	40,453	4,196	35,040	40,236	217*	1	4	5	3	8	1	4	5	3	8
„ November 4	40,693	5,100	35,229	40,329	364	—	2	2	7	9	—	2	2	6	8
„ „ 11	41,164	5,116	35,348	40,464	700	—	2	2	2	4	—	2	2	2	4
„ „ 18	41,643	5,117	35,397	40,514	1,129	—	—	—	2	2	—	—	—	2	2
„ „ 25	42,206	5,214	35,558	40,772	1,434	—	—	—	—	—	—	—	—	—	—
„ December 2	42,640	5,157	35,959	41,116	1,524	—	—	—	3	3	—	—	—	3	3
Average	42,040	—	—	—	—	—	—	—	—	—	—	—	—	—	—
Total	—	—	—	—	—	—	—	491	2,897	3,388	129	278	407	2,539	2,946

* The rising figures represent new comers.

	Percentage of*		Percentage of Deaths to Attacks in			General Rate.
	Attacks to Population.	Deaths to Population.	Once inoculated.	Twice inoculated.	Uninoculated.	
1. Dharwar	6*	4·4*	36·5	24·1	78·1	73·3
2. Hubli	8·06*	7· *	82·9†	82·9†	87·6†	86·9†
	Percentage of Deaths (Plague) to Population.*					
— Dharwar	—	—	3·2	·07	9·01	4·4
— Hubli	—	—	·3	· 6	6·04	7·

* Taken on average population, which is, for Dharwar, 21,808, and for Hubli, 42,040.

† These figures are not worth much. They arise from an unduly low proportion to the total inoculations and should not be generalised from. Also, there is the fact that many inoculations were done on persons already incubating plague. (*See* Dr. Leumann's Report, App. XV. in Volume I.)

	Attacks in		Deaths in	
	Dharwar.	Hubli.	Dharwar.	Hubli.
Once inoculated	115	491	42	129
Twice inoculated	29		7	278
Uninoculated	1,172	2,897	916	2,539
General	1,316	3,388	965	2,946

25,152. I believe you have a statement to make with regard to the enormous number of deaths among the uninoculated during the last period?—Yes, this statement which was embodied in my evidence, showed that in the third week of September there were only 143 uninoculated persons, and that there were 99 attacks among those, giving a percentage of between 60 and 70 per cent. for one week. Those figures had been reported by me to Government in my weekly report, but I pointed out in those weekly reports, what is a sufficiently obvious thing, that percentages drawn on very small figures are unsafe, and that was eminently so in this case, because those figures turned out not to be right. By Dr. Leumann's corrected record, the figures for that week should be:—Uninoculated persons 937, inoculated 38,205, and plague deaths among the non-inoculated 106, and among the inoculated 55. The percentage works out for the uninoculated to 113 per 1,000 per week, which is very different to the percentage formerly given.

25,153. That is exceptionally high?—Yes.

25,154. How do you account for this very high mortality among the uninoculated?—I do not account for it at all. In my evidence formerly given I stated that the virulence of the disease was very great at that particular time, and in that particular part of the country, and that the villages adjoining Dharwar and Hubli, which were similarly situated, and, if anything, in a better sanitary position, and where no inoculation was done, suffered to the extent of from 30 to 50 per cent. in the course of from six to 12 weeks, whereas the Hubli epidemic lasted 22 weeks.

25,155. One hundred and thirteen per mille would kill everyone in the town in less than eight weeks?—But 50 per cent. in six weeks will clear it out in 12.

25,156. Have you any other villages or towns of the same size as Hubli, showing a mortality from plague equal to that?—No. I would like to point out that, taking Dr. Leumann's figures, they are very small to draw percentages upon, and the deduction is not a very safe one.

25,157. We had it in evidence before us from several people, that the uninoculated population in Hubli was greater than that given in Dr. Leumann's Report. It has been estimated by several witnesses as being about 5,000?—I very much question that. No officer concerned with those operations could say, with certainty, that a census of that kind is absolutely accurate. There must be a margin of error, seeing the conditions in which those things are taken in India. But to assert that the census was out to that extent appears to me to be a statement very wide of the mark. The plague census was taken in exactly the same way as the Imperial Census was taken.

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25,158. When was the first census taken?—In October or thereabouts.

25,159. When did plague break out?—About that time. It was taken in exactly the same way as the Imperial Census was taken, that is to say, the houses were numbered, the town was divided into sub-divisions and wards, and a separate census paper was made out for each house. These papers were bound up in volumes, and constituted the record of each Supervisor and Superintendent, who went about with them daily in their hands. That was the first document on which the Supervisors and Superintendents worked. It was by these census records of each house that they tested whether people had arrived or had left, or were sick or had died. After the census was first made at the end of 1897, it was tested weekly during the operations by a corrected record being sent in for the purposes of that week's plague report.

25,160. Then you cannot account for that statement as given to us?—I think that statement errs very much on the side of exaggerating the error.

25,161. Is there anything else you would like to add?—I may say with regard to the discrepancy between Dr. Leumann's figures and my own, that the figures of incidence were never changed at all. They were his own figures, and there is no change to be made in them.

25,162. Do you put in maps showing the incidence of disease in Dharwar and Hubli?—Yes.*

25,163. (*The President*) Having regard to the Dharwar district generally, am I right in supposing that there has been a very remarkable amount of inoculation done in that district?—Yes, we have, up to date, inoculated nearly 100,000 people in the Dharwar district, nearly all of whom have been inoculated twice.

25,164. At what stage in the prevalence of plague was the inoculation commenced? Early or late?—Quite early in every case.

25,165. Am I right also in believing that there has been a very large amount of plague in your district?—Yes.

* *See* Appendices No. LXXIII. and LXXIV. in this Volume.

(Witness withdrew.)

SARDAR KHAN BAHADUR MUHAMMAD YAKUB KHAN called and examined.

Sardar Khan Bahadur Muhammad Yakub Khan.

25,166. (*The President.*) You are now on special plague duty in Bombay?—Yes.

25,167. When were you placed on this special duty?—On the 7th October last, in Bombay.

25,168. Previously to that you had been working in Karachi, I think?—Yes, on plague duty, but not immediately before that. Immediately before that I was Acting Deputy Commissioner in the Thar and Parkar district.

25,169. Were you on duty during both epidemics in Karachi?—Yes—the first being in 1897 and the second in 1898.

25,170. Will you describe the system which you found best suited for conducting plague operations?—Yes. My district there consisted of several villages of Musalmans. In each village I formed a committee of the leading members of the village, and conducted my operations through them, and with their co-operation.

25,171. What did these operations consist of?—Before I went there, there was opposition even to disinfection of houses, and to the segregation of contacts. In those days the sick were not removed to hospitals. After securing the co-operation of the Headmen, we arranged for segregation camps, and removed the contacts from the infected houses to the segregation camps and the sick to the sick camps. That was always done by the Headmen themselves.

25,172. What special advantages do you claim for your system?—The people were ignorant and excitable; through the Headmen they understood things better, and things went on more smoothly, and, I think, more quickly.

25,173. You got earlier information of plague cases, and were able more easily to remove the sick and the contacts?—Yes.

25,174. Was any evacuation of a large area attempted?—Yes, on a large scale. In the first year, three whole villages were moved into health camps one after another. That was arranged through the Headmen.

25,175. What was the population of each village?—In the first village it was 1,600. Half went away to the hills and the other half came into camp. The second village had 913 people.

25,176. They all went out?—Yes.

25,177. And the third village?—I have not the figures at hand now for the third village.

25,178. Was it smaller than either of the other two, or larger?—It was smaller than either of them.

25,179. Had plague already taken a strong hold in these villages before evacuation?—Yes.

25,180. In general terms, what was the result of the evacuation so far as the spread of plague was concerned?—With regard to the first village, we had no plague case in the camp after the first ten days. I say, from recollection, that there were only 18 cases in the first ten days.

25,181. How many cases had there been before evacuation in the first village?—There were about five cases a day before evacuation.

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25,182. How many days had that been proceeding?—Not less than ten days.

25,183. Now, with regard to the second village?—In the second village there were three plague cases only after evacuation. Before evacuation, we had two or three cases every day.

25,184. For how many days?—I should say for one week.

25,185. Now, with regard to the third village?—I have not the figures for that.

25,186. What is your opinion as to the value of evacuation in stopping an extending epidemic?—I am strongly of opinion that it stops it.

25,187. Did you have any experience in Karachi city?—My quarter formed part of the city. The people from Karachi city in the second epidemic camped outside in what is called the Trans-Lyari area. Half of that area was given to my charge. In all 30 voluntary camps were made by the people in my portion, and the population of that was 14,000. The other portion was given to another officer, and there were 11,000 people there.

25,188. Making a total of 25,000 all out at the same time?—Yes.

25,189. Had that a beneficial effect upon the spread of plague in Karachi?—Decidedly it had, I was watching each camp, and after ten days I did not find plague cases there. If there was any rare case found, I traced it to some infected people coming in without permission.

25,190. How long were the people kept out of their houses?—It varied slightly; on an average six weeks to eight weeks.

25,191. What was done with the evacuated houses before the re-admission of the people?—I was not concerned in that; it was done in the City Proper, but I know that there was an arrangement for general disinfection of all houses in the city. It was very thoroughly done.

25,192. Was it by chemical disinfectants?—Yes, under the supervision of medical officers specially appointed.

25,193. Were the houses opened up and unroofed?—Where it was necessary; not always.

25,194. What is your opinion of the duration of a plague epidemic if it be not interfered with?—I have not had experience of that.

25,195. If it be interfered with, what do you think is the general duration?—Four months.

25,196. That will depend upon how it is interfered with; you have already said it appeared to stop in ten days?—Yes, ten days for the people, but for the infected locality, I should say, two months. I would not see it re-occupied before two months.

25,197. Have you had any experience of plague affecting a second time a house in which it had occurred before?—Yes, my impression is that a large proportion of plague cases occur in those houses in which plague cases have previously occurred.

25,198. Is that after they have been vacated for several weeks, and disinfected?—No, I cannot say in those which had been vacated for several weeks, but in those which had been vacated for the ordinary period, namely, 10 days, and disinfected in the best manner laid down for us, cases did occur in the second epidemic.

25,199. Have you had any experience of inoculation?—Not much, except in respect of a portion of the population of two villages. In one village about 300 persons were inoculated, and I turned out about 200 from that village into a health camp when I found the village was getting infected.

25,200. The 300 people who remained were inoculated, and the 200 who were evacuated were not inoculated?—That is so. The 300 chose to get inoculated because they did not wish to leave the village. There were two cases of plague after inoculation, not before a fortnight I think.

25,201. Had there been many cases before evacuation?—Two per day.

25,202. Have you any other instance?—In another village 200 Memons were inoculated, they also got themselves inoculated because they did not wish to be turned out of their village. In that village there was about one case per day before the inoculations. After the people were inoculated there was no plague case in that village.

25,203. How many people were there in this village altogether?—These 200, and 100 more.

25,204. What became of the other 100?—I sent them to a health camp.

25,205. Before the inoculations?—Yes.

25,206. It is a similar case to the former one?—Yes.

25,207. In the case of plague becoming indigenous in a town of, say, 5,000 or 6,000 people, what would be your advice?—My advice would be to evacuate the town, if that were possible.

(Witness withdrew.)

Khan Bahadur B. B. Patel.

KHAN BAHADUR BOMANJI BYRAMJI PATEL called and examined.

25,208. (*The President.*) I believe you are a Justice of the Peace?—Yes.

25,209. What has been your relationship to the plague epidemic?—For the last two years I have been a volunteer in searching for plague cases.

25,210. (*Mr. Cumine.*) Are you a volunteer witness also?—Yes.

25,211. In what part of the city did your work lie?—In the Fort district.

25,212. From what period did it extend?—It commenced from March 1897.

25,213. Up to when?—Up to now.

25,214. To what class of the community are your observations chiefly confined?—Chiefly to the Parsees.

25,215. To how many of them?—Nearly 8,000. There are 360 houses in the block which is entrusted to me.

25,216. What proportion would that form of the total population in Bombay?—About one-sixth of the Bombay Parsee population.

25,217. How many plague cases among the Parsees have come under your immediate observation?—Nearly 125.

25,218. Have you noticed whether deaths among rats have generally preceded, or followed, attacks amongst human beings?—Preceded.

25,219. Is it, in your opinion, dangerous to continue living in a house where dead rats have been found?—Yes. I have explained the matter to several people, and told them not to live in houses where dead rats were found, but, notwithstanding, they live there. Within the last week I have seen plague cases where dead rats have been found.

25,220. Have you found that in a certain class of houses the death of rats is not necessarily followed by plague amongst human beings, and if so, in what class?—In some large and well-ventilated houses I have seen dead rats on the ground floor and on the upper storeys, and no plague cases have occurred, even on the ground floor.

25,221. Do you think segregation has had a good effect in Bombay?—It has had a very good effect among the Parsees.

25,222. How has that effect shown itself?—This year's mortality among the Parsees is about three-fourths less than the previous year.

25,223. Is there more segregation this year than there was before?—Yes. Nearly 2,500 people from the Fort have been segregated.

25,224. What do you call segregation?—The people live in the camp.

25,225. That is evacuation?—Yes. They have lived in camp on the Esplanade, on the Chowpati, and in six more other localities.

25,226. Voluntarily?—We persuaded some people to go out. Of course all did not go voluntarily. In several cases where dead rats were found, we persuaded them to go out.

25,227. Have you noticed whether plague cases this year occur sometimes in the same houses, or whether they occur in different houses?—Cases have occurred in the same houses, and I have seen cases occur even in the same rooms.

25,228. I suppose cases have also occurred to a large extent in the other houses and other rooms?—Yes, in other houses also.

25,229. Do you consider from what you have seen that plague is contagious?—No. Yet being a layman I cannot form an exact opinion, though I have seen people embracing, hugging, and kissing patients. Last week I saw in one family people embracing a patient.

25,230. What has the result been?—They have not been attacked.

25,231. Last week is rather recent?—I saw similar instances in January; but last week there was a plague case in my block, and I saw the people embracing the patient.

25,232. Have you had any considerable experience of Professor Haffkine's inoculations?—Yes.

25,233. How many people have been inoculated?—Amongst the Parsees 6,478 people have been inoculated since the year 1898, and there has been no death amongst them within the last six months from the date of their inoculations.

25,234. If there had been any deaths would you have heard of them?—Yes. We keep a register of deaths in our Towers of Silence. The register states whether the people are inoculated or not. It also gives the cause of death.

25,235. Has anybody examined that register to see to what extent inoculated people have died of plague?—All the particulars are in the register; whether they have died from plague or not, and whether they are inoculated or not.

25,236. Have you examined the registers often?—Yes.

25,237. You say over 6,000 people have been inoculated. That would be about one-eighth of the population. Then, even though the inoculated people died at the same rate as the uninoculated, you would expect to find in your register seven who died uninoculated to one who died inoculated, would you not?—Yes. Several people have died from plague one year, or nine months or eight months, after inoculation, but not within six months.

25,238. Have you worked out the figures from the registers?—No; from newspapers. I saw it in the Government Gazette. *Khan Bahadur B. B. Patel.* 14 Mar. 1899.

25,239. Has any case come under your notice where an inoculated Parsee has caught plague and died?—Yes; but the death has occurred after six months from the date of inoculation.

25,240. How many?—I cannot give the definite number, but I have seen some two or three inoculated cases having recovered.

25,241. (*Dr. Ruffer.*) How many inoculated people have died from causes other than plague?—A great many. Some have died of consumption, and some from fever and dysentery.

25,242. Can you tell us how many have died from causes other than plague?—No; I cannot tell you the exact number.

25,243. (*The President.*) Has your experience been chiefly amongst the Parsees?—Yes, amongst the Parsees only.

25,244. Do they dwell in rather better houses than the majority of the people?—Most of them dwell in better houses than the majority of the people. The 125 cases were mostly on the ground floors; and not on the upper floors.

25,245. When segregation was adopted this year, was it ever applied to people living in houses where there had been no plague cases, but which adjoined plague houses?—To people in houses in which plague cases occurred, or in which dead rats were found.

25,246. Not to adjoining houses?—No. We give people permission to go and live in the huts when dead rats are found, or when plague cases occur in their houses or in the neighbouring houses. When there are no plague cases, and when no dead rats are found, we do not give permission to the people to go and live in the huts.

25,247. So that the evacuation has been restricted to houses in which plague cases have occurred or dead rats have appeared?—Yes, so far as the Parsees are concerned.

(Witness withdrew.)

Dr. U. L. Desai called and examined. *Dr. U. L. Desai.*

25,248. (*The President.*) What are your qualifications?—M.B. and B.C.H., Victoria University, Owen's College, Manchester; L.R.C.P., L.R.C.S., Edinburgh; L.M., Edinburgh; L.F.P. and S.G., M.D., Belge, M.C.P., A.C.P., London; Consulting Physician Moah and Porwad Plague Hospital.

25,249. What is your opinion as to the cause of plague?—It is a manifestation of septicæmia by a specific virus in a filth diathesis.

25,250. In regard to the propagation of plague, what, in your opinion, are the chief causes of that propagation?—The chief cause is the infection by bacillus.

25,251. How is it spread or propagated?—By direct contact, by contagion, by inoculation from the earth, by aerial infection, by aqueous infection, by clothes and other fomites.

25,252. Have any of the lower animals got anything to do with it?—Yes.

25,253. What animals?—Rats, and other rodents.

25,254. Have you got any examples in which rats appear to have actually produced the disease in human beings?—I have seen one or two cases in which dead rats have been found in the houses, and the people living in the houses have vacated them at once, and then plague has broken out in the next house.

25,255. In the house without the rats?—Where the rats have not died.

25,256. How do you associate that contagion with rats if rats were not in the house in which plague occurred?—Before the rats came to that particular house where they died they must have been moving about; they do not stay in one particular locality.

25,257. These people might also have come in contact with plague contagion in other ways, might they not?—Yes, they might.

25,258. Have you anything to show that rats did it, anything more clear and definite?—No, except that I have observed plague invariably breaking out in houses not vacated after rats dying in them.

25,259. What is your opinion as to the origin; is it an introduction, or does it arise spontaneously?—From analogy, from cholera and other specific diseases, I think the disease can arise *de novo*.

25,260. The only reason for saying that is analogy?—Yes.

25,261. No fact in relation to plague itself?—No. It being a specific disease, it could not differ in its methods of origin and propagation from other diseases of the same class.

25,262. What are the most important predisposing causes of plague in your view?—Overcrowding, lowering of the vitality of the people by habitual derangement of health, as over fatigue, anxiety, habitual disregard of hygienic rules, diet, exercise, clothing, air, and water.

25,263. Everything insanitary?—Yes, both from a personal and a general point of view.

25,264. Amongst which things you place overcrowding first?—Yes. Overcrowding, and the pollutions of earth, air, and water resulting from it.

25,265. What about the exciting causes, or immediate causes?—Infection by a specific bacillus.

25,266. Has it come within your observation that any special classes of the community are more liable to plague than other classes?—The poorest, who live in damp filthy localities, are the most subject to this class of disease.

25,267. That is a new condition. You did not mention that before. That you distinguish from overcrowding?—Yes. They might act together or separately, overcrowding, filth, and dampness. Before plague broke out in Bombay, Poona, and Surat exceptionally heavy rains had fallen, increasing dampness and atmospheric humidity, besides the rise of subsoil water level.

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25,268. You think dampness in an open house might be a strongly acting cause?—Indirectly, because dampness will give a fit condition for the bacilli to grow in. A certain amount of moisture and heat are necessary for the growth of bacilli. Dampness supplies moisture.

25,269. Overcrowding you still keep in the first place?—Yes.

25,270. What communities are least liable to plague?—Europeans.

25,271. How do you explain their relative immunity?—By being removed from overcrowding and the causes of infection, in the first place; and in the second, by having more vitality in them.

25,272. What do you mean by vitality?—Capacity to resist the inroad of a disease. They are a better-lived class of people. Their nourishment is of a superior order to that of other communities that contract plague.

25,273. Is not that covered by what you mean by vitality?—Yes. Good nourishment means more vitality.

25,274. Are there any other causes?—The sanitary surroundings in which they live.

25,275. And that other great factor which you have referred to, overcrowding, has that anything to do with it?—Yes; it is much less amongst Europeans.

25,276. What do you think should be the plan of campaign against plague?—Battle against plague has to be waged not only as against a specific disease, but as against general causes of unhealthiness leading to lowered vitality of the masses, and consequent increased mortality. It will have to be a huge war against the continued forces of hereditary habits, religious prejudices, conserved customs, and extreme ignorance of the laws of personal, social, and public hygiene, which go to make up the social life of an Asiatic. It is to be a thorough meeting of the sanitary and medical needs of the country, by numerous compulsory enactments and institutions, which shall be bitter pills for the masses of India to swallow, and unless that is done, plague germs shall find fit nidus to grow, multiply, and cause ravages in various parts of India.

25,277. Therefore you think that a large improvement in the first place in the sanitary condition of the people is the fundamental reform required for diminishing plague epidemics; is that your view?—Yes.

25,278. What are the chief remedies you would propose against plague?—The chief remedies against plague are:—

Temporary.—Isolation of those attacked from the healthy, and removal of the inhabitants of the affected area to healthier localities.

Permanent.—For Bombay:—

1. To improve its drainage and sewerage. The most of the drains of Bombay are below sea level, some of it even below spring-tide low water mark, and nearly the whole of it too slightly elevated to admit of gravitation into the sea, at a distant point of outfall, and hence, improvement in the gradient is necessary.
2. An Act for the better housing of the people in Bombay, with a view to lessen the overcrowding in badly-constructed, ill-ventilated and ill-accommodated houses, and increase the responsibility of the proprietors of houses, that are let to lodgers.
3. Inspection of houses, as regards cooking, washing, sleeping, and other accommodations, and as regards walking barefooted on a filthy soil.
4. Necessity of a Rent Act, to control exorbitant rents by greedy proprietors.
5. Compulsory notification of infectious diseases, and to obtain this end, introduction of an Act for medical practitioners, qualified or unqualified, specifying the responsibility of such practitioners.

For Bombay and other Parts of India.

6. Food and Drugs Act.
7. A scheme to promote the knowledge of personal and General Hygiene among the inhabitants of India, as by instituting Degrees in Sanitary Science at Bombay, Calcutta, Madras, and Punjab Universities. To set a paper on this subject at the Matriculation and School Final Examinations, and make Sanitary Science a compulsory subject at the Indian Civil Service Examination.
8. To organise a Competitive Sanitary Civil Service for India.
9. A scheme for village sanitation and healthy water supply for human beings and cattle.
10. A scheme to supply better midwives for natives.
11. A scheme to direct the charitable instincts of the wealthy Indians to organise a system of medical relief in the homes of the poor.
12. An institution to supply nurses for infants and invalids among the Hindus and Muhammadans.
13. A scheme to encourage cremation of dead bodies of men and animals.
14. To enforce burning of all excretions and secretions of human beings and animals suffering from Infectious Diseases, particularly in Hospitals.

25,279. What do you mean by "Specifying the responsibility of such practitioners" as bearing upon the treatment of plague?—The first point is that, unless a man knows differential diagnosis, he cannot properly diagnose whether a case is plague or any other disease. It has actually come under my notice that a case of gonorrhœal bubo has been diagnosed as plague, and sent to a Plague Hospital in my charge.

25,280. You do not exclude unqualified practitioners?—I want to exclude them; but at present they are practising in India, and consequently I cannot exclude them. If an Act for Medical Registration is passed in India, the best thing would be to have an Act for qualified medical practitioners.

25,281. To register the qualifications of those who possess them?—Yes, and to give permission to qualified and registered practitioners only to practise, the same as it is in England.

25,282. Do you think the country is yet ripe for such a large measure?—Yes, I think it is quite ripe for the occasion, particularly the large towns of India, as Bombay, Madras, and Calcutta, where Medical Graduates of European and Indian Universities abound. In a city like Bombay the Act would work very well, now that the plague is on, for it will help the authorities to get at the real extent and home of plague which lies hidden under lies, quackery, and superstition.

25,283. I do not refer to the practitioners themselves, but in so far as the lower population is concerned. Do you think they are prepared for a measure of that kind?—When suttee and infanticide were practised, the measures taken for their suppression had to be strong measures. The will of the people is not to be taken into consideration, but their welfare is to be taken into consideration, and I think the British Government will confer the greatest boon upon India if they tried how this Act for qualified practitioners would work—at least in the city of Bombay. Numerous untold blessings to the ignorant masses will spring from this source.

25,284. What proportion, do you think, of the people do not employ qualified practitioners?—About three-fourths.

25,285. What proportion, do you think, do not employ any practitioners whatever?—At present the people refrain from taking any advice at all. They are afraid, if a case of fever is attended by a medical man, that he will report it as a plague case, and that the person may be removed to the isolation hospital.

25,286. I do not refer to the present period, but prior to this scare about plague. What proportion had no medical advice—no doctor?—Almost one-fourth.

25,287. And three-fourths do not have qualified practitioners?—No. This three-fourths include those that do not take any medical advice.

25,288. But still you think that a measure which prevented other than qualified practitioners from practising could be enforced—could be brought into operation?—Yes; provided vaids and hakims of a certain intellectual and moral standing already practising be given special licenses as qualified vaids and hakims, and an examining board for future vaids and hakims be instituted under Government supervision.

25,289. You think it would be very desirable?—It would be very desirable.

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25,290. (*Mr. Hewett.*) Do you know anything of any places outside the Presidency towns?—Yes. I have been in Surat and seen plague there.

25,291. Do you know anything about the feelings of Muhammadans in Northern India as regards the employment of vaids and hakims?—Yes.

25,292. What are their feelings?—The feeling is that they prefer vaids and hakims, but in places like the Punjab there are universities for vaids and hakims. The Punjab University has a scheme whereby it gives its qualifications to vaids and hakims after a certain source of study.

25,293. In European medicine?—No, in country medicine; but it is modified upon a European basis.

25,294. When you propose the registration of medical men, do you propose to register those who practise by the European system, or those who practise by native methods?—If a special examination is held for native medicines on the same lines as that of the Punjab University, they may be included as qualified practitioners, I would propose to register two classes of qualified practitioners, one practising the European system, enjoying greater freedom in the scope of the medical, surgical, and midwifery work to be performed by them, and with power to sign a certificate of death. The other a class of qualified vaids and hakims, whose sphere of work will be curtailed, so far as surgery, midwifery, and signing of death certificates were concerned. They might also be prevented from using certain poisonous drugs, as arsenic, aconite, mercury, morphia, &c. As the masses will realise the superior privileges enjoyed by the class practising the European system, the class of vaids and hakims will gradually diminish. I do not want to deprive the Hindus and Muhammadans of their vaids and hakims, as it partly is a religious question, but I wish to give them better vaids and hakims with some qualifications and a sense of responsibility, while practising a profession on which the life and death of a human being depends. I may mention that the question is apparently a complicated one, and is believed by many to be impossible of realisation, but I know for certain that the details are easy of arrangement; only that in the starting of this Act, vaids and hakims of a certain intellectual and moral standing already practising will have to be given licenses as qualified vaids and hakims without any examination, and a Board appointed under Government supervision to examine and grant qualifications as vaids and hakims in future.

25,295. Do you agree with the opinion expressed by many European medical men who have appeared before this Commission that the people do not like to be treated for plague according to European methods?—I do not agree with that opinion. I have been practising here for the last three years, and I know that they appreciate and prefer European medical attendance if they can afford it. The only bar is the heavy fees charged by European medical men. A love for skilful European medical and surgical practice is increasing even among the ignorant masses.

25,296. Are they in the habit of charging fees for attending plague cases?—Not now, because the plague cases are mostly attended in the hospital.

(Witness withdrew.)

(Adjourned till to-morrow.)

At The Secretariat, Bombay.

SIXTY-SIXTH DAY.

Wednesday, 15th March 1899.

PRESENT:

PROF. T. R. FRASER, M.D., LL.D., F.R.S. (*President*).

Mr. J. P. HEWETT.
Mr. A. CUMINE.
Dr. M. A. RUFFER.

Mr. C. J. HALLIFAX (*Secretary*).

Mr. C. H. Cayley, M.B.
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Mr. C. H. CAYLEY, M.B., called and examined.

25,297. (*The President.*) You are Master of Arts and Bachelor of Medicine?—Yes.

25,298. Have you an appointment here in connection with plague?—Not now.

25,299. You have had?—I was working in the laboratory with M. Haffkine.

25,300. As an assistant?—Yes.

25,301. (*Dr. Ruffer.*) You have made some observations on the presence of plague bacilli in the sputum?—Yes.

25,302. In cases of primary and secondary plague pneumonia?—Yes.

25,303. How did you examine the sputum in order to find the bacilli?—I dipped a glass rod in the sputum and inoculated a series of agar tubes with this glass rod, and immediately after inoculating the tubes, I covered them up from the light.

25,304. Do you find that an essential precaution?—I never found bacilli growing unless I did do that. Then the agar tubes were allowed to incubate for 24 or 48 hours, and from the colonies that developed fresh agar tubes, and also flasks of peptone broth, were inoculated.

25,305. Do you find the bacillus is often present in cases of primary plague pneumonia?—In all the cases I examined it was forming nearly pure cultures at first.

25,306. What is the sputum of plague pneumonia like at first?—A bright brick-pink and very glutinous, coughed up with difficulty. After a few days it gets looser and a darker colour, and as the case goes on it gradually becomes muco-purulent.

25,307. Do you find the number of bacilli increases or diminishes in the sputum?—It diminishes.

25,308. When does it begin to diminish?—In primary plague pneumonia, after the third or fourth day, they were distinctly less.

25,309. Most of the cases of plague pneumonia die?—Yes.

25,310. But you saw two cases which recovered?—Yes.

25,311. Did you make any examinations as to how long after the fever had subsided the plague bacilli could be found in the sputum?—Yes; I did not see either of these cases till after the fever had subsided.

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In one case it was 10 days after the fever had subsided that there was plague bacilli in the throat. There was no cough or expectoration.

25,312. Were you able to isolate the plague bacillus?—Yes.

25,313. And reproduce the disease in animals?—A rat was inoculated, and it died in 4½ days.

25,314. A rat inoculated with the sputum itself?—No, with ½ c.c. of an agar culture.

25,315. How do you know these cases were primary plague pneumonia if you did not see them?—I saw the doctor in charge, and got the history of the cases. They had been entered as plague pneumonia, and, in both cases, there had been consolidation of the lungs and no buboes.

25,316. What are the physical signs of primary plague pneumonia?—At first there are no signs pointing to the lungs at all. In the earliest cases I saw, there seemed to be no sign pointing to any particular organ at all. Then a cough developed, there was expectoration, and then I detected some consolidation of one of the bases.

25,317. Is the whole base affected?—No, only a portion.

25,318. It is a patchy sort of pneumonia?—Yes; in all the cases there seemed to be some distinct part where it started.

25,319. Does it affect both lungs or only one lung, as a rule?—In one of the cases both lungs were affected, but in most of the others it was one, and then there was general bronchitis.

25,320. The discharge from the mouth is sometimes extremely profuse, is it not?—Yes.

25,321. When do the secondary lung symptoms generally show themselves?—About the fifth or sixth day.

25,322. Is that the rule in every epidemic?—I do not think so. In Poona there were hardly any lung symptoms.

25,323. Have you made any observations in the present epidemic?—No, I have not.

25,324. You are referring to the epidemic of last year?—Yes; March, April, and May of last year.

25,325. The symptoms came on on the fourth or fifth day?—Yes; sometimes as late as the eighth day.

25,326. What physical signs did you find in the lungs?—Signs of acute bronchitis and patches of consolidation.

25,327. Did you ever find consolidation of the lung?—No; only a few patches where there was a little bronchial breathing, perhaps—no definite consolidation.

25,328. Did you ever notice signs of œdema of the lung?—In the later stages, when the patients were exhausted, at the back.

25,329. Did you notice signs of pleurisy?—No.

25,330. Is the sputum profuse in such cases?—Yes; it is fairly profuse, but not excessive.

25,331. Is not there a great deal of difficulty in bringing up the sputum?—Yes, there is.

25,332. The patients strain a good deal?—Yes.

25,333. Did you find the plague bacilli in such cases in the sputum?—In a fair number of cases.

25,334. In how many?—In 17.

25,335. Out of how many?—There were 43 after I had got the technique, and had worked out the best method for isolating the bacillus.

25,336. By cultures?—Yes.

25,337. In how many cases did you find it microscopically?—I found everyone of them had it, practically.

25,338. Did you find there were other bacteria present as well?—Yes.

25,339. What bacteria did you find?—A diplo-coccus was common at the start.

24,340. Did you find Friëdlander's diplo-coccus?—I never went beyond the fact that there was a diplo-coccus.

25,341. Did you find strepto-cocci common?—Yes.

25,342. For how many days did you find plague bacilli in these cases?—As a rule, about a fortnight after the lung symptoms developed; but there was one case which was 28 days, three days after the temperature was normal.

25,343. Did you isolate it in that case?—Yes.

25,344. Did you find that the plague bacilli in such cases were virulent?—I inoculated three rats; they took three or four days to die, and the primary pneumonia case died after 36 hours.

25,345. I think in one case you found the plague bacilli for five days only?—Yes.

25,346. In 13 cases the plague bacilli were found in periods varying from 10 to 14 days, in one case 15 days, in one case 19 days, and in another case 23 days?—Yes.

25,347. (*The President.*) You have tested clothes for the bacillus?—Yes.

25,348. Will you be good enough to tell us what was the result of your testing?—I never succeeded in finding the bacillus in any clothes which I examined.

25,349. These clothes had been worn by plague patients?—Yes.

25,350. In any cases had they been removed from a corpse?—Yes.

25,351. You examined portions of these articles of clothing and did not find any?—That is so.

35,352. You have examined the perspiration, I think?—Yes.

25,353. With what result?—I never found the plague bacillus in it.

25,354. You examined the saliva?—Yes.

25,355. With what result?—I found the bacillus in ordinary bubonic cases occasionally. I examined five cases where the parotid or submaxillary gland was affected, and I found it in each case. In one case I found it in the secretion 10 days after the temperature had become normal; the glands were still enlarged, but they had not suppurated.

25,356. Ten days after the apyrexia, you found it?—Yes.

25,357. Is that the longest period?—Yes; I never found it in any other case after the temperature was normal.

25,358. Have you examined the urine?—Yes: I failed to find it in the urine.

25,359. Have you made a large number of examinations?—About 15.

25,360. At what stages of the illness of the patients?—At all stages.

25,361. Will you describe the process you followed for separating the bacillus if it had been in the urine?—I collected the urine direct from the bladder with a catheter and passed it into a conical vessel and allowed the vessel to stand covered up from the air. After it had been covered up for three or four hours I took a small drop from the sediment at the bottom and inoculated broth with that. Drops of this inoculated broth were added to tubes of liquid agar, and plates and rolled tubes were prepared from them. Numerous colonies of bacilli developed, but I never succeeded in isolating the plague bacilli.

25,362. You did not centrifugalise?—No.

25,363. What has your experience been with regard to fæces?—I examined two or three cases of fæces. In one case where the motions consisted of nearly pure mucus I managed to isolate the bacillus, but that was the only case.

25,364. Out of how many?—About five altogether.

25,365. Was this the only case you examined in which the symptoms were specially abdominal?—Yes.

25,366. Have you examined the epidermis of the tongue?—Yes.

25,367. With what result?—I examined about 20 cases and I found it twice in the fur at the back of the tongue.

25,368. At what stage of the illness?—The patients had been ill for some time—about 10 days.

25,369. They were becoming convalescent?—Yes. They still had fever and were ill, but they were out of danger.

25,370. Have you anything to add to what you have said as to the examination of buboes?—No. I could

always get plague bacilli from the buboes before suppuration had taken place, but after suppuration I never succeeded in getting it—at least, not from the pus. I got it from the part of the bubo which had not broken down. There was one case of bubo which had not suppurated, and the patient was convalescent and going about. I tried it, but I did not succeed in getting any bacilli from it.

25,371. What is the latest period in the disease in which you found any bacilli in the bubo?—In all the cases in which I found it the temperature was still high.

25,372. Within 10 days?—Yes. I did not make any special attempt to find out how long.

25,373. But the maximum period is about 10 days?—Yes.

25,374. I think you made some examinations of earth in different localities?—Yes.

25,375. In what localities?—In Poona and Bombay.

25,376. Where in Poona?—From the floor of infected houses and the graveyards of Poona.

25,377. What were the results?—I never found it.

25,378. Taking infected houses, how many have you examined?—About 30 or 40.

25,379. Did you take a mixed sample of different parts of each floor or only one part?—It varied. Sometimes, if a patient had been lying on a particular place, I took a part from there. If I did not know anything about the matter I took samples from different parts, avoiding the fire-place and where the light shone in.

25,380. At any rate, you included, in your examination, earth which you knew had been very close to the patient?—Yes.

25,381. You examined also the earth of graveyards?—Yes.

25,382. With what results?—I never could find the plague bacillus.

25,383. The floors which you examined were cow-dung floors, were they not, in all the cases which you have referred to?—Yes.

25,384. Have you examined any dust in infected houses?—Yes.

25,385. Where did you collect it from?—I got it from all parts, from the ceiling and the walls, from niches in the walls, and from the sweepings from the floor.

25,386. What was the result?—The result was negative.

25,387. Were your examinations numerous?—Not with regard to the dust, particularly—that came in with the earth floors; I generally did some of the dust in the room when I did the earth floor.

25,388. That was a considerable number, apparently?—Yes; I suppose about 15 or 20 times I tried the dust.

25,389. Did you examine any of the hangings or the clothes in the room where patients had been living?—Yes.

25,390. What were they?—Mats in the room, hangings on the wall, and cotton or anything which I found lying about.

25,391. Did you examine any clothes?—Yes; soiled rags and anything I found left in the room.

25,392. Did you make a large number of examinations?—No; in about five or six houses.

25,393. What was the result of that?—In one case I found the bacillus on a piece of matting.

25,394. Had that been hanging up?—It was actually hanging up when I found it, but I was told it had been used for the bedding of the patient who had died, and it was certainly soaked with the discharge.

25,395. Fæcal or urinal?—Urinal and the saliva from the mouth.

25,396. Not fæces?—No.

25,397. I think you have made some examination of the temperature of plague in the steam apparatus and of the penetration of that temperature to bundles of clothing?—Yes. At Poona a steriliser was used at the railway station. The clothes of each passenger were tied up tightly into bundles and put in. To effect penetration to the centre of these bundles sufficiently to kill the plague bacillus, we had to get a pressure of 15 lbs. to the square inch and keep it at least for a quarter of an hour.

25,398. How large was the biggest bundle?—2 feet by 2 feet 6 inches generally, in long bags.

25,399. And you found that penetration extended to the centre?—Yes.

25,400. How did you ascertain that?—I got glass capsules with living plague growths in broth and placed them right in the centre of the bundles: and after the exposure to the steam, fresh broth and agar tubes were inoculated from the contents of these capsules.

25,401. You did not ascertain the actual temperature?—No; there was no registering thermometer.

25,402. (*Dr. Ruffer.*) What disinfector was it?—It was a locomotive boiler. The steam was introduced from a stationary engine.

25,403. How big was the bundle?—Just the clothes of the people who came up; they were all sizes. I suppose the biggest I tried was 2 feet 6 inches by 2 feet. They could not touch the bundles at all for 10 minutes or a quarter of an hour after their removal from the boiler. When they had only 10 lbs. pressure, the bundles could be handled directly they were brought out, the heat did not penetrate. 15 lbs. pressure for 10 minutes was not sufficient.

25,404. That corresponds to what temperature?—I had not a thermometer, but I suppose about 220° F. or 224° F.

25,405. Have you ever examined the blood of patients for plague bacilli?—Yes, microscopically.

25,406. Not cultures?—No.

25,407. Have you ever found it in a case of plague which recovered?—No, not in any case; but I saw some slides of blood which Dr. Marsh had taken.

25,408. And the patient recovered?—Yes. Apparently, they were plague bacilli.

25,409. (*The President.*) In these pneumonic cases which you have referred to, did you ever search for the pneumo-coccus either in the primary or secondary cases?—I did not search specially for it. I was only looking for the plague, but there was a diplo-coccus constantly present in the secondary pneumonia which was only occasionally present in the primary pneumonia.

25,410. You could not identify that with the pneumonia coccus?—I thought the secondary pneumonias were primary pneumo-coccic infections. That is the idea I got from the examination of the sputum.

(Witness withdrew.)

Mr. Sorabji Manekji Damaun-Valla called and examined.

25,411. (*The President,*) You are a volunteer witness, I think?—Yes. I was nominated by the Collector of Surat.

25,412. Will you state to the Commission what have been your opportunities of observing plague?—When the first epidemic broke out in Daman I was called there, and I saw the people dying of plague.

25,413. Have you had experience elsewhere?—No.

25,414. (*Mr. Cumins.*) When did your experience in Daman begin?—At the beginning of the first epidemic in March 1897.

24,415 Up to what point did your experience last?—To the end of the epidemic in July 1897. Then there was a recrudescence in March 1898 up to June, and the third epidemic is now going on.

25,416. Have you had experience of all three epidemics in Daman?—Yes; I was there all the time.

25,417. I think you have come to speak about inoculation chiefly?—Yes.

25,418. Have you done any inoculations yourself?—I have.

25,419. How many people did you inoculate yourself?—About 200.

25,420. Did you keep a watch upon these people to see what happened to them?—Yes.

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25,421. Upon the whole 200?—Upon all those who were inoculated through me.

25,422. How many would that be?—About 4,000, inoculated at my expense—of course from time to time. In the beginning of 1897 I sent for Dr. Kalapesi. He came thrice to Daman through the courtesy of Dr. Haffkine and inoculated about 2,000 people, and about 200 by Dr. Poiares, and then the Government sent for him, and I have nothing to do with that. Then, again, I sent for Dr. Dubash. He inoculated 1,115 persons, including inoculation by me, and then again this year he has inoculated 397. This includes my own inoculations also. Besides this about 200 persons of my village of Katheria were inoculated by Dr. Fernandes.

25,423. In what month were the 2,200 people inoculated?—In March, April, and May 1897.

25,424. In what month were the 1,115 and 200 inoculated?—In April 1898, within nine and four days respectively.

25,425. And the 397?—Last month—February.

25,426. What part of Daman did all these three sets of people live in?—In Daman Proper, where I live Katheria, which belongs to me, and Kariwari, a group, of three villages. I have brought a map here which will show the position and topography of Daman.*

25,427. To what extent were you able to keep a watch upon these three groups of inoculated people?—I had about half a dozen agents who know all the people. Whenever the people were inoculated I used to send them round every day to ascertain how they were getting on, especially in the first year.

25,428. To 2,200 people?—Yes. They are all known to me, and I have every facility to make inquiries. Then it is a small area, about 1¼ miles long and three-quarters of a mile broad. I, myself, used to visit them whenever I could, and I was all the more interested, as I had introduced inoculation and I did not know what effect it would produce.

25,429. What practical effect did it produce?—It had a wonderful protective effect. Those who were inoculated were protected from plague, and a very small ratio died—about two per cent.—whereas the uninoculated died in numbers—about 70 per cent. taking all the three seasons together.

25,430. How do you know how many of the inoculated died?—I had my servants, who were always going about. Then those who were inoculated had instructions to let me know if there was any sickness among them. Then I had my sepoys and the servants at the burial and burning grounds always from the beginning of plague, and they brought information if my other people failed to give it to me.

25,431. How did your subordinates and the sepoys at the burial grounds know whether certain persons were inoculated or not?—They inquired whether they were inoculated and what was the name, and reference was always made to the inoculation registers.

25,432. How are you satisfied that the answers they gave you were true answers?—Because whenever they were ill I used to know it, and gave them medicine as I gave to others also.

25,433. How did you know it?—Generally people came to me running whenever they got ill, and my men always used to go and inquire who was ill amongst the inoculated people. It is necessary to know, first of all, that Daman is a very small area, where it is not difficult to make inquiries, and then I, myself, and my men, know all the people. Then, owing to the cordon (British) which was very strict, nobody was allowed to go out of Daman. It was very easy to ascertain, especially in the first year.

25,434. Did you have a register kept in which each person's name was entered as he got himself inoculated?—Yes.

25,435. After the close of the first epidemic did anybody go round with this register in his hand to every house in the town and see with his own eyes whether the inoculated persons were alive or not?—Yes.

25,436. Who did it?—My men. I have about half a dozen men appointed for the purpose, and, as I said, I was specially interested and anxious in making inquiries during the first year in order that the preventive measure which was entirely a new one, and which I had introduced, may not prove injurious and bring a slur upon me. Then, again, Dr. Haffkine was very particular about the statistics.

25,437. Was this done at the close of the epidemic when people had ceased to die of plague?—No. Then there was no need, because my men who were at the burial grounds used to bring information.

25,438. Out of the first set of 2,200 people, how many got plague and how many died?—26 people died of plague up to May 1897, who were inoculated in the first season of the same year, when Dr. Lyons came to Daman for inspection. All the information was supplied to him by me.

25,439. How many got the plague and did not die?—That I have not got at hand. I have the register, but I have not taken out the figure.

25,440. Now with regard to the second lot; how many of those who were inoculated got plague, and how many died?—There were people who were inoculated in 1897 who died in 1898, and also who died in 1899. Up to date, yesterday, 83 persons have died, out of which 36 died during the first season.

25,441. Of those 2,200 inoculated in the first season, how many died in the first season, and how many in the second, and how many in the third? Then, as regards those inoculated in the second season, please tell us how many died in the second, and how many in the third season?—Of the 2,200 of the first season, 46 died in the first season (1897), 19 in the second season (1898), and two in the third season (1899). Of those inoculated in the second season (1898), 12 died in that season (1898), and four died in the third season. Fortunately, there has not been a single case of death among those inoculated in the third season (1899).

25,442. Was everybody who came to be inoculated in Daman inoculated, or did you reject some people because they were too old or too young?—Yes. Those who were too old, or those who had recent fevers or who had fever, or suffering from any other disease at the time, were not accepted. Then the old people, cripples, and small children under six months, and pregnant women, were rejected.

25,443. Did you observe whether inoculation had any marked effect upon the mortality from causes other than plague?—In the first epidemic, I did.

25,444. What was the effect?—After the epidemic had ceased the health of Daman was marvellously good, as the death register will show.

25,445. I am asking about the inoculated people. Had inoculation a markedly good effect upon them with regard to diseases other than plague?—I have not come to learn of any complaint, and when the inoculated people died, whatever they died of was registered. The register shows they did not die of any disease up to date, except the plague.

25,446. Amongst the 2,200 people inoculated in the first season, have there been, up to date, any deaths from causes other than plague?—Amongst the inoculated I have not found a death from any cause other than plague.

25,447. (*The President.*) I think I heard you say that after the epidemic had disappeared the health of the people seemed to be specially good?—Yes.

25,448. During the second epidemic, was there much sanitary improvement effected in the district?—I used to give disinfectants and fumigation. People had great faith in burning rosin and sulphur, and they were freely given to them.

25,449. Was there any special scavenging or cleaning of the place?—It was not what it ought to be.

25,450. Was there more done than before the plague?—Yes.

25,451. During the epidemic you specially attended to the scavenging?—Yes.

25,452. Did you open up many of the houses in which plague cases had occurred?—Not I; the Government did.

25,453. Were only the houses in which plague cases had occurred disinfected?—Yes. It is being done in a real systematic way this season.

25,454. Did they constitute a large number of the houses?—Yes.

25,455. Therefore, you had been specially active during the epidemic in improving the scavenging, cleansing

* Not printed in the proceedings of the Commission.

the place, and opening up a considerable number of the houses?—Yes. It is especially the case this season under Dr. Pinto, the Special Plague Commissioner.

25,456. And after the epidemic subsided the health of the population was greatly benefited?—Yes. If I am permitted, I should like to say that several cases were treated with living frogs. Living frogs were applied to the buboes. The first frog so applied died within five minutes, the second within 10 or 15 minutes, the third within half an hour, and so on; the fifth frog lived about two or three hours. I have even known it live five or six hours, and the patient found considerable relief.

25,457. From killing these frogs?—Not from killing, but from application. The colour of the frog was changed into a greenish-yellow when dead.

25,458. How are the frogs fastened on to the patient?—They were tied on to the bubo, on the abdomen side.

25,459. With a cloth?—No, without a cloth; they were put on and then bandaged. Some cut the frog transversely, and then applied it dead, and kept it on for some hours and then applied another. This is a sort of root (*handed in*) which was applied by many people, and some found relief. The general treatment which the poor people applied was branding of the bubo. I have reason to believe that many escaped by mere branding than by any other treatment. Then there were one or two marvellous cures. A Parsee woman, while pregnant, got diarrhœa, and had it for a year and a half until she was inoculated. As soon as she was inoculated the diarrhœa disappeared on the third day without any medicine. The head Parsee priest, of Daman, is a gouty man, and used to get very severe attacks very often, lasting over weeks. After he was inoculated the attacks have become very mild and far between.

25,460. How many cases were treated with frogs?—About five.

25,461. Did they all recover?—Three recovered and two died. *Mr. S. M. Damaun-Valla.* 15 Mar. 1899.

(The following letter was subsequently submitted by witness:—I produce a receipt which I have obtained from a native doctor, and which was said to be very efficacious:—

"Recipe for bubonic fever given by a native doctor (raid), Brahmachari Boa Raghranand, residing in Kandavady, Bombay, who has treated and cured hundreds of people with the following medicines:—

"*For External Application.*

℞:

"*Bhilama* (marking nut) cut into small pieces - - - - -	1 lb.
"*Isus* (Incense or Crude Mastic) in coarse powder - - - -	1 lb.
"Wax, in pieces - - - -	½ lb.
"Oil of *Sarsi* (Pongamia Globra) - -	¼ lb.

"Mix together and distil. Apply the distilled oil or ointment as a plaster on the buboes, and ferment them with *Neem* tree leaves (Azadirakhta Indica) mixed with Ajawan and common salt.

"*For Internal Use.*

℞:

"Cow's urine - - -	To be taken in equal parts.
"Juice of *Neem* tree leaves - -	
"Juice of green (*i.e.*, fresh) ginger	

"To be taken when there is fever or not. Dose, ½ oz. for adults once or twice a day. If the fever is very high, give thrice a day. When the fever is less, reduce the dose."

The aforesaid native doctor is well known in Bombay, has great experience of native herbs and medicines, and has cured some very serious diseases. He assures me that he has treated hundreds of cases of plague with the above medicines, and cured most of them.)

(Witness withdrew.)

Mr. Shrinivasa Rau, M.B., recalled and further examined. *Mr. Shrinivasa Rau, M.B.*

25,462. (*Dr. Ruffer.*) I believe you have made some observations on the samples of soil found at depths of 1,420 and 1,620 feet respectively in the Mysore mines?—Yes.

25,463. What sort of soil was that?—More or less pulverised rock.

25,464. Was it mixed with any organic material?—Yes.

25,465. Where did that organic material come from?—I believe it must have been brought in by the miners who went down.

25,466. You do not know whether the micro-organisms which you found there are due to the organic material which was brought in by the miners, or whether they were in the rock itself?—That is so.

25,467. What micro-organisms did you find?—Only four kinds of bacilli, three of which were spore-bearing, and one was non-spore-bearing, but it was motile.

25,468. Did you find anything else?—No.

25,469. I suppose the soil on which the coolie lines are built is full of all sorts of material?—Yes.

25,470. Did you find the plague bacillus?—No.

25,471. Did you find that cockroaches which were inoculated died of plague?—A cockroach which was experimented on died, but I do not think it died of plague. There was no moisture in the bottle in which it was kept, and it must have died of that.

25,472. (*The President.*) Why did you make experiments with cockroaches?—I was specially asked to do so to see whether there was any chance of their carrying the plague.

25,473. (*Dr. Ruffer.*) You fed some cockroaches on plague-infected material, but you did not find that it affected them?—No; at least, on the fifth day after I fed them on infected material they were quite well.

25,474. What are "cyanide tailings"?—Pulverised rock mixed with a solution of potassium cyanide.

25,475. Did you find any micro-organisms in that?—No.

25,476. Did you make any experiments as to the effect of fumes given off by blasting gelatine?—I sent my assistant to make them.

25,477. What did he do?—He simply exposed pure cultures of the plague bacillus to the fumes for five, ten, and fifteen minutes. He opened a cotton plug, and also opened a Petri's dish and exposed them to the fumes. I was not satisfied with the way in which the experiments were done, and I have not drawn any conclusions from them.

25,478. Did the plague bacillus die as a matter of fact?—No.

(Witness withdrew.)

(Adjourned to Friday, March 17th.)

At The Secretariat, Bombay.

SIXTY-SEVENTH DAY.

Friday, 17th March 1899.

PRESENT:

PROF. T. R. FRASER, M.D., LL.D., F.R.S. (*President*).

Mr. J. P. HEWETT.
Prof. A. E. WRIGHT, M.D.
Mr. A. CUMINE.
Dr. M. A. RUFFER.

Mr. C. J. HALLIFAX (*Secretary*).

Major J. Crimmin, V.C., I.M.S.

17 Mar. 1899.

Major J. CRIMMIN, V.C., I.M.S., recalled and further examined.

25,479. (*Mr. Hewett.*) Can you supplement the figures you gave us at your last examination regarding medical examination of the crews and passengers of ships proceeding from Bombay, first to ports out of India, and secondly, to ports in India?—Since my previous examination on 2nd December 1898, the Port Health Staff examined, up to 15th March 1899, 330 vessels proceeding to ports out of India. They carried a crew of 20,273 and 15,745 passengers. The number of vessels examined and proceeding to ports in India, including Burma, was 22,950. They carried a crew of 169,799 and 218,069 passengers. The inspections in the harbour from 1st January 1897 up to 15th March 1899 show that 51,213 incoming vessels, with a crew of 404,934 and 520,679 passengers, were examined, and that 110,851 outward bound vessels, with a crew of 930,448 and 962,573 passengers, were examined, making a grand total of 162,064 vessels of all sorts, with 2,858,634 souls as crew and passengers. The total outward-bound vessels since 1st January 1897 I have classified into (*a*) those going to Aden, Red Sea, and Europe; (*b*) those to ports out of India, other than Aden, Red Sea, and Europe; and (*c*) those going to ports in India.

	Total Number of Crew and Passengers who left for Aden, Red Sea, and Europe.	Number rejected.	Total Number of Crew and Passengers who left for Ports out of India, other than Aden, Red Sea, and Europe	Number rejected	Total Number of Crew and Passengers who left for Indian Ports, including Burma.	Number rejected.
In 1897 - -	56,448	181	32,862	206	621,404	2,935
In 1898 - -	57,988	552	33,705	547	830,135	13,812
From 1st January 1899 until 15th March 1899 -	18,711	107	9,118	139	282,750	6,532
Total -	133,147	840	75,685	892	1,734,289	23,279

Total number of outward bound crew and passengers - - - 1,943,122
Total number of rejected at the examinations - - - - 25,011
Total number of plague cases found among outward-bound crew and passengers - - - 243*

25,480. Can you give us information to show the number of persons you prevented from sailing on outward-bound vessels?—The number prevented from sailing on outward-bound vessels since 1st January 1897 up to 15th March 1899, is 840 to Aden, Red Sea, and Europe; 892 to ports out of India, other than the above; and 23,279 to ports in India.

25,481. How many of these people got plague?—The number of outward cases that we know of is 243. The total number caught at the outward and inward inspections, as well as in harbour craft up to date, is 374. The number of plague cases I have given do not represent the total number prevented from sailing, because in the early days we had no camps to send the rejected to. They were turned into the city, and lost sight of.

25,482. Does your examination, in your opinion, act as a deterrent?—Yes, very much. Nowadays we get very few fully developed plague cases. In the early days we got fully developed cases; but since the people found our inspections were so strict they would not run the risk of being detained at the examinations. A person may have a rise of temperature—99° or 100° F., and he or she is rejected and sent to a camp. If the temperature falls to normal he is let out. If not he is kept until we are certain that the fever from which he is suffering is not due to plague. They have an idea that we knew when they came from a plague-infected house, and that they would be detained if they suffered from fever.

25,483. Does the fact that you have discovered and stopped a certain number of persons who have subsequently developed plague while under your observation, taken with the fact that very few cases have developed after the ship has left port, lead you to infer anything as to the period of incubation?—It is very difficult to give the period of incubation; I have cases in my mind of men who came from non-infected ports, and who developed plague within two or three days after after arriving in Bombay. These men died. There was one the other day that came from London. He was a paid off crew of one of the P. and O. ships. Being paid off he was booked as a passenger, and he came in here on the 25th February and was dead from plague on the 1st March.

25,484. (*Dr. Ruffer.*) Was he a European?—No, he was a native. The only case of plague I have found in a European, in connexion with ship inspections, was the case of a postal official. He was born in this country.

25,485. (*Mr. Hewett.*) I want to ascertain whether you think that the fact that so few cases develop after your examination, when you turn out all the people who have fever, tends to show that the period of incubation is very short?—I believe that in the majority of instances it is from three to five days. That is the period I put it at. Longer periods may be given, but I believe they are, more or less, mild cases and are not detected in the early stages of the illness.

25,486. When you were under examination last, you said you did not disinfect the clothing and the effects of any people proceeding by these ships; do you do so now?—At present we disinfect the clothes and bedding and boxes of all native crews proceeding to all ports out of India, either directly, or by first calling at another Indian port, such as steamers going to Europe *viâ* Karachi.

25,487. What is your method of disinfection?—What we do is as follows:—The new native crews are collected in the disinfection shed by some official from the shipping office who brings a copy of the Articles. Native crews from ships in the harbour are brought by one of the officers of the ship. Roll-call from the Articles takes place so as to make certain that we have the total number of native crew as well as their property. The shed is divided into two portions. In the larger portion, the crews' kit is sorted. The crew dress themselves in the cleanest clothing they have, and the remainder of their clothing and bedding is put through an Equifex stove, and subjected to saturated steam at

* This number is exclusive of 47 cases found on vessels lying in the harbour, and 84 which developed among people arriving from coast ports near Bombay. For detailed monthly figures, *see* App No. X. in Volume I. and App. No. LXXV. in this Volume.

a pressure of 10 lbs. to the square inch. This pressure is said to give a temperature of 239° F., which is said to give absolute disinfection in the case of the most resistant organisms yet isolated. Clothes and bedding are kept in the stove for 15 minutes, and during that time they are subjected to three changes of steam. They are then dried for about five minutes with hot air. Boots and shoes and rain-coats are disinfected with a spray of perchloride of mercury of the strength of 1 in 1,000, or, if they are greasy, they are sprayed with live steam through a hose from the boiler. The crews' boxes are brushed inside and out, or washed, if they are very dirty. They are then rolled about in a large tub containing a solution of perchloride of mercury, 1 in 1,000. The kit and boxes are then passed into a second part of the shed, where they are packed, and are then conveyed to the ship under a Police escort. One of the European Sanitary Police goes on board to see that no bedding is stowed away in the forecastle, fore-peak, or other parts of the ship. Sometimes they say they have no bedding at all, but we make them find it somehow or other. The floor of the shed is then swept, and the sweepings destroyed in the furnace of the stove. I have a copy of the Rules which have been drawn up. They are as follows:—

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RULES for DISINFECTION of KIT at PILGRIM SHED.

I. The boxes and kit of crews to be collected and placed in the southern half of the building.

II. The Officer in charge of the disinfection should see that the entire kit, clothes, and bedding, and all the boxes of the crew are produced for disinfection. This can be ascertained by having a roll-call from the ship's articles. Each man, as his name is called, should point out his property.

III. The officer in charge should detail one of the Sanitary Police officers to see that the forecastle has been completely emptied of all kit and bedding, and that kit has not been concealed in cupboards, forepeak, stokehold, or other places on board.

IV. All clothes, including those worn at the time, and bedding, should be passed through the stove.

V. Fifteen minutes should be given for disinfection with steam at a pressure of 10 lbs. The steam at this pressure will have a temperature of 239° F. There should be three changes of steam during the 15 minutes, and 5 minutes should be taken for drying with hot air.

VI. The following articles need not be put through stove:—Leather goods, books, velvet, silks, rain-coats, and such other articles which, in the opinion of the Medical Officer, it is unnecessary to disinfect, such as clean white clothes, ironed shirts, &c.

VII. Leather goods should be sprayed with perchloride of mercury, or, if greasy, steamed with carbolised steam.

VIII. The boxes should be brushed inside and out, and then disinfected with perchloride solution, 1 in 1,000. When this has been done, they should be moved to the north side of the shed to receive disinfected kit.

IX. The kit, when disinfected, should be placed in the north side of the shed, packed there and conveyed on board the ship under escort.

X. A certificate, showing the number of saloon, deck and engine-room crew, whose kit has been disinfected, should be sent to the office, so that it may be attached to the vessel's papers for the information of the Examining Medical Officers.

XI. Crew should be advised to wear shoes or boots when on shore, and should clean their feet on returning to the ship at all times.

XII. When the disinfection of kit is finished, the floor should be swept and the sweepings destroyed in the furnace.

(1.) In order to meet the letter and spirit of the Venice Convention, the crews may be examined in the shed while the kit is being disinfected. The names of any of them who have a marked rise of temperature should be taken down and sent to the office.

25,488. Have any cases of plague been detected recently on steamers going to Europe after their departure from Bombay?—No, I do not know of any.

25,489. A statement was recently made in the newspapers that one or two cases had developed on board the *Caledonia*: can you give us any information as to this statement?—At our examination, on the 19th November 1898, we found a man with fever. He was sent for observation, and the doctors in charge of the hospital reported that it was a case of plague. The information was telegraphed to Suez by Government, and on the arrival of the vessel at that port two native crew suffering from fever were removed from the ship. Reuter telegraphed, as an item of news, that they were plague cases. I wrote to Egypt for details of the cases, and at the same time asked for a list of plague cases imported from Bombay during the year 1898. The President of the Quarantine Board replied that neither of the men removed from the *Caledonia* could, in his opinion, be considered to have developed plague, and further that no genuine case of plague had been imported from Bombay during the year. I have the letter here and can read extracts from it.

25,490. (*Dr. Ruffer.*) By whom is it signed?—It is signed by Dr. Cathcart Garner. He says: "The ship, "on arrival at Suez, was disinfected and allowed to "proceed through the canal in quarantine." He goes on to say: "The first of these cases which I have "described as 'slight,' was up and about the next day, "and the temperature normal. He had a small simple "gland only in the axilla." Then he describes the other, and says, "This case has been treated all "through as 'suspicious,' although considering the "absence of certain clinical symptoms together with "the absence of bacteriological confirmation, I am "inclined to believe it was not really a case of "plague."

25,491. (*Mr. Hewett.*) Have any cases of plague occurred on ships going from here to Rangoon?—During the present epidemic which commenced in December 1898, only one case of plague, as far as we know, has got away from Bombay on a vessel bound to any port outside the Bombay Presidency. This was a native cook on board the *Booldana*, which left this port on the 18th of January 1899. At the time of the inspection of the *Booldana* one of the saloon-crew—a native cook—was missing. It now turns out that the missing cook was at the time suffering from fever on shore and did not like to face the inspection. He died of plague on shore a few days after the vessel sailed. The man who developed plague on board after leaving this port was another native cook who we now find resided in the same house as that in which the first cook who died came from. The second cook, who had plague on board, was therefore a contact. That is the only case, so far as we know, that has got outside the Bombay Presidency during the present epidemic.

25,492. Has any case of plague got by sea from Bombay to Ceylon?—No, it was another of those cases reported by Reuter. We made inquiries, and we found he was a man who went by land from Bombay to Tuticorin. He embarked at that port for Colombo, and on arrival he was found to have an enlarged gland and fever. I presume the doctors segregated him as a precautionary measure so as to be on the safe side. It was admitted later on in the newspapers that it was not a case of plague. The man did not die.

25,493. Have you heard this from Ceylon?—Yes; I hand in a copy of the letter from the Port Surgeon, Colombo, saying that it was not a plague case, as follows:—

"Port Health Office, Colombo,
"SIR, 25th February 1899.

"I HAVE the honour to acknowledge your letter No. 307, of 1899, dated 14th February, and to inform you that the case referred to occurred at Galle, and has been officially pronounced to be not one of plague. The patient, Carolis Jayesuria, travelled to Ceylon by way of Tuticorin, and passed through Colombo after disinfection. He was treated at Tuticorin for venereal disease, and the glands of the groin had evidently enlarged sympathetically.

The Port Health Office,
Bombay."

25,494. Is there any trade between the port of Bombay and Madagascar?—There has been some trade, apparently. I find that the last steamer which left Bombay for ports in Madagascar, either directly or by first calling at an intermediate port, sailed on the 9th of August 1897. She had a doctor on board, and during the voyage one death took place. This is certified in the log-book to have been due to paralysis, 41 days after leaving Bombay, and after an illness of 30 days. As far as we know plague broke out at Tamatave, in November or December 1898, that is 15 months after the last steamer left Bombay for

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Madagascar. I have a special report on the subject drawn up by Dr. Evans of my staff, which I hand in.*

25,495. Can you give us any facts about plague on the *Bhundara*?—The agents of the *Bhundara* say there were seven cases of plague during the voyage, of whom five died, and in addition there was a death certified to be due to acute lobar pneumonia. The first death took place on the 16th December 1898, the day after leaving Karachi. It took place among the coolies from some part of the North of India. There were no cases among the crew who left Bombay, and there was no illness of any kind resembling plague among the crew from the time they left Bombay until they returned to India *viâ* Colombo.

25,496. You have ascertained that from the agents?—Yes, and I hand in a copy of the log-book of the vessel, as follows:—

* *See* App. No. LXXVI. in this Volume.

VOYAGE No. 121.

COPY of OFFICIAL LOG, S.S. *Bhundara*

(*See Note at end of Copy of Log Book.*)

Date.	Entry.
16.12.98 5.30 P.M.	1. A railway coolie named Alla Ditha died from hyperpyrexia, and was buried at 8.5 p.m. by his co-religionists. Signed—J. E. SYLVESTER, Doctor. P. H. STUNT, Chief Officer. A. E. STEBBING, Commander.
20.12.98 6 A.M.	2. A railway coolie named Navab died from acute lobar pneumonia, and was buried at 8.45 a.m. by his co-religionists. Signed—J. E. SYLVESTER, Doctor. P. H. STUNT, Chief Officer. A. E. STEBBING, Commander.
20.12.98 10 A.M.	3. A railway coolie named Essa died from uncertain causes, whether cholera (sporadic) or plague, symptoms pointing to either or both, more towards the latter, and was buried at 10.45. a.m. by his co-religionists. Signed—J. E. SYLVESTER, Doctor. P. H. STUNT, Chief Officer. A. E. STEBBING, Commander.
20.12.98 10.30 A.M.	Effects of Essa (deceased) were destroyed. Signed—J. E. SYLVESTER, Doctor. P. H. STUNT, Chief Officer. A. E. STEBBING, Commander.
20.12.98 11 A.M.	After burial of Essa, the two cabins set aside for the hospital in the port alleyway, and one other second-class cabin next thereto, were thoroughly washed out, fumigated, and disinfected. Signed—J. E. SYLVESTER, Doctor. P. H. STUNT, Chief Officer. A. E. STEBBING, Commander.
21.12.98	4. A railway coolie named Jalal Deen died. Cause of death uncertain, whether cholera (sporadic) or plague, symptoms pointing to either or both, more towards the latter, and was buried by his co-religionists at 10 p.m. Signed—J. E. SYLVESTER, Doctor. P. H. STUNT, Chief Officer. A. E. STEBBING, Commander.
21.12.98 10 P.M.	Effects of railway coolie Jalal Deen were destroyed. Signed—J. E. SYLVESTER, Doctor. P. H. STUNT, Chief Officer. A. E. STEBBING, Commander.
23.12.98 9.10 A.M.	5. A railway coolie died, Feroz Khan. Cause of death plague, and was buried at 9.45 a.m. by his co-religionists. Signed—J. E. SYLVESTER, Doctor. P. H. STUNT, Chief Officer. A. E. STEBBING, Commander.
23.12.98 9.50 A.M.	Effects of Feroz Khan (deceased) were destroyed. Signed—J. E. SYLVESTER, Doctor. P. H. STUNT, Chief Officer. A. E. STEBBING, Commander.
24.12.98 6 A.M.	6. A railway coolie named Maula Dad died. Cause of death plague, and was buried by his co-religionists at 6.25 a.m. Signed—J. E. SYLVESTER, Doctor. P. H. STUNT, Chief Officer. A. E. STEBBING, Commander.
24.12.98 6.25 A.M.	Effects of railway coolie, Maula Dad (deceased), were destroyed. Signed—J. E. SYLVESTER, Doctor. P. H. STUNT, Chief Officer. A. E. STEBBING, Commander.
24.12.98	After the burial of the above, the cabins used as hospital were washed out with carbolic acid solution, and all bedding destroyed. Signed—J. E. SYLVESTER, Doctor. P. H. STUNT, Chief Officer. A. E. STEBBING, Commander.
28.12.98 1.30 P.M.	Fumigated No. 1 'tween deck with tar and sulphur, all ports, ventilators, and hatches closed whilst under fumigation for two hours. Signed—J. E. SYLVESTER, Doctor. P. H. STUNT, Chief Officer. A. E. STEBBING, Commander.
29.12.98 2 P.M.	Fumigated No. 2 'tween deck for two hours with tar and sulphur. Ports, ventilators, and hatches closed whilst under fumigation. Signed—J. E. SYLVESTER, Doctor. P. H. STUNT, Chief Officer. A. E. STEBBING, Commander.
30.12.98 2 P.M.	Fumigated 'tween deck alleyways with tar and sulphur for two hours. Ventilators, ports, &c. closed whilst under fumigation. Signed—J. E. SYLVESTER, Doctor. P. H. STUNT, Chief Officer. A. E. STEBBING, Commander.

Date.	Entry.
31.12.98 2 P.M.	Fumigated No. 3 'tween deck for two hours with tar and sulphur. Ventilators, ports, and hatches closed whilst under fumigation. Signed—J. E. SYLVESTER, Doctor. P. H. STUNT, Chief Officer. A. E. STEBBING, Commander.
1.1.99 2 P.M.	Fumigated No. 4 'tween deck for two hours with tar and sulphur. Ventilators, ports, and hatches closed whilst under fumigation. Signed—J. E. SYLVESTER, Doctor. P. H. STUNT, Chief Officer. A. E. STEBBING, Commander.
4.1.99 11 A.M.	'Tween decks and alleyways washed with a solution of Jeye's fluid and whitewash all over the deck and three feet up the sides fore and aft. Signed—J. E. SYLVESTER, Doctor. P. H. STUNT, Chief Officer. A. E. STEBBING, Commander.
4.1.99	Lascars' and fireman's forecastles washed with Jeye's fluid and quicklime solution. Signed—J. E. SYLVESTER, Doctor. P. H. STUNT, Chief Officer. A. E. STEBBING, Commander.
5.1.99 2 P.M.	All flooring in the first-class saloon, also ship's bilges, washed with a solution of Jeye's fluid and quicklime. Signed—J. E. SYLVESTER, Doctor. P. H. STUNT, Chief Officer. A. E. STEBBING, Commander.
1st to 5th January 1899.	All clothes, bedding, and effects of coolies in No. 1 'tween decks (where all but one of the cases occurred) were soaked into a hot solution of corrosive sublimate for several hours. The coolies themselves were made to bathe in a similar solution. The plague attendants segregated for a few days, and after the necessary actions being carried out, were allowed to mix themselves up with the others. Signed—J. E. SYLVESTER, Doctor. P. H. STUNT, Chief Officer. A. E. STEBBING, Commander.
6.1.99 9 A.M.	A fracas occurred amongst the deck passengers, in which two of them were injured about the head. One of the offenders, who was caught red-handed, was tied up and kept on water only during the day, under the supervision of the Commander. He was released at 5 p.m. The injured were attended to by the Surgeon, one being retained in hospital for treatment, and the other, after having his wounds dressed, being discharged. Signed—P. H. STUNT, Chief Officer. A. E. STEBBING, Commander.
18.1.99	Paint work on upper deck. Saloon and second-class cabins all scrubbed round with carbolic powder. Signed—J. E. SYLVESTER, Doctor. P. H. STUNT, Chief Officer. A. E. STEBBING, Commander.
20.1.99	Boats used for landing passengers scrubbed and disinfected with carbolic powder. Signed—J. E. SYLVESTER, Doctor. P. H. STUNT, Chief Officer. A. E. STEBBING, Commander.
21.1.99	Crews' bedding and effects exposed to the sun's rays for six hours this day. Signed—J. E. SYLVESTER, Doctor. P. H. STUNT, Chief Officer. A. E. STEBBING, Commander.
22.1.99	Crews', passengers', and ship's dirty linen put into the port and starboard cutters and soaked in a solution of corrosive sublimate, as ordered by Health Authorities, to enable the ship to get pratique. Signed—J. E. SYLVESTER, Doctor. P. H. STUNT, Chief Officer. A. E. STEBBING, Commander.
22.1.99	Washed the 'tween decks, fore and aft tops, sides, and deck with corrosive sublimate, as per above orders. Signed—J. E. SYLVESTER, Doctor. P. H. STUNT, Chief Officer. A. E. STEBBING, Commander.
22.1.99	Put corrosive sublimate into all the bilges and pumped them out fore and aft. Signed—J. E. SYLVESTER, Doctor. P. H. STUNT, Chief Officer. A. E. STEBBING, Commander.
23.1.99	Took the linen out of soak and dried all during the day. Signed—J. E. SYLVESTER, Doctor. P. H. STUNT, Chief Officer. A. E. STEBBING, Commander
24.1.99	Destroyed by fire 40 bundles dry fish marked a.m. or c.a.m. under instructions from the Port Authorities. Signed—J. E. SYLVESTER, Doctor. P. H. STUNT, Chief Officer. A. E. STEBBING, Commander.
25.1.99	Destroyed by fire the following packages, under instructions from the Port Authorities, being rat-eaten :— A.M.J. - - 3 Bags Indian Corn. ,, - - 3 Bags Jawaree. B.A.K. - - 1 Bag Jawaree. E.A.K. - - 1 Bag Jawaree. Signed—J. E. SYLVESTER, Doctor. P. H. STUNT, Chief Officer, A. E. STEBBING, Commander

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The Medical Officer's Register shows six deaths from plague and two recoveries according to the following extract from the Register of the *Bhundara* on the voyage from Bombay to East Africa, *viâ* Karachi:—

No.	Names.	Disease.	Remarks.
3	Alla Ditta	S. Plague	16th. Died.
4	Nawab	S. Plague	16th. Died 20th.
5	Esa	Plague	19th. Died 20th.
6	Jalal	Plague	21st. Died 21st.
7	Feroz Khan	Plague	23rd. Died 23rd.
8	Mooladad	Plague	24th. Died 24th.

Besides the above, two more railway coolies are shown to have been admitted suffering from plague on the 25th December, Surbar Kutal, temperature 104·2; and Khuda Bux, temperature 103·4. Both had tender inguinal glands, and both recovered.

25,497. So that all the cases occurred among the coolies?—Yes, all occurred among the coolies, none of whom had left Bombay. The first death, as I say, took place on the 16th December 1898, the day after leaving Karachi. The second death occurred on the 20th December, the third on the 20th, the fourth on the 21st, the fifth on the 23rd, and the sixth on the 24th of December.

25,498. Do you wish to say anything with regard to some previous evidence which has been given here about the discharge at Mandvi in 1896 of the cargo of a ship laden with wet grain?—Dr. Weir, in his evidence on the second day, refers to a ship laden with wet grain, and states that notwithstanding his protest, the grain was landed and stored at Mandvi, and that a large number of rats died.* I remember the ship very well. She was the *Knight Companion*, which returned to Bombay after meeting with rough weather in 1891. The agents reported that the grain cargo was damaged and stinking badly, and wished the Port Health Officer to condemn it, apparently for insurance purposes. We stated that we had nothing to do with damaged cargo, and had no power to condemn it, and suggested, if it was stinking, that it would be advisable to send it out to sea on an ebb tide. The agents then consulted the Municipal Health Officer, and afterwards, presumably with his knowledge and permission, sent the grain cargo into the city. It would appear that the Municipal Health Officer has, or had, no power to order the destruction of bad or damaged food-stuffs unless actually exposed for sale as human food. Most of this cargo, I remember, was boiled down to make size of for stiffening piece-goods.

25,499. (*Dr. Ruffer.*) How do you ascertain the temperature in your steam sterilizer?—It is guaranteed by the constructors.

25,500. Have you ever made any comparative experiments?—No. I do not think there is an electric signal thermometer available in India. The results of the experiments showing the temperature at various pressure are given in a book of instructions sent with the sterilizer.

25,501. Yo do not take the temperature at each disinfection?—No; it is impossible, unless we have a patent signal thermometer, which I understand are very liable to get out of order.

25,502. (*Mr. Hewett.*) Is there anything else you wish to bring before our notice?—I understand some people are rather sceptical as to the way in which we carry out harbour inspections, and the success which has attended them. I should be delighted if the Commissioners would give me an opportunity of accompanying them around, so that they can verify for themselves what is done.

25,503. Have you any facts to show that certificates of inoculation are tranferred from person to person?—We have come across certificates, occasionally, where the description in the certificate does not correspond with the person presenting it.

25,504. Can you give specific instances of this?—As a specific instance, I remember one case which was brought to the notice of Government in connection with another matter. The card showed that Janki Dhakoo, a woman, age 40, was inoculated on 15th February 1899. The card was presented on the 20th February by a lad of not more than 17 or 18 years of age.

25,505. Will you please give us the number of deaths that occurred in 1895 and 1896 on board ships coming to Bombay from Hong Kong, with details of each case of death that occurred?—I cannot give deaths in those years, except for part of 1894, when plague was epidemic in Hong Kong. I have the counterfoils of the pratique book for 1894, which showed that 11 vessels arrived during the time that restrictions were in force against arrivals from Hong Kong, with a total of 1,269 crew, and 342 passengers from Hong Kong between the 29th June and 25th September 1894. During the voyage, three deaths occurred, and none of the persons who died came from Hong Kong. It is stated on the counterfoils of the pratique book, that many of the 11 vessels refused to take native passengers from Hong Kong. The log-books of six of the ships are filed in the Bombay Shipping Office, and the following is the history of the three cases who died:—

Case I.—The *Clyde* left Hong Kong on 2nd August, arrived Bombay 19th August. Mohamed Abdulla died from malarial fever on 17th August. He was transferred at Colombo from the s.s. *Shanghai*, which was on a voyage homeward bound from the East.

Case II.—The *Rosetta* left Hong Kong 16th August, arrived Bombay 3rd September. Bellal Jeewa died on 2nd September from beri beri. He was transferred at Colombo from the s.s. *Australia* which was on a voyage homeward bound from Australia.

Death 3.—The *Rohilla* left Hong Kong on 13th September, arrived Bombay 1st October. Abdulla Sallim died on 27th September from pneumonia. He was transhipped at Colombo from the s.s. *Mirzapore*, which was on a voyage outward bound from London to Calcutta.

The above three deaths had been certified to by the Surgeons on board. All the 11 vessels had doctors on board, with the exception of the *Kong Bing*. This latter vessel had no passenger leaving Hong Kong or on arrival at Bombay.

The above information and the fact that none of the intermediate ports became infected is against the hypothesis that plague was imported into Bombay from Hong Kong by human agency.

25,506. A case, supposed to be plague, has just occurred in a man who has arrived in a ship from Hong Kong, and is at the Modi-Khana Hospital. Will you please let us have particulars about that case, including the following, viz.:—

(1) When the ship in which the man came left Hong Kong;
(2) Where it touched;
(3) When it arrived here;
(4) How long the man was reported ill before arrival;
(5) Whether there was a doctor on board or not;
(6) Whether the man's illness was noted in the log-book or not; and
(7) How the case has been diagnosed to be plague?

—(The following particulars were supplied later by witness):—The following is a statement of information obtained from the Captain of the *Bormida*:—

Questions.	Answers.
"1. When did the *Bormida* leave Bombay on the last occasion, and for what ports?	She left Bombay on 28th January 1899 for Singapore and Hong Kong.
"2. Was her native crew's kit disinfected before leaving Bombay?	No. There was no such rule in force at the time.
"3. What crew and passengers did she take with her on the 28th January 1899?	Eighty-four crew and 20 passengers.

* *See* Question No. 996.

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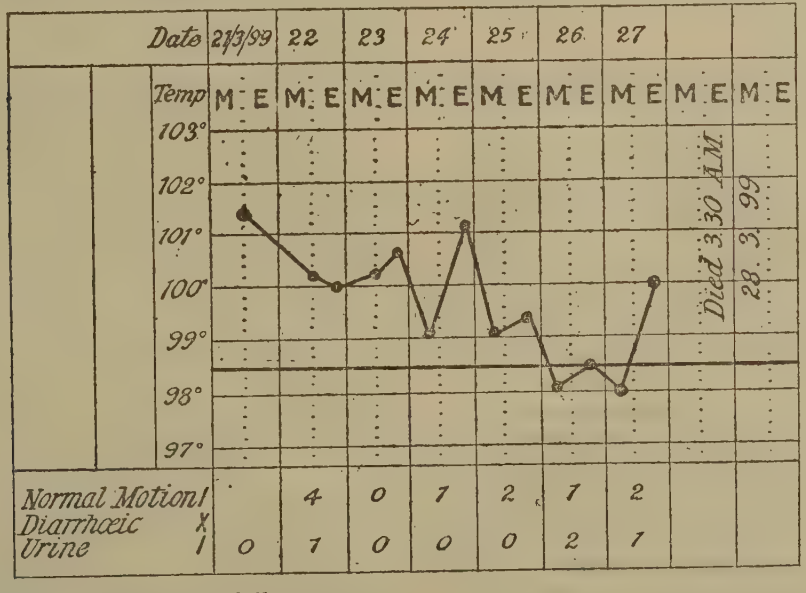
Questions.	Answers.
"4. What illness did the crew and passengers suffer from between Bombay and Hong Kong?	None.
"5. When did the *Bormida* arrive at Hong Kong, and how long was she at that port?	On 24th February, and remained there for four days.
"6. When did she leave Hong Kong for Bombay, and with what number of crew and passengers; giving the nationality to which they belonged?	Left on 28th February with the same number of crew, and with 366 passengers Chinese, including two Europeans.
"7. What ports did the vessel touch at between Hong Kong and Bombay, giving dates?	She touched Singapore on 6th March and left on the 9th. She touched Penang on the 11th morning, and left in the evening the same day.
"8. What illness did the crew and passengers suffer from on her voyage to Bombay from Hong Kong?	None whatever.
"9. When and where was the Chinaman cook Kwoknam shipped?	From Hong Kong.
"10. Was he examined before the vessel sailed from Hong Kong, and by whom?	Yes, he was examined along with others by the Health Officer of the Port.
"11. Was any medical examination of the crew and passengers made by the Port Health Authorities at Hong Kong, and on what date?	Yes, on the 28th February, the day of sailing at 11 a.m.
"12. On what date did Kwoknam commence to suffer from fever?	The first notice of his illness was made by the ship's doctor on the 21st March.
"13. Was he on shore at Hong Kong, and if not, had he an opportunity to go on shore if he wished?	He was, of course, on shore, because he came on board from Hong Kong, replacing one Chinaman in service on board.
"14. What cargo did the vessel take in at Hong Kong?	Sugar, camphor, chinaware from Hong Kong, and planks, rattens, tin, and coffee from Singapore and Penang.
"15. Did any rats die during the voyage?	None.

G. DELAYN,
Bombay, Commander S.S. *Bormida*.
24th March 1899."

The Italian s.s. *Bormida* left Hong Kong on 28th February 1899, touched at Singapore 6th March, Penang on 11th March, and arrived at Bombay on the evening of 20th March 1899. Two days before she left Hong Hong a Chinaman named Kowk Nam was shipped as a cook. There was a medical practitioner on board, but no reliable history can be obtained about this Chinaman cook, and no entry of his being ill was made in the log-book. On inquiry on board some of the Chinamen said that the cook had slight fever for two days, and some said for seven days, while others said he had buboes from the time he left Hong Kong. It could not be ascertained if the man was in the habit of smoking opium to excess. On arrival in Bombay the man's temperature was 101·4° F., and he had enlarged glands in each inguinal region as well as an old sore below the navel. He became delirious on the 21st March, and was taken first to a non-plague hospital, where he was refused admission. He was then admitted into the Modi-Khana Plague Hospital, where the case was reported as one of plague. A copy of the man's temperature chart is as follows:—

Name, Kwok Nam; Sex, Male; Age, 30; Caste, Chinese; Occupation, Cook.



The clinical notes on the case are as follows:—

R. No. 2,412.
Name, Kwok Nam.

Date, 21.3.99.

Age.		Disease.	Duration of Disease.	Occupation.	Locality of Residence.	Caste.
Years.	Months.					
30	—	Plague. 1 bubo L. groin, 1 bubo R. groin.	7 days -	Cook - -	S.S. *Bormida* -	Chinese.

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Clinical notes—*cont.*

Date.	Symptoms.	Prescriptions.
—	Admitted into the hospital with fever and gland in both the groins. The patient is delirious and violent. Pulse is weak and frequent. Has an ulcer over the abdomen. The gland in the left groin is larger than the right, it is very tender. The relative of the patient says that he is suffering from fever for the last 7 days, became delirious this morning. The duration of the gland and ulcer is unknown. Came from s.s. *Bormida* from Hong Kong.	℞ Mist. Strych. ℥ iv. Sig. ℥ i every 4 hours. ℞ Morphia grs. ½. Liq. Strych. m. v. Fiatinjectio. 6.40 p.m.
22nd	Is delirious, but less than last night. Gland in the left groin is hard and prominent. Tongue is red at the tip. Pulse weak and quick. Culture taken by Dr. Liston from bubo in left groin. This culture showed a pure growth of bacilli pestis.	22nd. do.
23rd	Is quiet. Rigbt bubo more enlarged. Culture taken from an incision near the ulcer.	23rd. do.
24th	Slept fairly, is conscious, pulse frequent and fairly strong. Tongue is red at the tip, coated at the back. The ulcer is drying. Cut over the left gland is ulcerated.	24th. do.
25th	Is quiet. Pulse fairly strong. Slept fairly. Tongue is coated. Eyes are injected.	25th. do.
26th	Is quiet. Ulcers are drying. The gland in the right groin getting soft. Tongue coated.	26th. do.
27th	Is weak. Does not take nourishment properly. Pulse soft. Died 3.30 a.m. 28.3.99.	27th. do.

It will be seen from the chart and notes that the temperature fell to 99° on the 24th, and that he was reported to be conscious. As the case was rather a peculiar one, I asked the Senior Medical Officer to have a bacteriological examination made. This was done on the 22nd and 23rd March, and it was reported that plague microbes were found. The bacteriological report is as follows:—

"Bacteriological and Clinical Report on Chinaman from Hong Kong, landed in Bombay 21st March 1899 suffering from Fever.

"On 22nd March I received instructions from Dr. Wilkins to make a bacteriological examination of the case of a Chinaman who had come from Hong Kong suffering from fever, and who was then lying at the Modikhana Plague Hospital.

"I saw this patient on the afternoon of the 22nd March. He was then delirious and talking in some unknown language and in broken English. His speech was not slurred or stuttering. His pulse was rapid, large, but soft. The tongue was flabby, coated with a thick white fur. The conjunctivæ were not injected. Temperature in axilla 100·2.

"Just below the umbilicus occupying the middle line but somewhat more inclined to the left side was an ulcer about ¾-inch broad by about 1¼ inches long (the longest diameter being vertical). The centre of the sore was occupied by a black slough; the edge of the sore was slightly elevated and infiltrated, and of a red colour. The infiltration did not extend for more than half an inch from the edge of the ulcer. In either groin an enlarged gland could be detected, largest on the left side. An incision with aseptic precautions was made into the gland in the left groin and with a platinum needle, agar tube cultures were made.

"The patient was again examined on the 23rd. Delirium was not so marked, he was weaker and the glands in the groins were decidedly larger, otherwise his condition was much as before. An incision was made into the infiltrated area at the side of the ulcer in the abdominal wall and agar tube cultures and blood films prepared therefrom. The blood films when stained showed one or two bacteria morphologically resembling plague bacilli.

"On 24th, an abundant growth was evident in the agar tubes obtained from the bubo on 22nd, the characters of the colonies, their rate of growth, and microscopical specimens resembled plague, but to ensure the diagnosis a mouse was inoculated intraperitoneally on the 24th with one drop of a broth emulsion from one of the tubes planted on the 22nd. The mouse died on the 25th. Plague bacilli abundant in all the organs, but particularly in the liver.

"On 25th the growths in the tubes planted from the edge of the abdominal ulcer showed a mixed growth of staphylococcus albus, and plague bacilli.

"The previous history of this case is as follows:—The patient was a cook on board the S.S. *Bormida* which left Hong Kong on the 5th March, calling at Penang and Singapore on the way to Bombay. Seven days before arrival in Bombay he began to suffer from fever. The duration of the ulcer in the abdomen was unknown, but from clinical evidence must have been of at least seven days' duration before it was seen by me on the 22nd. He began to suffer from delirium on the 21st, his temperature being 102·4 at 7 a.m. on that date. After landing he was first taken to the Goculdas Tejpal Hospital, and then transferred to the Modikhana. I am indebted to Dr. Turkhud, in charge of the Modi-Khana Plague Hospital, for the above information.

Wm. Glen Liston, Lt. I.M.S.,
attached Indian Plague Commission."

The man died on the 28th March, or six days after the first bacteriological examination was made. As the ship had a crew of Italian and Chinese, it was difficult to obtain any reliable information regarding the Chinaman cook. For this and other special reasons I would recommend that no conclusion be arrived at regarding plague. There was another Chinaman on the same vessel at the same time with a temperature of 102° and an old left inguinal bubo. We took him out of the vessel on her departure from Bombay, not that he was a plague case, but so that the vessel may not run the risk of quarantine at a foreign port in the event of the Chinaman being found with fever and a bubo. We know all about this man's old bubo, as the fact had been noted by us in the official papers when the vessel was leaving Bombay on a previous occasion. The man was all right next day, and has continued so.

(Witness withdrew.)

Mr. J. H. Du Boulay, I.C.S. recalled and further examined.

Mr. J. H. Du Boulay, I.C.S.

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25,507. (*The President.*) There are a few other points about which we require some further information from you?—Certainly.

25,508. (*Dr. Ruffer.*) Can you tell me how many Plague Hospitals there are in Bombay under the Municipality?—The Arthur Road, the Mahratta, the Modi Khana, and the General Muhammadan are those directly under the Municipality, and we have considerable control over several others—I mean we have greater control over one or two others than we have over the absolutely private hospitals.

25,509. What is the medical staff of the Modi Khana hospital?—Dr. Turkhud is in charge of it. There are six nurses, three English and three local.

25,510. How many patients are there?—Seventy-two plague patients at present.

25,511. And how many other patients?—Forty-six observation and other diseases.

25,512. Practically about 120 patients?—A hundred and eighteen, yes.

25,513. Has the doctor in charge got any other duties besides his hospital duties?—I think he has duties in connexion with the Leper Asylum.

25,514. Do you think it is possible for a man to look after 120 patients and do duty somewhere else as well?—He only spends a few hours there, but I am hardly competent to give an opinion about a medical man's ability.

25,515. Do you think it is possible?—He is obviously away for a certain number of hours a day, and he cannot be attending to patients while he is away.

25,516. How many nurses are there?—Six.

25,517. Three day and three night nurses?—Yes, I think that is how it is arranged. These arrangements are all under Colonel Wilkins, and they do not directly come to me.

25,518. How many Hospital Assistants have you got?—Two.

25,519. Would you go to the next hospital?—The Arthur Road. The Medical Officers are Dr. Choksy and Dr. Fraser.

25,520. Does Dr. Choksy practice as well as look after the hospital?—I hear of cases where he attends on plague patients.

25,521. Has he any other duties besides at the Arthur Road Hospital?—I do not know of any others. He used to be in charge of the Leper Asylum, but he has given that up.

25,522. What is the total number of patients?—One hundred and thirty-four plague cases, and one hundred and five observation and other diseases; total 239.

25,523. The ordinary staff consists of Dr. Choksy and Dr. Fraser: anybody else?—There are no other doctors. There are 13 nurses, four Hospital Assistants, two compounders, and 46 ward boys.

25,524. Are these Hospital Assistants allowed to do any work outside the hospital?—I think they practically never leave the hospital.

25,525. Will you go on to the next?—The Mahratta Hospital has got Dr. Dhargalker and Dr. Lam, and there are two people who are called House Surgeons—they are not fully qualified—Suntoodji Ramji and Eknath Hate. There is also a Vaidya who takes charge of the considerable number of patients who prefer native treatment.

25,526. How many plague patients are there?—In the Mahratta Hospital 222 plague cases, and 54 observation and other diseases.

25,527. Do you think the medical staff is sufficient for that number of cases?—I think so. There are three Hospital Assistants, and three medical students. Of course the people do not like excessive attendance, they prefer being left alone as much as possible, that is the great thing in the Mahratta Hospital.

25,528. That is a hospital where they do the bathing treatment chiefly?—Yes, they prefer the Mahratta Hospital to the Arthur Road.

25,529. What is the other hospital?—The other is the Northbrook Gardens. There is only a native hakim in charge of that. That is a Musalman hospital.

25,530. How many nurses are there?—I do not think there are any nurses there. There are three ayahs; they do not like English attendants.

25,531. In going round the hospitals, I noticed that a great number of the patients are tied up in the beds: do not you think that could be prevented if you had a larger number of nurses?—If you had a very enormously increased staff, it could be done. We should have to have a nurse for every two or three patients.

25,532. Is it not a very painful process to be tied up in bed when you have fever?—It has never been done to me. I think the patients are mostly delirious.

25,533. Do you think it is a pleasant process?—The nurses, I presume, would merely hold them instead of tying them.

25,534. I want to ask you about disinfection. You disinfect a good deal with perchloride of mercury?—Almost entirely.

25,535. What steps did you take to see that the perchloride of mercury used was of good quality?—I have had one specimen analysed within the last two or three months, and that was practically pure.

25,536. It was never analysed before two or three months ago?—I do not know what the Plague Committee did. I notice in their report that they took frequent steps to have it analysed. The perchloride of mercury here is received in packets from the manufacturer. If there is any break in the seal, the sample is rejected.

25,537. How long ago is it since the samples were analysed?—Within the last two months.

25,538. What was the result of the analysis?—The perchloride was practically pure.

25,539. What impurities did it contain?—That is all I have got from the Chemical Analyser—that it was practically pure.

25,540. Have you ascertained the strength of the solutions which are used?—Yes, I have had samples analysed from time to time.

25,541. Could you give us the result of those analyses?—One was made by Professor Wright, taken from a bucket containing 1 in 3,000.

25,542. Was it acid? The solution that you used is supposed to be acid?—Yes, the solution was slightly acid.

25,543. What is the quantity of acid that it is supposed to contain?—About 1½ to 1 of perchloride.

25,544. It should be strongly acid, should it not?—I cannot give an opinion about that.

25,545. Could you give us the result of the other analyses which you have?—I got from the Chemical Analyser an opinion on the point of acidity. He suggested the use of hydrochloric acid one lb., with one lb. of mercuric chloride to make a hundred gallons of solution. The above proportions are correct for pure water. To meet the possibility of ordinary alkaline salt when it is being used, it might be advisable to increase the hydrochloric acid to 1½ lbs. per hundred gallons of solution. That rather strengthens my belief that it is sufficient.

25,546. If it is only slightly acid, it simply shows that the amount of acid has not been added. It should be strongly acid if it contains 1½ per cent. of hydrochloric acid?—I do not know. I think the hydrochloric acid is useful for other purposes, but of course it should be there if you put it in. I went to another district and made some of the stuff myself, and took that out in the district and took a sample of that, as well as of what they were actually using; and that came to 1·206 per 1,000 as the strength of the sample I myself prepared. What they were actually using before, with the preparation of which I had not anything to do, came to ·610 per 1,000. In another case I did the same thing. I made some myself, and took a test of what they were actually using. What I made myself came to 1·475 per 1,000, and what they were actually using came to ·612 per 1,000. In another case I took a test of what they were actually using, and it came to ·7 per 1,000. In another case I took a test of what they were actually using, with some that I prepared myself, and what they were actually using came to ·09 per 1,000; and that which I prepared myself came to 1·08 per 1,000. I think it right to point out

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that that district has been without a district officer for some time. These, of course, were tests I took for my own information originally, not for yours. I also took some tests later. After I began taking these tests, and was rather unsatisfied about them, I asked one district officer to put a larger quantity of perchloride in the solution. In taking a test later I prepared a sample myself, as I wanted to see particularly what was the effect of the metal pump which they were using. I took one sample straight from the tube before the pump was put in, and another sample I pumped with a metal pump through a long greased tube in order to see what the effect of the metal was. The pump immediately became covered with silver—mercury I suppose—but the Chemical Analyser reported that both samples were 2 per 1,000.

25,547. The difference would be exceedingly small. Have you anything else?—I have told you of the perchloride I took, and I have just made a rough note of other disinfectants that were tested last year. Carbolic acid powder was tested five or six times during 1898. It was found to contain 13 per cent. of acid. Carbolic acid was tested two or three times, and each time found to be of good quality. In another case there was an analysis of perchlorde of mercury in use taken by Dr. Ruffer which gave 1·25 per 1,000 as the result.

25,548. How did you ascertain the temperature in the sterilising stoves?—By the pressure outside—by watching the gauge outside.

25,549. Do you think that is a good method? Have you ever found that the interior of the bundles of clothing that were put inside, was not up to the proper temperature?—Yes, on the occasion that I went round with you, sir, we found that, but you had to search for these places where the clothing was cold.

25,550. Part of the clothing was cold?—Yes.

25,551. But would it not be better to ascertain the temperature in a bundle of clothing each time; would it not be a more efficient method?—It would be more efficient I think. It was contrary to orders that that clothing was put in in bundles; the clothing ought to have been put in loosely, as is done now.

25,552. (*Prof. Wright.*) What method do you think is the best in your experience of getting information of plague cases while the patients are actually alive? Have you found a system of reports by doctors to be satisfactory?—No.

25,553. Have you found that the system of reporting by volunteers gives you satisfactory results; that you receive information of all the cases of plague that occur?—No, we never had received information of all the plague cases that have occurred before death, but then the cases are very often very virulent, and die in six or seven hours sometimes.

25,554. What do you think about the system of discovering plague cases by house-to-house visitation?—It is absolutely intolerable to the people; that is my experience of it.

25,555. What systems do you at present adopt—the system of getting reports from medical men, and the system of reports from volunteers?—The reports from volunteers; we also find out where every death occurs—not quite every death, but we find out all but about five per cent. of the deaths that occur in the city.

25,556. I am talking about living cases?—Those houses where deaths are occurring come under closer observation by the staff itself; they search those houses. Very often we get a great deal of information from the friends of the patients, and we send the cases to hospital; we also get a good deal of information from the neighbours who object to having plague cases near them; and we get a certain amount of information from spies, and we have all sorts of minor sources of information.

25,557. What percentage of plague cases do you think are reported to you during life? Do you receive information of one per cent. of plague cases by all these means?—It is very difficult to put it in that form straight away; I should have to work it out. I could hardly, without thinking it over and working all the figures out, give you an idea.

25,558. When you find the plague patients during life, what is your policy? Is it not the case that patients were removed to hospital by force?—I know of no occasion where force has ever been used; they have always been persuaded eventually.

25,559. Your policy is to remove them to hospital?—Yes.

25,560. Do you in any case leave a patient behind in his own house?—Occasionally.

25,561. In what cases do you leave him behind in his own house?—If he is moribund; it practically comes to that. Unless the man is certified by a medical expert to be in a very dangerous state he never is left in his house. District officers have a discretion to leave patients at home if proper arrangements for isolation and treatment can be made. It is very rarely exercised.

25,562. Supposing you find great opposition to his removal, then would you under the present system leave him in his house?—Opposition is nearly always backed by a medical certificate that the man is moribund, or in a very precarious condition.

25,563. Do you think it good policy to remove plague patients to hospital; do you think that it arouses much opposition, and leads to much hiding of cases?—I suppose it does, yes.

25,564. Do you think it would be a better system not to remove anybody to hospital, except people who were really anxious to go?—I should say not. I do not know how this infection is spread, but I imagine that the patient in the house is the centre of infection. Most of our houses are big chawls in Bombay, and you cannot isolate patients in those places.

25,565. The advantage of removing them to hospital is that you remove the source of infection, and the disadvantage is that you lead to the secretion of cases. I am asking you to judge whether you think the advantage of removing the source of infection counterbalances the disadvantages which are associated with the secretion of cases?—I do not feel that I am able to pronounce an opinion about that.

25,566. I want to know whether you have arrived at an opinion about it, whether it would be a wiser policy?—I have not arrived at an opinion about it.

25,567. What steps do you take when you leave a patient in a house to prevent him disseminating infection? Supposing you leave a moribund patient in his house, what steps do you take to prevent his becoming a source of infection?—Nothing beyond giving instructions to the people in the house, and beyond talking to them seriously if they disobey.

25,568. There is no system of periodical disinfection of floors when the patient is left behind?—That is occasionally done. The people are told to disinfect, and are given disinfectants to use, but the houses are not disinfected as a rule by our staff in those cases.

25,569. Do you think it would be practicable in those cases to go into their houses and disinfect their floors and walls and clothes periodically, or do you think it would not be practicable?—It would not be practicable. You could not disinfect the most infected part of the room very well; the patient would be lying there, and you cannot very well disinfect his bedding while he is on it.

25,570. You think it would be difficult to do it satisfactorily?—Yes, and there would be a certain amount of opposition too. It would not be so bad as removing them, of course, but they would say, "Why cannot you let us do this?" and there would be a certain amount of opposition.

25,571. Your measures that you take to prevent the spread of infection from living patients consists chiefly of removing them to hospital?—Yes.

25,572. When you remove the patient to hospital do you disinfect the room from which he has been removed?—Yes.

25,573. What steps do you take to prevent infection occurring after a death from plague has occurred?—We go at once to the room and burn all the clothing we find lying about.

25,574. How do you receive information that a patient has died of plague; do you receive the medical reports?—We get from our volunteer agents the same sources of information as we get about living patients, and in addition we have the cemetery reports.

25,575. You have reports from doctors, reports from volunteer agents, and reports from the cemetery?—Yes, and reports from our own staff, and spies, and the Municipal staff.

25,576. How many reports do you get from doctors, private practitioners, of cases of death from plague?—Not a great number, about 30 or 40 a week, or something of that sort.

25,577. That means about five a day, and your mortality may be sometimes 300 a day on the average?—Yes.

25,578. So that that is a very ineffective method?—I am speaking of private practitioners, who are not volunteers; the volunteers work harder.

25,579. Do you think that represents the number of plague cases that are actually attended by doctors?—No.

25,580. You think there is a good deal of secretiveness, and the doctors do not report cases?—I should think so.

25,581. You have your reports from volunteers, and spies, and at the graveyards; will you tell us what is the agency you employ at the graveyard?—There are Hospital Assistants there who ask the symptoms from the people who bring the body, and naturally the people are not in a hurry, as a rule, to say that it is plague, but still there are a certain number who give the information, and as soon as that information goes down to the district the house is dealt with. The Medical Officers visit every house where a death has occurred and make inquiries as to the cause.

25,582. I understand that on one particular day you had a death rate of 391: do you remember how many of those deaths on that day were discovered to be plague by means of this agency at the graveyards and burning ghauts?—I cannot tell you that.

25,583. How many were put down as plague out of that?—I think it was certainly under 200.

25,584. And your normal mortality would be about 80; is not that so?—Yes.

25,585. And probably 310 died of plague?—Yes, about that—there is a good deal of measles now.

25,586. At any rate, it would be about two-thirds of the probable deaths from plague that you find out by these means?—I was going to tell you that we do not accept the cemetery reports as final that such and such a case is plague. My medical men go round to the houses, and make inquiries from the neighbours regarding every death.

25,587. On a day like that you would have 391 deaths; how many of them would be investigated afterwards?—Every death is investigated whether it is put down as plague or not.

25,588. Is a man sent to the houses?—A medical man goes to every house where a death occurs; but of course they know a great number of them before—they are cases that passed through their own hands.

25,589. Do you mean to say that 391 visits were paid by medical men to discover the cause of death?—You would probably have to knock off over 200 to start with, of cases they knew about beforehand. There is a great deal of sickness reported to our medical men beforehand; they know all about those cases, and about all those that they have had to deal with before. They are responsible to go and find out what the cause of death was in every case where they do not already know it.

25,590. There are Hospital Assistants at the cemeteries and burning ghauts, and they write a list of cases?—The cemetery officers merely record the opinions of the people who come with the bodies there. They say, "What symptoms had he and what has he died of?" And from the answers they diagnose cases as plague or as the case may be.

25,591. They send a list then of the deaths that appear to them to have been caused by plague; who does that list go to?—The record of all the deaths is sent to the Health Department.

25,592. Are they sorted out there?—They are sorted out at the Health Department, and the Health Department send the various lists down to the Inspectors.

25,593. Who sorts out the cases of plague?—They do not sort out the cases of plague.

25,594. On the day when there were 391 deaths, a list of the cases would be sent to the Health Department, and 200 of these would be deaths that occurred from plague. What happened to that list in the Health Department?—You insist on speaking of the plague cases; the deaths are all jumbled together; they do not sort out the plague cases there.

25,595. What process takes place at the Health Department?—The clerks sort them out according to the different sections of the city to which they belong, and put down the names and addresses and the causes of death. I have a copy of each section made, which I send to the District Officer responsible for that section.

25,596. Do you send it the same day?—The same night, and he gets to work on it the following morning.

25,597. (*Dr. Ruffer.*) Twelve hours afterwards?—Yes.

25,598. (*Prof. Wright.*) In the Health Office they are sorted out according to the sections in which these deaths occur, and a list is sent to your Divisional Officers. What steps are then taken?—They check these by their registers and books.

25,599. What registers and books?—They keep registers of all sickness and deaths that they hear of in their district, and of all plague cases. If he has a plague case, and removes it to the hospital, and the case dies, the death appears in the cemetery list, and it comes back to the district office, and he looks at the book to see if he has treated that man or not. If he has, there is no further action to take, as he has already disinfected the place when he took the patient to hospital. Similarly when a death occurs, although he may not have heard of the sickness, he has very likely heard of the death before the cemetery list comes in, and he may have gone there and disinfected the place at once, if he thought it was plague. If that has happened, he ticks it off, and it is disposed of. The cemetery list would come to him 24 hours later, but every death in the city, which is not sent from a Plague Hospital, or sent with a certificate from a volunteer, is supposed to be caught by the police, who give earlier information than is received from the cemetery.

25,600. I have not understood this yet. The list goes to the Health Office, I understand, and then it goes to the Divisional Officer; the Divisional Officer looks through the list of the day, and he recognises the cases of people whom he has previously dealt with?—Or there may be a large number that may not require disinfection—who were not plague.

25,601. There are a large number he may not have heard of before, and who are on this list?—They are not a large number; I should say about ten in thirty.

25,602. About 30 per cent. of the cases were not heard of before?—Yes.

25,603. He may have received reports of two-thirds of the sickness occurring in Bombay?—Of the deaths.

25,604. This is the first notification of the deaths?—No, I have said that the police also give us information of deaths. As a funeral party goes along the street, a policeman will go up and inquire where they came from.

25,605. To whom do the police give the information?—To the Divisional Officer through the Registration Office. There are recording offices all over Bombay—seven or eight. The police instantly give information there, and the recording stations pass it on to the Divisional Officers.

25,606. You have a further record of deaths supplied by the police?—I do not call it a record; it is merely an auxiliary means of getting early information of deaths.

25,607. Does that go to the Health Office?—No.

25,608. How does that get to the Divisional Officer?—It gets to him from the recording station. There are seven or eight recording stations which receive the information from the sepoys who find the house, and this is sent on to the District Officers.

25,609. That is information of death; it does not convey any information as to the cause of death?—None whatever.

25,610. I mean in a large majority of these deaths the Divisional Officer does not know whether they are due to plague or not,—he does not know anything about the cause of death; is not that so?—Yes, there are a large number of cases when he only learns the cause of death by subsequent inquiry. I wanted to point out to you that this information from the police is prior to the information that they give us in the

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cemetery, so that when District Officers receive information from the cemetery they have already inquired into the death. They have only a few left on the fever sheets.

25,611. Does the Medical Officer visit each house in which a death has occurred?—Yes.

25,612. In the case of the 391 deaths on that day, a medical officer would have visited each house and made a report?—Yes, but of course there are a certain number of cases of death that we cannot trace, owing to wrong addresses being given.

25,613. When he has not seen a patient, he has only been able to make inquiries from the relatives. The information with regard to the cause of death is from inquiries made from relatives at the graveyard, and inquiries from relatives at the house?—Yes; but there are a number of sick people, and dying people, and dead people, either of plague or not of plague, whom the medical officers have seen before the body is removed to the cemetery.

25,614. Those who have been seen during life are a very small proportion of the total cases, are they not?—I could not give the proportion; it would require a lot of working out.

25,615. What means have you of arriving at a conclusion as to cause of death in the case of the people who die in Bombay? Will you put in all the information in answer to that question? As far as I have understood it is from inquiries by Hospital Assistants at the graveyards?—You leave out the cases the district officer knows before.

25,616. That is a small percentage?—It is a large percentage.

25,617. I want to know what means there are of determining the causes of death in Bombay?—(Witness subsequently supplied the following information):—From an examination of my weekly records for the week ending 4th March 1899, I find there were 2,307 deaths reported from the cemeteries and burning grounds. Of those 2,155 were traced in the city, 1,077 were found to be due to plague, 513 occurred in hospital, and a considerable proportion of the remainder were seen by the Sectional Medical Officers either before or after death. I am unable to give the exact proportion, because being satisfied that my officers do not put down deaths as due to plague without proper verification, and finding rather that the tendency is to accept the statements of relations that death is due to some ordinary cause, I have not exacted from them any systematic account of the grounds on which they put down a death as due to plague. Of the remaining 1,078 deaths that were traced—

287 were seen by the Sectional Medical Officers before or after deaths.
90 were certified by qualified practical practitioners.
1 was certified by a medical practitioner (unqualified).
38 were seen before death by members of the volunteer Plague Committees.
44 died in the public hospitals.
2 died in the Lunatic Asylum.
1 in Military Lines.

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giving a total of 463 where there was fairly satisfactory evidence that death was not due to plague. In the remaining 615 cases the Sectional Medical Officers visited the houses where deaths occurred, and made inquiries to the best of their ability from the friends, residents, and neighbours. In 290 cases they satisfied themselves that death was not due to plague. In the remaining 325 cases the results of their inquiries were unsatisfactory, and they were put down as suspicious.

To sum up, out of 2,307 deaths, 1,077 were undoubtedly due to plague. In 325 cases the results of their inquiries lead the Sectional Medical Officers to put the deaths down as suspicious. In regard to 463 there was fairly satisfactory evidence that they were *not* due to plague. In 290 cases the Sectional Medical Officers satisfied themselves by inquiries that death was *not* due to plague. In 152 cases it was impossible to make inquiries owing to failure to trace the houses where the deaths occurred.

The average death rate for the corresponding week in years before the outbreak of the epidemic was 543. In the week under examination, putting on one side the 1,402 cases of plague and suspicious, there were 905 deaths in regard to which no action was taken for plague. The difference (362) between 543 and 905 should, however, be reduced by 103 deaths due to an epidemic of measles, in order to arrive at the approximate number of plague deaths which altogether evaded detection. For the week under examination I estimate this number at 259.

25,618. Could you not receive more complete information than you do at present by a system of corpse inspection?—Undoubtedly.

25,619. Do you think corpse inspection would arouse much opposition?—Undoubtedly it would.

25,620. Do you think it would arouse less opposition if conducted at the graveyards, or burning ghauts, than if conducted at houses?—I think it might be conceivable, by very very slow processes, to arrive at a certain amount of compulsion that they should be examined in the houses, but it would be a very slow process, and probably take a couple of years to work up to.

25,621. If you find out that you know very little about the causes of death in the large majority of cases, then you feel inclined to think that a system of corpse inspection should be gradually introduced, you think it would be wise, and that it would be an advantage to introduce it?—Yes.

25,622. (*Mr. Hewett.*) Is a certificate required from a duly qualified qualified medical officer before anybody can be sent to the Plague Hospital?—Yes, but it is not insisted on in every case; if there is no doubt, and the people do not object.

25,623. Have you any knowledge of the number of cases sent to the Plague Hospital under certificates, who have turned out to be not suffering from plague?—I have no idea whatever.

25,624. Is it a considerable proportion?—The doctors in charge of hospitals are inclined to make rather a fuss about such cases. One or two cases have come to my knowledge, but not a large number. The tendency is rather on the other hand to send cases for observation, which are really plague, I think.

25,625. Do you find any tendency among the relatives and friends of patients to send them to hospital?—There are a certain number who go every week voluntarily. It is a very fair proportion of those who do go to hospital now.

25,626. Have you any facts to show the tendency of a second case to occur in a house where a case has already occurred?—I had some statistics collected, but I really do not think they are very valuable, but I have got them here if you wish to see them. I have not worked them out myself, but that is the belief I always had till I got these statistics, that there was a tendency for cases to recur in a house. These statistics seem to show that the number of houses where single cases occurred is rather larger than I anticipated, but whether that is due to disinfection or not, I do not know. There are the facts.

25,627. Have you found a tendency for a second case to occur in those instances in which you have left moribund patients in their houses?—No, I have not found that particularly, but all these things are questions of close observation, which we have not got the means of carrying out in Bombay—we have not got the establishment, nor the knowledge of the cases that occur.

25,628. Have you permitted people who are not moribund to be treated in their own houses here?—No, except in thoses cases that I spoke of.

25,629. Are you still taking all the contacts to contact camps?—We have not got what you would call a contact camp, I mean the restrictions are almost entirely abolished. All we can say is that, "you must "bring your cooking pots to camp, and you must live "there." They can go to their work, but they are expected to sleep there; they do not go to their own houses.

25,630. Do you think that you know little about the cause of death in the great majority of cases of death?—I do not think so. Of course we do not know much about the ordinary causes of death. We do not bother very much, as long as we are satisfied that it is not plague. We take more or less what the cemetery clerks put it down as, if there is nothing suspicious.

To reduce it to the minimum, I should say that there are 300 cases a week which we make mistakes about, that are probably due to plague, and which we have not treated as suspicious.

25,631. Your meaning, then, is that by the facts ascertained by district officers, and by reports in hospital, supplemented by the subsidiary inquiries which are sent to the Health Department, you manage to ascertain the cause of death in all but about 300 cases a week?—That is about what I worked it out last week.

25,632. (*The President.*) Do you find it now more easy to induce people to go to hospital than in former epidemics?—My experience of former epidemics was not very great. Of course there was a great deal more force displayed always in the time of the first outbreaks, and I do not think the people cared to face the possibility of a struggle with the authorities then. As a matter of fact, now, there has been no case in which, to my knowledge, a man has absolutely resisted being taken to hospital. A case did occur to me one day, but that was eventually got over. He wanted to leave the room, but the doctor stood in the way: that was all.

25,633. In regard to moribund cases, which have been left at home for treatment, have you any knowledge whether such cases have or have not been the means of communicating plague?—I have no figures that I could put before you. I do not think it is possible to form any real opinion about it in Bombay, because, as I say, our men are all working hard, and they have not time to carefully study each case, and, also, there are a certain number of cases which escape observation; so that any observation would be very incomplete; but so far as I have heard from the district officers here, I have not heard it stated that moribund cases left at home have had a tendency to spread plague. Cases occur afterwards, but then they have in many houses two or more cases, so that that again would not attract much attention; they could not say that it was because any case was left in the house that it caused the spread of plague. A district officer having a badly infected locality told me that he thought it was entirely due to one or two cases being left in the street; but there, again, it is an unsound inference, it seems to me; there were a number of cases, and two cases had happened to be left there.

25,634. You have not followed up the question sufficiently in order to answer it definitely?—No, it is a very large question indeed.

25,635. I understand, therefore, that you do treat certain cases in their own houses?—We leave moribund cases at home.

25,636. Only the moribund?—I think I explained that—that there were a certain percentage of cases, mostly rich people and influential people, who obtain certificates to the effect that the patient is in an exceedingly precarious condition, and that the removal will cause a great risk of death; and if the opposition is very great, we give it up. One does not like to enforce the removal, and to face that risk.

25,637. (*Prof. Wright.*) You said in answer to Mr. Hewett that you knew the cause of death in all but 300 cases a week?—Yes, I worked it out last week.

25,638. You mean that there were 300 cases of plague which you do not recognise as plague?—That is the conclusion I came to. There are a certain number of cases that are treated as suspicious. There is no record, but they disinfected in those cases. These suspicious cases are excluded from the 300.

25,639. When you recognise a case as plague you disinfect the house, do you not?—Not the whole house.

25,640. How much, when it is a chawl?—The room itself, and generally the two adjoining rooms.

25,641. How many houses would be disinfected in a day when, for instance, the mortality is 391? That would involve a disinfection of over 300 houses?—Probably.

25,642. Have you the means for disinfecting 300 houses in a day? How many gangs have you got?—I cannot tell you how many gangs, but there are about six or seven men in a gang, and there are about 600 men actually employed.

25,643. That would be 100 gangs?—About 100 gangs.

25,644. Have you got plant for 100 gangs?—The plant is not very much.

25,645. I mean, such as it is; you would want a bucket and a pump, and you would also want an Inspector?—We have not got an Inspector to every gang. We have got a muccadum, who is the head coolie. The Inspector goes round to three or four or five gangs.

25,646. Is the muccadum a responsible man that you can trust?—He is merely a head coolie; he is held responsible by the Inspector.

25,647. All I want to arrive at is this: when you have 300 houses to disinfect in a day, have you got the men and the staff to do it in a way that you would consider satisfactory?—Yes, it is done.

25,648. Are you satisfied whether it is done properly?—It is a question for you to decide whether it is satisfactory.

25,649. I want to know whether you are satisfied with it?—I am satisfied with it to this extent, that if you want to make it perfect you must go bankrupt at once.

25,650. Are we to write down in our minds that it is an effective or ineffective method of disinfection, or is it possible that a large number of the disinfections are not properly done?—I do not think it is really satisfactory. Every house ought to be disinfected by a doctor, I understand.

25,651. With the staff and the plant that you have got, how many houses do you think you could manage to do in a day in a way that would enable you satisfactorily to make this a basis for a further inference as to the way in which plague spreads?—I have never attempted to draw a plan of that. If the district officers tell me that they find it difficult, they are at once given more coolies and supervising staff. I have never attempted to form any estimate of the exact number of coolies required for so many cases. If they find their men are overworked, the work is done in the same way, only it means that the men go on working longer hours, and they go to houses a longer time after the infection has occurred.

25,652. You do not think you can infer, with a death-rate of 391 in a day, that 300 houses are cleared from infection by your disinfecting operations on that day?—I think, from the analysis of my disinfections, it occasionally does occur that the work is not properly done.

25,653. (*Dr. Ruffer.*) What do you mean by occasionally; that is rather an elastic term? Do you mean 10 a day, or 20 a day? "Occasionally," in my mind, means 1 in 100. I want to know what it means to you?—I should say 3 or 4 in 100 is my idea of it, but it is impossible for anyone to say.

25,654. I want to know about the cemeteries. Is it true that the Assistant Surgeon at the cemetery is on no account to examine bodies there?—He has orders not to.

25,655. Under any conditions?—Yes.

25,656. I went to a cemetery the other day, and a child was brought in, and the relatives said it had suffered from asthma. I believe the child's certificate in the registration in the cemetery afterwards was entered as "General Debility"?—Was not it the other way round?

25,657. It would come to the same thing. It was given to me as asthma?—I saw the certificate of the Plague Authority, that it was "General Debility," which I believe came in afterwards, that is to say, after the cemetery clerk had made his diagnosis.

25,658. The relatives told me the child had asthma. How did the Plague Authority come to the conclusion that it was general debility?—I did not ask him in that particular case, but he had probably seen the child suffering for months previously.

25,659. He had not seen the child at all. The friends told me that it had not been seen by a medical man at all?—This is a Plague Authority, who is not necessarily a medical man.

25,660. The Plague Authority wrote it down, and it was entered in the register as general debility, but the friends said it suffered from asthma. I want to know how the plague officer arrived at that conclusion?—It depends in each case; generally it is that he has seen the case suffering. You say the friends of the child told you that he had not?

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25,661. That is my impression, but we will ask Dr. Weir?—You cannot trust what the people say. You and Dr. Weir were there, and they were probably frightened, and perhaps thought that you were going to do something; so they might have given you a different story from what was on the certificate. I have to add, in regard to the question of compulsory removal to hospital, that we do not attempt to deal with the Health Department sweepers. They are difficult to manage, and the disastrous possibilities of a strike among them compel us to leave them alone as a matter of policy. They are isolated, so far as possible, in their own chawls, and dealt with entirely by the Health Department itself.

(Witness withdrew.)

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Lieut.-Col. T. S. WEIR, I.M.S., recalled and further examined.

25,662. (*The President.*) When you were before us previously we asked you for certain statistics with which you are now in a position to supply us?—Yes.

25,663. You were asked about a case of plague which was communicated by a rat-bite and about cases of importation by sea?—I have not been able to get the information as yet.

25,664. Then there was a question raised as to the period of incubation?—That information is not ready yet. I have sent circulars round to all our medical men.

25,665. You were asked if you could supply us with the number of cases of plague in each ward or district in Bombay since the origin of plague?—I have a return showing the mortality since 1896, as follows:—

STATEMENT showing the NUMBER of DEATHS from BUBONIC PLAGUE from 1st September 1896 to 16th March 1899.

Wards, &c.	Sections.	From 1st Sept. to 31st Jan. 1896.	1897.	1898.	From 1st Jan. 1899 up to 16th Mar.
A.	Upper Kolaba	1	50	31	5
	Middle and Lower Kolaba.	77	189	311	186
	Fort, Southern	2	7	6	2
	Fort, Northern	46	216	923	229
	Esplanade	1	33	91	52
B.	Mandvi	308	334	911	251
	Chuckla	58	81	464	95
	Umarkhadi	84	259	746	208
	Dongri	95	267	716	304
C.	Market	87	182	1,044	328
	Dhobi Talao	110	367	1,034	307
	Fanaswadi	32	126	548	270
	Bhuleshwar	68	151	724	259
	Khara Talao	51	118	348	144
	Khumbharwada	83	391	974	330
D.	Khetwadi	48	272	655	270
	Girgaum	42	258	850	419
	Chowpati	7	52	174	106
	Walkeshwar	5	125	101	37
	Mahalakshmi	7	219	192	101
E.	Mazagaon	93	553	732	348
	Tarwadi	20	207	343	251
	2nd Nagpada	29	196	303	100
	Kamathipura	227	619	1,005	339
	Tardeo	31	363	524	316
	Byculla	88	919	1,432	676
	1st Nagpada	27	165	244	137
F.	Parel	23	603	759	318
	Sewri	13	122	59	108
	Sion	5	460	188	113
G.	Mahim	12	666	383	272
	Worli	41	460	382	158
	Water Division	2	8	10	3
	Non-Resident and unknown.	113	1,965	978	42
	Total	1,936	11,003	18,185	7,084

25,666. You were asked if you could supply us with the number of houses marked unfit for human habitation?—I have that return from the Engineer, and I put it in, as follows:—

STATEMENT showing the Numbers of Houses marked "UHH" in each Ward and in each Section in Bombay in 1897 and 1898.

Ward.	District.	No. of Houses marked "UHH."
A.	Kolaba	47
	Fort	20
	Esplanade	11
B.	Mandvi	27
	Chuckla	35
	Umarkhadi	38
	Dougri	16
C.	Market	5
	Dhobi Talao	84
	Fanaswadi	2
	Bhuleshwar	15
	Khara Talao	12
	Kumbharwada	70
D.	Khetwadi	7
	Girgaum	4
	Chowpati	2
	Walkeshwar	7
	Mahalakshmi	1
E.	Mazagaon	28
	Tarwadi	17
	2nd Nagpada	14
	Kamatipura	164
	Tardeo	8
	Byculla	36
	1st Nagpada	1
F.	Parel	5
		665

25,667. Can you give us tables showing the density of population in each ward?—Yes, the following statement shows what is required:—

STATEMENT showing POPULATION, and the DENSITY by SECTIONS and the AREA of each SECTION.

Wards.	Sections.	Population.	Area in Square Yards.	Mean Area in Yards to a Person.	Square Acre.	Persons to an Acre.
A.	Upper Kolaba	4,835	1,220,267	281·49	252·12	17·19
	Middle and Lower Kolaba.	13,622	723,733	53·13	149·53	91·09
	Fort, Southern	3,951	670,400	169·67	138·51	28·52
	Fort, Northern	32,847	562,667	17·13	116·25	282·55
	Esplanade	10,064	3,315,200	329·39	684·95	14·69
B.	Mandvi	37,295	963,644	25·83	199·10	187·31
	Chuckla	32,127	248,000	7·70	51·23	628·47
	Umarkhadi	52,466	504,000	9·60	104·13	503·85
	Dongri	30,317	1,321,600	43·59	273·05	111·03
C.	Market	44,751	404,800	9·04	83·63	535·10
	Dhobi Talao	39,945	505,600	12·65	104·46	382·39
	Fanaswadi	24,069	544,000	22·60	112·39	214·15
	Bhuleshwar	38,363	347,200	9·05	71·73	534·82
	Khara Talao	27,035	195,200	7·22	40·33	670·34
	Kumbharwada	32,209	252,800	7·84	52·23	616·67

Wards.	Sections.	Population.	Area in Square Yards.	Mean Area in Yards to a Person.	Square Acre.	Persons to an Acre.
D.	Khetwadi	28,814	1,870,400	64·91	386·41	74·56
	Girgaum	26,999	563,000	21·03	117·35	230·07
	Chowpati	11,512	532,800	46·28	110·08	104·57
	Walkeshwar	12,990	2,873,600	221·21	593·71	21·87
	Mahalakshmi	17,014	3,252,800	191·18	672·06	25·31
E.	Mazagaon	33,640	2,907,200	86·42	600·66	56·00
	Tarwadi	21,208	2,481,600	116·51	512·72	41·36
	2nd Nagpada	18,768	203,200	10·82	41·98	447·07
	Kamatipura	29,203	340,800	11·67	70·41	414·75
	Tardeo	18,980	795,200	41·89	154·29	115·52
	Byculla	47,403	2,843,200	59·97	587·43	80·69
	1st Nagpada	11,133	160,000	14·37	33·05	336·85
F.	Parel	28,740	2,907,200	101·15	600·66	47·84
	Sewri	6,063	2,436,800	401·91	503·47	12·04
	Sion	19,601	16,316,311	832·42	3371·13	5·81
G.	Mahim	18,505	7,708,800	416·57	1592·72	11·61
	Worli	25,493	11,390,400	446·80	2353·38	10·83

25,668. How far do you think the figures in that table are correct?—The table must be interpreted with a number of reservations. For instance, there is the section of Dongri. Dongri is the section immediately to the north of Mandvi and includes Dongri Hill, which is inhabited on one side, and the quarry-holes on the east and also open land. Then there is the migration of population backwards and forwards.

25,669. You mean that a large portion of this section has no houses?—Yes; so that really the populated portion of Dongri is much more thickly populated than would appear by the table. It is the same with regard to Fort North and Fort South. The area for each person is less than would appear from the table. Then, of coure, there is a great migration of population backwards and forwards from Bombay. They go from Bombay to the country for work, and during plague years there is a great migration of population from the town proper into the suburbs, and also migration from the suburbs into the town proper.

25,670. At what period of the year were these figures collected?—They are taken from the census, from an enumeration which was made in February.

25,671. What was the migration then?—Very large numbers of people came into Bombay for work.

25,672. And they remained there from February till when?—Till the beginning of the monsoon, in June.

25,673. Therefore the numbers would only apply to half of the year?—Practically. That is one of the conditions which you have to remember in interpreting these figures.

25,674. (*Dr. Ruffer.*) With regard to the mortality as compared to the area you stated, I think, that it was smaller in certain wards?—The density of population in Bombay varies from seven square yards to each person, to over 800 square yards in the extreme suburbs.

25,675. Was the plague worse in the more densely populated quarters?—No. It is a most extraordinary fact that the mortality in the buildings in the most densely populated portions of the town has absolutely been less than in small villages in the suburbs, according to our statistics.

25,676. Are their houses as much overcrowded in the small villages as in the town?—No; they are not so overcrowded.

25,677. How do you know that?—From the census figures which show a decreasing population from the city outwards, in dwellings; 37 per cent. of the population live over 3½ per cent. of the surface area. You have, in proportion to your house area, a larger number of rooms than you have in the suburbs. In the next place you have people living in the town whose necessities compel them to overcrowd each other more. And you have a population in house-passages and in the streets.

25,678. But that is only an impression?—No; that is undoubtedly so. There are no houses in the suburbs so overcrowded as in the city.

25,679. Have you a census of the villages showing that as compared to their area they are more overcrowded than the smaller part of the town?—We have statistics which could, of course, be found, showing the number of persons per houses in villages in which cholera or plague has occurred.

25,680. Then, according to your evidence, overcrowding is not the chief factor in producing plague?—No, that is not my point; I believe it is a very important factor. But there is this fact in our statistics that the plague mortality was highest in the suburbs.

25,681. Do you think that is accurate enough to base a conclusion upon?—The statistics are absolutely correct; there is no doubt whatever as to that, and there are explanations.

25,682. What is your explanation?—Let us take, for instance, the mortality in the House of Correction. The mortality there according to the population was really higher than occurred in some of the most crowded chawls of Bombay. It is very difficult to give an explanation of that, but there is the fact. The only explanation I can give is that it is a place for prisoners, and in order to keep prisoners you must build a wall, and the wall round the prison and the walls inside it may possibly obstruct the perflation of air and give less perflation of air than exists in the most crowded chawls of the town. That is the only explanation I can give, and it is not a very satisfactory one.

25,683. (*The President.*) How long does a prisoner remain in the House of Correction?—It is a short term prison.

25,684. Where do they usually come from?—From Bombay itself.

25,685. From the better or worse localities?—From the worse localities.

25,686. Therefore there is a constant stream passing through the House of Correction of persons from the worst localities?—Yes.

25,687. Are not these prisoners likely to have come in contact with plague?—One would think so, but we have the fact that, except for the small outbreak that occurred in the House of Correction, the place has suffered very little from plague. What occurs to me is this, that if the continual stream of people from the poorer classes into the House of Correction was the cause of the outbreak in 1897, it would be a constant cause in operation since.

25,688. You mean that there was a local outbreak, and not that the House of Correction was a place where plague had been especially prevalent?—That is so.

25,689. (*Dr. Ruffer.*) Has there been any plague in the Jail since 1897?—I am not quite sure till I inquire: there have been, I think, three cases in 1898 and none in 1899.

25,690. (*The President.*) What kind of houses are these suburban houses where plague has been prevalent?—They are the old-fashioned houses which have a number of inner rooms, not communicating directly with the external air, and very ill-ventilated.

25,691. In which of these rooms do the people live and sleep generally?—In the inner rooms.

25,692. Where there is very bad ventilation and darkness?—Yes, and the ground is damp.

25,693. Further than that, is it at all probable that when plague is bad in the town there might be an exodus to these suburban houses in any large extent?—Yes, there is an exodus to them, but against that there is also an exodus from the suburbs into the town. You have at one period a migration into the suburbs and, at another, a migration from the suburbs.

25,694. During plague?—Yes.

25,695. When do they come from the suburbs into the town?—When the disease becomes prevalent in the suburbs.

25,696. And already it has become prevalent?—Yes.

25,697. Before the time it became prevalent there might have been an exodus from the city to the suburbs?—Yes.

25,698. And, therefore, a condition of special overcrowding at the time when in the suburbs plague was most prevalent?—Overcrowding in this way, that you

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have a certain increase of the house population and also a larger number of people living on the open ground in camps.

25,699. But, above all, although these are suburban dwellings, they are not free from the defects which are commonly found in the centre of a city with regard to lighting and ventilation of the inhabited parts of the houses?—That is so.

25,700. (*Mr. Hewett.*) Have certain portions of the streets in Bombay in recent years been rebuilt?—Yes, they have been widened.

25,701. Has any alteration been made in the houses in those streets?—A good many alterations have been made in the houses.

25,702. In the particular streets to which I am referring?—Yes, to improve light and ventilation.

25,703. Can you specify a particular street in which it has been done?—Yes, Girgaum Road.

25,704. Any other?—Yes. Abd-ul-Rahman Street.

25,705. Has anything been done in Arthur Road?—No; that is part of a new street.

25,706. It has not been widened, but it is a comparatively new street?—Yes. The portion between Chinch Pooghly Station and Parel Road has been built upon within the last ten years.

25,707. And Cotton Road?—Cotton Road is one of the new streets.

25,708. In these particular places are the lighting and ventilation and the perflation of air through the houses better than in other parts of Bombay?—Undoubtedly. Take, for instance, a new road, a road constructed in 1885, Ripon Road, and also Morland Road, immediately east of the Byculla Club. They are new streets with new houses.

25,709. Is the lighting and ventilation better there?—Undoubtedly, very much better. There are a number of chawls in these streets very fairly ventilated indeed.

25,710. Have you any statistics to show the ratio of attacks from plague and deaths from plague in these new streets, compared to the ratio of attacks and deaths in the other portions of the town?—We have the statistics, but we could not give the ratio.

25,711. Do you know the population of these particular streets?—Not exactly.

25,712. Not approximately?—With the continual changing of the population of Bombay and the construction of new buildings, it is extremely difficult to get it even approximately, and it is most uncertain. What we can do is to give the actual number of cases and attacks and the population of the last census. To do more it would be absolutely necessary to take a census.

25,713. Was plague bad or not in these particular streets to which you have just referred?—Plague undoubtedly was bad in one street, and in chawls off or close behind another—Morland Road. I am convinced that it would have been very much worse if the streets and houses had not been as well-ventilated as they are.

25,714. Was plague worse in Morland Road than it was in the badly overcrowded portions of the town?—No; I would not say it was worse, but it was very bad. The chawls were as much affected as in the most densely populated portions of the city.

25,715. I want to know whether the attacks and mortality from plague in Morland Road were as bad or worse than in the worst portions of Bombay?—I can only answer in a very general way. The figures are here. I think they were just as bad, although Morland Road has hundreds of yards of open ground on one side.

25,716. What is the average area per person in the suburbs?—It varies. You have from an average of 832·42 square yards for each person in Sion to 7·22 square yards to each inhabitant in Khara-Talao section.

25,717. You say that during the course of a plague epidemic there is an exodus from Bombay to the suburbs, and also at times a corresponding exodus from the suburbs to Bombay?—Yes, a migration backwards and forwards.

25,718. Does the population of the suburbs become as congested or more congested than the population of the town at any period in the epidemic?—Never; it never becomes so congested.

25,719. Can you give us the total mortality per month from the 23rd September 1896 to the end of February 1899?—Yes, it is as follows:—

TOTAL MORTALITY during the following YEARS.

1896.				1897.											
September.	October.	November.	December.	January.	February.	March.	April.	May.	June.	July.	August.	September.	October.	November.	December.
925	2,994	3,024	6,244	7,627	6,928	5,419	3,620	2,433	2,326	3,112	4,076	3,616	3,389	2,984	3,736

(*continued*).

1898.												1899.		
January.	February.	March.	April.	May.	June.	July.	August.	September.	October.	November.	December.	January.	February.	March 1st to 16th.
6,802	8,283	9,303	5,185	3,021	2,106	2,435	3,264	3,618	3,664	2,646	3,311	5,796	7,576	5,426

25,720. Will you give us the normal mortality for the same period?—I will prepare that table. (The following figures were subsquently supplied by the witness):—

STATEMENT showing MONTHLY AVERAGE MORTALITY for 5 years from 1891 to 1895.

January.	February.	March.	April.	May.	June.	July.	August.	September	October.	November	December.
2,150	2,132	2,322	2,480	2,480	2,078	2,246	2,423	2,188	2,099	1,938	2,093

25,721. Will you give us the total recorded plague mortality for that period?—Yes, it is as follows:—

STATEMENT showing the NUMBER of PLAGUE DEATHS in the following years.

1896.				1897.											
September.	October.	November.	December.	January.	February.	March.	April.	May.	June.	July.	August.	September.	October.	November.	December.
79	313	273	1,271	1,857	3,083	2,266	1,316	328	99	42	70	168	178	255	644

(*continued*).

1898.												1899.		
January.	February.	March.	April.	May.	June.	July.	August.	September.	October.	November.	December.	January.	February.	March 1st to 16th.
2,934	4,498	4,973	2,171	524	150	263	543	731	728	217	470	1,591	3,040	2,452

25,722. What is the average normal mortality?—Our average mortality is about 28 per thousand.

25,723. Can you tell us to what extent the Health Department has been added to recently?—Yes; I have prepared a table showing that. It is as follows:—

RETURN of STAFF employed in the HEALTH DEPARTMENT up to the end of SEPTEMBER 1896, and that employed up to the end of FEBRUARY 1899.

—	Divisional Health Officers.	Deputy Health Officers.	Inspectors.	European Sub-Inspectors.	Native Sub-Inspectors.	Ward Clerks.	Muccadums.	Patels.	Cart Drivers.	Bigarees Men and Women.	Halalkhores.				Gully Flushing Establishment.					Special Medical Officer on Plague Duty.	Hospital Assistants.	Rat Killers.	Report Hospital Bearers.
											Sub-Inspectors.	Cart Drivers.	Trip-Markers.	Muccadums.	Halalkhores Men and Women.	Engine Drivers.	Firemen.	Sub-Inspectors.	Muccadums.	Bigarees Men and Women.				
Permanent establishment up to 30th September 1896.	—	—	7	7	44	7	57	22	639	1,950	2	127	—	47	1,646	—	—	—	—	—	—	—	—	—
Permanent establishment up to 28th February 1899.	4	7	8*	5	36	7	44	29	640	1,908	2	139	—	55	1,865	7	7	6	7	154	—	—	—	—
Extra establishment on account of plague up to 28th February 1899.	—	—	—	18	53	8	68	—	—	555	—	—	13	49	152	—	—	—	—	—	31	5	8	6

* One acts as Inspector.

The Tardeo Flats, Night Branch, and Veterinary Establishments are not shown in this statement.

25,724. In ordinary circumstances have you any Assistant Health Officers here in Bombay?—Previously to 1896 I had no medical Assistant Health Officers.

25,725. Under ordinary circumstances the Health Officer would have to do the whole work himself?—Yes, except during epidemics, when a medical staff was engaged.

25,726. Can he do the work at ordinary times, and so protect the town against epidemics?—I do not think he can now without a large medical staff.

25,727. Your establishment has been considerably increased?—Very much increased.

25,728. How many Medical Officers have you under you in the Health Department?—Eleven for Health Department duties.

25,729. Is that more than you want?—No. The total number of Medical Officers absolutely now employed on plague duty and Health Department duty is 42.

25,730. How many of those 42 are Europeans?—Five. There are a number of Eurasians. There are seven medical men who are Eurasians, and they are all qualified.

25,731. Can you give the total number of persons who have been sent to public and private hospitals since the beginning of the epidemic, supposed to be suffering from plague?—Yes, as follows:—

STATEMENT showing the NUMBER of PLAGUE PATIENTS admitted into all PLAGUE HOSPITALS, PUBLIC and PRIVATE, from March 1897 to March 1899.

—	No. of Plague Patients.
From March to June 1897:—	
Public hospitals	1,531
Private hospitals	1,244
From July 1897 to April 1898:—	
Public hospitals	3,957
Private hospitals	4,157
From October 1898 to March 1899:—	
Public and private	5,818
	16,707
	184
	16,891

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25,732. Can you give us any figures for any particular hospital or hospitals to show what proportion of the persons supposed to be suffering from plague when they were sent to hospital were afterwards found not to be suffering from plague?—Yes. About 66 of the patients admitted in the Mahratta Hospital turned out last year to be cases of simple or relapsing fever. About 56 of the patients turned out to be suffering from diarrhœa, relapsing fever, or some other cause, from January 1899 up to 14th March 1899. About 10 per cent. of the patients sent to the Arthur Road Hospital do not suffer from plague but from some other diseases. From the 1st of August 1898 to the 15th of March 1899, 2,021 of the patients admitted in the Modikhana Hospital were not suffering from plague.

25,733. Were those people who were subsequently found not to be suffering from plague sent to the Arthur Road Hospital, by Medical Officers or by other people?—That I could not say, they may or they may not have been.

25,734. Can you give us any information as to the proportion of persons sent (1) to public hospitals, and (2) to private hospitals, who died within 24 hours of admission?—Yes. At Arthur Road Hospital about 38 per cent. died within 24 hours of admission.

25,735. You are probably familiar with Section 449 of the Bombay Act of 1888 regarding the registration of deaths, which places an obligation on certain persons to report death at or about the time the corpse of the person is disposed of?—Yes.

25,736. Have any subsidiary rules been issued on this matter?—No.

25,737. Do people habitually report deaths, and their cause?—At the cemeteries.

25,738. That is how that Section is interpreted?—Yes.

25,739. The report you get is the report from the cemetery?—Yes; there may be a certificate from a medical man.

25,740. For the purposes of this rule, if it is reported at the cemetery, that is taken to be a compliance with the law?—Yes. I have in my last Annual Report on the Health of Bombay, 1897, discussed the measures that should be taken for the improvement of the system of registration of deaths. I have described the steps to be taken towards correct registration. The importance of improvement is very great. The law must be altered, medical men must be registered, and medical relief given to the poor, who must be treated while living or examined after death, the latter a very serious alternative in this city not lightly to be recommended. The same object can be secured in a different way by treating every death as a death due to an infectious disorder and disinfecting the bedding.

25,741. (*Mr. Cumine.*) With regard to the drainage of Bombay, in what parts of the city do drains exist?—Roughly speaking up to Clark Road, a line from east to west through Clark Road. More than a quarter of the island is without drains.

25,742. On which side are there drains?—On the south side of Clark Road

25,743. Are the houses connected with those drains?—The houses are all connected, but only a certain proportion of the houses have house-connections on the new system.

25,744. Are the connections in working order?—Yes.

25,745. Did not Mr. Baldwin Latham, the famous expert, come to Bombay to advise the Municipality on drainage?—Yes.

25,746. Did they accept his advice?—The advice was accepted, but his proposals have not all been carried out yet.

25,747. On what system did he advise Bombay should be drained?—He practically approved of the present system except with regard to the outfall: he suggested another outfall

25,748. Is Kolaba drained, and the Fort?—Yes.

25,749. Chowpati?—Yes.

25,750. Malabar Hill?—No, Malabar Hill has open storm water drains.

25,751. Is the drainage of the rest of the town proceeding at present?—Yes; a great deal of work has been done since 1896.

25,752. Has it all been done on the same system, or has the Shone system been introduced anywhere?—It is being introduced into certain districts in which the levels will not permit of gravitation into the general system.

25,753. Now, with regard to rain-water?—That is separated from the sewage. The removal of rain-water is arranged for in a separate system of drains.

25,754. Have the houses got closets?—There are only a few hundred closets in Bombay. In the drained districts the solid matter is received in baskets or in pans at the bottom of the privies, and removed by Halalkhores, and the fluid portion flows into the drainage system. The Halalkhores remove the solid matter in baskets to depôts. There are a number of depôts which are the points on which the Halalkhores work. Each depôt is an unit of organisation. All the depôts in the drained districts are connected with the drainage system. The night-soil in the baskets is emptied into hoppers in the depôts and discharged through the sewage system.

25,755. And eventually the sewage goes to the sea?—Yes.

25,756. How does it reach the sea? Is it pumped up? Yes.

25,757. (*The President.*) Do you mean the solids as well as the liquids?—Yes.

25,758. The whole?—Yes. The difference in the drained districts is simply this, that the night-soil, instead of flowing direct from the water-closet to the sewer, is carried in baskets from the privy to the depôt, and discharged into the sewers.

25,759. I suppose in these closets there is a tube connected with the drain for liquid matters?—Yes. A connection.

25,760. There is a percolation from the baskets?—Yes; the bottom of the basket is porous. In the houses which have been connected on the new system the liquids flow into a pipe, and the pipes are connected with the street sewers. The districts where house connections on the new system have not been carried out, the liquid matter flows from an open gati into the street sewers. In districts which have not drains the solid matter is removed to depôts, and the liquid matter is received in cesspools.

25,761. (*Mr. Cumine.*) What is the old system and what is the new system?—The old system is a system of open galis or drains; the new system is a system of pipe connections, with intermediate portions of properly constructed open drains.

25,762. Is the new system being introduced universally now in supersession of the old system?—Yes.

25,763. What is to prevent the solid matter from going into the drains direct?—The receptacles.

25,764. To the north of Clark Road I understand there are no drains?—Practically no drains.

25,765. What is the system there of removing the night soil?—The same as in the town. It is removed to the depôts, and from there it is carried back to the nearest district where there is a sewer, and emptied in the sewer.

25,766. And the liquid matter?—That is received in cesspools, and a small portion of the cesspool matter is also removed to the depôts and discharged into the sewer. The remainder either soaks into the ground, or is spread over the ground, or is used for irrigating the ground.

25,767. Have you noticed that the existence or the non-existence of drains has any marked effect upon the health of the people?—I have no doubt whatever that the existence of the drains has a most favourable effect. Considering the density of our population and the miserable economic conditions under which they live, I think the relative low mortality is due in a great measure to the drainage that we have. The mortality from plague has been made lower in Bombay and Madras than in other cities in India.

25,768. How is the drainage system better for the health of the people than the system of removing by hand which prevails to the north of Clark Road?—The system which prevails to the north of Clark Road is only possible in very sparsely populated districts. For that system you have ground for a cesspool, and you must also have ground on to which a portion of the matter can flow.

25,769. If the drainage system has a good effect upon the health of the people, I presume the people who live

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n the north of Clark Road have not such good ealth as the people who live on the south of it?—That is not so always in all districts, except since plague came.

25,770. On what do you base your opinion that the rains are good for the people?—On statistics, and ecause the filth is removed from the neighbourhood f the dwellings. Where there is no system of drainage he filth accumulates and percolates into the ground.

25,771. That is a theoretical reason?—Absolutely so. have no doubt as to its truth.

25,772. Is it within your knowledge that the health f the people in any portion of Bombay south of Clark Road has improved since the drainage system has been introduced?—Yes. In certain districts the health has improved undoubtedly.

25,773. Which are they?—The Market, for instance, nd the Fort, Khetendi, and others. The districts hat have been drained have shown a progressive mprovement in health.

25,774. Why are solids in the drained portion of Bombay removed by hand in this way? Why are they ot allowed to pass straight into the sewer?—On ccount of the difficulty of getting the people to use ater closets, and the danger of having water closets.

25,775. What is the danger?—The danger is that hey would be improperly used and become obstructed.

25,776. (*Prof. Wright.*) Are the present pipes obtructed?—Obstructions do occur, but they are nfrequent.

25,777. (*Mr. Cumine.*) Is the basket movable?—Yes.

25,778. Is it found that the people remove baskets nd introduce something into the pipe which obstructs t?—Into the basket, or it may be over the basket, ieces of cloth are thrown, also stones, pieces of stick, and pieces of earth. A certain proportion of the population use balls of earth after ablution. The women throw down the privy the cloths which they use in a certain portion of their life, and if those cloths happen to fall into the gali trap or pipe they may cause an obstruction.

25,779. (*Prof. Wright.*) Can you give us the comparative number of attacks in segregation camps as compared to those among the rest of the population?—I have sent that in.

25,780. You have not given us the number of people in the segregation camps, and therefore we are not able to work out the percentage of attacks?—I am afraid it would be rather difficult to get the number; there is no record for some of the camps in 1898.

25,781. Will you see whether you are able to obtain that information?—Yes. For some it can be given and in some it cannot. (Witness supplied certain figures, but they were incomplete, and he was unable to supply such figures as were required.)

25,782. (*The President.*) I suppose your drainage system in Bombay is not yet completed?—That is so.

25,783. Therefore you are not in a position to accurately estimate the advantage which you will derive from a complete system?—We are not yet able to fully edstimate the advantages of the system.

25,784. With regard to Morland Road, are there any special conditions and circumstances in the houses there which are favourable to the prevalence of plague?—Yes; there are a very large number of poor people and outcasts living in very crowded rooms, and rooms which they crowd with screens and purdas.

25,785. Were the conditions in Morland Bay exactly the same as in the most crowded buildings in the towns?—Yes; they are exactly the same inside the rooms only, but not outside.

(Witness withdrew.)

Lieut. H. V. Firth, I.S.C., called and examined.

Lieut. H. V. Firth, I.S.C.

25,786. (*Mr. Cumine.*) Your first experience of plague measures was, I think, as officer in charge of the railway detention camp at Bhusawal, in the early part of last year?—Yes.

25,787. In what month did you come to Bombay?—In June.

25,788. What ward were you placed in charge of?—E Ward East.

25,789. What parts of the town did that comprise?—Mazagaon, Tarwadi, and Byculla.

25,790. With regard to disinfectants, have you any reason to disbelieve in the efficacy of a single application of perchloride of mercury?—Yes; I did not see it was of much use. You empty the chawls, disinfect once, and put the people back again after a fortnight or a month, or any length of time, and as soon as you put them back again they get plague just as badly.

25,791. Therefore, what did you do?—I disinfected the room in which a plague case had occurred every day for a week with perchloride.

25,792. Having done this, did you find that the people on re-admission again got plague?—Not in the rooms that had been disinfected for a week, but in the other rooms. I believe in disinfection once if you get it in the early stages. For instance, if you find dead rats, and disinfect at once, that will stop plague; but if plague once gets a hold on a chawl, once disinfection is no good at all.

25,793. When you came to your ward in June 1898, was it badly infected?—No, there were only cases here and there.

25,794. As it gradually became infected to what did you attribute the infection?—At first, people with plague came from other wards, nine-tenths of the cases. When the plague staff got energetic in other wards they came into my section. Also, I think rats spread the disease. I do not know whether the rats got it in the houses or brought it in, but when we were taking the tiles off, we found rats everywhere, all over Mazagaon and Byculla.

25,795. Did the death of the rats appear to precede plague in human beings?—Yes.

25,796. With regard to the removal of patients to hospital, I think that soon after your arrival in Bombay you allowed at first a small number, and latterly a considerable number, of patients to remain in their rooms?—Yes.

25,797. Did you always leave them in their rooms?—No; sometimes we left them in their rooms and sometimes we put them in huts in the compound.

25,798. When you left the patients in the rooms, what did you aim at specially in order to prevent the contacts from getting plague from them?—We used to mop the floors and walls with a strong solution of perchloride, and then we used lime on the floors to keep them dry. We used to take the patient into the passage while we did this. We did not use the perchloride every day, but we used lime every day, and we gave the patients pans for spitting into and for droppings.

25,799. Did you allow the patients to sleep upon the floor?—No; we gave them a cot and blankets, or whatever they wanted. We used to burn all the infected clothes first of all, and then give them saris or dhotis and whatever was necessary.

25,800. Did you leave the family of the patient?—Yes.

25,801. In cases where the patient died, what did you do after the death?—We disinfected the rooms and sent the contacts to a contact camp. If the patient did not die, we left the contacts there and disinfected the room, and allowed them to go on living there as if nothing had happened.

25,802. In, approximately, how many cases did you leave the patient in the room or in a hut in the compound?—I have a record of about 50, but I think it must be over 60.

25,803. So far as you can see, did the contacts catch the plague?—Not so far as I could see. If you did not disinfect the room the contacts would get plague, not from each other, but from the poison in the room.

25,804. Where you did disinfect the room?—Not a single contact took plague.

25,805. After a time had you to give up this system of leaving patients in their rooms and send them to

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hospital?—Yes; they were all sent to hospital at the beginning of December, I think.

25,806. Under what system was there was most concealment?—When we sent them to hospital. The more you worry them the more they conceal cases.

25,807. Have you lately reverted to the system of leaving the patients in their houses?—Yes, without disinfection.

25,808. How long is it since you reverted to this system?—I suppose about three weeks or a month.

25,809. You do not disinfect now?—No.

25,810. What is the reason of that?—Because the disease is of such a virulent type. It is no good doing so. They are generally dead by the next day.

25,811. Is the type a little milder now?—Yes, during the last week it has become so. Some patients have lived for three or four days.

25,812. Do you now, in any cases, remove people to the hospital against their will?—At present, we only send those who are willing to go, but we try to persuade them all to go.

25,813. Do you find a certain number of people are willing to go?—Yes, some are willing to go. I find when I go on my rounds that many have gone to hospital. On the first day, when the patient is not delirious, they will not send him to hospital, but on the second or third day they want him to go. So long as the patients have only simple fever, they like to treat the case as such.

25,814. Do you mean the patients themselves are willing to go to hospital?—No; I mean the friends desire to send them.

25,815. Under the system of no one being sent to hospital, unless his friends desire that he should go, do you have much concealment of cases?—I think I find nearly every case now, unless there is sudden death. I can only do it privately. If I leave one case in a chawl then they show me the other cases. But if I did not leave that case they would conceal all the other cases.

25,816. Do you consider that in order to have any measure of success in Bombay, the confidence of the people must be acquired?—Yes, you ought to work with the people. The more you gain their confidence the more they will help you, and the better all round. Only about 30 per cent. of cases are found now whereas 70 per cent. die without your discovering them. The less stringent you are the more cases you find, you would probably find 50 or 60 per cent.

25,817. Do you send any people to contact camps?—Yes, if we can find them, but they generally bolt.

25,818. Who are the people you send to contact camps?—When a patient dies of plague, the rest of the people in the room know you will go round and send them to contact camp, and by the time you get there they have bolted. They hate going to camp.

25,819. Why do they hate going to camp?—Contacts naturally hate going to camp; they like to go into another room. They are probably in the next room, if you could only find them; but it would take too much time to hunt up all the contacts. One is told that they have gone up country, when they are probably in the same chawl.

25,820. When you send them on to the Government camp, do you keep them there as long as they choose to stay?—No, only 10 days.

25,821. Do you think it is possible that the feature they dislike in going to a Government camp is the being turned out again after 10 days?—Yes.

25,822. Supposing you thought it wise to turn the people out of a chawl, how would you, yourself, think it best to treat them? Would you send them to a Government contact camp?—No; they do not like going to camp, because they are only allowed to stay there 10 days. If they have to go there they would like to stay there for a few months at least.

25,823. Do they like you to allow them to put up their own huts?—Yes, and that does not cost Government any money. I spent 300 rupees a day at one time, but now I do not spend anything on evicts.

25,824. Would the people think it a hardship to have to put up their own huts?—No.

25,825. Would they require the Government to provide materials for them?—No, because they do not pay rent if you give them a piece of land. They would be grateful for any assistance, but it is not absolutely necessary.

(Witness withdrew.)

(Adjourned till Monday, 20th March.)

At The Secretariat, Bombay.

SIXTY-EIGHTH DAY.

Monday, 20th March, 1899.

PRESENT:

PROF. T. R. FRASER, M.D., LL.D., F.R.S. (*President*).

Mr. J. P. HEWETT.
Prof. A. E. WRIGHT, M.D.
Mr. A. CUMINE.
Dr. M. A. RUFFER.

Mr. C. J. HALLIFAX (*Secretary*).

Major W. L. Reade, R.A.M.C.

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Major W. L. READE, R.A.M.C., recalled and further examined.

25,826. (*The President.*) We shall have to trouble you again for some further information with regard to Poona?—Certainly.

25,827. We wish to learn what has occurred in Poona with reference to the plague since we had the pleasure of seeing you there?—The disease has now been

stamped out. 26 cases of plague have been imported into Poona since 22nd of February. The type of plague imported is of extreme virulence. As an example of its intensity, I may instance the following:—A family of nine persons came from Bombay on 1st March 1899, and were under surveillance. One of the family died on the 4th March. On inspection by a medical officer the cause of death was pronounced as due to plague. Three other members were also suffering from plague, and the remaining members also showing signs of illness were admitted into the General Plague Hospital, when they all died, the last on the 10th instant. The virulence of the disease is also shown by the proportion of deaths to cases. To the 18th of March 290 cases have occurred, and of these 228 have already died; of the 26 imported cases of plague 24 have died. This importation of the disease is due to the large number of people who have come from Bombay, reaching Poona by rail and road. This influx from Bombay has been reported to me by the Plague Superintendent of the Station, who stated that two-thirds of the people from Bombay alighted at Poona. At Talegaon, a station 20 miles from Poona, where all Bombay trains stop, it has been reported to me by the Hospital Assistant on surveillance duty that a considerably larger number of people than usual have recently been alighting daily. The normal is about 200 a day, and latterly they have been arriving at the rate of 280 a day. There is little doubt that a large number of people come into Poona by road from this station in order to escape the restriction which they think will be imposed in Poona. On a house becoming infected, the inmates are sent to an observation camp where their clothes are disinfected and their names and thumb impressions are taken; these people are then allowed to carry on their ordinary avocations during the day and return at night to sleep. After four days' observation the names of the contacts are brought on the surveillance list of their Pet, where the remaining period of surveillance is undergone. Among the contacts 15 developed plague and nine died out of a total of 2,013 under observation, all these cases being within the incubation period of the disease. This is shown in the following statement:—

No.	Admitted Segregation.	Came from	Hospital.	Result.
1	14 February -	Budhwar, 348 -	18·2·99	Died.
2	23 „ -	Sadashiv, 649 -	—	Died in town 23·2·99
3	24 „ -	Vetal, 547 -	24·2·99	Died.
4	14 „ -	Budhwar, 348 -	20·2·99	Died.
5	24 „ -	Shukrawar, 1,500	27·2 99	Alive.
6	28 „ -	Kasba, 990 -	4·3·99	Alive.
7	28 „ -	Raviwar, 801 -	1·3 99	Died.
8	2 March -	Gunj, 763 -	6·3·99	Died.
9	5 „ -	Raviwar, 372 -	7·3·99	Alive.
10	5 „ -	„ -	8·3·99	Died.
11	5 „ -	„ -	9·3·99	Died.
12	11 „ -	Shukrawar, 1,590	13·3·99	Alive.
13	11 „ -	Sadashiv, 682 -	13·3·99	Died.
14	11 „ -	„ -	14·3·99	Alive.
15	9 „ -	„ -	16·3·99	Alive.

15 contact cases of plague, of whom nine have died.

Under the present system of house census a larger number of contacts in proportion to the number of cases were kept under observation, that is to say, the average number observed to each case of plague is now seven, whereas in the last epidemic it was three. The ratio of attacks amongst the contacts is now one in 134; in the previous epidemic it was one in 98. In no instance have we been able to trace any infection due to a want in the disinfecting process, or to the short period which elapses between the time of removal and the return of the inmates. Only in two cases were patients allowed to remain for treatment in their own houses; the result was so disastrous that the trial was given up. I am of opinion that unless a private house can be placed under the conditions of a plague hospital the treatment of cases in such houses should not be attempted.

25,828. Can you give us details about the two cases you refer to?—I have the history of two cases which were treated in their own houses, which I will read to you. "On the 20th of February 1899 a boy from house "No. 20, Shukrawar, went to house No. 284, Sadashiv, "and died there of plague. The next day, on 21st "idem, a woman in house No. 20, Shukrawar, was "attacked with plague and was allowed to be treated "in the house which was disinfected on the next day. On "the 24th the brother-in-law of the woman got ill and "was removed to the Sadashiv Pet Hospital, where he "died of plague on 26th February 1899." I should like to say with regard to this first one that it is not a desirable house, nor one which one would have sanctioned in the ordinary way. They said the woman was moribund. It is not a house one would deliberately have chosen.

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25,829. In this particular case you left the patient there because she was moribund, and not because the house was a particularly good one to try experiments with?—Quite so. "At house No. 78, Shukrawar, on "the 13th of February 1899, one case of plague occurred "in the house. On 19th February a boy in the same "house got plague, and was allowed to be treated in the "house, which was disinfected the next day. The father "of the boy died of plague in the house on 24th February. "On the 27th the brother of the patient caught plague, "and was sent to the hospital, where he died. The "patient died on 1st March 1899. Next day the wife "of the boy's brother got plague and died the same day "in the Sadashiv Pet Hospital." My conclusions with regard to treating patients in their own houses are as follows:—I think that, unless it is possible to assimilate a private house to the condition of a Plague Hospital—it ought to be detached, or more or less detached—I do not think it is possible to treat any large number of people in their own houses. These two houses caused me a great deal of anxiety. One had to tell off a soldier to constantly disinfect the floors. I admit that the floors were properly disinfected, but one could not daily look after the infected clothing, and the hundred and one things which have to be done in a Plague Hospital. Then there is the question of a guard. As it is extremely difficult to place a house under the same condition as a hospital, personally, from the little I have seen, I do not think I should be inclined to recommend the system. In fact, in Poona we have practically done away with it, because it is impracticable.

25,830. Treatment in houses under any circumstances?—If you have a bungalow in a cantonment surrounded by a good compound, and there are reasons for it, say, in case of a native gentleman or a native noble of high rank, where the question of expense is a bagatelle, I think it is possible to do it, but I am sure you could not do it efficiently in the case of a large number of houses in a crowded town.

25,831. You say that the system is applicable only in those cases where there is a large compound and other conveniences, and therefore in a town, where some of the houses are better than the others, you think it would be inappropriate even for the better houses?—I should be inclined to lay down that the house must either be detached or semi-detached. I do not think that in a long row of houses it would be possible. Then there is the question of rat infection. If you have an infected house in the midst of a crowded quarter, you get infection carried by rats from that house to others. Even if you bring the house under the same conditions as a Plague Hospital, in the midst of a crowded town there are more rats than you would have in a hospital, in a more or less isolated place. We have evidence now with regard to rat infection, and it shows that within a small radius there is undoubted infection by rats, though a very localised one. Rat infection has not played a large part in the recent outbreak in Poona. As a source of infection to man it is one I have had little knowledge of up to the present date, but from our recent experience in Poona I incline to believe that the influence of this infection has been much exaggerated. In house No. 5, Shukrawar, three plague cases occurred, and on disinfection of the house, which was a grain shop, 25 dead rats were discovered in a heap on the floor of one of the rooms. A row of small houses adjoining this shop were also disinfected and no further cases occurred. On March 1st a family of nine persons came from Bombay and took up their residence at house No. 1622, Shukrawar, and were placed under surveillance. On the third day a child died, and in the same room eight other persons were found ill, all of whom were sent

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to the General Plague Hospital. The infection spread to house No. 1621, where four cases of plague occurred, three of whom died. In house No. 1623, 18 dead rats were discovered in a room on the ground floor. The house was disinfected. The inmates of this house, on discovery of the dead rats about 8th March, left the house, with the exception of the head of the family, my head clerk, who remained. He developed plague on the 17th and died on the 19th. No more cases of plague occurred in the neighbourhood of these houses. These instances are cited, as they go to show that infection by rats would appear to be of a local character.

25,832. Is that all you wish to put before us?—That is all.

25,833. (*Mr. Hewett.*) Could you separate the figures, so as to show the total reported cases of plague and deaths, and the total mortality, and the average mortality, from the time that we went to Poona up to now?—I have a statement of the actual total mortality from the 14th of February, which was the date of the outbreak, to the 17th of March, and also one showing the mortality for the same period in the years 1891 to 1895. They are as follows:—

MORTALITY.

STATEMENT of FIGURES of NORMAL and ACTUAL TOTAL MORTALITY from 14th February to 17th of March, during the Years 1891 to 1895.

Date.	1891.	1892.	1893.	1894.	1895.	Total.	Average of 5 Years.	Total, 1899.
14 February	5	4	1	4	8	22	4⅖	18
15 „	7	7	4	9	1	28	5⅗	22
16 „	3	9	25	4	13	54	10⅘	16
17 „	7	7	3	11	8	36	7⅕	20
18 „	10	7	4	9	17	47	9⅖	15
19 „	9	6	7	4	6	32	6⅖	18
20 „	6	10	9	13	10	48	9⅗	26
21 „	6	8	6	2	5	27	5⅖	16
22 „	4	6	9	4	9	32	6⅖	22
23 „	4	6	6	3	7	26	5⅕	17
24 „	6	7	5	7	29	54	10⅘	20
25 „	5	8	8	13	51	85	17	28
26 „	4	3	12	4	10	33	6⅗	16
27 „	4	6	23	15	9	57	11⅖	13
28 „	6	6	15	21	9	57	11⅖	34
1 March	15	9	5	3	8	40	8	25
2 „	5	13	4	1	6	29	5⅘	24
3 „	8	9	—	6	—	23	4⅗	25
4 „	5	8	8	4	8	33	6⅗	26
5 „	4	5	8	4	13	34	6⅘	20
6 „	6	5	9	5	7	32	6⅖	23
7 „	8	9	15	13	4	49	9⅘	27
8 „	4	7	4	4	15	34	6⅘	25
9 „	7	10	7	8	3	35	7	20
10 „	3	11	9	11	—	34	6⅘	29
11 „	9	—	11	12	14	46	9⅕	28
12 „	5	5	6	11	15	42	8⅖	17
13 „	3	1	9	10	8	31	6⅕	24
14 „	6	10	12	1	7	36	7⅕	20
15 „	6	13	7	17	2	45	9	27
16 „	7	9	7	4	9	36	7⅕	20
17 „	3	3	16	3	—	25	5	33
Total	190	227	274	240	311	1,242	248⅖	714

25,834. The total mortality is 714, compared with an average of 248?—Yes.

25,835. What was the number of ascertained plague deaths?—There were 290 cases and 228 deaths, according to the following statement:—

STATEMENT of RECORDED ATTACKS and DEATHS from PLAGUE from 14th February 1899 to 17th March 1899.

Month and Date.	Poona City. Imported. Cases.	Imported. Deaths.	Imported. Whence imported.	Indigenous. Cases.	Indigenous. Deaths.	Total. Cases.	Total. Deaths.
14 February	—	—	—	1	1	1	1
15 „	—	—	—	1	1	1	1
16 „	—	—	—	—	—	—	—
17 „	—	—	—	4	2	4	2
18 „	—	—	—	—	—	—	—
19 „	—	—	—	13	4	13	4
20 „	—	—	—	4	6	4	6
21 „	—	—	—	2	2	2	2
22 „	—	—	—	2	2	2	2
23 „	1	1	Bhore	6	7	7	8
24 „	—	—	—	9	8	9	8
25 „	—	—	—	9	4	9	4
26 „	2	1	Bombay and Bhore.	6	4	8	5
27 „	—	—	—	10	1	10	1
28 „	2	2	Bombay	13	8	15	10
1 March	2	2	„	6	6	8	8
2 „	1	1	„	15	12	16	13
3 „	1	1	—	12	12	13	13
4 „	—	—	—	16	15	16	15
5 „	4	3	Bombay and Kothrud.	4	2	8	5
6 „	1	1	Bombay	5	8	6	9
7 „	—	2	—	5	6	5	8
8 „	3	3	Bhore and Bombay.	6	5	9	8
9 „	—	—	—	7	2	7	2
10 „	1	1	Induri	10	5	11	6
11 „	2	3	Kudji and Bombay.	11	11	13	14
12 „	1	—	Sholapur	7	10	8	10
13 „	1	—	Jintia	7	5	8	5
14 „	2	1	Bombay and suburban limits.	11	9	13	10
15 „	—	1	—	16	15	16	16
16 „	—	—	—	16	7	16	7
17 „	—	—	—	15	11	15	11
18 „	2	1	Bombay and Loni-Kalbhor.	15	13	17	14
Total	26	24	—	264	204	290	228

25,836. The number of ascertained deaths from plague amounts to half the excess mortality?—Yes.

25,837. (*The President.*) How do you account for the other deaths?—During January and February we have had an extremely virulent outbreak of measles and influenza—diseases, more particularly, which cause bronchial affections among children.

25,838. Has the mortality among them been excessively large?—There were 208 deaths among children under the age of five in January.

25,839. What was the ordinary average in other years?—Fifty-nine.

25,840. (*Mr. Cumine.*) As regards the two instances which you gave us of treating plague patients in houses, did you give them to us because they are the only two instances, or because they especially illustrate the danger of leaving patients in houses?—They were the only two cases that were treated so.

25,841. As a rule you remove all patients, except the moribund, to hospitals?—Yes.

25,842. Do you find that the knowledge of the fact that patients will be removed to hospital produces concealment of plague illnesses?—I do not think so.

when the case is known to be plague. Directly a case is diagnosed as plague the relatives are very anxious to get rid of it in Poona. If you go to a house and say it is a case of fever, they say, "Will you leave it until "definite symptoms have come on"? But if you say it is a case of plague you never have any difficulty. They are generally quite ready to have the person removed. We have generally explained to them that even if it is a case of fever it is very necessary that it should be under observation. Directly any definite symptoms develop there is never any further argument. That is the general rule.

25,843. Do you find that the people conceal the illness as long as possible, or do they report it as soon as it appears, even though it has not shown definite symptoms of plague?—Latterly they have done so. On the other hand, we have certainly had a great many corpses in the town; but I have attributed that a great deal to the extreme virulence of the disease. The ordinary history of an attack would be this: The man would be apparently pretty well in the morning; he is attacked in the afternoon; and is dead either the same evening or on the following morning. We have had very numerous instances of that kind.

25,844. In the two cases where you left the patients in the house, did you put them upon beds, or did they lie on the floor?—In both cases, which were sanctioned, as it were, officially, the patients were on the floor.

25,845. Did you give them any pans of disinfectants for the sputum and excreta?—Yes, we gave them everything we could in that way. I was extremely busy during that period. I told off a soldier to go there, and gave him directions what to do. Of course, I cannot answer for personal supervision. I can only say that the instructions I gave were carried out by the soldiers as far as possible. Even with this precaution the cases caused me great anxiety.

25,846. Were directions given to sprinkle disinfectants upon the floor daily?—I tried to take all the ordinary precautions that are taken in a Plague Hospital.

25,847. Do you find that plague reappears among the contacts on their return to sleep in their houses after 10 days?—I have not had a single case beyond the incubation period.

25,848. Before they are allowed to return, that is to say, in the course of these 10 days, has the room where the case occurred been disinfected only on one occasion or repeatedly?—Only on one occasion.

25,849. (*Prof. Wright.*) Of the 290 plague cases that have recently occurred, do you know how many were derived from plague existing in Poona when we were there, and how many were derived from freshly-imported cases?—To put it roughly, each imported case would do a certain amount of damage. We have the evidence I have given you of one child coming into a house.

25,850. When we were in Poona there was one focus of infection, which had been caused by an imported case; since then there have been a great many imported cases, and, of course, you have to deal with these new foci of infection. Can you find out how many of these 290 cases occurred in houses in which imported cases died—and how many have occurred in houses into which no imported cases can be traced?—It is not possible to get the information even approximately. The imported case on 14th February 1899 was the cause of 25 attacks before the disease was stamped out of the infected quarter.

25,851. Your excess mortality was 228 cases due to plague, and 238 deaths which were not due to plague?—Yes.

25,852. The latter 238 cases would labour under the suspicion of being plague cases?—Yes.

25,853. Could you rebut that by showing that very few plague cases originated in those houses in which the 238 deaths occurred which you do not attribute to plague? I want to find out whether there were any deaths from plague, or whether cases of plague subsequently occurred, in these houses which were not disinfected because the deaths that occurred in them were regarded as non-suspicious deaths?—I put in a statement, as follows:—

Pet.	Number of Houses in which suspicious Deaths occurred from 1st December 1898 to 28th February 1899.	Number of Houses in which occurred ordinary Deaths from 1st February 1899 to 16th March 1899.	Number of Houses in Column 2 in which Plague subsequently occurred.	Number of Houses in Column 3 in which Plague arises subsequently occurred.	Time which elapsed between the suspicious or ordinary death and the plague death.		
					Under 10 days.	Under 15 days.	Over 15 days.
1.	2.	3.	4.	5.	6.		
Gharpode -	1	13	—	—	—	—	—
Nyhal -	1	8	—	—	—	—	—
Raste -	1	16	—	—	—	—	—
Bhamburda	1	21	—	—	—	—	—
Naragon -	2	15	—	1	—	—	—
Nana -	7	41	—	2	—	1	1
Gunj -	9	38	—	1	1	—	—
Somwar -	1	24	—	—	—	—	—
Mangalwar	1	9	—	—	—	—	—
Budhwar -	8	34	1	2	—	—	2
Shukrawar	14	81	2	5	3	—	3
Shanwar -	6	53	—	—	—	—	—
Raviwar -	11	74	2	10	10	—	2
Ganesh -	5	30	—	—	—	—	—
Kasba -	13	95	—	—	—	—	—
Vetal -	5	24	—	1	—	—	1
Bhawani -	6	27	—	—	—	—	—
Sadashiv -	10	44	2	7	5	1	3
Musfer Ganj.	—	2	—	—	—	—	—
Total -	102	649	7	29	19	2	12

25,854. Are the contacts only kept under observation?—They are sent to camp, where their clothes are disinfected; then they are allowed back to their houses, but they must return to camp at night.

25,855. How soon are they allowed to go away entirely from the contact camp?—After three days they are put on the "contact surveillance register" of the Pet they live in. We make a person sleep for three nights in camp, and then a notice is sent to the Chief Volunteer of his Pet: "This man is transferred from "the observation camp; take him on the surveillance "of your Pet."

25,856. (*The President.*) Under what circumstances have you had to re-disinfect a house?—If one of the people during the period of observation developed plague, the house would, of course, be re-disinfected.

25,857. That is to say, a person who had access to one of these disinfected houses is sometimes found to develop plague, and in that case you have to disinfect the houses?—If a person develops plague after an original attack the house is again disinfected. A family, we will say of five people, are sent to the observation camp. Two days after observation one of them is found suffering from plague; he is sent to the Plague Hospital and his house is again disinfected, as he is one of the family of the same house.

25,858. Has that person been in that house which you are going to re-disinfect, or has he been in the camp all the time?—He has slept in the camp at night and in the day time been in the house, and after three days he is allowed to occupy the house day and night.

25,859. At what intervals after the houses are re-entered have attacks occurred?—Attacks varied from the first day up to the sixth.

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25,860. What has been the progress of the plague on the occasion of the third outbreak here? Is it extending rapidly or slowly? Would you have expected it to extend more rapidly, or has it extended in the same way as in the first and second epidemics?—I consider the feature of this outbreak is its very rapid spread, in fact it is quite different to the former outbreaks in Poona, and all outbreaks of plague of which I have had experience. The cause of this outbreak, an imported case from a Bhatia's family, infected 25 other cases in a few days' time, before it was possible to stamp the disease out of the infected quarter. One of the prominent features of this disease, *i.e.*, its slowness of progression, has not been found true in this outbreak. The second and also the present epidemic of plague in Karachi may be cited as evidence in corroboration of this statement. It would appear that the disease, having once got a favourable soil for its propagation, increases in virulence with every repeated outbreak. The recent outbreak here is the most virulent type of the disease that I have seen.

25,861. You have, however, the advantage of the circumstance, that you are now dealing with plague at the commencement of the hot season?—Yes.

25,862. Is it not a fact that at that time you generally have found that plague does not extend very rapidly?—At Karachi they had a virulent outbreak in very hot weather.

25,863. What has been your experience at Poona in the previous outbreak?—The 1896-7 outbreak was got under before the end of May.

25,864. That is to say it subsided in the hot season?—It commenced earlier than on the present occasion.

25,865. I believe you have had a great deal of voluntary co-operation in Poona?—Yes, a very large amount.

25,866. Is that more intelligently and more effectively being applied during this third epidemic than during the previous epidemic?—I think it is very much. All the Pets showing the best results and the best organisation are those in which voluntary agency is more largely represented.

25,867. Do you attribute some of the relative success in dealing with the epidemic on the present occasion to the greater efficiency of voluntary co-operation?—Certainly, I do.

25,868. How do you account for this voluntary co-operation being now more effective, efficient, and hearty than it had been on previous occasions?—I think it has only been developed towards the middle portion of the last epidemic; ever since then it has been kept up. In the first epidemic there were volunteers, but there was practically no voluntary organisation.

25,869. At the same time I suppose the people in Poona have more true information than they had before as to what your aims are, and what plague means?—Certainly they have.

25,870. And they have reason, therefore, more heartily to co-operate?—Yes.

25,871. They know that whatever you are doing is being done as far as possible for their good?—Yes.

25,872. They appreciate that?—I think they do.

25,873. I suppose while the epidemic is going on you cannot give any decided view as to the success of your methods?—I think they are extremely hopeful.

25,874. You appreciate the fact, however, that the epidemic is now decreasing?—I do not say that. Until it diminishes in Bombay, of course, we have to look out for a great many imported cases. I think we have been able to cope with our imported cases, and the amount of infection which has spread from them.

25,875. But your information has not been enough to enable you to judge absolutely?—No, I could not say that.

(Witness withdrew.)

Mr. S. R. Arthur, I.C.S.

Mr. S. R. Arthur, I.C.S., recalled and further examined.

25,876. (*The President.*) I believe you wish to give us some information in addition to that which you gave in your examination here on the 11th of February?—Yes, regarding inoculation in the Kolaba district, of which I am, at present, Collector.

25,877. (*Prof. Wright.*) Would you describe the circumstances under which the inoculations were done?—Yes. Inoculation was commenced at Alibag on 2nd February 1899.

25,878. What is the population of Alibag?—According to the last census, the population of Alibag is 5,888, but owing to the visitation of plague in two consecutive years I think it is probable that, even before the present attack, the population was less. The average population is now about 5,000, I should think. Since the inoculations commenced 1,385 persons had been inoculated, men, women and children, including old men up to 90 years of age, and infants two months old. The present attack commenced on the 24th December, and was heralded by a mortality among rats. As far as I have been able to ascertain, the disease was not imported, but started spontaneously, presumably from last year's germs which have meanwhile remained dormant. On the first cases occurring there was a considerable exodus of well-to-do people, including nearly all the non-official Municipal members. At the beginning of February 1898, I issued an order that no one uninoculated should sleep in the town at night, and that those who did so would be prosecuted if discovered. Latterly, the greater part of the population have been encamped in huts outside the town. When inoculation was first introduced the people were much afraid of it, and all sorts of rumours as to the evil effects of inoculation were in circulation. The results, however, have been so successful that I think this fear is now entirely removed. I will now lay before the Commission a few of the most striking instances of the immunity enjoyed by the inoculated as compared with the uninoculated. The first instance I can give is this: On 21st February there were in the Police Lines about 130 inoculated persons, and about 21 uninoculated. They were being gradually inoculated, but these 21 had remained till the end. One of these, Rakmi, wife of Gangnak, Head Constable, was attacked with plague on the above date and died on the 22nd. Another, Rama Shripatrao Sinda, was attacked on the 18th and died on the 24th—two cases. The remaining persons then uninoculated were inoculated within the two or three days following, and though 150 persons have since been living in the lines, and are living there now, there has only been one other case of plague there. This was Parbati, wife of Ragnak Rannak, who, having been inoculated a fortnight previously, was attacked with plague on the 8th of March. She had high fever one day, was better the second day, and on the third day she was almost well; and within a very few days was restored to perfect health. Beyond that there has not been another case in the Police Lines.

25,879. Are these police lines situate in an infected quarter?—Yes. It was the place first attacked. It was attacked last year also. The second instance I wish to bring to notice was also a striking one. A Koli family of seven members was living in the town, of which six members had been inoculated in February. The remaining one, Hasu, wife of Nathu, having been delivered of a child only two months ago was uninoculated. She caught the infection about the 8th instant, and died on the 14th in hospital. None of the other members of the family were affected, and the child was weaned from her, inoculated, and is now thriving. The third instance is also that of a Koli family of three members living together in the town. Of these, two were inoculated on the 9th and 17th February respectively. The third, a little girl of 12 years of age, who was to be inoculated later, was attacked with plague on the 1st instant, and died on the third. The other two members were not affected. The fourth instance is that of a family of goldsmiths living in the town. Govindshet was inoculated in February. His daughter. Rembai (uninoculated) was attacked on the 7th instant, and died on the 8th. His wife, Bhagirthi, was inoculated the same day, and has not been affected. The fifth instance is that of a Musalman family of eight; of these, three were inoculated in February.

Ismail, the son of one of them, eleven years of age uninoculated), left Alibag on the 2nd and showed symptoms of plague on the 4th. He died on the 14th. The remaining members of the family were inoculated on the 4th, and none of the seven members now remaining have been attacked.

25,880. (*Dr. Ruffer.*) Had they been inoculated before that first case or after?—Three were inoculated before, the remainder were inoculated on the case occurring. When inoculation was first introduced in Alibag, I did not allow the members of affected families to be inoculated, lest the plague should be incubating in them, and lest their death from plague should be wrongfully ascribed to the effect of inoculation, but latterly, inoculation having become more popular, I have revoked this order, and have allowed the families to be inoculated, and have then allowed them to go to their homes instead of being segregated. Sixty-seven persons, who would otherwise have been segregated as contacts have been set free, and there has not been a single case of plague amongst them. The experiment was, perhaps, a somewhat hazardous one, but it has, I think, been justified by the result.

25,881. (*Prof. Wright.*) Have you any figures to show how many cases you expected among these 67 contacts?—I could not say how many, but I should certainly have expected some cases in the ordinary course.

25,882. Is that an inference from what you have seen in other contact camps? — Yes. Generally some members of the family are attacked here and there; but the fact that among the 67 not one was attacked seems to be extraordinary, and I ascribe their immunity to inoculation.

25,883. In your case these contacts are left in the houses?—They get a certificate from me that they have been inoculated, and they are allowed to go where they like.

25,884. They are not removed from the source of infection into camp?—No. They were exactly where they were before. Among the whole of the inoculated population of Alibag there have up to date been five deaths. Of these, three have been certified to be due to natural causes, while two have been due to plague. Of the latter, both were attacked within eight days of inoculation. One, Natu Koli, was inoculated on the 10th, had high fever with perpetual vomiting for three days and died on the 13th, three days afterwards. He had no buboes, but as there were two deaths among the uninoculated near his hut, just before, it is almost certain that he died of plague contracted prior to inoculation. The other person who died of plague after inoculation was one Balaji Bandhari. He was inoculated on the 5th, attacked with plague on the 12th, and died on the 14th. Other cases occurred in the neighbourhood among the uninoculated, and it can be taken as probable that he had caught the infection prior to inoculation. The total number of attacks and deaths from plague in Alibag since the 24th of December is 71 and 61 respectively, and, if the period up to 31st January be excluded, for the remaining period they number 54 and 47 respectively. If the fact that I ordered the evacuation of Alibag by all persons not inoculated at the beginning of February—that subsequent to that a very small proportion remained, and that a few prosecutions reduced even that small proportion—be taken into consideration, it will be seen that the mortality among the uninoculated living in Alibag has reached a somewhat high percentage. And yet, at the present time, some 700 uninoculated people are living in the town with impunity. I think the above recorded facts testify strongly to the value of inoculation.

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25,885. You are not able definitely to get at the numbers of the uninoculated living in Alibag?—There cannot be more than a very few indeed. I went round the town myself at night about a month ago. I think we got 40 people then.

25,886. You think there may be a hundred or two?—I do not think there are anything like that. Four persons were prosecuted, and since then I think there are very few indeed—mostly those remaining are uninoculated members of families which are undergoing inoculation, and who will be inoculated afterwards. I should not think there are more than 20 people in Alibag now who are not inoculated.

25,887. (*Dr. Ruffer.*) Is plague going on now?—Yes.

25,888. Then plague must be going on amongst the inoculated, if they are all inoculated?—Cases are now occuring principally among people in the huts outside Alibag.

25,889. (*Prof. Wright.*) Now and then every day or two, you get a case amongst the uninoculated people remaining in Alibag?—Yes, up to the last few days.

25,890. Did you get very severe symptoms sometimes after inoculation which frightened the people?—No, mostly not. There was high fever often for one day. Generally, the fever subsided in 48 hours. Of course those inoculated had swollen arms for some little time. One man had very high fever and required some attention for three days. He has recovered completely. I am not aware of anyone having suffered from inoculation.

25,891. Have there been any cases in which there has been very little reaction?—Yes, I have heard of such cases: chiefly old men and little children who were given less doses.

(Witness withdrew.)

Lieut.-Colonel E. Lawrie, I.M.S., recalled and further examined.

Lieut.-Col. E. Lawrie, I.M.S.

25,892. (*The President.*) There are one or two matters on which we desire some further explanations from you?—Yes.

25,893. (*Dr. Ruffer.*) Do you wish to add anything to the evidence which you gave in Hyderabad—to the first proof of your evidence which was sent to you?—I have had a second proof sent to me, and I do not wish to add anything to that.

25,894. We examined Dr. Mullunnah with regard to a "Memorandum on experiments on rabbits with "Haffkine's fluid, performed by order of the Plague "Commissioner, Hyderabad."* I only want to ask you one question about that. In paragraph 6 of your statement you said, "The effects of inoculation of the "sterilised fluid in rabbits, and also, as far as our "limited experience goes, in man are simple. It "produces 'reactionary' fever alone without, as a "rule, any evil consequences. The question is, What "is this 'reactionary' fever due to? There can be "little doubt that it is due to plague." Will you tell us the reasons why it is due to plague?—I do not see what else it can be.

25,895. You say, further on, "One of us, Mullannah, "says it is due to the plague toxines"?—My opinion is that it produces a mild attack of plague. He preferred to put it that it was due to the plague toxines, and as he objected to put it my way, I put in his statement as well as my own.

25,896. What evidence have you got to show that it is plague?—It is not curative at all; there is no evidence except the effect that it acts as a prophylactic.

25,897. Supposing you take another disease—supposing you take diphtheria, for instance, and supposing you filter a culture of diphtheria, or sterilise a culture of diphtheria by carbolic acid, you get a certain amount of immunity, but you would not say you gave the animal diphtheria?—What else would the immunity arise from? You mean the products?

25,898. Yes?—But do not they necessarily produce the disease?

25,899. Not necessarily. It does not produce the disease. You would not say that animal had diphtheria, would you?—I should say if it was produced by the toxines, that it would be a modified form of the disease.

25,900. In the same way you say it is a modified form of plague?—Yes.

25,901. But you do not find the plague bacillus?—No.

25,902. So that this form of plague could not be contagious to other persons?—I should think not.

* *See* Appendix No. LXIX. in this Volume.

Lieut.-Col. E. Lawrie, I.M.S.

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25,903. I also want to ask about cotton. We have had it in evidence from Dr. Mullannah, that, in his opinion, he was not able to isolate the bacillus of plague from the cotton he examined. Are you still of the opinion that the bacillus that was isolated was the bacillus of plague?—Yes.

25,904. Why do you think so?—Because it had exactly the same appearance as all the other preparations of the plague bacilius that I have seen.

25,905. You have based your diagnosis on the microscopical examination alone?—I did not see anything else. I know that he was not able to propagate the bacillus afterwards, but I also know that in this cotton, in which he got the bacillus, there were numerous other organisms; and I thought it was quite likely on that account that it was not susceptible to propagation

25,906. He was unable to reproduce plague with this?—Yes.

25,907. So that the only evidence of plague you have got so far is the microscopical appearances of the culture?—And the death of the rabbit in which the plague bacillus was found.

25,908. In which that bacillus was found?—What I called the plague bacillus.

25,909. Do you think that is sufficient to make a diagnosis of plague?—It satisfied me completely at the time, but I reported the matter to the Commission for further investigation.

25,910. (*Mr. Hewett.*) When you were before us before, I asked you a question as to railway inspection, and I asked you to provide the figures from the 1st of October, showing the number of persons examined, and the number of persons detained, and the number of persons who developed plague in each case?—Yes.

25,911. Dr. Mullannah produced a statement (*see* Question No. 5,789) the other day—I do not know whether it was on your authority—in answer to that question?—I believe the number of passengers was omitted in that.

25,912. Will you supply us with that information?—Yes. It was subsequently intimated that all through passengers, as well as those who alighted at stations, were examined, but as no record of the number of through passengers is kept, the number of persons examined cannot be exactly stated. It is estimated that not less than 12,000 persons are examined during the month, making a total of 180,000 in a year and a quarter.

25,913. (*The President.*) I want some information from you with regard to the progress of plague now in the Hyderabad State?—We have got a hundred villages infected in the Naldrug, Gulburga, Lingsagur, and Kopbal divisions.

25,914. Is it increasing or diminishing at the present stage?—A lot more villages were infected in the last cold weather, but the villages that are infected we have now got well in hand, and it is not spreading.

25,915. What is the condition of plague in Hyderabad city itself?—There has been none in Hyderabad city.

25,916. How do you account for the non-appearance of plague in Hyderabad city?—We have had pretty strict detention on the railway, and the roads from the infected areas are so bad that infected people cannot travel into the city.

25,917. It is therefore very easy to isolate the city?—Yes, comparatively.

25,918. Of course, people are entering the city constantly from the outside, and I suppose from infected areas also?—They do, but if they come from infected areas they are nearly always detained at Gulburga or Raichur, and if they come into the city they are kept under observation for 10 days.

25,919. How long has plague been prevalent in the Hyderabad district?—Since December 1897.

25,920. So that these measures which you have adopted have been all this time successful in preventing indigenous plague in Hyderabad city?—Yes, they have so far.

25,921. Are these measures which you have just stated all the measures in force for protecting Hyderabad, or are there any others?—We have this cold weather, as we have had in the previous cold weather, patrols on all the roads; that is to say, every road leading from Hyderabad to the infected areas has been patrolled, and the villages on either side of the road have been searched throughout the whole area leading from Hyderabad to the infected frontiers.

25,922. You have practically a cordon round Hyderabad?—We have got that ready to put on, but it has not been put on yet, because there is no plague in the villages round Hyderabad between it and the infected areas.

25,923. How many are there on each patrol?—There are eight parties, each consisting of one Medical Officer, and one Commissioned Officer, and from 5 to 10 sepoys. The previous year we sent out parties of sawars—that is, cavalrymen—but we found that those men did not do the work so well as the infantry, so this year we have changed them to infantry.

25,924. Are they in charge of the medical men?—Each party is accompanied by a medical man.

25,925. Have they been successful in discovering plague cases?—No, they have not discovered a single case.

(Witness withdrew.)

Major W. B. Bannerman, I.M.S.

Major W. B. Bannerman, I.M.S., recalled and further examined.

25,926. (*Prof. Wright.*) I believe you have some facts to tell us with regard to the results of inoculations on the 26th Madras Infantry?—Yes.

25,927. Would you describe the condition of the regiment when you went down to inoculate it?—I arrived in Belgaum on the 23rd of December 1897, and the inoculations were begun next day. I found the whole of the regiment in camp round about Belgaum, some on one side of the town, and some on the other.

25,928. Why had they moved out into camp?—Because they had been so badly attacked by plague. Within the previous six weeks they had had 78 cases with 49 deaths, and on account of this they had gone out into camp—they had vacated their lines.

25,929. Had they been long in camp when you went there?—The whole of them were out by the 23th of November. I arrived about a month afterwards—the 23rd of December; so that they had been out in camp some weeks.

25,930. This large mortality that you spoke of occurred before they moved out into camp?—Not the whole of it. The first recognised case of plague in this regiment occurred on 12th November 1897, in the person of Private 2,224, Govindasami, who died the same day. Next day another sepoy was attacked, and also died. On the 15th a drummer was attacked; on the 17th the disease appeared among the followers, a bhisti of C Company succumbing on that day. By the 21st November 13 attacks had been reported from the lines, four of these being among the sepoys. On the 22nd, B Company went into camp, followed on the 23rd by C and G, and portions of A, D, and F Companies. By the 28th the whole regiment was in camp, and the lines were being disinfected. During this transition period 15 more attacks took place, six being among sepoys. In the 10 days following removal to camp 13 sepoys and 21 among families and followers were attacked. Then the cases fell off after that, and there were only one or two cases occurring in a week by the time I arrived.

25,931. The epidemic was practically over then before the inoculations began?—Yes.

25,932. Will you describe your inoculations. Did you inoculate everybody in the Regiment?—With the exception of some 81. I operated on 1,665 out of a total population of 1,746. Those were all operated on, and the Regiment then immediately went back to their lines, the lines in the meantime having been disinfected. After the inoculations were complete, that is, on the 6th of January, they had two attacks during the first epidemic in Belgaum, one in a sepoy, and one in an European Officer.

25,933. Were those among the inoculated, or the uninoculated?—They happened both to have been inoculated, but these were the only attacks after the inoculations were complete. These both took place in January.

Major W. B. Bannerman, I.M.S.

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25,934. These were two inoculated persons?—Yes. There were none among the uninoculated. There happened to be 81 uninoculated, but for the next six months there were no more cases among either group. From the figures that I got from Colonel Peters, Civil Surgeon of Belgaum, of the deaths in the city and cantonment, the epidemic seems to have been at its height in January, that is, in the month when they had only these two cases in the Regiment. Then the epidemic gradually died out, till, I believe, in May there were no cases at all. Then the second epidemic began in June. It became pretty bad in July, when there were 215 deaths among the civil population. In July there was only one death in the Regiment, a sepoy, who had been inoculated in December.

25,935. What is the population of Belgaum?—I am not quite certain, but I think about 38,000 or 40,000, if I remember rightly. Then, in August, there were two cases, and in September there were two cases.

25,936. All these among the inoculated?—The two who were attacked in August were both inoculated. In September two also were attacked, one inoculated, and one not inoculated. Then in October there were four cases.

25,937. Were these among the inoculated?—They were all inoculated, except one. Then in November there were two cases, one inoculated, and one not inoculated. In December there was one case of an inoculated man. These were all the cases that occurred in the Regiment during this second epidemic.

25,938. Do you know how many there are altogether from January onwards? There were three cases among the 81 uninoculated; is not that so?—Yes, three cases.

25,939. I want to know the total mortality in the Regiment, amongst inoculated and uninoculated, so as to compare the figures?—Amongst the inoculated there were 11 cases with three deaths; in the uninoculated there were three cases with three deaths, that is, 14 cases altogether. During the second epidemic there were 1,801 people inoculated.

25,940. You re-inoculated them?—They were re-inoculated at the end of August. The second inoculations began on the 18th of August, and were complete practically by the beginning of September. Between September and November various people were inoculated or re-inoculated as they came back from furlough, or from recruiting duty, or as families came in that had been away during the first inoculations. After the second inoculations there must have been very few inhabitants of the lines who had not been operated on. Less than the 81 left after the first inoculations.

25,941. Did you do these re-inoculations?—No.

25,942. Do you know why they were undertaken?—I believe because the sepoys wanted it themselves. I spoke to a number of sepoys and Native Officers recently when I was up there, and they told me that they were just waiting for a third epidemic to break out to be inoculated for a third time.

25,943. The symptoms are not sufficiently severe to frighten a soldier off from re-inoculation?—No.

25,944. Did you see any bad results in those first inoculations among the sepoys?—No.

25,945. Did you see them after inoculation?—A great majority of them I did.

25,946. Those are all the pertinent facts, are they not?—Yes, I think so.

25,947. (*Dr. Ruffer.*) As I understand, the Regiment was first evacuated, then inoculated, and then allowed to go back?—Yes.

25,948. How many cases did you get between the evacuation and the return to the lines?—On the 22nd of November the Regiment began to go into camp. The evacuation was complete by the 28th. During this transition period 15 more attacks took place, six being among sepoys. In the next 10 days 34 cases occurred, and before the return of the Regiment to the lines was complete 16 more were attacked, making altogether 78 cases from the commencement counting the 13 which occurred before removal to camp.

25,949. How many attacks were there among inoculated people?—There were not any inoculated at that time. The inoculations did not begin till about a month afterwards.

25,950. How long after the inoculation was finished did you allow the people to go back?—They went back at once—the next day.

25,951. But the houses had been evacuated for about two months before they were allowed to return to them?—The houses were evacuated on the 28th of November, and they were all back by the 30th of December. They had been empty for a month.

25,952. Had they been disinfected?—Yes.

(Witness withdrew.)

Dr. D. Galeotti.

Dr. D. Galeotti recalled and further examined.

25,953. (*The President.*) I think that you expected, on appearing before us for further examination, to give us some notes of experiments on animals with serum obtained from five horses which had been immunised in Florence by Professor Lustig?—I spoke to Professor Lustig when he came here, about this, and he told me that he had sent to the Municipal Commissioner a memorandum about this thing. I think the Commissioner has the memorandum, and I cannot say more.

25,954. This memorandum does not contain any details; it only summarises some of the experiments, without any details. These experiments were not made by you?—No.

25,955. So that you cannot give us any further information than is contained in this report?—No.

25,956. Perhaps you will put in this statement in your evidence?—Yes, it is as follows:—

MEMORANDUM BY PROFESSOR LUSTIG, ADDRESSED TO THE MUNICIPAL COMMISSIONER, BOMBAY.

"1. Having obtained from the Italian Government leave only up to the 10th of February, I must start from Bombay, and I regret it is impossible for me to give my evidence to the Plague Commission.

"2. I beg to forward to the Commissioner a memorandum giving briefly my opinion on the different subjects on which I have been requested to express my views.

"3. Further explanations can be given by my Assistant Dr. Galeotti, Lecturer at the University of Florence, and by myself by letter, if the Commission find it necessary.

"4. The aim of my coming to Bombay has been to pay a visit to the laboratory arranged by my Assistants, Drs. Galeotti and Polverini, and to have personal information of the arrangements for the preparation of the curative serum.

"5. I am very pleased to be able to express to the Commissioner that I have found the work of Drs. Galeotti and Polverini completely satisfactory in every regard, and I should be very glad if the Plague Commission would pay a visit to this laboratory, and examine the operations for the preparation of the serum, and the experiments for the immunization of horses and other animals.

"6. Now it will be seen, from the several works published by myself and Dr. Galeotti in different and scientific papers, that we had in our researches the object of extracting from the plague bacilli a very active substance of known constitution able to induce in the animals, in which this substance has been injected, a strong local and general reaction, by means of which the serum of the blood of these animals attains specific bactericidal and antitoxic properties.

"7. Our experiments were, in the beginning, performed on animals (rabbits, rats, guinea-pigs, monkeys) and later on (July and August 1897) in Bombay and Poona with a serum prepared in Florence. With this serum we treated 30 patients in different stages of disease, and of these, 24 recovered.

"8. In February 1898, I was charged by the Bombay Municipality to prepare serum in my laboratory at Florence, and to send it here for the treatment of the plague patients in the hospitals of this town.

"9. Every time, before the sending out of the serum, I tested its preventive and curative power on rats, injecting them in the peritoneal cavity with a small quantity of a very virulent plague culture.

"10. I did not determine the minimum curative doses, because I think that such determinations have

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no conclusive value, it being impossible to have a standard of the strength of the injected virus.

"11. I can only say with certitude that $\frac{1}{3}$ or $\frac{1}{4}$ c. c. of serum was able to save a white rat of 100 grammes weight, if the serum was injected under the skin, two or three hours after the intraperitoneal infection with cultures able to kill rats in 12 to 18 hours.

"12. These experiments show that the serum of the No. 4 horse was the most active and that of the No. 1 horse was the weakest.

"13. The results are in accordance with the experiments made with the same serums on plague patients.

"14. With these experiments the Plague Commission have been acquainted by Dr. Galeotti and by other witnesses who had occasion to examine the treated cases, and it would be useless to report these results.

"15. Now I have found the horses treated by Dr. Galeotti in good condition, and I hope they will give an active serum.

"16. The first essays made by Dr. Galeotti on the rats gave very satisfactory results, and these results will be communicated to the Plague Commission, together with those of the following experiments by Dr. Galeotti himself.

"17. In accordance with my personal experience on the preparation of the curative serum in general, and specially in regard to the preparation of the anti-plague serum, I must say that not all the horses are able to give a serum endued with sufficient curative properties, and that some horses are able to give a good serum, only after a long process of repeated inoculations.

"18. I believe that the strength of the serum prepared, following my method, is due to the presence in the blood of substances elaborated by the cells of the organism of the horses, during the general reaction which follows every inoculation.

"19. When the activity of the serum is in good proportion to the No. 1 strength of these reactions, the horses able to give strong reactions will furnish surely an active serum.

"20. On the contrary, the horses in bad condition of nutrition and in which the material exchange is abnormal, show, after the inoculations, phenomena of impoverishment and torpor, and are not able to give a sufficiently strong serum.

"21. For this reason, it is necessary to keep the horses quiet for some time after the first blood letting and before making the inoculations.

"22. I have requested Dr. Galeotti to give to the Plague Commission the most detailed information about the experiments which will be done on plague patients with the serum of the horses which are now under treatment, and also, in regard to the researches designed, to determine on rats, with the greatest exactness possible, the strength of the serum of every treated horse.

"23. Being requested by the Municipal Commissioner to express my opinion on the importance of rats in the diffusion of the plague, I offer the following:—

"1st. I have performed experiments on hundreds of rats of different races, and living in the most different conditions, and I have found that almost all are able to get plague.

"2nd. Keeping in a cage several infected and non-infected rats, all die in a short time, and in their bodies plague microbes are always to be found.

"3rd. Mixing a virulent plague culture with the food given to some rats, all these generally die from plague, and it is quite common to find the bacilli in their fæces.

"4th. Generally, in the infected rats, the microbes enter the blood, and with great facility they pass into the urine. Thus this urine becomes a very infectant liquid.

"5th. In 1897, I had occasion to examine at Poona some dead rats found in a granary, and in these I found plenty of microbes. I investigated also the corn of this granary, but I did not discover any plague microbes in it. Nevertheless, I know that two cooks who ate of this corn died from plague a few days after.

"6th. I believe that the rats represent one of the important means of diffusion of the disease, because they are able to carry everywhere virulent microbes.

"7th. That the destruction of the rats is one of the most important measures to be taken against the plague. For this purpose poisons can be very useful, and specially would it be useful to provoke amongst the rats another epidemic, with a microbe not virulent to man. For this purpose I believe that the Laser's microbe would be the best, because it was used with satisfactory results in Thessalia some years ago. I got a culture of this microbe and tried it on white mice, and obtained a culture which was very virulent, but I had not success into the black rats. I know that the same was experienced by Dr. Galeotti, to whom I sent one of my cultures of Laser's microbe. Dr. Galeotti will receive in a short time from Prague another more virulent culture of the same microbe, and will repeat the experiments."

25,957. I think you spoke previously of 24 patients who had been treated at Poona with the serum, but you were not then able to tell us what was the nature of the individual cases, whether they were mild, or whether they were severe, or at what stage in the course of the disease the serum treatment had been adopted. Can you now give us these points of information?—I was not in Poona, and I had only had verbal information from Professor Lustig, and he told me that the cases were of different strength. Some of them were very serious, and some not very serious, and that for the most part they were all of the bubonic form, one or two being of the pneumonic form.

25,958. You cannot say anything more definite; you have only got the information from another source, not from your own observation?—That is so.

25,959. I think you also told us that you were then engaged in endeavouring to produce a more active curative serum?—Yes.

25,960. Would you tell us what stage you have reached in that production now?—I have had six horses here from the beginning of November, and I have tried now the serum of five horses.

25,961. How long did you continue the process of immunization from the beginning of November?—Till the end of January, three months.

25,962. What was the maximum quantity of virus which you were able to give these horses ultimately?—I inoculated the horses five or six times, injecting a different quantity each time.

25,963. A larger quanity?—Not very large.

25,964. Was the second quantity larger than the first, and the third larger than the second, and so on?—Yes, always increasing the dose. The biggest dose I injected was two grammes and a half.

25,965. Of what?—Of the substance which we inject into the horses in order to immunise them.

25,966. What was that substance?—A nucleo-proteid, extracted from the microbes of plague, following the method I have described to the Commission.

25,967. A concentrated toxine?—Yes, a pure toxine.

25,968. Free from anything living, therefore without any living organisms?—Without any living microbes.

25,969. Have you any idea what is the quantity of it which is required to kill a horse?—One of my horses died a few hours after the immunising injection of 49 centigrammes.

25,970. That is a great deal more than the smallest quantity required to produce death?—Yes.

25,971. What was the largest quantity of this fluid which you ultimately injected?—I could not inject more than two grammes and a half because the horses suffer very much from this injection.

25,972. Two grammes and a half produced all the initial symptoms of the first injection with the smallest amount?—Yes, very big swelling, very big œdema in the place of the injection, and afterwards a high fever in the horse, and for some time after, the place of the injection has formed a centre of large exudation.

25,973. The local effects were very pronounced?—Yes, and sometimes there was necrosis of the skin of the horse.

25,974. Was there anything further, any other general symptoms?—The animal does not take its food, and it is very weak.

25,975. Did it lose weight or not?—Yes, it lost weight very much.

25,976. Have you tested this substance against the living organism as well as against the toxine?—No, it has no effect on the living bacteria.

25,977. What effect has the serum obtained from the horse?—The serum of the horse has bactericidal

power, and the microbes are agglutinated by the serum, and they died in a short time.

25,978. Is it very actively bactericidal?—Yes, it has very great bactericidal power.

25,979. What is its anti-toxic power?—The anti-toxic power does not seem to be very great.

25,980. It is mainly a bactericidal serum?—Yes, mainly bactericidal.

25,981. Have you made any observations on human beings, plague cases, with this serum?—Yes, the serum has been used at the Arthur Road Hospital—the Municipal Hospital—by Dr. Choksy.

25,982. In how many cases?—I think now we have treated 120 cases, only I have not heard a report of all these treated cases. This report has been made, however, and has been forwarded to the Government, and I shall send you a copy in a few days.*

25,983. Can you give us the general results?—I can tell you generally the results, In the beginning the type of the disease was very serious, and, especially, there were very many pneumonia forms, and almost in every patient there was complication of the lungs, localisation of the infection in the lungs, and the first results were not very satisfactory, but now, with the serum of the horse No. 4, the results are much better.

25,984. Is that a horse which has been carried further in immunization than the other horses?—No, almost all the horses were treated in the same way, but the serum of the horse which had been used lately has given much better results, and especially for the general clinical observation of the patients.

25,985. Was the nucleo-proteid used in the first inoculation the same as in No. 4 horse?—Yes, horse No. 4 was immunised in the same way, in the same order.

25,986. With the same nucleo-proteid?—Yes, and the same result in the symptoms of the disease, especially the temperature and the action of the pulse and the action of the heart. I remember that this serum has been injected into 47 patients, and 22 of them died, but I think that almost all the remaining patients will recover. The cases treated with this serum were almost all very bad, and some of them had also a complication of the lung, and I observed the microbes in the sputum.

25,987. That is a much more favourable result than with the other experiments?—Yes.

25,988. You did altogether 120?—Yes, I do not know the exact number, but you will get that information.

25,989. There are, therefore, about 70 other patients who were treated with serum, were there not?—Yes.

25,990. But that serum, you think, was not a good serum?—I do not think that the serum was much weaker than that of the No. 4 horse, but I think the patients were much worse.

25,991. These were not such bad cases?—They were all very serious cases.

25,992. The previous cases were very serious; these last were not so serious?—These were a little milder, not very mild, because mild cases are now very rare.

25,993. While these 47 cases were being treated there were other cases in the same hospital which were not being treated?—Yes.

25,994. What was the mortality of the other cases, and what was the number of the other cases?—The mortality would be about 85 per cent., or, perhaps, more.

25,995. It was a very strong contrast, therefore, this mortality with the mortality in your cases treated with serum?—Yes, very different.

25,996. What was the dose employed? How much did you use in the 47 cases?—The doses injected were from 60 to 120 c.c.

25,997. Was that the total quantity or the dose?—The total quantity.

25,998. In what doses?—15 or 20 c.c. for every dose.

25,999. Within what intervals of time were they given?—Sometimes the injection was done twice every day.

26,000. How many hours was the second injection after the first?—For example, if the first injection has been done in the morning, the second would be done in the afternoon or in the evening, and the third in the morning of the day after. The following injections depended upon the conditions of the patients; when the patient was better, the injections were suspended. If there was an increase of the temperature, or if the patient was getting weak in any way, the injections were repeated once or twice every day.

26,001. Are these differences about the same as in the previous 70 cases?—Yes.

26,002. And the average total given to one patient was about the same?—Almost the same for every patient.

26,003. Therefore, the explanation of the more favourable results in the second series is, that the cases were not quite so bad?—Yes; I think it is partly due to the fact that the cases are not so fatal as in the first series, and that the serum of the fourth horse has been proved more active on animals.

26,004. Have you made any experiments on the lower animals?—Yes, on monkeys.

26,005. (*Dr. Ruffer.*) I suppose you base your serum treatment on man upon your experiments on animals?—Yes.

26,006. So that if the serum were found to be useless in animals, you would not feel justified in trying it on man?—It would depend on circumstances.

26,007. What experiments have you made upon the animals with the serum of horse No. 4?—I have tried it on three monkeys.

26,008. What kind of monkeys?—Brown monkeys. I had four monkeys, one being used as a control. I injected the monkeys with 2 c.c. of the plague culture, which was very virulent. Three hours after the injection I injected the serum upon three of those monkeys 5 c.c. to each monkey. After 24 hours I injected another 5 c.c. of the same serum.

26,009. That is 10 c.c. altogether?—Yes. Two of the injected monkeys recovered.

26,010. Were they ill at any time?—I did not take the temperature, but they refused food on the first day. After that they began to eat again, and they recovered completely. One died after six days of plague, and I found the bacillus in the bubo. The control died in five days.

26,011. Will you give us the other experiments you have made on animals?—Yes; I put in the following statements with regard to them.

* *See* App. No. LXII. in this Volume.

Experiments with the Serum of Horse No. 3.

Animals.	Weight.	Infection made on the 7th February.	Curative Injections.	Results.
White rat, 1st.	100	Piercing the tail with an infected needle.	After 3 hours, $\frac{1}{4}$ c.c. After 27 hours, $\frac{1}{4}$ c.c.	Cured.
White rat, 2nd	—	Ditto	After 3 hours, $\frac{1}{4}$ c.c. After 27 hours, $\frac{1}{4}$ c.c.	Cured.
White rat, 3rd	—	Ditto	After 7 hours, $\frac{1}{4}$ c.c.	Dead on the 10th February
White rat, *control*	—	Ditto	—	Dead on the 8th February.
Monkey A.	4 lbs.	Injection of 2 c.c. culture	After 5 hours, 5 c.c. After 24 hours, 5 c.c.	Cured.
Monkey B.	4 lbs.	Ditto	After 5 hours, 5 c.c. After 24 hours, 5 c.c.	Cured.
Monkey, *control*	4½ lbs.	Ditto	—	Dead on the 12th February.

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EXPERIMENTS with the SERUM of HORSE No. 5.

Animals.	Weight.	Infecting Injections.	Curative Injections.	Results.
White rat, 1st	120	$\frac{1}{4}$ c.c.	After 3 hours, $\frac{1}{2}$ c.c. After 27 hours, $\frac{1}{4}$ c.c.	Cured.
White rat, 2nd	100	$\frac{1}{4}$ c.c.	After 3 hours, $\frac{1}{2}$ c.c. After 27 hours, $\frac{1}{4}$ c.c.	Cured.
White rat, *control*	110	$\frac{1}{4}$ c.c.	——	Dead on the 13th January.
Monkey 1	4 lbs.	2 c.c.	After 3 hours, 5 c.c. After 27 hours, 5 c.c.	Cured.
Monkey 2	5 lbs.	3 c.c.	After 3 hours, 5 c.c. After 24 hours, 5 c.c.	Dead on the 18th January.
Monkey, *control*	4 lbs.	2 c.c.	——	Dead on the 16th January.

The animals were all injected on the morning of the 12th January, with an emulsion of an agar culture of 48 h.

EXPERIMENTS with the SERUM of No. 1 HORSE.

Animals.	Weight.	Infection made on the 20th February.	Curative Injections.	Results.
White rat	—	Injection of $\frac{1}{8}$ c.c. under the skin.	After 3 hours, $\frac{1}{4}$ c.c. After 24 hours, $\frac{1}{4}$ c.c.	Cured.
White rat, *control*	—	Ditto	——	Dead on the 21st February.
Monkey C.	5 lbs.	2 c.c. of culture	After 3 hours, 5 c.c. After 24 hours, 5 c.c.	Cured.
Monkey D.	4 lbs.	Ditto	After 3 hours, 5 c.c. After 24 hours, 5 c.c.	Dead on the 28th February.
Monkey, *control*	4 lbs.	Ditto	——	Dead on the 25th February.

EXPERIMENTS with the SERUM of No. 4 HORSE.

Animals.	Weight.	Infection made on the 25th February.	Curative Injections.	Results.
Monkey, *control*	4½ lbs.	1 c.c. of culture	——	Dead on the 2nd March.
Monkey E.	5 lbs.	1 c.c. of culture	After 5 hours, 5 c.c. After 27 hours, 5 c.c.	Dead on the 3rd March.
Monkey F.	4 lbs.	1 c.c. of culture	After 5 hours, 5 c.c. After 27 hours, 5 c.c.	Cured.
Monkey G.	5 lbs.	1 c.c. of culture	After 5 hours, 5 c.c. After 27 hours, 5 c.c.	Cured.

26,012. Did you find that all brown monkeys which you injected with plague died, or that a certain number of them recovered?—The brown monkeys are not very susceptible to plague, and it is necessary to have a very virulent culture in order to communicate plague to them—a culture which has been passed through many animals.

26,013. Do you consider that a culture which takes four days to kill a monkey is very virulent?—Yes, it is very virulent.

26,014. Have you ever found a culture which killed a monkey after two days or 36 hours?—Yes, by injecting large quantities.

26,015. It is not the most virulent culture you can get, is it?—Yes; it was the most virulent I could get which I used for these experiments,

26,016. Were these experiments with the serum of horse No. 4, in which you treated 47 patients and had 22 deaths, carried out by Dr. Choksy?—Yes.

26,017. In the hospital?—Yes.

26,018. Can you tell me how many patients, on an average, come into the hospital in a moribund condition?—I think more than half the patients that are admitted.

26,019. How many come in a hopeless condition?—More than half of the patients.

26,020. So that you cannot contrast the mortality of 85 per cent. with your mortality of 22 per cent.?—I do not contrast them.

26,021. Were these 47 patients all patients whom you thought had a chance of recovery?—Some of them were injected when they were in a hopeless condition.

26,022. How many?—I cannot say.

26,023. Did you exclude all moribund cases, and all cases which you thought were bound to die?—Yes.

26,024. Did you exclude all pneumonic cases?—Yes, but sometimes pneumonia is not recognised.

26,025. Did you exclude all cases which you knew had pneumonia?—Yes, all those who had very evident pneumonia.

26,026. Some of them had secondary pneumonia?—Yes; sometimes they were injected patients who were believed by Dr. Choksy at the beginning not to have pneumonia, but who afterwards developed pneumonia.

26,027. Did you exclude all septicæmic cases?—No, because we did not examine the blood before the injections.

26,028. You say that injection had an effect on the pulse; how long after the injection did you notice that effect on the pulse?—Four or five hours.

26,029. Do you think that might not have been due to the patients resting in bed?—No, because such improvement was not observed in the patients not treated with the serum. There is also an effect on the delirium.

26,030. What is the effect on the delirium?—Sometimes the delirium subsides after the injection.

26,031. In how many cases did you find that it subsided?—In almost all the patients which recovered.

26,032. On what day did the delirium subside?—In two or three days after the injection.

26,033. It had not an immediate effect on the delirium?—Sometimes it had, but not generally.

26,034. Had it an immediate effect on the lung symptoms?—No.

26,035. How many of these patients had microbes in the sputum?—I should think two-thirds.

26,036. In the cases which you treated which had lung symptoms, how many recovered?—I cannot say.

26,037. (*Professor Wright.*) What is your evidence for the statement that your serum possesses any bactericidal power?—I have done experiments in vitro.

26,038. What did these experiments consist of?—I mixed the serum with the plague culture in bouillon.

26,039. Did you ever find by that method that all the plague bacilli were killed?—There was a decrease in the number of the colony, developed on Petri's dishes inoculated with the culture mixed with serum. After about 24 hours there generally is no more development of microbes.

26,040. With what serum did you make these experiments?—With the serum sent me from Florence.

26,041. Have you made the same experiments with the serum you have now?—I have just begun these experiments.

26,042. Did you determine whether the serum which you gave to the Commission possessed any bactericidal power?—The experiments on the subject are not yet completed. I have examined the action of the serum on the microbes under the microscope.

26,043. You saw agglutination under the microscope?—Yes.

26,044. With the serum?—Yes; I tested it several times.

26,045. Did you find that you got satisfactory agglutination with serum No. 1?—I tried No. 3 and No. 4.

26,046. Did you get satisfactory agglutination with serum No. 3?—Yes; there was always agglutination.

26,047. How much did you dilute the serum?—I only put a few drops of the serum in the bouillon culture.

26,048. Have you done any experiments on animals with the serum with which you have provided the Commission?—Yes.

26,049. With which serum?—The serum from horse No. 1 and horse No. 3.

26,050. Have you tried any experiments on man with the serum?—Yes.

26,051. With a portion of the serum with which you have provided us?—Yes. With serum No. 3, I injected 17 patients, of whom 10 died and 7 recovered.

26,052. What were your results with serum No. 1?—I do not remember because that was done by Dr. Choksy.

26,053. Did Dr. Choksy also try serum No. 3?—It was tried in the hospital.

26,054. Can Dr. Choksy provide us with the notes of the cases which were injected with sera Nos. 1 and 3?—Yes.*

26,055. Cases injected with the same serum with which you provided us?—Yes.

26,056. What experiments did you make in order to determine whether the serum possessed any anti-toxic power?—I have made no experiments in that direction in Bombay.

26,057. Have you ever found that serum No. 1 or No. 3 made the animals die sooner than the controls?—No.

26,058. Have you never seen that occur with any of the sera which you produce?—I have seen it in small white mice. At the beginning I tried to do the experiments for testing the serum on white mice, but they cannot shake off the shock of the injection and they die within a short time after the injection. I tried to inject them also with a physiological solution or with bouillon, and they died in the same way.

26,059. Do you think a horse which is itself suffering in health, that is, which has not recovered from the injection, can provide a serum capable of benefiting a plague patient?—I never tried to bleed a horse when it had still fever, so I cannot say. I have always bled a horse after the fever had gone.

26,060. But I understand you bleed your horses before they have recovered their original state of health; is that so?—When I bleed the horses they are in good condition, they have no fever, and they take their food properly.

26,061. Do you think a horse which has not yet recovered from the effects of inoculation can give a good serum?—I cannot say that.

26,062. If you found that the animals which received the serum died before your controls would you still proceed to do experiments on man with that serum?—I should repeat the experiments on different animals.

26,063. But if you tried it two or three times on different animals?—I do not think that the serum can do any harm to the patient. I think if one serum is not effective it can be completely harmless.

26,064. But if you saw the animals which received the serum dying sooner than the control animal, would you not think that the serum did the animals harm?—Yes; if I were sure to have eliminated every cause of errors in the experiments.

26,065. If you had a serum which you knew would do animals harm, would you give that serum to man?—It depends upon the conditions of the experiment.

26,066. Would you yourself take such a serum?—If the animals were really killed by the serum I certainly would not inject it into a patient.

26,067. And if the animals which were injected with the serum died quicker than the controls, you would not advise anybody else to give that serum to man for curative purposes, would you?—It depends upon various considerations.

26,068. Therefore, I ask you if you would take the serum yourself if you found it killed animals?—No, I would not take a serum proved poisonous.

26,069. (*The President.*) Have you had any opportunity of trying any anti-toxic serum, not only plague, but any other anti-toxic serum, possessing more poisonous qualities than a normal serum?—No, I have not.

26,070. You do not know that it is the case that some anti-toxic sera are more poisonous than others?—No.

26,071. Supposing it had come to your knowledge that anti-toxic sera are capable of producing poisonous symptoms as a rule, that is, that the tetanus anti-toxine or dyphtheria anti-toxine, as a rule, are more poisonous than normal sera, would that deter you from using the plague anti-toxine in the treatment of plague?—I would not use it. If these toxic properties were proved, I would not use it.

26,072. Suppose they are in existence in the anti-tetanus serum, would that deter you from using it in the treatment of tetanus?—It depends upon the gravity of the toxic properties.

26,073. Have you ever tested the toxic power of this plague serum which you have been working with? Have you administered it alone to any of the lower animals?—No, I have not.

* *See* App. No. LXII. in this Volume.

(Witness withdrew.)

Mr. C. C. James called and examined.

26,074. (*Dr. Ruffer.*) You are one of the Engineers to the Bombay Municipality?—Yes, Drainage Engineer for the City.

26,075. What experience of plague have you had?—I was first of all on plague duty under the Municipal Commissioner from September 1896 to March 1897, and then I was on the Plague Committee from March 1897 to May 1898, when I went on leave.

26,076. You put in Chart No. 1, showing the mortality from all causes which occurred in Bombay from August 1st, 1896, to March 15th, 1899?—Yes.*

* *See* App. No. LXXVII. in this Volume. The chart was subsequently continued to the end of May, 1899.

26,077. Will you kindly describe the chart?—The two upper black lines are the maximum and minimum temperatures. The thin blue line is the population.

26,078. Could you tell us how the maximum and minimum temperatures were ascertained?—They were obtained from the Observatory at Kolaba.

26,079. How was the number of the population obtained?—That was obtained from the Bombay tramway statistics, checked with the Government returns supplied by the Collector of Bombay.

26,080. What do you mean by saying they were checked by the Government returns?—The population is assumed at the start to be 850,000, which is obtained

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in the following way:—The Census for 1891 gave 821,000 for the city. That gave us an increase of so many thousands on the previous census, and carrying that calculation forward we got a population in August 1896 of 850,000. That is carried forward, and the tramway people, taking that as correct, have worked out on their receipts a rule for population, and we check their numbers for population with the returns which the Collector of Bombay gives us every month. We find that it comes within 10,000 or 12,000—quite near enough for this purpose.

26,081. Do all classes of people use tramways?—I think practically only the working classes.

26,082. But do the lowest classes use tramways?—I think not, not to a great extent.

26,083. Then, how can the tramway statistics be correct; would the lowest and very poor classes leave the town?—There is a very large moving population here.

26,084. But you base your total population on the tramway statistics. Now, supposing a large number of people emigrated from Bombay, surely they would be mostly people who would use tramways, the higher workmen and the lowest middle-class people. Would they not leave in larger proportion than the coolies?—No, I do not think so.

26,085. Would the coolies leave in as large a proportion?—I think so.

26,086. What evidence have you of that?—I have not any evidence. That is the best information we can give you with regard to population.

26,087. It is only approximate?—Yes.

26,088. Within what number is it approximate?—Within 10,000 or 15,000. We know what people leave the city, and what people come in, because we get the returns from the railways, the steamer companies, and of the roads; in fact, the people leaving by every exit from Bombay are counted. These returns cannot be very far out.

26,089. Why do you take the tramway statistics instead of taking the figures of people leaving and coming to the city?—We take both; but the tramway company were the first to publish returns of population, and as they were found to be nearly correct we accepted them. I think that the figure given this year is slightly in excess of the actual, owing to so many persons having sent their wives and children out of Bombay.

26,090. Are the figures you give here the tramway figures?—Yes, checked with those returns.

26,091. The figures actually entered on the chart are the tramway figures?—Yes.

26,092. Will you continue your explanation of the chart?—The green line shown here is the humidity. The thick red line is the daily mortality, and the thin red line is the normal mortality based on an average of five years previous to 1896. The purple line is the velocity of the wind, and the brown line is the clouds.

26,093. Do you think there is any relation between either the maximum or minimum temperature and the mortality in Bombay?—It is difficult to form any real opinion from the chart. The chart is very contradictory. I cannot say there is any very great relation between them.

26,094. Is not the mortality greater during the winter months?—Yes.

26,095. Is it the largest in the same months in the three preceding years?—Yes.

26,096. Is not the mortality much larger now than it was in February 1897?—Yes, it is. The number of deaths is greater.

26,097. When was the highest mortality in 1896-97?—On the 14th January.

26,098. In 1897-98 it was in February, I think?—Yes.

26,099. And in 1898-99 in March?—Yes.

26,100. Do you think the population is larger now than it was in 1896-97?—Yes.

26,101. If you calculate the mortality according to the population, do you find the mortality this year is smaller than it was in the preceding years?—Yes; I put in Chart No. 2,* which shows the epidemics for three seasons, 1896-97, 1897-98, and 1898-99, with the weekly death-rate plotted per mille per annum.

26,102. So that the conclusion is that, although at first sight the mortality appears to be higher this year, it is actually lower in proportion to the population?—Yes.

26,103. Did you find any relation between humidity and plague?—Yes. Northerly and easterly winds, which are always dry, seemed generally to have an unfavourable influence, often immediately so, on the mortality.

26,104. To what you attribute that? Do you think it is possible that northerly and easterly winds, being cold, drive people into their houses?—Yes, undoubtedly. The result of that is that, instead of a large number of people sleeping out of doors, as is generally the case during the hot weather, they all sleep indoors, and moreover they shut their doors and windows. It is calculated that 350,000 people sleep in the streets here during the hot season.

26,105. Were there any special meteorological conditions just before the advent of plague?—Yes, in 1896 they were very interesting. The mean annual temperature for 1896 was 80·07, being the highest but one on record for the last 50 years. The total fall of rain for the year was 87·65 inches, which again is abnormal, being nearly 15 inches above the average of the last 50 years. These facts in themselves would, without further explanation, mean little, but it is necessary to consider the details of this fall of rain. June gave us 28 inches, being 8 inches above the average, July was 36·44 inches, being 11·7 inches above the average, August gave us 20·8 inches, being 7·5 inches above the average, and after the end of August only 1·61 inches of rain was registered, being 10 inches less than the average.

26,106. Have these same conditions prevailed in 1898-99?—No.

26,107. So that they would not explain the continuance of the plague in those years?—No.

26,108. Is there any relation between the height of the subsoil, water and plague?—There is no doubt that high subsoil water predisposes to disease; I think in that way there is a connection. There is an interesting point in Chart No. 1, I would mention, and it is the deaths from plague from August 1896 to end of June, 1897; 46,886 people died from plague. The average number of deaths for this period, calculated on five years previously, is 18,289, and the difference, 27,597, must be taken as plague. From July 1st, 1897, to May 1st, 1898, the deaths from all causes was 50,150, and the number on the average of five previous years was 24,192, and the difference—plague—25,958. From May 1st, 1898, to 20th March, 1899, the total deaths from all causes was 44,362, while the average is 25,759, and the difference, 18,503, is plague. This gives, for the first epidemic, 27,597 deaths from plague, the second 25,958, and the the third, to March 20th, 18,503 total, 72,058.

26,109. You have some other charts which you wish to put in?—Yes, the three plans of Bombay,* showing the progress of plague.

26,110. Will you kindly explain them?—These three plans represent the three epidemics. The seven primary colours have been taken there to represent months. When a case occurs in a district, unless the district is epidemic, it is shown by a coloured circle, appertaining to the month. For instance, take Byculla; in September 1896, as will be seen from the pink circle, there were 3 deaths from plague, in October 9, in November 2, and then in December the whole district became epidemic, having 112 deaths recorded as shown in the margin of the plan; in January 205 deaths occurred, in February 318, and in March 177. After the epidemic is over, the deaths are shown in a scored and coloured circle, showing that in April there were 50 deaths, in May 15, in June 6, in July 5, and in August 2. All the districts are worked in that way, so that it is possible to tell where each district has become epidemic. For instance, all these districts (Walkeshwar, Worli, Mahim, Sion, Parel Sewri) were epidemic in January. Mandvi was epidemic in October, so that you see the course of the disease, which is from south to north. That is probably because the people fled from Mandvi and that part of the city to the outlying districts. Then this plan also

* *See* App. No. LXXVIII. in this Volume.

* Not reprinted with the Proceedings of the Commission.

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hows that the whole of the island was epidemic in 396–97, except one district, namely, the Esplanade. Those districts which escaped being epidemic last year were Esplanade, Upper Kolaba, Walkeshwar, Mahalakshmi, Sewri, and Sion. These districts have also remained practically free in the third epidemic. Sion is a large district with several small villages, and those villages in proportion to their population have suffered much more than the population of the districts in the middle of the city. The last plan, viz., for 1898–99, is very interesting, because it shows a small district called Chuckla, a crowded Muhammadan part of the city, which has only had 68 deaths recorded in this epidemic; it is densely populated. Whether that is accidental or whether the deaths have not been registered it is impossible to say. I cannot account for it. Then Mahim, for instance, is very badly affected; it is a very low-lying district, full of water; no sun gets there.

26,111. (*Mr. Cumine.*) Is it worse now than it was?—Mahim is very bad just now. I think this plan also shows a further improvement again in the number of districts which are not epidemic:—Sion, Worli, Walkeshwar, Mahalakshmi, Chowpati, Esplanade, Upper Kolaba, and Chuckla, eight against seven last year. I think there is some connexion between plague and a high sub-soil level, from the fact that in nearly all the low-lying districts where the sub-soil water is high, plague has been scarcely ever absent during the last three years.

26,112. Has the plague got out of Mandvi now?—There is not much plague there at present, it is rather a recrudescence than anything else.

26,113. (*Dr. Ruffer.*) Are the low-lying districts the most thickly populated districts?—Very often they are.

26,114. So that the larger mortality may be due either to overcrowding or to the fact of the districts lying low?—Yes.

26,115. Is there anything else you wish to add?—I have four other plans with me which show the actual houses infected in the districts.

26,116. I do not think we shall require those?—I should like to say that one of the most interesting things in Chart No. 2 is that both the previous epidemics stopped about the same time. If the third one is going to stop as the previous ones have, and as it appears to be going to do it, this would rather point to the fact that next year the epidemic will start later than it has done this year and finish again at the same time, and the following year follow the same course until the lines meet. You will see that the two lines cross one another at the end of April.

26,117. (*The President.*) Would it be possible to get the charts kept up to date?—Yes, I will endeavour to do so. (Note.—Witness supplied the Commission with additions to the charts, to bring them up to the end of May 1899, as shown in App. No. LXXVII. and App. No. LXXVIII. in this Volume.)

(Witness withdrew.)

(Adjourned till to-morrow.)

At The Secretariat, Bombay.

SIXTY-NINTH DAY.

Tuesday, 21st March 1899.

PRESENT:

Prof. T. R. FRASER, M.D., LL.D., F.R.S. (*President*).

Mr. J. P. Hewett.
Prof. A. E. Wright, M.D.
Mr. A. Cumine.
Dr. M. A. Ruffer.

Mr. C. J. Hallifax (*Secretary*).

Capt. C. G. Spencer, R.A.M.C., called and examined.

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26,118. (*The President.*) I believe you are in the Royal Army Medical Corps?—Yes.

26,119. And I think you have been engaged for some time on special work in connection with the plague bacillus?—Yes.

26,120. What previous opportunities have you had for engaging in bacteriological investigation?—I have done bacteriological work at Netley, but since then I have not done any bacteriological work.

26,121. How long ago was that?—That was five-and-a-half years ago.

26,122. In the work which you have just been doing I believe you have arrived at certain conclusions with regard to the recognition of the plague bacillus, have you not?—Yes.

26,123. Will you kindly tell us what conclusions you have arrived at?—The plague bacillus in the blood and tissues of an animal can be recognised, as a rule, with some certainty. Its characters are constant and distinctive. In a pure culture the characters of the bacillus are not at all so constant. They differ from the appearances seen in animals, and it is much more variable in a pure culture.

26,124. The bacillus as obtained from an animal affected by plague has a different appearance from a bacillus grown in a culture?—Yes.

26,125. What are the differences?—The bacillus in cultures is smaller; its shape is different; the size is more variable; involution forms are more numerous; and the bi-polar staining—one of the most constant appearances in blood—is very much less marked in the culture.

26,126. The bacillus culture is the more virulent?—I cannot say whether it is more virulent.

26,127. You do not consider the microscopic diagnosis is sufficiently conclusive?—No; I would never rely upon microscopic appearances alone.

26,128. What would you take in combination with microscopical appearances, to make yourself quite certain?—Experiments on animals, the appearance of the bacillus in pure culture, the mode of growth, and the rate of growth.

26,129. Have you found anything worthy of note with regard to the growth of the plague bacillus by Hankin's method?—Yes. I made two series of experiments with Hankin's reducing solution.* In each case I took five

* *Vide* Question, No. 8604. The solution referred to consists of:—

Ferrous sulphate	10 parts.
Tartaric acid	10 parts.
Citric acid	1 part.
Water	100 parts.

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tubes of broth and added from one to five drops of Hankin's solution, and then added the pure culture of plague to the broth. These tubes were kept at the temperature of the room and examined at various intervals for a week. Decolorisation was very slightly marked, and occurred in only one or two of the tubes. I did not recover the plague bacillus from any of the tubes.

26,130. What is your opinion with regard to the method?—It does not seem to be a reliable method for detecting the plague bacillus.

26,131. I believe you have observed the growth of plague in earth also?—Yes.

26,132. Will you state the result?—The method was as follows. Twelve small glass tubes, each pierced with a lateral hole were taken, and these were filled with earth. The earth was obtained from Calcutta, and was not supposed to be infected. They were plugged with cotton wool, and sterilised. The lateral hole of the tube was guarded with a tight-fitting india-rubber band. The earth in six tubes was saturated with sterile water, and the six other tubes were kept dry. All were inoculated with one drop of a fresh plague culture dropped on the surface. The tubes were kept at the ordinary temperature of the room in diffused light. After four days they were examined and no plague bacillus could be recovered, either from the surface of the tubes or at different depths below the surface from one-sixteenth of an inch to one inch.

26,133. By what process could that not be recovered, by microscopical examination, or what?—By attempting to grow it in broth.

26,134. Have you examined any samples of earth which you knew probably contained plague, or were obtained from places where the earth might contain plague?—Not by direct microscopic examination.

26,135. You have done it by other methods?—Yes.

26,136. What methods?—By making agar plate cultures, by Hankin's method, by cultivating in carbonic acid, and by inoculating animals.

26,137. What was the result of the agar plate cultures?—Seven specimens of earth from the floors of plague-infected houses were examined by agar plates, and the plague bacillus was not found at all. Four different bacilli, which could not be distinguished from plague by the microscope, were found. These were proved not to be plague by their method of growth. Three of them were chromogenic, and only when observed a little longer formed spores.

26,138. Were inoculation experiments made with them?—Not with them.

26,139. What was the result of your trials with Hankin's method?—Ten specimens of earth from plague-infected floors, and one specimen of dust from an infected pucka floor, were examined by mixing the earth with sterile broth and inoculating the emulsion into a series of five broth tubes containing from one to five drops of Hankin's solution. In none of these were plague bacilli found. Bacilli resembling plague in microscopical appearance were found in four of these specimens. These were tested by inoculations on animals, and we came to the conclusion that they were not plague.

26,140. You mean there was no pathogenic power?—No.

26,141. With regard to the carbonic acid method, what is that?—I examined seven specimens of earth from plague-infected floors by the carbonic acid method, and from two specimens bacilli closely resembling plague were obtained; but whether they were plague or not I cannot say, as my experiments are not completed.

26,142. Have you done anything further; have you taken other steps for identification, and inoculated animals?—I have inoculated animals.

26,143. What has been the result?—One animal inoculated with one of these bacilli died within 12 hours; but by the time it was found it was so far decomposed it was impossible to draw any conclusion from the examination. In the other case the animals were inoculated three days ago, and they are still living. Those experiments I shall repeat.

26,144. So that you are not in a position to say anything definite about that?—No. I have also examined three specimens of unsterilised garden soil to which small quantities of a plague culture had been added. From two of these three specimens I have recovered the plague bacillus.

26,145. Could you give us the details?—A small quantity of unsterilised garden soil was added to sterile broth and a pure culture of plague bacillus was added to the mixture of earth and broth. Carbonic acid gas was passed through. The tubes were sealed and incubated for from four to seven days and examined at different intervals. On examining these cultures microscopically I did not see anything resembling plague; but on making agar stroke cultures, from two of the specimens bacilli were isolated, which resembled plague microscopically. They grew rather faster than plague on agar. On inoculating animals, the animals died, and plague bacilli were found in the blood.

26,146. What were the pathological appearances in these animals?—They were inoculated intraperitoneally. One of them had buboes.

26,147. Where?—The buboes were in both groins. The liver and spleen were congested and dark.

26,148. Only one had buboes?—Yes, only one had buboes.

26,149. But in both cases you recovered a bacillus from the blood which very closely resembled the plague bacillus?—Yes.

26,150. Have you made any experiments as to the effect of saprophytic organisms on the growth of the plague bacillus?—Yes. The object of these experiments was to ascertain if any saprophytic organism could be found which influenced the growth of the plague bacillus either favourably or unfavourably. The method employed was to obtain a pure growth upon broth of the organism under examination, to filter the broth culture through porcelain, then to pour some of the filtrate on to the surface of a sloped agar tube, allow it to soak into the agar for a few minutes, and then pour it off again. Plague bacilli were then planted on the agar tube thus treated, and also on an untreated agar tube to serve as a control. The tubes were kept at the temperature of the room, and the amount, character, and rate of growth were compared from day to day, and any differences noted; 22 different organisms were isolated. These were all obtained from the floors of plague-infected houses. Of these 22 organisms 15 were bacilli, of which seven were spore-bearing, and five were chromogenic; six were cocci, three of which were chromogenic, and one was a sarcina; 15 of these organisms were tested by the above method as to their effects on the growth of the plague bacillus. In no case was any definite effect, favourable or unfavourable, on the growth of the plague bacillus detected. Bacillus coli communis was also tested by the same method. The growth of the plague bacillus in the tube treated with the filtrate of a broth culture of bacillus coli was at first decidedly retarded, but after five or six days it grew better, and by the tenth day there was little difference between the growth in this tube and that in the control tube. The experiment was also tried of growing the plague bacillus in the filtrate from a broth culture of bacillus coli. It grew slowly at a temperature of 37° C.; and sub-culture from this tube made on agar and kept at the temperature of the room grew, but very slowly at first. As tested by these methods the effect of bacillus coli on the growth of the plague bacillus is not very powerful.

26,151. Are there any other points you would like to bring before us?—Yes. I have tried the direct inoculation of animals with earth from plague-infected floors.

26,152. Perhaps you will give us information about that?—One specimen of dust from a pucka floor, and eight of earth from plague-infected floors were mixed with sterile broth, and the emulsion injected under the skin of rats.

26,153. Were the floors infected?—The earth I used was collected in sterile test tubes from floors of plague-infected rooms. As soon as I received information that a case or a death from plague had occurred, I went myself and collected the earth, in some cases before the body had been removed, and in every case before disinfection.

26,154. What parts of the floor did you take your specimens from?—As far as I could ascertain from close beside where the patient had been lying.

26,155. How many specimens did you take?—18.

26,156. Did you take specimens from only one part the floor?—In some cases from different parts, and other cases from one part.

26,157. What was the maximum number of parts?—70. Of the nine rats inoculated six survived and had symptoms; three died, two on the third day, and on the sixth day. The *post mortem* appearances re not those of plague, and no plague bacilli were und in the blood or organs, or at the sites of oculation.

26,158. What conclusion do you draw from that ries of experiments?—I have come to the conclusion at that method is not a satisfactory one for discovering plague in infected floors. I may mention at two specimens were also inoculated into rabbits. e of the two rabbits survived, and had no symptoms. e other died on the ninth day. No plague bacilli re found in the body.

26,159. Is there any other matter you wish to bring fore the Commission?—Yes. In addition to the ove experiments, a specimen of water and another of id from a pond behind the Mahratta Plague Hospital re examined. Waste water from a ward had been owed to run into this pond, and the clothing of some tients had been washed in it. Several cases of ague had occurred among people using the pond ter. Two c.c. of water injected into a rat killed in three days. No plague bacilli were found in the dy. By Hankin's method a bacillus microscopically sembling plague was found in the water. This was jected into a rat, which died in five hours, but no ganisms of any kind were found in the body. This cillus was not isolated in pure culture.

26,160. What was the pathological condition of the ad body?—There were no plague bacilli found in e body. I cannot state the exact pathological pearances.

26,161. Can you say whether there were or were not buboes?—There were no buboes. The mud from the nd was mixed with broth and injected into the rat, hich died in three days. No plague bacilli were und in the body. Hankin's method gave negative sults.

26,162. Was the quantity injected into these animals rge or small?—The quantity of water injected was c.c. In the other case 1 c.c. of broth in which a small antity of mud had been shaken up was injected.

26,163. Therefore it was rather dilute?—No, not ry dilute.

26,164. It would depend upon how much broth you ixed?—It was a pretty thick emulsion of mud and oth.

26,165. (*Dr. Ruffer.*) You said you made some icroscopical examination of earth. I do not think ou gave us the results?—Six specimens of earth from alcutta were examined microscopically. In four of ese diplo-bacilli morphologically resembling plague ere seen. In two the diplo-bacilli were exceedingly undant, some occurring in every field of the icroscope. Judging from other appearances these mples had been taken from the surface of the earth.

26,166. How can a plague patient infect the floor?—y sputum, and by the urine.

26,167. And possibly by the fæces?—Yes, possibly the fæces.

26,168. What is the largest quantity of earth which you have examined, either by inoculation or by the plate method at each examination; would it amount to a gramme, or to a very small part of a gramme?—It would amount to several grammes in some instances.

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26,169. How many grammes altogether?—I did not weigh it.

26,170. Roughly speaking, will it be 2 or 3 grammes?—Yes, quite.

26,171. So that the possibility of finding the plague bacillus in the earth will be extremely small?—Yes.

26,172. (*Prof. Wright.*) Have you come to the conclusion that the results obtained by the method of direct examination as to the presence or absence of plague in earth are likely to be fallacious?—I have formed the opinion that they are very likely to be fallacious.

26,173. With regard to your experiments of isolating plague by Mr. Hankin's solution, do you happen to know if Mr. Hankin attaches any importance to this method?—I believe he does not attach any importance to this method.

26,174. But in view of the fact that we have had evidence before us that plague has been detected by this method, you desire to record that, in your experience, the plague bacillus is generally killed off when it is treated by this method?—Yes.

26,175. So far it would be a very ineffective method of looking for the plague bacillus?—Yes.

26,176. Further, you have come to the conclusion that bacilli resembling the plague bacillus are able to survive in this fluid?—Yes.

26,177. It would therefore be a fallacious method?—Yes.

26,178. Therefore you would not lay any reliance on the method—as a method of detecting the presence of plague bacilli in fæces, for instance?—I should place no reliance on the method at all.

26,179. With regard to your methods of seeking for plague in earth, have you been more successful with the carbonic acid method than you had been with other methods?—It is the only method by which I have succeeded in isolating plague from earth.

26,180. Do you know how this method was suggested?—It was suggested by Dr. Marsh's experiments on the effect of burying plague cultures.

26,181. Would you describe accurately the carbonic acid method for detecting plague in earth; what method did you employ?—A small quantity of earth was added to a tube of broth and shaken up. Carbonic acid gas was passed through and the tube sealed, and incubated for from four to seven days.

26,182. Do you think there are high percentages of carbonic acid in the fluid?—Yes. I think it was saturated.

26,183. Do you think the atmosphere above was saturated?—Yes; I think the atmosphere above was saturated.

26,184. Has Dr. Marsh made any experiments upon the discovery of plague in earth by means of the carbonic acid method?—I do not know any details about his experiments.

(Witness withdrew.)

Lieut. Stewart R. Douglas, I.M.S., called and examined.

Lieut. S. R. Douglas, I.M.S.

26,185. (*The President.*) How long have you been ngaged on special plague work?—Since the 17th of ecember 1898.

26,186. (*Dr. Ruffer.*) You have been making some xperiments on the treatment of patients affected with ague, with Roux' serum, have you not?—Yes.

26,187. Can you tell us under what conditions these xperiments were made?—Forty-nine cases were eated at Bangalore. Every case that came into the ospital was taken, except those that were obviously ot plague, or those that were convalescent from ague—that is to say, people that had suppurating uboes.

26,188. You made no choice of the patients you noculated and those you did not inoculate?—No.

26,189. Can you put in a table of cases with the notes of all these cases that you inoculated?—Yes.

26,190. Each case has the temperature chart, the pulse chart, and the respiration chart added to it?—Yes.

26,191. And each case gives the number of inoculations, the time of inoculation, and the quantity of fluid injected?—Yes, I hand in the tables.*

26,192. Can you tell us whether you have noticed any evil effects from Roux' serum?—I have never noticed any evil effects.

26,193. Did you ever have any joint pains after the inoculation of Roux' serum?—In one case, but I did not treat it myself.

* *See* Appendix No. LXXIX. in this Volume.

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26,194. Can you tell us whether you noticed any effect of Roux' serum either on the pulse, or the temperature, or the respiration?—Never a constant effect. No constant effect was noticed in any series of cases.

26,195. Can you tell us whether the effect of Roux' serum was to reduce the mortality in your cases?—At Bangalore I had to take the North Camp as a control against the South Camp. The previous mortality in the South Camp up to the date of my arrival had been 58·7 per cent. While I was there, there were 73 admissions—some of those were not plague, some were convalescents, and some died before I saw them. The percentage of deaths, during the time I was treating with Roux' serum, was 47·9 per cent. The bubonic cases that came in during that period were 39, and the number of deaths 20, giving a percentage of deaths of 51·3. The total cases treated with Roux' serum were 49, and the deaths were 31, giving a percentage of 63·26. Against that it must be said that I took all the serious cases, because the convalescents were not treated by Roux' serum.

26,196. What was the mortality in the North Camp before you came?—In the North Camp up to the 19th November—that was before I came—it was 75·13 per cent. Between the same dates as when I was treating cases in the South Camp, the percentage of deaths was 53·7. The total bubonic cases admitted during that time were 14, of whom 10 died, giving a percentage of 71·42.

26,197. So that the reduction in your mortality by Roux' serum was not greatly marked?—No, not greatly marked.

26,198. (*The President.*) What was the reduction?—We cannot tell exactly what the reduction was. The South Camp Hospital gave a better balance all along. During the time I was there, it was about 5 per cent. better in the South Camp than the North Camp.

26,199. That is, of those treated with Roux' serum?—No.

26,200. Then what about those that were treated with Roux' serum?—With regard to those treated with Roux' serum, I have no direct controls there.

26,201. You did not control the cases there?—No, not at Bangalore.

26,202. (*Dr. Ruffer.*) I think you made some experiments here with Roux' serum?—Yes.

26,203. Can you give us the result of them?—I treated 15 cases with 15 controls. They were taken as they came into the plague ward of the Modi Khana Hospital, and they were handed over to us, and we took every second one that came in,—Lieutenant Walton and myself. I treated 15, with 15 controls, and he treated 13, with 13 controls. Amongst my cases of those treated with Roux' serum, there were two recoveries; amongst the controls there were four recoveries.*

26,204. Was your experience on the whole here the same as your experience at Bangalore?—Yes.

26,205. You noticed no effect either on the pulse, or the respiration, or the temperature, or on the general condition of the patient?—No, none at all.

26,206. Nor on the delirium?—No, they seemed very much the same.

26,207. Is there anything else you would like to add about your experiments with Roux' serum?—Taking the whole of the cases treated by Dr. Walton and myself, there were 27 cases treated by Roux' serum, and 27 controls. Of those there were five recoveries in the Roux cases, and four recoveries in the controls. It works out to a percentage of 81·48 per cent. of deaths for the Roux cases, and 85·18 per cent. for the controls—practically the same.

26,208. Have you made any experiments with Roux' serum on animals?—Yes.

26,209. Could you give us the details of those experiments?—A series of experiments was made with Roux' serum and Galeotti's serum.

26,210. Give us the general results of those experiments?—The results are given in following table:—

TABLE I.—EXPERIMENTS made to TEST the PROTECTIVE POWER of ROUX' and GALEOTTI'S ANTI-PLAGUE SERA.

Number of Guinea-pig.	Amount of plague culture injected.	Amount and kind of serum which was injected.	Date on which foregoing inoculations were made.	Result.	Remarks.
Number 1 (control) -	$\frac{1}{10}$th agar tube	Nil.	18.2.99	Died 22.2.99	
„ 2 (control) -	„	„	„	„ „	
„ 3 - -	„	$\frac{1}{4}$ c.c. Roux' serum, *Note* 1 (Horse 21).	„	„ 23.2.99	
„ 4 - -	„	$\frac{1}{2}$ c.c. Roux' serum, (Horse 21).	„	„ 21.2.99	
„ 5 - -	„	1 c.c. Roux' serum (Horse 21).	„	„ 1.3.99	Much local re-action, *vide infra Note* 3.
„ 6 - -	„	2 c.c. Roux' serum (Horse 21).	„	„ 2.3.99	Much local re-action, *vide infra, Note* 4.
„ 7 - -	„	$\frac{1}{4}$ c.c. Galeotti's serum *Note* 2 (Horse No. 3).	„	„ 22.2.99	
„ 8 - -	„	$\frac{1}{2}$ c.c. Galeotti's serum (Horse No. 3).	„	„ 25.2.99	
„ 9 - -	„	1 c.c. Galeotti's serum (Horse No. 3).	„	„ 22.2.99	
„ 10 - -	„	2 c.c. Galeotti's serum (Horse No. 3).	„	„ 20.2.99	

In each case the serum and plague culture were injected within a few minutes of each other into opposite sides of the body. In each case the presence of plague bacilli in the body was verified after the death of the animal.

NOTE 1.—This serum was kindly placed at the disposal of the Plague Commission by M. Roux, of the Pasteur Institute, Paris. The following particulars with respect to the method which was employed for the preparation of this serum were also very kindly communicated by M. Roux. "The anti-plague serum from horses 21 and 31 was prepared in the following way:—The "horses first received dead cultures of plague bacilli subcutaneously; afterwards they received dead cultures by intravenous "injection. Lastly, they received living cultures by intravenous injection."

NOTE 2.—This serum, which was prepared by Dr. Galeotti for the Municipality of Bombay, was furnished to the Plague Commission by that gentleman. It was accompanied by the following note with respect to the procedure which was adopted in its preparation. "Horse No. 3 received the following inoculations of nucleo-proteid which was prepared by Professor Lustig's "process from culture of plague bacilli:—

"Nov. 23rd 1898. 0·11 grammes.
"Nov. 17th „ 0·92 „
"Dec. 9th „ 2·85 „
"Jan. 2nd 1899. 1·53 „
"Jan. 10th „ 1·59 „
"Feb. 5th „ 2,000 c.c. blood extracted. The serum was employed on plague patients in the Arthur Road Hospital.
"Feb. 7th „ 4,000 c.c. blood extracted. The serum is sent herewith to the Plague Commission."

continued on page 333.

* *See* Appendices No. LXXX. and LXXXI. in this Volume.

26,211. The general result of that experiment was that Roux' serum appeared to have a certain effect in delaying death?—Yes, considerable.

26,212. And Galeotti's serum rather the opposite effect?—Yes. There was another series of experiments done in the same way.

26,213. Can you give us those?—Nine guinea-pigs were taken, and all received the same dose of plague. Four received 2 c.c., 1 c.c., ½ c.c., and ¼ c.c. respectively of Roux' serum; another four received 4 c.c., 2 c.c., 1 c.c., and ½ c.c. of Galeotti's. The results are given in the following table:—

TABLE II.—Further Experiments made to test the Protective Power of Roux' and Galeotti's Anti-plague Sera.

Number of Guinea-pig.	Amount of plague culture injected.	Amount and kind of serum which was injected.	Date on which foregoing inoculations were made.	Result.	Remarks.
Number 1 (control) -	$\frac{1}{10}$th agar tube	Nil.	27.2.99	Died 4.3.99	
„ 2 - -	„	¼ c.c. Roux' serum, (Horse No. 31) *Note* 1.	„	„ 5.3.99	Marked local reaction.
„ 3 - -	„	½ c.c. Roux' serum (Horse No. 31).	„	„ 6.3.99	do.
„ 4 - -	„	1 c.c. Roux' serum (Horse No. 31).	„	„ 29.2.99	No cause of death detected, no plague bacilli were detected either microscopically or by cultivation.
„ 5 - -	„	2 c.c. Roux' serum (Horse No. 31).	„	„ 8.3.99	Marked local reaction, a few plague bacilli in spleen.
„ 6 - -	„	½ c.c. Galeotti's serum (Horse No. 1) *Note* 2.	„	„ 3.3.99	
„ 7 - -	„	1 c.c. Galeotti's serum (Horse No. 1).	„	„ 3.3.99	
„ 8 - -	„	2 c.c. Galeotti's serum (Horse No. 1).	„	„ 3.3.99	
„ 9 - -	„	4 c.c. Galeotti's serum (Horse No. 1).	„	„ 3.3.99	

The method of experimentation was the same as in the former series of experiments.
Plague bacilli were detected after death in all the guinea-pigs with the exception of guinea-pig No 4.

NOTE 1.—This serum was kindly placed at the disposal of the Plague Commission by M. Roux.
The particulars with regard to the method of preparation which was employed for the preparation of this serum, which were furnished by M. Roux, have been already given. (*Vide* Note 1 on page 322 of this Volume *supra*.)

NOTE 2.—This serum was prepared by Dr. Galeotti for the Municipality of Bombay. The following particulars with regard to the method of preparation were kindly furnished by Dr. Galeotti:—

Horse No. 1.

Nov. 17, 1898.	Plague-nucleo-albumen	0·18	grammes.
Nov. 28, „	„	0·57	„
Dec. 12, „	„	1·38	„
Dec. 29, „	„	3·97	„
Jan. 21, 1899.	„	1·23	„
Feb. 15, „	Bled 2,000 c.c.		
Feb. 17, „	„ 3,500 c.c.		

This horse was inspected by Professor Wright and Dr. Ruffer on March 2nd, 1899. He was then in a fairly good condition and had a sleek coat. He was said to have reacted less to the injections than any of the six other horses.

26,214. (*The President.*) What was the time between the administration of the virus and of the serum?—They were done at the same time. The serum was injected into one side, and the virus of plague simultaneously into the other.

26,215. (*Dr. Ruffer.*) Practically the same result was obtained in this case? That was Galeotti's serum from horse No. 1, and Roux' serum from horse No. 31?—Yes. The effect of Galeotti's serum was also tried on rats. Seven rats were taken, two were kept as controls, having received ½ c.c. of an emulsion of plague bacillus. All the others received the same dose. One of these rats received at the same time 2 c.c. of Galeotti's serum in the opposite side of the body; another rat received 1 c.c., another ½ c.c., another ¼ c.c., and another ⅛ c.c. of Galeotti's serum. The results of those up to date are that one control died in four days; the 2 c.c. Galeotti died in two days, and the ¼ c.c. Galeotti died in three days. In all these plague bacilli were found after death. The other control, and the remaining rats who received the serum are well up to date, and most probably immune.

26,216. Practically, that experiment agrees with the former series of experiments?—Yes.

26,217. Have you any more experiments in regard to the serum on animals?—Yes, but not as regards the protective value of Roux' or Galeotti's.

26,218. (*Prof. Wright.*) You made an experiment with the normal blood, comparing with Roux', in order to ascertain whether it had any immunizing power?—It is going on now. The control guinea-pig, and the guinea-pig which received normal horse blood, are dying. (Note by witness on revising proof of his evidence, "since dead.") The guinea-pig which received 2 c.c. of Roux' serum from Horse No. 21 is still alive, but with marked local reaction.

26,219. (*Dr. Ruffer.*) Did one receive Galeotti's serum?—No, not in that experiment.

This horse was inspected by Professor Wright and Dr. Ruffer on March 2nd, 1899. He was in very bad condition, was apparently feverish, was lame, and was covered with unhealthy half-healed sores.

NOTE 3.—Particulars of *post-mortem* examination of Guinea-pig No. 5. Abscess at seat of inoculation on left side of abdomen, bubo in left axilla. Pleural effusion on left side. Nodules in left lung. In all of these situations plague bacilli were detected on microscopic examination. No generalised infection. No plague bacilli were detected either microscopically or by cultivation in spleen, liver, or blood.

NOTE 4.—Particulars of *post-mortem* examination of Guinea-pig No. 6. Abscess at seat of inoculation on left side of abdomen. Suppurating buboes in both axillæ and both groins. Effusions in both pleural cavities. Lungs filled with hard greyish-yellow nodules. Plague bacilli were found scattered in small clumps through these nodules. Bacilli were recovered by cultivation from the suppurating buboes. Only a single colony was obtained in the cultivation made from the spleen.

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26,220. Can you give us any facts as to the diagnosis of plague by sedimentation and agglutination?—Three sets of capillary sedimentation tubes were put up. Two of these were filled in with plague bacilli and with the diluted serum of a convalescent plague patient. The dilutions employed were respectively 1 in 10, and 1 in 50. Two other capillary sedimentation tubes were filled in with plague bacilli and with my own serum in the same dilutions and a further capillary tube was filled in with an equal quantity of normal saline solution, and of an emulsion of plague bacilli. In all of these the sedimentation was equal. The agglutination test has also been applied several times under the microscope, using a mixture of plague bacilli and normal saline solution as a control, and no marked difference can be seen.

26,221. So that you do not know of any method at present by which you can diagnose plague either by sedimentation or agglutination?—No.

26,222. That is a diagnostic method which is useless at the present time?—Yes.

26,223. Have you any other experiments on the same point?—With regard to Pfeiffer's reaction, that has been tried three times in the case of guinea-pigs, with Roux' serum. It gave doubtful results in the first case, and very doubtful positive results in the other two.

26,224. Would you kindly explain exactly how you did it?—A guinea pig was taken, and into the peritoneum of this guinea pig I introduced a small quantity, about 1 c.c. of Roux' serum, and about 1 c.c. of thick emulsion of plague. Then at intervals of one minute, two minutes, three minutes, and 5 minutes, portions of the peritoneal fluid were drawn off, and examined under the microscope. In the first case, I thought spherulation was seen very well, but in the other cases it was not at all marked. The same experiment was performed with Galeotti's serum, and the spherulation seemed to be fairly marked in that case.

26,225. How long was it before it showed itself?—It showed it best in five or six minutes.

26,226. Did any of those animals die?—I know of two of the Roux' animals that died, but I am not sure about the third. It was well some time afterwards when I saw it.

26,227. Can you give us any facts as to the involution forms of plague bacilli as seen in animals after death?—In a cat that was inoculated here, at the seat of inoculation there was a quantity of lymph thrown out, and in this there were some forms of longish bacilli, and they looked like the involution forms seen on agar cultures. Also in the peritoneal exudation of a guinea pig that had been inoculated under the skin, the same appearance was seen in one case.

26,228. Did you see it anywhere else?—No, those are the only two cases.

26,229. Did not you see it in the rabbit that was inoculated with the urine of a plague patient?—No.

26,230. Did not these involution forms look very much like saprophytes when you saw them first? Could they not be easily mistaken for some other kind of microbe?—Certainly.

26,231. So that there is a possibility of error?—Yes.

26,232. Have you any other facts bearing on that?—No.

26,233. Have you any facts bearing on the growth of plague in various gases?—I have some facts bearing on the growth of plague in carbon di-oxide. These researches in question were suggested by Dr. Marsh's experiments on the effect of burying plague cultures in earth. I believe Dr. Marsh also did this experiment. It was thought that CO_2 might have an influence on the growth of plague. A broth tube was inoculated with plague bacilli, and a stream of carbon di-oxide passed through it, and the tube was then sealed, a control being made from the same source at the same time. The growth in the tube containing the carbon di-oxide appeared to be more abundant two or three days afterwards.

26,234. Have you got one experiment on that or several?—Several.

26,235. In each case it was the same?—No, not in each case; the results have not been constant.

26,236. (*The President.*) What were the results in the other cases?—In some cases the tubes were contaminated, and therefore you could not get a result at all. In others there did not seem to be any difference in the growth.

26,237. (*Dr. Ruffer.*) It is not a constant phenomenon?—No.

26,238. Can you tell us anything with regard to the channels of infection of various animals? I think we had better begin with monkeys. The monkeys that you used were all brown monkeys, were they not?—Yes. Monkeys inoculated under the skin have died, as a rule, in about five days. One monkey that had a few drops of plague culture dropped into its nose, took seven days to die. In two cases in which plague bacilli were inoculated into the trachea of monkeys, they each died in three days, and in those cases the lungs were almost solid—like the lungs in croupous pneumonia. One monkey was inoculated in the vagina, some plague culture being put up the vagina, and this died in seven days, and plague bacilli were found in its organs. There was in this case a good deal of local re-action. One monkey had a small quantity of plague put into one eye.

26,239. On the conjunctiva?—Yes, on the conjunctiva, and that died in six days.

26,240. Did you find that the glands nearest to the point of inoculation were always more enlarged than the other glands?—In all these cases, except in the cases of the trachea, where there was no marked enlargement of glands.

26,241. Did you try to infect monkeys by the mouth?—No, not monkeys; but some were inoculated intravenously.

26,242. What were the results of your intravenous injections?—One monkey recovered. In one monkey, the plague bacilli seemed to have leaked into the wound and affected the glands just above.

26,243. Did that animal die?—Yes, that animal died with a bubo above the wound. Another monkey received intravenously eight-tenths of an agar culture injected by means of a glass cannula, and it died two days later with very few bacilli found in its organs, and none in its blood. Cultures were made, but I have not examined them yet. Another monkey received four-tenths of an agar tube, intravenously, and died three days after injection. It had plague bacilli in its organs—more than the one which I have referred to above—but I could not see any in its blood, and the cultures have not developed yet. Another monkey received two-tenths of a tube, intravenously, and died in four days.

26,244. Did you find any bacilli in its blood?—Yes, in that one they were swarming.

26,245. Have you any other experiments on monkeys that you can give us? I think in some cases you obstructed the circulation of a limb in which plague had been injected?—Yes, in one case. The right femoral and the left brachial artery were tied, and then into each of the four limbs ¼ c.c. of the same plague emulsion—that is one-fortieth of an agar culture—was inoculated, and that monkey died in three days. Buboes were found in all four limbs, in both axillæ and in both groins.

26,246. Were they most marked on the side in which you had tied the artery?—No, there was no difference noticed.

26,247. Have you any other experiments bearing on the same question?—The right femoral and left brachial veins were tied in one monkey, and one-fortieth of a tube was injected into each limb; that monkey took four days to die. No difference was found in the size of the buboes.

26,248. Did you find when an animal was injected, and died, after inoculation in the veins, that the blood and the organs contained more or less bacilli than when it is injected under the skin?—When the animals died quickly, there were decidedly fewer bacilli in the organs.

26,249. They died quicker than when injected under the skin?—They had very much larger doses.

26,250. You cannot make any comparison between the two?—No, not definitely.

26,251. Can you tell me anything about the presence of bronchial symptoms in plague patients during the present epidemic?—They seem to be exceedingly numerous. I think, out of my 13 cases that I treated with Roux' serum, that six or seven had bronchial symptoms.

26,252. What do you mean exactly by bronchial symptoms—not pneumonia?—No, not pneumonia.

26,253. You mean any other symptoms connected with the affection of the lungs—such as bronchitis, or œdema of the lungs?—Yes, and any slight symptoms or physical signs.

26,254. Most of those patients have a cough, have they not?—Many of them cough, but not all of them cough that have the symptoms.

26,255. Do they expectorate?—Freely.

26,256. Can you tell me anything about the presence of the bacilli in the sputum in non-pneumonic cases, in bubonic cases?—Out of 13 bubonic cases, bacilli were found microscopically seven times, and five of those had buboes other than in the cervical, or sub-mental regions.

26,257. Were you ever able to isolate it in non-bubonic case by culture?—No, not from the sputum.

26,258. But there may be quite enough microbes in the sputum to make a diagnosis of plague?—They were exactly like plague.

26,259. Were you ever able to find plague bacillus in the discharge from the nose?—In one case where the patient had sub-mental buboes, and a bloody discharge from the nose.

26,260. You were able to prove those by inoculation, were you not?—Yes, I proved by inoculation that these bacilli were true plague bacilli.

26,261. Can you tell me anything about the presence of plague bacilli in the blood?—In a total of six examinations of the blood of plague patients I found plague bacilli in three; one man recovered, one man died 24 hours afterwards, and the other died five days afterwards.

26,262. In these cases, were the plague bacilli numerous?—In the case of the patient who died the next day, they were very numerous; in the other two they were very few.

26,263. In one of them it did not grow in agar, but it did in bouillon?—Yes, in one case a growth was obtained in bouillon but not in agar.

26,264. Did you examine the pus from the buboes? Were you ever able to find the plague bacillus in a bubo which had opened spontaneously, or had been opened after suppuration?—No, never.

26,265. You found a lot of other micro-organisms, did you not?—Yes, in three cases other micro-organisms were found.

26,266. What micro-organisms did you find?—Staphylo-coccus pyogenes aureus in two cases, and a bacillus resembling the bacillus coli in other cases.

26,267. Did you find the bacillus coli in other cases resembling plague?—I found the bacillus coli in an abscess in a boy's scalp.

26,268. Were you able to find the plague bacillus in the urine of plague patients?—I examined for it in three cases. In none of these was a cultivation of the plague bacillus obtained. The method employed was as follows:—The urine was drawn off with a sterilised catheter, and passed straight into a tube of bouillon. These tubes were always found contaminated. In five cases in which the urine was inoculated into the peritoneal cavity of an animal, one gave a positive result, the animal died with typical plague bacilli in its organs.

26,269. (*Prof. Wright.*) Have you any facts that bear upon the sterility of Haffkine's fluid? Have you, for instance, any knowledge that some of the bottles are obviously putrid?—Some of the bottles when opened smelt very strongly, a very foul smell.

26,270. Do you think a putrid smell would arise if carbolic acid had been added to the particular bottles you speak of?—It might, I think.

26,271. We have had it in evidence that M. Haffkine standardises his prophylactic by shaking up the sediment, and judging of the turbidity of the fluid. Do you think it is probable that the sediment contains other substances also, in addition to bacteria?—It certainly might.

26,272. What experiments have you made to determine that there is an equal amount of sediment in the doses prescribed on the bottles?—A series of filter papers was folded up and then dried and sterilized at 110° C. in a hot chamber. They were then put into test tubes which had also been dried, corked, and allowed to cool. That done, the tubes with the contained filter papers were weighed on a chemical balance. Each filter paper was then placed in a funnel, and a bottle of Haffkine's fluid was opened, and the contents of this, after they had been measured, were filtered through the dried filter paper. The filtrate which passed through the filter was in all cases *water-clear*. The paper and the contained sediment were then dried at 110° C. This done, the filter papers were put back in the same test tubes, corked with the same corks, and allowed to cool, and then re-weighed. The difference in weight gave the amount of the sediment in the quantity of Haffkine's fluid which had been filtered. The dose which was prescribed for that particular bottle was ascertained from the label on the bottle, and the number of doses which were contained in the quantity which had been filtered was then calculated out. Finally, from these data the weight of sediment which was contained in each dose was ascertained.

26,273. Were the bottles which were tested selected at random?—Yes.

26,274. What were the results?—In all, ten bottles, derived from ten different brews, were examined. The results which were obtained are exhibited in the following table:—

TABLE III.—Showing the different WEIGHTS of SEDIMENT which may be contained in a prescribed DOSE of HAFFKINE'S PROPHYLACTIC.

Number of Brew.	Volume of the Prophylactic which was passed through the Filter.	Volume of Prophylactic which was prescribed as a Dose.	Combined Weight of the Test-Tube, Cork, and dried Filter Paper.	Combined Weight of the Test-Tube, Cork, dried Filter Paper, and Sediment.	Weight of the Sediment in total Quantity filtered.	Weight of Sediment in prescribed Dose.
4552	100 c.c.	10 c.c.	13·225 grm.	13·3065 grm.	0·0815 grm.	0·0081 grm.
6482	70 „	5 „	10·1135 „	10·265 „	0·1515 „	0·0108 „
6038	69 „	6¼ „	10·055 „	10·2075 „	0·1525 „	0·0138 „
4243	100 „	5 „	17·343 „	17·657 „	0·314 „	0·0157 „
6174	62·5 „	6¼ „	17·3565 „	17·517 „	0·1605 „	0·016 „
3923	105 „	15 „	9·129 „	9·242 „	0·113 „	0·016 „
5312	62·5 „	6¼ „	11·3305 „	11·5055 „	0·175 „	0·0175 „
5182	75 „	10 „	13·148 „	13·4225 „	0·2745 „	0·036 „
4823	62·5 „	12·5 „	12·2735 „	12·4675 „	0·194 „	0·038 „
5108*b*	42·5 „ *Note* 1	15 „	9·640 „	9·7995 „	0·1595 „	0·0563 „

NOTE 1.—The bottle of prophylactic which was employed for the estimation contained only 42·5 c.c. of fluid.

26,275. I understand the weight of the sediment contained in a dose of Haffkine's prophylactic is in some cases seven times larger than in other cases?—Yes.

26,276. And if the toxine in the fluid is in proportion to the sediment, it would result that one dose of prophylactic may contain not only seven times more of the sediment, but also seven times more of the dissolved toxine, than another dose of the prophylactic?—Yes.

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26,277. Have you made any experiments to determine whether Galeotti's serum is more toxic than normal horse serum?—Yes, I made the following experiments:—

TABLE IV.—COMPARISON of the TOXICITY of GALEOTTI'S SERUM with the TOXICITY of NORMAL HORSE SERUM.

Number of Guinea-pig.	Weight of Guinea-pig.	Amount and Kind of Serum which was injected.	Relation of foregoing Amount to Body Weight of Animal.	Result.
	Grammes.		Per Cent.	
Number 1	230	Galeotti's serum 11·5 c.c.	5	No toxic symptoms observed.
„ 2	220	Normal horse serum 11 c.c.	5	Do.
„ 3	260	Galeotti's serum 19·5 c.c.	7·5	Do.
„ 4	260	Normal horse serum 19·5 c.c.	7·5	Do.

26,278. You draw from these experiments an inference that Galeotti's serum is not appreciably more toxic that the normal serum?—To normal animals it does not seem to be appreciably more toxic.*

26,279. In view of the fact that you found that animals that have received Galeotti's serum have died faster, when inoculated with plague, than normal animals, do you infer that there are probably unneutralised toxines in Galeotti's serum?—Yes.

* For further confirmation of this result *vide* Table V. in Lieut. Liston's evidence below.

26,280. Have you made any experiments with a view to determining whether a more effective therapeutic serum could be obtained by combining together two or more varieties of such sera?—Yes, I took two rabbits and inoculated them preventively, with a very large dose of Roux' serum. I then further gave them an injection of plague bacilli. These rabbits had high temperature after this, but they both recovered. One rabbit had received 35 c.c. of Roux' serum, and the other had received 55 c.c. After ten days they seemed perfectly well, and some of their blood was drawn off, and the serum collected. Then the following experiments was made:—

TABLE V.—EXPERIMENTS made to DETERMINE the PROTECTIVE POWER of various ANTI-PLAGUE SERA and of various COMBINATIONS of ANTI-PLAGUE SERA.

Number of Guinea-pig.	Nature and Quantity of the therapeutic Serum which was injected.	Dose of Plague Bacilli which was injected.	Date on which the foregoing Injections were made.	Result.	Remarks.
No. 1 (control)	Nil	$\frac{1}{12}$ agar tube	14.3.99	Died 18.3.99	
„ 2 (control)	Nil	„	„	„ 19.3.99	
„ 3	2 c.c. Roux' serum (horse 21)	„	„	„ 23.3.99	Much local reaction.
„ 4	2 c.c. Galeotti's serum	„	„	„ 17.3.99	
„ 5	2 c.c. serum from immunized rabbits.	„	„	„ 19.3.99	
„ 6	1 c.c. Roux' serum 1 c.c. Galeotti's serum	„	„	„ 21.3.99	Much local reaction.
„ 7	1 c.c. Roux' serum 1 c.c. immunized rabbits' serum	„	„	„ 23.3.99	„ „
„ 8	1 c.c. Galeotti's serum 1 c.c. immunized rabbits' serum	„	„	„ 17.3.99	
„ 9	0·5 c.c. Roux' serum 0·5 c.c. Galeotti's serum 0·5 c.c. immunized rabbits' serum	„	„	„ 19.3.99	
„ 10	1 c.c. immunized rabbits' serum	„	„	„ 22.3.99	Much local reaction.
„ 11	0·5 c.c. immunized rabbits' serum.	„	„	„ 17.3.99	

In each case the serum and plague bacilli were injected into opposite sides of the body. The serum was in each case injected a few minutes before the plague bacilli. In every case plague bacilli were found after death in pure culture in the internal organs.

Each of the guinea-pigs which were employed for this experiment weighed approximately 350 grammes.

26,281. Have you noticed a characteristic smell in plague cultures?—Yes.

26,282. Do you think that the smell is an important aid in diagnosing plague?—I do not think it is very important. I have never found anything smelling like it myself.

26,283. Have you made any experiment to determine whether it is possible to infect an animal with plague by means of the food?—Two experiments were made. In the one case two rabbits were taken. They each had a whole agar culture of plague, emulsified in broth, put into the stomach by means of a stomach tube. One of those died 21 days afterwards, but no plague bacilli were found in any of its organs, and neither did we find anything resembling plague. The other one is still alive. In another case, a mouse was fed upon the spleen of an infected rat, and it died. Plague bacilli were found in its organs, but you cannot say that that is a proof that it died from eating the spleen.

26,284. With regard to the monkey that died after the injection of plague bacilli into the vagina, were there buboes in the groin?—There were buboes in both groins, and the lumbar glands were very much enlarged.

26,285. Do you think that the prevalence of inguinal buboes may stand in relation with the fact that plague may be absorbed into the system by way of the genital tract?—They might be.

26,286. (*Dr. Ruffer.*) Did you try to infect a male monkey by means of the genital tract?—Yes, but it was unsuccessful. The monkey had been used for another experiment.

26,287. (*The President.*) In those experiments in which you tried the antidotal power of the serum, how did you

pproximately determine the dose of virus so as to now that you gave the same dose of virus to each of he animals?—A mass of culture plague was taken, nd a certain quantity of broth was put into that; all he bacilli were scraped off the surface of the agar, and nade into an emulsion with this broth; then it was lrawn up into a syringe, and an eqnal quantity given to each.

26,288. What was the relationship of this dose to the smallest dose which you gave of the serum? How far lid it exceed the smallest quantity?—One-tenth of a nass of culture of an agar tube was given in one case, nd in the other I think it was the same.

26,289. Have you any data to show what relation-hip the dose you administered would bear to the smallest dose that would kill the animal?—I do not know what the smallest dose is, it being impossible, as far as I know, to find out the minimum lethal dose of a living virus.

26,290. You do not know whether it was 100 times more or 50 times more?—Certainly not.

26,291. I suppose it is impossible, if you have an enormous amount of virus introduced into an animal, to prevent death, by any anti-toxic serum, in that animal?—Certainly; all the animals died. Those who were inoculated with Galeotti's serum died before the animals which had nothing but plague.

26,292. You do not know how much above the quantity that was capable of producing death, the dose of the virus which you administered in these test experiments had been?—Certainly not.

26,293. It may have been 100 times?—It may have been 100 times.

26,294. (*Prof. Wright.*) Do you infer from the fact that the controls lived for four or five days in each experiment that the dose of plague administered cannot have been very great?—It cannot have been very excessive.

26,295. Do you think it could have been 100 times?—I should think that it would have killed quicker if it had been 100 times.

26,296. (*The President.*) Did you record the weight of the animals in all those cases?—The guinea-pigs were picked as nearly the same size as possible. They were not weighed.

26,297. In the series of observations on patients which you have made, I observe that you do not always state the day of illness. Could you put that in?—It is put in in every case except one in that series.

26,298. There are two or three here in which it is not put in. Do you think it would be possible to obtain that information? If you could fill in this column it would be very important?—It is filled in on the notes if it is not filled in on the chart.*

26,299. In those cases where you have found the plague bacillus in the sputa, which also were cases in which the lungs or bronchi were affected, did you look for pneumo-cocci?—We looked for them microscopically.

26,300. Did you find any pneumo-cocci?—Certainly.

26,301. Always?—No, not always; we found it as frequently in ordinary normal sputum.

26,302. Did you examine any discharges from the nares which had not blood in them?—No. I only did one case.

26,303. Did you make any experiments by introducing the virus with food substances into the alimentary canal?—It was a pure culture that was put down into the stomach of a rabbit by means of a stomach tube.

26,304. Not mixed with food?—No.

* *See* Appendices Nos. LXXIX., LXXX., and LXXXI. in this Volume.

(Witness withdrew.)

Lieut. H. J. Walton, M.B., F.R.C.S., I.M.S., called and examined.

26,305. (*The President.*) I believe you are in the Indian Medical Service?—Yes.

26,306. You have also been engaged in special laboratory investigations with plague virus, I think?—Yes.

26,307. What are your medical qualifications?—M.B., London University, F.R.C.S., England.

26,308. Would you tell us what previous experience you have had in similar work?—I had fifteen months' plague duty before I came here; I had five months' experience of plague work on the railways, two months in Bombay city, three months at Satara, and five months in the Punjab.

26,309. What laboratory experience have you had prior to this?—I have worked at St. Bartholomew's Hospital, under Dr. Kanthack, and I have worked at Netley.

26,310. You first endeavoured to determine the susceptibility of various animals to plague?—Yes, I did.

26,311. Will you give us the information you obtained with regard to rats?—I find the Bombay rats are very frequently immune. If you have two rats and give them equal quantities of the same plague emulsion, one will sometimes die and the other will show no symptoms at all. I have notes of five rats which were inoculated with plague from a source which was known to be virulent to other rats, and they showed no symptoms at all. One rat I have now which was inoculated three times with very large doses, but it has never had any symptoms of any kind. I have also tried the effect of passing plague through rats—killing a rat with plague; taking its spleen and emulsifying it and injecting it into a second rat, and when the second rat dies, into a third and so on. I have done two series of experiments. The second series was a double series, so that practically there were three series of experiments. I have never succeeded in killing more than three rats in succession. The spleen was emulsified at once and injected at once; it was not allowed to grow in broth, it was emulsified and injected immediately. I emphasize that because I think it is possible that if a spleen were taken from a rat and then allowed to grow in broth, in a short time it might become more virulent. I have no experiments on the subject, as I have not had time. I find that by this method of taking a series of rats the first rat usually dies quickly—in three or four days; I have killed them in two days. The second and third rats generally take longer to die. I have never got beyond the third rat. One of these third rats, died after six days. Its blood and all its organs were examined but no bacilli were found at all. Its heart's blood was planted on agar and nothing grew. I have also tried to infect rats through the nose by dropping broth cultures of plague upon the nostrils. I have done that twice, but I did not succeed in killing the rats.

26,312. The result seems to show that there is no intensification of the virus?—Yes; but on the contrary, I think the virulence diminishes.

26,313. There was attenuation?—I think so. This is probably not quite the method of Nature. The spleen was taken out as soon as the animal was dead and immediately injected.

26,314. You have not endeavoured to obtain cultures from the blood or to isolate the bacillus in the blood in each of those generations?—I have in the third rat, of the second series. I did not in the first series, because I expected to kill more rats. But in the second series I did, and as I say, I could grow nothing and I could see no organisms in it.

26,315. Have you made experiments with pigeons also?—I have made three experiments on pigeons by injecting large quantities of plague under the skin, but none of the pigeons died.

26,316. Have you experimented on cats?—I only inoculated one cat. That was inoculated under the skin also and it died after six days. It seemed to be rather resistant. There was a good deal of local reaction—I think more local reaction than I have seen in any other animal. Its blood and all the organs were full of plague.

26,317. Did you give a very large dose?—Yes, it had a large dose.

26,318. What has been your experience with regard to monkeys?—I have inoculated monkeys subcutaneously, intravenously, by the trachea, by the conjectiva, and by the nose, and they have died with all those methods. The shortest time was the subcutaneously inoculated monkey which died in two days. Three or four days is about the average. I have notes here of two, three, and four days.

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26,319. In the case of the intravenous injection?—One monkey died after two days. Here again, I have four, four and two days. In one of those four I am not quite sure it was really intravenously inoculated; I think that the cannula may have leaked a little.

26,320. Have you administered plague virus by the digestive canal?—I have tried that on mice, but not on monkeys.

26,321. What was the usual period between stomach-administration and death?—Subcutaneously, I think, in monkeys it was between three and four days.

26,322. With regard to mice, did you find them very susceptible?—I found mice very susceptible indeed.

26,323. How did you ascertain that?—I once intended to inoculate one mouse with plague, but the tube from which I was going to inoculate it was mislaid. I had already used up the contents of the syringe on another mouse, so I pricked this mouse with the empty syringe. There might have been a very little adhering to the inside of the needle. The mouse died as quickly as the one that had received a considerable dose under the skin. Another thing is that they die very quickly. You can kill a mouse with quite a small quantity of plague in four hours. I have killed mice in four hours by injecting into the peritoneum. Under the skin, I think the shortest time was about eight hours. These were all tame mice. I have not tried any wild ones.

26,324. Did you find the virus of plague in these mice?—Yes. All the organs were full of plague bacilli and the blood was full of plague bacilli. One mouse I fed on the spleen of a plague rat. The spleen was very full of bacilli and the mouse was given water, but no other food. I examined the mouse before I put it in, and I saw no abrasions or cuts of any sort. It died in two days. Some of the spleen had disappeared. It had no other food. The spleen and the liver contained numerous plague bacilli.

26,325. Within what time did death occur?—Two days. That is the only experiment I have done by the mouth.

26,326. Were plague bacilli found in the organs?—Yes, in the spleen and in the liver.

26,327. Have you administered the virus in any other way in the case of mice?—Yes. I have injected into the peritoneum and subcutaneously.

26,328. Are those the only two ways?—Yes, and that one instance by the mouth. Those are the only ways I have tried.

26,329. You have made experiments with earth, I believe?—I did some experiments with earth to see if I could find any bacillus that resembled plague, or bacilli that were plague. Dr. Spencer was examining Bombay earth and I was trying earth from other sources. I examined a sample sent from Calcutta. There did not seem to be anything special about the history of the earth. It came from under a staircase in a house, but there was no note that there had been plague in the house. I plated this earth on agar after putting it in broth. A great many things grew, and I managed to isolate seven distinct micro-organisms. Amongst these there was a bacillus which looked like plague under the microscope. It stained in the way that plague does, and it was very much of the same shape as plague. On the original plate this bacillus grew unlike plague; I had no suspicion; it did not look at all like plague. It was a small round pale colony with concentric rings round it. It grew very slowly; I think it was six or seven days before I saw this colony on the plate. I have a note here that at the end of six days the diameter of the colony was about that of a pea. It was not a very fast-growing bacillus. The bacillus is about the size of the plague bacillus, but it differs from it in being distinctly motile. It does not show a stalactite growth in broth. Another thing I found was that if I made repeated sub-cultures on agar, in the later sub-cultures it grew very much faster, although originally of slow growth. After 24 hours there was a good growth in the later sub-cultures. It showed well-marked bi-polar staining, and it showed forms like the involution forms of plague. In fact, when I showed the bacillus to anybody I usually with a view to astonishing them, told them that it was not plague, because it looked so much like it.

26,330. Is it pathogenic?—Yes, it is pathogenic. It kills mice and guinea-pigs when injected subcutaneously or into the peritoneum. Mice die in about six hours when a small quantity of emulsion is injected into the peritoneum. I also tried pigeons and rats, but it killed neither.

26,331. Did you find these bacilli in mice?—Yes, but I find that there is some difference between this bacillus recovered from animals and this bacillus as it grows on agar tubes, although, in the former case, it is still rather like the plague bacillus. I have noticed that the blood and the organs of animals which have been killed by this bacillus show many bacilli which are usually distinctly longer and more pointed than those from cultures. In some cases this character is very marked indeed. Sometimes this bacillus stains very irregularly, especially when from the peritoneal cavity; in fact, the conditions in which it most closely resembles plague are when it is taken from a young culture on agar. Morphologically, it is then almost identical with plague. It is pathogenic; it kills mice and guinea-pigs, but it differs from plague in being motile and in being more pointed. In animals it is usually more pointed, but sometimes it is not. It looks very much like plague even from animals.

26,332. What are the pathological changes?—The animals seem to die with intense septicæmia; the blood and all the organs are full of bacilli.

26,333. There is no enlargement of the glands?—No, it produces no buboes.

26,334. You consider it is extremely like plague?—Yes. It was suggested to me that it might possibly be one of the forms of bacilli which produce lactic acid fermentation. I tried lactose broth, but it does not ferment it at all.

26,335. Are you quite certain in your own mind that it is not the plague bacillus?—Yes. I feel quite sure that it is not. It grows much faster than plague. When you look at it very critically it is not quite the same as plague. It does not produce buboes in animals, and it is a motile bacillus.

26,336. You have also made experiments as to the escape of plague from animals?—Yes, but I have not done very many, and I have not had much personal experience of it. I have tried human urine. I have done five or six cases. I once drew off some urine from a plague patient in one of the hospitals seven hours before he died. This was centrifuged, and the sediment was planted out, and I grew from that a bacillus which, in its cultural characteristics and morphologically, closely resembled plague, but it did not appear to be pathogenic, as it failed to kill a rat and a mouse.

26,337. Was it alive?—Yes, it grew. From the blood of the same case a bacillus was grown, which was apparently the bacillus of plague, which did kill rats. At another time I grew a bacillus from the urine of a dead monkey which had been killed by plague; this bacillus seemed to be that of plague. In both these cases, when plague was grown from urine, the plague grew very slowly. I have an impression in my own mind, from these cases and from other cultures I have seen from urine, that the bacillus of plague as obtained from urine grows very slowly. The first cultures, at any rate, grow more slowly when it is from urine than from the blood.

26,338. Are there any other fluids which did not retard the growth of plague?—I do not know. I have no experience of them.

26,339. Have you made experiments with sputum?—Yes. There was one case here at the Port Trust Hospital. The sputum was sent up to the laboratory one day; it contained a great many micro-organisms and also a great many bacilli which appeared to be the bacilli of plague, they were exactly like plague, under the microscope, and they stained like plague. I went down to see this case and all I could find was that the patient was said to have been ill for six days. He had the physical signs of broncho-pneumonia on both sides of his chest; there were no definite areas of dulness; there were râles and rhonchi all over his chest, and he had no bubo. He was treated by intravenous injection by Roux' serum; he had 40 c.c. The next day I heard he was very much better. I saw his sputum again; there were no bacilli like plague in it and the man recovered.

26,340. Have you found much difficulty in recognising the plague bacillus in animals?—Yes.

26,341. What are the chief difficulties?—One difficulty is that it appears to be sometimes associated with other bacilli. When an animal dies of plague, you sometimes find other bacilli in it, as well as plague

bacilli. There was a monkey which was killed by injecting plague culture into its trachea, and I made cultures from the lung, the heart's blood, the spleen, and the liver. The cultures from the liver and spleen grew like ordinary plague; the cultures from the lung, which showed a copious growth on agar after 24 hours, looked like plague when examined microscopically, with many involution forms. It was emulsified and injected into the peritoneum of a mouse, which died in four hours. In the peritoneal fluid from this mouse there was a capsulated bacillus. I think it was a bacillus, or at least, a cocco-bacillus. I tried a few experiments with the bacillus. It did not stain by Gram's method, and there was a suggestion that it might be Friedländer's bacillus. I tried this bacillus in lactose broth, and it certainly decomposed it. A great deal of gas was given off, but the gas apparently was not carbon dioxide, and it did not combine with caustic potash. The growth from the heart's blood of the same monkey grew abnormally fast, much faster than I have ever seen plague grow, but when it was injected into the peritoneum of a mouse, the peritoneal fluid and organs contained bacilli, apparently those of plague. One other micro-organism was found in the peritoneum of a monkey which was killed by subcutaneous injection of plague. This was a diplo-coccus, which also was capsulated. I have not done any further experiments with this; it only happened recently, and I know nothing further about it.

26,342. Then I believe you have made experiments on the effect of various external conditions upon the growth of the bacillus?—Yes, I have. I have tried the effect of light, darkness, and temperature on the growth of plague. I find that plague grows fastest in the incubator at a temperature of about 37° C. It grows faster at 37° C. than it does in the ordinary room temperature, taking that to be between 24° C. and 27° C. I tried to see if there was any difference in the rate of growth in darkness and in ordinary diffused light, but I could not satisfy myself that there was. I also tried experiments as to the effect of exposure of plague to direct sunlight. I have done a good many experiments on that, and the result has been very constant in one respect: I have never been able to recover from culture tubes living plague, either from broth or from agar, which have been exposed to sunlight for four hours. I have done several experiments, and exposed it for shorter periods. I have twice recovered it after three hours. In one experiment I did, a broth culture was exposed to the sun for three hours. A sub-culture was then made on agar. Nothing was seen for five days, when a few very minute colonies were noticed, microscopically very much resembling plague. After eleven days there was very little growth. The effect of sunning it for three hours apparently was that it retarded the growth. This culture was emulsified and injected into the peritoneum of a mouse, which died two days afterwards, with numerous bacilli, apparently plague, in the spleen, liver, and blood. Then I repeated the above experiment with an agar tube, and there again I recovered plague after it had been exposed for three hours to sunlight. I also did some experiments by exposing to sunlight plague infected pieces of calico. In one experiment I took six square pieces of thin calico, steeped them in plague culture, and exposed them for three hours to strong sunlight in sterilized Petri dishes in the following manner:—One was covered with one layer of calico, one was covered with two layers of calico, one was covered with three layers of calico, one was covered with four layers of calico, one was covered with five layers of calico, and one was covered with six layers of calico, and exposed for three hours to sunlight. Each piece of calico was then placed in broth for several days. No growth took place, even from the one which had been covered by six layers of calico. I did another sunning experiment with flannel. I took five square pieces of flannel; they were well washed and sterilized, soaked in plague culture, and exposed for 3½ hours to sunlight in a Petri dish with the cover off. One was covered with one layer of flannel, one with two layers, one with three layers, one with four layers, and one with five layers of flannel. When removed and placed in bouillon, typical plague growths resulted, but No. 1 only showed growth after four days. Another experiment was done with the hem of an ordinary calico pocket handkerchief. It was soaked for three days in plague culture; it was then exposed for three hours to strong sunlight in a Petri dish. It was then placed in bouillon. It showed no growth for nearly a week, but then grew vigorously. That rather confirms the previous experiment as to the retardation of growth by exposure to sunlight. Another experiment was done with cotton wool. Ordinary cotton wool was twisted so as form a thickness of ordinary knitting wool. This was soaked in a plague culture, removed to a Petri dish, and exposed to strong sunlight for three hours. When removed to bouillon it showed vigorous growth within 24 hours.

26,343. Have you, in any of your experiments, found sunlight to completely kill the bacillus?—I have never grown it after four hours' exposure.

26,344. What have you to say with regard to desiccation?—Pieces of calico were thoroughly soaked in a plague culture, and then removed to sterilized Petri dishes, and allowed to dry at the ordinary temperature of the room, and in diffused light. Cultures were obtained from a piece of calico for the first four days, and then the experiment was interrupted for six days. On the 11th day no cultures could be obtained. Apparently for the first four days plague can be recovered. I know nothing between four and 11 days; on the 11th day I could not recover it. I also tried another way, by introducing calcium chloride into the same Petri dish. I put sterilized cotton fabric soaked in plague emulsion into a Petri dish in which calcium chloride was placed. At the end of 18 hours the cotton was placed in broth. No growth resulted. Of course, the calico was very dry indeed.

26,345. What has been the general result of your desiccation experiments?—That exposing a plague culture for at least four days to ordinary drying does not kill plague. Of course, there is no reason why it should. It depends upon the amount of broth you use. Apparently it takes four days for a piece of cotton soaked in broth to become so dry that plague will not grow upon it, yet if it is dried very rapidly with calcium chloride, the period of time is much shorter.

26,346. After absolute dryness, how long will the plague bacillus live?—I could not obtain it after 18 hours in one case: I am still experimenting on that subject.

26,347. I believe you have also tested the resistance of plague to various disinfectants?—I have done a few experiments with regard to the resistance of plague to disinfectants. In one experiment I did, an emulsion of plague was made, and of this ½ c.c. was added to the following tubes:—1 in 1,000, 1 in 2,000, 1 in 5,000, 1 in 10,000 and 1 in 20,000 perchloride of mercury. After half-an-hour a loop-full from the above tubes was planted in broth for 12 hours, in order, as far as possible, to eliminate the admixture of the bacillus with the disinfectant. Then sub-cultures were made on agar from each broth tube. I did this with plague, and also with a coccus, which I have found very resistant in other experiments. I found that I could grow the coccus from the tube of 1 in 20,000, but I could not grow plague from any of them. Plague did not grow after being exposed to perchloride of mercury 1 in 20,000 for half-an-hour.

26,348. Did you observe any characteristic odour in the cultivations from broth?—Yes; I think that plague cultures have a very characteristic smell. I think you can smell it in agar, but you can smell it best in broth. I once distilled a broth culture, and the distillate smelt very strongly indeed. It was the same smell, but in a more concentrated form.

26,349. Can you describe the smell?—It is very difficult to describe a smell. It is a very unpleasant smell; I do not think I can describe it any better than that.

26,350. Have you made any experiments on the disinfection of houses?—I went to one house in the C. district in Bombay. Scrapings from the floor and wall were collected before and after disinfection by the disinfecting gang. I was informed that the disinfectant used was perchloride of mercury in the strength of about 1 in 500. A few c.c. of broth were added to the earth samples, and the pledgets of cotton wool which had been used to collect samples of the wall were dropped into broth. The samples were collected immediately after disinfection, while the floor and wall were still wet. Consequently a good deal of the disinfectant was present in the samples. They were plated in Petri dishes. After 24 hours there was no appreciable difference in the number of colonies from the wall before and after disinfection. I do not know what the explanation is. Perhaps it is that the wall is greasy, and the disinfectant does not remain there very long;

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but that is how it was. But in the floor samples there was a difference. The method of disinfection which they adopted was, first to enter the room and squirt vigorously on the floor; then they squirted the walls and the roof, and the disinfectant ran down on to the floor, so that the floor was a kind of pond at the end of the process. I found that the floor samples, which had been taken before disinfection, grew very vigorously, but no growth occurred for the first 24 hours in floor samples which had been taken after disinfection but after three days there was a growth in all of the samples taken, but not of plague. I was not looking for plague; I was trying to find if the process destroyed the ordinary organisms.

26,351. In none of these cases did you identify the plague bacillus, I suppose?—No. There was another experiment which I did, namely, trying the effect of carbolic acid on a wall. A wet sterile cotton wool plug was wiped on the wall and a broth tube inoculated with it. Then five per cent. carbolic acid was squirted over a portion of the wall, which was covered with a Petri dish to prevent air organisms getting there, and left for 10 minutes. Then a second broth tube was inoculated in the same way as the first had been. After 24 hours there was abundant growth in the control tube, and after 48 hours the second tube showed colonies of a spore-bearing bacillus. The result was not very satisfactory.

26,352. It was not very much in favour of carbolic acid?—No.

26,353. What has been your experience with regard to the treatment of plague cases with anti-toxic serum?—I have only treated 15 cases altogether.

26,354. With what serum?—With Roux' serum, from horse No. 31 and horse No. 21. One case I treated at Bangalore. It was a hopeless case. The girl had extensive broncho-pneumonia and was very weak when she came in. She had 28 c.c. of serum and died. Most of my cases were done at the Modi Khana Hospital in Bombay. Another case that I treated, and which I wish to be considered in a separate category, was one at the Port Trust Hospital here. The reason I put it into a separate category is that I had no control. In the Modi Khana cases I had controls to all. This case, in the Port Trust Hospital, is the man I spoke about before, in whose sputum I saw what I believed to be plague bacilli. He had 40 c.c. of the serum injected into his veins and recovered.

26,355. Within what time had he the 40 c.c.?—He had that at once. Then at the Modi Khana Hospital I treated 13 cases with 13 controls.* Alternate cases were taken. I had no voice in the selection of the cases. The doctor in charge of the hospital gave me a case as it came in and said "So-and-so is your control"—that was, as a rule, the next man who came in. I treated 13 cases, of whom 10 died, being a mortality of 76·9 per cent. All the 13 controls died. This was about six weeks ago, when plague was exceedingly virulent. The mortality of the cases treated was 76·9 per cent., and the mortality of the controls was 100 per cent., but of course the numbers are so small that you cannot draw any positive conclusions from those experiments.

26,356. Have you made any experiments on the effect of the serum on the pulse?—I could detect no constant effect of the serum on the pulse, respiration, temperature or symptoms.

26,357. Was the duration of life longer or shorter in the case of those treated with the serum than in the case of the controls?—At first I rather thought myself that it was somewhat longer, but I took the trouble to count the number of days that the fatal cases lived, both those treated with the serum and controls, and I found there was no difference.

26,358. What were the separate doses and total amounts given of the serum?—The most I gave was 220 c.c. to a man who recovered.

26,359. In what time?—He received on an average about 60 c.c. a day.

26,360. For about four days?—Yes.

26,361. What was the largest amount you gave at one time?—The most I ever gave at a time was 40 c.c.

26,362. This was to the man who had 220 c.c. altogether?—Yes, he had 40 c.c. at a time, but so had most of my cases.

26,363. What were the other totals?—They varied between 40 c.c. and 220 c.c.

26,364. I suppose they often died before they had received much serum?—Yes. Very often I would give a man one injection and when I went round in the evening he was dead, so that he never received more than 40 c.c.

26,365. Did you notice much local irritation produced by the larger injections?—No; I have never noticed the slightest local irritation. There is one point about these cases which I should like to mention, and that is with regard to the frequency of lung symptoms. By lung symptoms I mean symptoms of broncho-pneumonia, which seem to be very common in the present epidemic. I find that of the 13 cases I treated, eight had lung symptoms.

26,366. What about the controls?—I cannot tell you about them, but I presume they would come out about the same, because they were not picked in any way. Two of the cases which recovered had lung symptoms, and there was also that case at the Port Trust Hospital which had lung symptoms and recovered.

26,367. On the whole these cases were very unfavourable for any treatment, were they not?—Yes.

26,368. Have you anything else to add?—I have nothing else to say, except with regard to the mortality at the Modi Khana Hospital, among cases treated with Roux' serum. Lieutenant Douglas and I were working together there. We added our cases up and found that the percentage mortality was 81·48, and that in a similar number of controls the percentage mortality was 85·18. But the total number of cases was only 27, so that I do not think you can draw any conclusions in favour of Roux' serum from that.

26,369. (*Prof. Wright.*) You do not think the efficacy of the disinfection of a house can be properly tested by merely comparing the number of bacteria before and after disinfection?—That seems to me to be the only way available. Of course the fact that you cannot find plague is no evidence that a house has been satisfactorily disinfected, because under the most favourable circumstances it is very difficult to recover plague bacilli.

26,370. When you find that the number of bacteria is not reduced by the disinfection process, does it always follow that the house has not been completely disinfected against plague?—No, I do not think so at all. I think there may be resistant micro-organisms and spore-bearing micro-organisms which would not be destroyed by a process which would kill plague.

26,371. You think the disinfection of a house would be adequate if you found that all the bacteria which survived had more resisting power than the plague bacilli?—Yes, I think it might be adequate.

* *See* App. No. LXXXI. in this Volume.

(Witness withdrew.)

Lieut W. G. Liston, M.B., I.M.S.

Lieut. W. G. Liston, M.B., I.M.S., called and examined.

26,372. (*The President.*) You are a member of the Indian Medical Service, and have been on special duty with the Indian Plague Commission?—Yes.

26,373. You have been making special researches in connection with plague?—Yes.

26,374. When did you commence them?—On the 5th December.

26,375. I believe you have endeavoured to determine the sterility or otherwise of Haffkine's fluid?—Yes, I have examined twelve bottles. To test each bottle, two agar tubes were used. The first of these was inoculated with a small quantity of undiluted vaccine. The second was inoculated with exactly the same quantity of vaccine as the previous tube, but this time diluted with four volumes of sterile broth. In all twelve bottles were examined. The date on which the vaccine was prepared was not noted on the bottles. Care was taken to choose only those bottles that had the sealing wax on the corks intact, otherwise the bottles were chosen at random from samples of almost every brew which M. Haffkine had in his laboratory at the time, and which he had kindly sent for examination. Of these twelve bottles nine were found to be sterile, viz., brews

Nos. 9029, 6174, 3927, 6161, 4552, 6112, 4739, 6053, 3993. Three were found to contain micro-organisms, viz., brews Nos. 6151, 3895, and 3911.

No. 6151. Only a single colony grew in the undiluted tube.

No. 3895. Copious growth in undiluted tube of an oval bacillus non-motile, and forming small transparent colonies; similar growth in the diluted tube.

No. 3911. Thirty to forty colonies in the undiluted tube. Nine small colonies in the diluted tube.

26,376. In any of these cases were there any organisms which at all resembled the plague organisms?—No.

26,377. Do you know whether any of the detected organisms are pathogenic or not?—No.

26,378. You have made experiments on the relative toxicity of the supernatant fluid and sediment of Haffkine's fluid?—Yes, and also protectively afterwards. In this experiment six guinea-pigs were used, three receiving doses of the supernatant fluid and three doses of the sediment. The brews used in the experiment were the following:—

Brew. No. 5344, dose 6¼ c.c.
" " 5338, " 6½ c.c.
" " 5344, " 6¼ c.c.
" " 5315, " 6¼ c.c.
" " 5975, " 6¼ c.c.
" " 4422, " 2½ c.c.
" " 4414, " 7½ c.c.

These bottles had been standing in the laboratory for some time, and their contents had separated into two portions, a supernatant brown clear fluid and a muddy greyish brown deposit. The bottles were carefully opened and the supernatant fluid drawn off with a sterile pipette. The sediment remaining was freed, as far as possible, of excess of fluid by means of the pipette; a thick emulsion was thus obtained. The supernatant fluids from all the brews were mixed in one flask and the sediments in another. The details of the experiment are given in the following table:—

TABLE I.

DETERMINATION of TOXICITY of the SEDIMENT and SUPERNATANT FLUID of HAFFKINE'S PROPHYLACTIC.

Number of Guinea-pig.	Component of the Prophylactic which was injected.	Dose of the foregoing Component which was injected.	Result.
Number 1 -	Supernatant fluid - -	1 c.c. per 100 grammes of guinea-pig's body weight.	Survived.
" 2 -	"	2 c.c. per 100 grammes of guinea-pig's body weight.	Died 5 days after inoculation. (*Note* 1.)
" 3 -	"	4 c.c. per 100 grammes of guinea-pig's body weight.	Died 4 days after inoculation. (*Note* 1.)
" 4 -	Sediment -	½ c.c. per 100 grammes of guinea-pig's body weight.	Survived.
" 5 -	"	1 c.c. per 100 grammes of guinea pig's body weight.	Survived.
" 6 -	"	2 c.c. per 100 grammes of guinea-pig's body weight.	Survived.

Note 1.—No micro-organisms could be detected after death in the bodies of these animals.

26,379. I understand that anything above 2 c.c. of the supernatant fluid killed them?—Yes, any dose above 1 c.c.

26,380. What is the minimum lethal dose of the supernatant fluid?—2 c.c.

26,381. The doses you used of the sediment did not exceed 2 c.c.?—No.

26,382. You had only one experiment in which you administered the supernatant liquid in the lethal dose of 2 c.c.?—Yes, we used larger doses of the non-carbolized supernatant fluid, and compared its toxicity with that of the nutrient broth in which the bacillus is grown. A series of guinea-pigs were inoculated with doses of 5, 10, 20, 30 c.c. of broth and non-carbolized supernatant fluid, respectively, eight guinea-pigs being used in all. These guinea-pigs were all inoculated on the 15th March. On the 19th all the guinea-pigs were well except one, which received 10 c.c. supernatant fluid, which died on the 18th, and was found to have in its liver and spleen a few groups of cocci, which stained by Gram's method, so that it is possible that this was an accidental death.

26,383. And could not have been due to the inoculation?—That is so.

26,384. It must have been poisoned in some other way?—Yes. The details of the series of guinea-pigs are as follows:—

TABLE II.

ESTIMATION of the RELATIVE TOXICITY of (*a*) the ORIGINAL NUTRIENT MEDIUM which is EMPLOYED in the PREPARATION of M. HAFFKINE'S PROPHYLACTIC; and (*b*) of the same NUTRIENT MEDIUM (un-carbolized but heated to 65° C. and filtered), after it has served as a CULTIVATION MEDIUM for the PLAGUE BACILLI. —(*Note* 1.)

Number of Guinea-pig. *Note* 2.	Kind of Broth, and Amount of same which was injected.	Temperature of Guinea-pig on the Day after Inoculation.	Final Result, One Week after Inoculation.
Number 1 -	Haffkine's nutrient medium, 30 c.c.	103° F.	Quite well.
" 2 -	Haffkine's nutrient medium, 20 c.c.	102·2° F.	" "
" 3 -	Haffkine's nutrient medium, 10 c.c.	102·8° F.	" "
" 4 -	Haffkine's nutrient medium, 5 c.c.	102° F.	" "
" 5 -	Filtrate from plague culture grown in above medium, 30 c.c.	102·6° F.	Quite well, except for fact that there is a small patch of necrosis at site of inoculation.
" 6 -	Filtrate from plague culture grown in above medium, 20 c.c.	102·4° F.	Quite well, except for small patch of necrosis at site of inoculation.
" 7 -	Filtrate from plague culture grown in above medium, 10 c.c.	96·8° F.	Died, probably of intercurrent infection (*vide* text *supra*) 3 days after inoculation.
" 8 -	Filtrate from plague culture grown in above medium, 5 c.c.	103·4° F.	Quite well.

Note 1.—M. Haffkine kindly furnished the material which was employed in this series of experiments. The material consisted of (*a*) a large flask of freshly prepared nutrient medium, such as he employs as a cultivation medium for plague bacilli in the preparation of his plague-vaccine; (*b*) a large flask of the above nutrient medium which had served as a cultivation medium for the plague bacilli, and which had just been heated to 65° C. with a view to converting it into plague-vaccine.

Note 2.—The guinea-pigs which were employed for this experiment were afterwards (*vide* Table VI. *infra*) submitted to a test with a view to ascertaining whether they had acquired any immunity.

It would, therefore, appear that the toxic element of the supernatant fluid of Haffkine's vaccine for guinea-pigs is, probably, the contained carbolic acid.

26,385. You have made some experiments with regard to the protective power of the fluid and of the sediment?—Yes, I made the following experiment. The guinea-pigs which were used were those which survived the experiment reported in Table I. above, and one control guinea-pig. There were three guinea-pigs which survived the doses of sediment and one which survived the dose of supernatant fluid.

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TABLE III.

DETERMINATION of the respective PROTECTIVE POWER of the SEDIMENT and SUPERNATANT FLUID of HAFFKINE'S PROPHYLACTIC.

Number of Guinea-pig.	The Guinea-pig had previously been injected with	Interval that had elapsed since the previous Injection.	Amount of Plague culture which was now injected.	Result.	Remarks.
No. 1 * *Note* 1 (control.)	Nil	-	½ agar culture	Died 6 days after	Plague bacilli; abundant small nodule in spleen.
No. 2 (No. 1 of Table I. *supra*)	Haffkine's supernatant fluid, 1 c.c. per 100 grms. of body weight.	8 days	Ditto	Died 24 hours after.	No local reaction or buboes; no nodules in organs; abundant plague bacilli.
No. 3 (No. 4 of Table I. *supra*)	Haffkine's sediment, ½ c.c. per 100 grms. of body weight.	Ditto	Ditto	Died 9 days after.	Marked local reaction, nodules in lungs.
No. 4 (No. 5 of Table I. *supra*)	Haffkine's sediment, 1 c.c. per 100 grms. of body weight.	Ditto	Ditto	Died 5 days after	Some local reaction.
No. 5 (No. 6 of Table I. *supra*)	Haffkine's sediment, 2 c.c. per 100 grms. of body weight.	Ditto	Ditto	Died 5 days after	Some local reaction.

* *Note* 1.—It is to be noted that this guinea-pig may have been, to some extent, immune against plague, inasmuch as he was an animal which had survived an epidemic of plague which had occurred among the guinea-pigs in another laboratory.

26,386. You have further made some experiments on the longevity of the bacillus in grain and gunny bags?—I made the following series of experiments:—

I took two pieces of gunny bag and two samples of wheat. I placed each of these in a separate test tube and sterilized them and labelled them respectively tubes 1, 2, 3, and 4. I then thoroughly moistened the contents of these four tubes with a broth emulsion of a plague culture on agar. The contents of the tubes were then allowed to become air dry by placing them in an incubator.

Two days afterwards I introduced some sterile broth into tube 1 (one of the tubes which contained grain), and shook it up after allowing it to remain in contact with the grain for a little time. A portion of the washings of grain which were thus obtained were introduced into an agar tube. The rest of the washings were injected into *guinea-pig No.* 1. This guinea-pig remained well.

At same time some of the grain from tube No. 2 was introduced into a tube of sterile broth.

At same time a piece of gunny bag from tube No. 3 was treated in the same way as the contents of tube No. 1. A portion of the washings of the gunny bag was introduced into an agar tube. The rest of the washings were injected into *guinea-pig No.* 2. The guinea-pig remained well.

At the same time a portion of gunny bag from tube No. 4 was introduced into a tube of sterile broth.

Two days after I had performed the above operations, both of the agar tubes (*i.e.* the agar tubes which had been inoculated respectively with the washings of the grain and with the washings of the gunny bag) showed three or four colonies of bacilli, which had the microscopical appearance of plague bacilli. At the same time both the broth tubes (*i.e.* the broth tubes which had been inoculated respectively with a portion of dried grain from tube No. 2 with a portion of dried gunny bag from tube No. 4) showed a growth.

I inoculated 1 c.c. of the broth, into which the dried grain had been introduced into *guinea-pig No.* 3. Further, I inoculated 1 c.c. of the broth, into which the dried gunny bag had been introduced into *guinea-pig No.* 4. Two days after the *guinea-pig No.* 4 *died.* Plague bacilli were recovered from its spleen and liver. On the next day, *guinea-pig No.* 3 *died.* Plague bacilli were recovered from spleen, liver, and peritoneal fluid.

In view of the above results a further portion of the grain and a further portion of the gunny bag which had been infected with plague seven days previously were introduced into two further sterile broth tubes.

Two days after this 1 c.c. of the contents of each of these broth tubes was introduced respectively into *guinea-pigs* 5 *and* 6.

Guinea-pig No. 5 (*i.e.*, the guinea-pig into which the broth culture which was derived from the gunny bag was injected) died three days after he had been inoculated. There was evidence of considerable local reaction, but plague bacilli were recovered from spleen and liver.

Guinea-pig No. 6 (*i.e.*, the guinea-pig into which the broth culture which was derived from the grain was injected) showed slight local reaction three days after he had been inoculated. This guinea-pig developed no further symptoms.

In view of the result which was obtained with guinea-pig No. 5, a further portion of the gunny bag was planted out into sterile broth 12 days after it had been infected with plague.

This broth was cultivated for three days, 1 c.c. of the broth was then introduced into *guinea-pig No.* 7. The guinea-pig remained perfectly well.

It has thus been possible to recover the plague bacillus by continued inoculations from infected grain up to the third but not so late as the seventh day. In the case of the infected gunny bag it has been possible to recover the bacillus up to the seventh day, but not as late as the twelfth day after infection.

26,387. You made some experiments as to the growth of plague bacilli in an atmosphere of hydrogen?—Yes. The plague bacillus was grown in broth in an atmosphere of hydrogen gas. The growth was not rapid, and it did not differ much from the growth in broth exposed to air. In view of the fact that the plague bacillus appeared thus to be a facultative anaerobe an attempt was made to obtain the plague bacillus from earth floors of three infected houses by making, first, a cultivation in hydrogen and afterwards a cultivation in air.

Sources of the earth:—

I. Collected on 8th February 1899, from 25, Bori Street, one fatal case of plague had occurred in the house.

II. Collected on 8th February 1899, from hut in Victoria Bandar, fatal case of plague had occurred in the house on 8th February 1899.

III. Collected on 8th February 1899, from hut in Victoria Bandar, a plague case was sent to hospital from the hut on 8th February 1899.

Small quantities of the earth were mixed with broth and a portion of this broth (about ½ c.c.) was added to sterile broth. Hydrogen gas was then passed through the broth and the tubes sealed. A control broth culture of pure plague was at the same time made in hydrogen, and when this tube showed a distinct growth (36 hours later) the tubes in which the earth organisms were being cultivated were planted out in three dilutions on agar plates. These were examined from time to time during four days for any colonies resembling plague; any such were examined microscopically always with negative results. A similar series of experiments were done with the same samples of earth which had been and were artificially infected with plague bacilli. Not even under these circumstances could the plague bacillus be isolated from the earth by this method.

26,388. The whole series has been purely negative? Yes.

26,389. You have endeavoured to find the bacillus in the urine of plague patients?—Yes. The urine of 50 plague patients was examined bacteriologically, and the plague bacillus was isolated in two cases only. The

ıethod of collecting the urine is open to some riticism. It was accomplished in the following ıanner:—The patient was made to pass water, and fter a small quantity had been passed, a little was ollected in a sterile tube. This urine was allowed to tand for a few hours till the mucus, &c. sank to the ottom of the tube. With a sterile pipette a small uantity (about ½ c.c.) of the urine from the surface f the deposit at the bottom of the tube was with-rawn and planted on agar. In only one case was he urine thus obtained found to be sterile. Almost lways a rapidly growing micro-coccus was found.)ccasionally other bacteria were found. Any colonies vhich at all resembled plague colonies were examined, ınd when a bacillus microscopically resembling plague vas found it was isolated and grown in pure culture. Some of the pure culture then was inoculated into a rat, ınd the micro-organisms were again recovered from the lead animal. In only two cases was the plague bacillus hus found, viz., in cases Nos. 65 and 280, Mahratta Hospital. In the latter case, 280, a sample of the blood obtained from the finger at the same time as the ırine gave a very abundant growth of the plague bacillus.

26,390. Can you give us some information as to the stage of illness when it was taken, and also how long before death?—Yes.

Case No. 65 was admitted into the Mahratta Hospital on 6th of January with right axillary bubo. His urine was obtained on the 12th, five and a half hours before death.

Case No. 280 was admitted into the Mahratta Hospital on 18th January 1899, with right femoral bubo. His urine was obtained on the 19th, four hours before death. The blood from the finger, taken at the same time, gave an abundant growth of plague.

26,391. What was the result of your experiments with regard to fæces?—I have tried four cases by plate cultivation and by successive strokes on agar tubes, three tubes being used; but in no case was I able to get the plague bacillus. The great difficulty was the rapidly growing contaminating micro-organisms.

26,392. Perhaps, in this instance, you will also state the period of illness?—Yes.

Case 226.—Bubo left inguinal. Admitted 16th January 1899; sample taken on 17th; died on 18th.

Case 284.—Right central bubo. Admitted 18th; sample taken on 18th; died on 19th.

Case 437.—Left axillary bubo. Admitted 24th January; sample taken on 25th. This patient recovered.

Case 530.—Left femoral bubo. Admitted 27th January; sample taken on 28th. Patient died on 30th January 1899.

26,393. You have examined the sweat in several cases?—Yes, and I never discovered plague bacillus.

26,394. Were they fatal cases?—I could not say.

26,395. Can you tell us the period of illness?—I am afraid I cannot give you that information.

26,396. You have also examined buboes, I think?—Yes. One bubo was examined by hypodermic needle; a small quantity of lymph withdrawn and planted on agar. This patient was recovering at the time of operation; his temperature being normal. Several colonies of staphylo-coccus albus developed, and a small diplococcus, but no plague bacilli. Another bubo was examined when opened, the patient still suffering from fever, and died some days later. A pure growth of plague bacillus was obtained from this case.

26,397. Was the bubo opened during the time of operation?—It was being opened. The discharge from another bubo, which had been opened some days, was examined, but no plague bacilli were found.

26,398. Have you ever found bacilli in a bubo?—Yes; on the one occasion which I have mentioned.

26,399. You have also examined the blood?—Yes. I have examined the blood from the finger on several occasions. I have no notes of the exact number of patients which I examined, but I frequently found the plague bacillus in the blood. It was from this source that we got nearly all our plague cultures.

26,400. Can you tell us the longest period before death when you found the bacillus?—The longest period that I ever found the plague bacillus in the blood before death was 51 hours.

26,401. I suppose you also found it at the moment of death?—Yes. I examined one case about one hour after death, and found the bacilli abundant in the veins.

26,402. What is your opinion as to the value of microscopic examination alone in detecting the plague bacillus?—It cannot be relied upon at all alone. The detection of plague by the microscope alone is exceedingly hard, indeed impossible. I have seen many bacteria, which, microscopically, look very like plague, and which, on more critical examination, as by rate of growth, character of colonies, and inoculation experiments on animals, have proved not to be plague. The size of the plague bacillus varies considerably, both in different growths and as regards the individuals in the same growth. A quickly growing plague bacillus on artificial media is, as a rule, a small bacillus, while a larger variety is a more slowly growing one. The individual members of a growth frequently vary from an almost coccus-like form to long thread-like individuals. Similar involution forms may be found in animals, but very rarely, the individuals are much more uniform in size, and, as a rule, stouter than the original growth on agar; bi-polar staining, too, is much better marked.

26,403. Have you any observation with regard to the persistence of plague after the death of animals?—With regard to the persistence of the plague bacillus after death, it is difficult to make sure that an animal has died of plague when decomposition has set in. Several such animals have been examined, and frequently a large thick spore-bearing bacillus has been found in all the organs and in the blood which apparently obscures or in some way destroys the plague bacilli. With a view to ascertaining whether any change occurred in an animal which was known originally to contain many plague bacilli the following experiment was made. A guinea-pig, which died of plague, was first examined shortly after death. Many plague bacilli were found in all the organs, bubo, and heart's blood. Other examinations were made two hours, six hours, and 26 hours after death, with the result that, despite the fact of the presence of many contaminating bacteria, the plague bacilli were abundantly evident even after 26 hours.

26,404. Were the bacilli dead or living?—I did not make cultivations.

26,405. What media have you used to cultivate the plague bacillus?—Various media were tried for the cultivation of the plague bacillus. An almost neutral agar or broth was found most suitable for ordinary use. The bacillus grew well, too, on glycerinated agar, but not well on glucose agar. Boiled white of egg did not make a good media, although the bacillus did grow slowly, forming peculiar involution forms with squarer ends than usual and many long forms. Sterile plantain was also tried, but the growth was not good or characteristic. The addition of varying proportions of anti-plague serum (Roux) to bouillon was tried. Those tubes in which the largest proportion of serum was present seemed to grow slightly faster. The serum of convalescent patients had no restraining influence on the growth of plague. Roux' serum on the surface of agar formed an excellent medium in which to grow the plague bacillus.

26,406. What do you infer from that fact with regard to the bactericidal property?—I infer that Roux' serum and the serum of convalescent patients outside the body has no bactericidal properties on the plague bacillus.

26,407. Have you paid some attention to the pathological appearances in the several animals on which you made experiments?—Yes, on rats. Little, generally, could be learnt as to whether a rat had died of plague by external examination. Buboes, although found on one or two occasions, were very rare. Inguinal axillary and parotid buboes were seen. As a rule, no indications were evident to point to the path of infection. The liver and spleen are always enlarged, soft, and friable. Frequently, no enlarged lymph glands in any situation could be detected. Microscopically, plague bacilli could be found in the heart's blood, often very few in number, but abundantly found in the liver, and particularly in the spleen. Pure cultures of plague bacillus were easily obtained from these situations. Buboes in which suppuration had taken place very rarely gave a growth of plague.

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26,408. They did sometimes?—A cultivation was obtained on one occasion.

26,409. Usually, you did not get it?—That holds true when suppuration has occurred. The *post-mortem* appearances found in guinea-pigs varied much more than in rats. Certain guinea-pigs showed a considerable degree of resistance. This resistance was seen in three different sets of animals. It was seen (1) in animals which had been vaccinated by the sediment in Haffkine's prophylactic; (2) in animals which had been injected with Roux' serum; (3) in animals which had survived an epidemic of plague which broke out among the animals kept in a shed in M. Haffkine's laboratory compound.

A non-resistant guinea-pig died of plague in from two to five days after hypodermic inoculation. There was, as a rule, little reaction at the seat of inoculation. A small bubo was generally found on the same side as the point of inoculation. The liver and spleen were enlarged, soft, and friable; hæmorrhages were frequently found, subcutaneously, near the seat of inoculation, and subperitoneally; also and perhaps most frequently around the kidneys. Frequently, also, some subserous punctiform hæmorrhages were found along the alimentary tract. Plague bacilli were abundantly found in liver and spleen and bubo, also at the seat of inoculation, less numerous in heart's blood. Peritonitis was found on two or three occasions showing microscopically an abundant and pure growth of plague.

A more resistant guinea-pig took longer to die, often more than five days. There was marked reaction at the seat of inoculation, a hard mass being felt which might cover an area of from one to two square inches. Buboes were larger, often more numerous than in rats. Suppuration was usual at seat of inoculation, occasionally also in the buboes. The spleen was enlarged and contained small tubercular greyish yellow hard nodules. Bacilli were abundantly found in these nodules, particularly at the periphery of the nodules for the centre of the nodule was degenerated or necrosed and did not stain well. Although the pulp of the spleen contained bacilli, their numbers were not so great as in the nodules. This was evident both by microscopical examination and by cultures on agar, a more abundant growth being obtained from a nodule than from the spleen pulp. The nodules varied in size from small just visible points giving a mottled look to the spleen (under these circumstances they were very numerous) to larger nodules from one-sixteenth to one-eighth inch in diameter, the nodules were then much fewer. Still more resistant guinea-pigs (and when I speak of these I refer particularly to those which received from one to two c.c. of Roux' serum at the time of inoculation with plague) showed very marked reaction at the seat of inoculation, which was generally the side of the abdominal wall. The infiltrated area often extended across the middle line and buboes were found in both axillæ and both groins. The buboes were largest on the side of inoculation; pure cultures of plague were obtained from all the buboes. The lungs showed small hard fibrous looking nodules which varied in size from one-eighth to one-quarter inch in diameter. The portions of lung tissue not occupied by the nodules were dark coloured and resembled the appearance seen in hypostatic pneumonia. Very little healthy lung tissue was left. In one case there was pleural effusion (hæmorrhagic) on one side. The spleen and liver were not much enlarged if at all, no nodules were noted in the spleen. Growths were abundantly obtained from the buboes and nodules in the lung, but not from the rest of the lung tissue. No growth was obtained from the liver, and on two occasions one colony from the spleen only. Microscopic specimens showed plague bacilli abundant in buboes and in the nodules in the lungs, but not in the lung tissue or other parts. The arrangement of the bacilli into little clumps in the lungs and spleen was very marked. Agar growths from these guinea-pigs gave many very long filamentous forms. Phagocytosis was frequently noted. On one occasion it was very marked in a guinea-pig, which had previously been vaccinated against plague, and which some time after received a large dose of living plague. In another case it was noted in the peritoneal exudation of a guinea-pig which had died of plague in the epidemic which broke out among the animals at M. Haffkine's laboratory. Another case of interesting phagocytosis was seen in the liver of a human fœtus. The mother aborted and died. An hour later, a pure growth of plague bacilli was obtained from some blood which was drawn from the basilic vein after death. A mixed growth containing plague bacilli was obtained from the vaginal discharge. The umbilical cord, liver, and spleen of the fœtus was examined microscopically; only in the liver were any plague bacilli seen, and these were all found phagocytosed in large mononuclear cells. Unfortunately no growths were made from the organs of the fœtus, as it had been placed in some rum to preserve it before it reached me.

26,410. (*Dr. Ruffer.*) Did you sterilise grain before you added the plague culture?—Yes.

26,411. Did you make any experiments on non-sterilised grain?—I am making them now. I have not finished.

26,412. How do you find the presence of bacilli in grain? Do you test it by cultures?—By placing the grain in broth and then planting out an agar and also examining the broth.

26,413. Do you test it in any other way?—I am making a second series of experiments. I am inoculating animals.

26,414. Have you had any results?—Not yet.

26,415. Have you been able to produce plague from the cultures obtained from the grain?—No.

26,416. In the case of gunny bags, how did you inoculate them; did you steep them in plague cultures?—No. I poured a few drops all over the surface of the gunny bags from a sterilized pipette.

26,417. Of the broth culture?—Yes.

26,418. It would contain more food than rat's urine, for instance?—Yes, and probably more bacilli also.

26,419. Did you test the presence of this bacillus in the gunny bags by inoculation?—I have not done so yet, I am doing that now.

26,420. So far as you have gone you have not been able to reproduce the disease from gunny bags?—No.

26,421. Did you test it by making plates from gunny bags?—No.

26,422. How do you test for the presence of the bacillus after 48 hours?—A portion of infected gunny bag was placed in the broth and any growth which formed was examined.

26,423. Did you examine it microscopically?—Yes; and then planted it on agar in a tube.

26,424. You found no plague bacilli?—That is so.

26,425. Did you ever inoculate the broth which you got from gunny bags afterwards?—No.

26,426. Did you ever find the plague bacillus in the blood of plague patients who recovered?—Never.

26,427. You said just now that you have been able to grow the plague bacillus on a sterilized plantain?—Yes.

26,428. Does it grow well on a sterilized plantain?—I did not find it grow well.

26,429. Does it grow?—Yes.

26,430. So that you can get the growth of plague by purely a vegetable medium?—Yes.

26,431. In the case where you found plague bacillus in an open bubo, had that been opened artificially, or had it burst?—It had been opened artificially.

26,432. (*Prof. Wright.*) Will you, with a view to further testing whether any toxine passes out from the plague bacilli into the nutrient medium, test the respective toxicity, firstly of your peptone broth, and secondly of the filtrate from a plague culture which has been grown in that peptone broth. Further, will you determine whether that filtrate becomes less toxic when it is heated to 60° or 100° C. respectively?—(Witness afterwards submitted the following statement):—

A peptonised broth was made and sterilised in the usual manner.

This was divided into two portions.

Portion 1 was placed on one side to be used to determine the toxicity of the culture medium.

Portion 2 was inoculated with plague and was cultivated for three weeks.

After that time the broth was filtered off from the bacilli, the filtrate was found to be sterile.

It was then divided into three portions, Portion A., Portion B., and Portion C.

Portion A. was employed for determining the toxicity of the unaltered filtrate.

Portion B. was heated to 60° C. for one hour in order determine the toxicity of the filtrate after heating 60°.

Portion C. was heated to 100° C. for five minutes in der to determine the toxicity of the boiled filtrate.

The result of the experiments which were made were follows :—

TABLE IV.

ELATIVE TOXICITY of PEPTONE BROTH and of the HEATED and UNHEATED FILTRATE from a PLAGUE CULTURE which had been GROWN on that MEDIUM for THREE WEEKS.

Number* of Guinea-pig.	Amount and kind of Fluid which was injected.	Result.
uinea-pig No. 1 -	Original peptone broth, 10 c.c.	Survived.
„ „ 2 -	Original peptone broth, 20 c.c.	do.
„ „ 3 -	Original peptone broth, 30 c.c.	do.
„ „ 4 -	Unheated filtrate from plague culture, 10 c.c.	do.
„ „ 5 -	Unheated filtrate from plague culture, 20 c.c.	Died in 24 hours.
„ „ 6 -	Unheated filtrate from plague culture, 30 c.c.	Died in 36 hours.
„ „ 7 -	Filtrate from plague culture heated to 60° C., 20 c.c.	Died in 30 hours.
„ „ 8 -	Filtrate from plague culture heated to 60° C., 30 c.c.	Died in 24 hours.
„ „ 9 -	Filtrate from plague culture heated to 100° C., 20 c.c.	Very ill 36 hours after. (*Note* 1.)
„ „ 10 -	Filtrate from plague culture heated to 100° C., 30 c.c.	Died in 24 hours.

Note 1.—No further report as to the result of the experiment as since been received.

* The guinea-pigs in the experiment varied between 350 and 400 grammes in weight.

A soluble toxine appears thus to be developed when he plague bacilli is cultivated in peptone broth.

Further, it would appear that the soluble toxine is naffected by heating to 60° for one hour and by heating to 100° C. for five minutes.

26,433. Will you kindly further make experiments to determine the respective toxicity of (*a*) normal horse serum, (*b*) Galeotti's anti-plague serum, and (*c*) Roux' anti-plague serum ?—Yes. (Witness afterwards submitted the following statement of results) :—

TABLE V.

RELATIVE TOXICITY of NORMAL HORSE SERUM, of ROUX' ANTI-PLAGUE SERUM, and of GALEOTTI'S ANTI-PLAGUE SERUM.

Number of Guinea-pig.	Amount and Kind of Serum injected.	Result.
Number 1 - -	Normal horse serum, 20 c.c.	Survived.
„ 2 - -	Normal horse serum, 30 c.c.	do.
„ 3 - -	Roux' anti-plague serum, 20 c.c.	do.
„ 4 -	Roux' anti-plague serum, 30 c.c.	do.
„ 5 - -	Galeotti's anti-plague serum, 20 c.c.	do.
„ 6 - -	Galeotti's anti-plague serum, 30 c.c.	do.

Nothing in the general condition of these animals suggested that there was any difference in toxicity between these various sera.

26,434. Further, will you kindly determine in the case of the guinea-pigs which you employed for the experiments which are tabulated in Table II., whether the injection of the filtrate (uncarbolized) from Mr. Haffkine's prophylactic confers on guinea-pigs any greater protection against plague than the original nutrient medium which he employs ?—Yes. (Witness afterwards submitted the following statement) :—

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TABLE VI.

RESPECTIVE IMMUNIZING POWER of HAFFKINE'S ORIGINAL NUTRIENT MEDIUM and of the FILTRATE (*i.e.*, of the FILTRATE obtained from a CULTURE of PLAGUE which had been GROWN in HAFFKINE'S NUTRIENT MEDIUM) from HAFFKINE'S PROPHYLACTIC.

Number of Guinea-pigs.	Amount and Kind of Fluid which had been previously injected.	Date of the Test Inoculation with living Plague-Bacilli.	Interval which elapsed between the previous Inoculation and the Test Inoculation.	Result.
No. 1 - (Control.)	Nil - - -	5.4.99.	- - -	Died in 24 hours.
No. 2 - (No. 1 of Table II.)	Haffkine's original nutrient medium, 30 c.c.	„	18 days -	Died in 2 days.
No. 3 - (No. 2 of Table II.)	Haffkine's original nutrient medium, 20 c.c.	„	„	Died in 4 days.
No. 4 - (No. 3 of Table II.)	Haffkine's original nutrient medium, 10 c.c.	„	„	Died in 1 day.
No. 5 - (No. 4 of Table II.)	Haffkine's original nutrient medium, 5 c.c.	„	„	„
No. 6 - (No. 5 of Table II.)	Uncarbolised filtrate from Haffkine's prophylactic, 30 c.c.	„	„	Died in 3 days.
No. 7 - (No. 6 of Table II.)	Uncarbolised filtrate from Haffkine's prophylactic, 20 c.c.	„	„	„
No. 8 - (No. 8 of Table II.)	Uncarbolised filtrate from Haffkine's prophylactic, 5 c.c.	„	„	Died in 1 day.

If we take the three guinea-pigs, Nos. 6, 7, and 8 (*vide* above table), who received the filtrate from Haffkine's prophylactic, and if we compare them with the three guinea-pigs, Nos. 2, 3, and 5, who received comparable quantities of Haffkine's original nutrient medium, and if we add up the number of days which these two sets of guinea-pigs survived after the plague inoculation, it will be obvious that the protection which we afforded by the original nutrient medium is neither less nor greater than the protection which was afforded by the filtrate from Haffkine's prophylactic.

26,434A. Will you kindly further make some experiments on disinfection with a view to determining the efficacy or otherwise of the methods of disinfection which are applied to plague houses in Bombay ? Further, in making experiments on the subject, will you take as your test of the efficacy of all these disinfection processes, not the presence, or, as the case may be, the absence, of the plague bacillus, after the completion of the disinfection (for you have no satisfactory means of ascertaining that presence or absence), nor yet the destruction or non-destruction of a certain number of non-pathogenic bacteria in those houses. But will you, instead of either of these tests, take as your test of the effectiveness of a disinfection process the destruction, or, as the case may be, the non-destruction of all bacteria, except such as may prove themselves to be more resistant to disinfectants than the plague bacillus ? — Yes. (Witness afterwards submitted the following statement) :—

EXPERIMENTS ON DISINFECTION.

Eight disinfected rooms were examined generally, as shortly after disinfection as permitted the drying of the antiseptic on the floors. All the houses examined had pucka floors, *i.e.*, cement or stone floors.

The disinfected houses were all in the Mandvi district, and had been disinfected with a solution of perchloride of mercury, supposed to be of the strength of 1 in 500.

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The method of obtaining the cultures was as follows:—

The cotton wool plug was removed with antiseptic precautions, from an empty sterile test-tube, the inner end of the plug was rubbed over a considerable area of the floor, about one square foot, some dust and dirt was thus collected on the wool plug, which was again inserted into the sterile tube. In the laboratory a portion of the soiled wool was introduced into two broth tubes. These were placed in the incubator. Of the eight samples of dirt examined by this method one was found sterile. Three others were sterile except for the presence of a mycelial fungus. The specimens from the four other floors gave bacterial growths; these were planted out on agar plates, and the various bacilli found were isolated and grown in broth. In all, four varieties of bacteria were found. These were one micro-coccus, and three species of bacilli. The resistance of each of these bacteria was tested by an exposure for 15 minutes to the following dilutions of perchoride of mercury solution. The resistance was compared with the resistance of the plague bacillus when exposed for the same time to the same dilutions of perchloride solution.

The strengths of perchloride of mercury solutions were—

1 in 1,000.
1 in 2,000.
1 in 5,000.
1 in 10,000.
1 in 20,000.

It was found that in all cases the surviving organisms of the disinfected floors were less readily killed than the plague bacillus when tested by the above solutions.

The method of testing the resistance of the bacteria was as follows:—

All the bacteria were obtained in pure culture in broth. By a capillary pipette, a small quantity of the broth growth was placed in the sterile perchloride solutions of the above-mentioned strengths, and left for 15 minutes. After that time, with another sterile pipette, a small quantity was withdrawn from the bottom of the perchloride solution where the broth growth had sunk as a flocculent precipitate, and this was placed in a fresh broth tube.

The bacteria which were found survived 15 minutes' exposure in all dilutions of perchloride, even 1 in 1,000, except one bacillus, a small thin bacillus, producing yellow pigments. This last only survived a strength less than 1 in 2,000. Plague bacilli tested in a similar manner never survived 15 minutes' exposure to a strength of 1 in 5,000. In one series of experiments the plague bacillus did not survive exposure for 15 minutes to a solution of 1 in 10,000. It would thus appear that the bacteria which were obtained from these disinfected floors were in all cases more resistant to the action of perchloride than the plague bacillus.

26,434B. Will you further endeavour to ascertain whether the plague bacillus can, when it has been added to unsterilized earth, be recovered from it by making cultivation in CO_2?—Yes. (Witness afterwards submitted the following statement):—

Two series of experiments were done in this connection:—

A sample of ordinary garden earth was obtained, to this a considerable quantity of living plague germs was added. Some of the infected earth was emulsified in broth, and a portion of this emulsion was added to six broth tubes prepared for anærobic growths. The six tubes were divided into three series, two being treated to saturation with CO_2, two were treated to almost saturation, two were treated with just an excess of CO_2. They were incubated for three days. After this interval a control broth tube in which plague was growing in saturated CO_2 showed a distinct growth. Agar plates in three dilutions were then made and examined after two days, and again, after three days, but no bacillus conforming to all the tests for plague was found, although in two samples a bacillus, morphologically resembling plague, was found. The last-mentioned bacillus when grown on agar, in pure growth, grew more rapidly than plague. Further, when a portion of this growth was emulsified in broth and was inoculated in considerable dose into the peritoneal cavity of mice, the inoculated animals survived.

These anærobic tubes, when opened, had all a very strong smell, apparently of sulphuretted hydrogen.

The effect of H_2S gas by itself and the effect of mixture of H_2S and CO_2 on plague growth was there fore investigated. Another series of six tubes wa inoculated with freely infected garden earth.

In the present undiluted H_2S plague did not appea to grow, although a muddiness resulted in the brot from the deposition of sulphur from the H_2S.

In a mixture of CO_2 and H_2S there was slight growth H_2S was first passed in, then CO_2, so that while th atmosphere in the sealed tube contained little of H_2S some of this gas, probably, remained dissolved in th broth. The growth of plague occurred in chains, as i does in ordinary CO_2 broth cultures.

The six tubes which are in question were incubate for three days. They were then planted out on aga plates in three dilutions. These plates were examine daily for three days; I did not succeed in detectin bacilli that answered to all the tests of plague.

This is hardly to be wondered at, for agar plates o plague grow very slowly, whereas the bacteria foun in the earth grow very rapidly, by the end of thre days nearly all the plates were covered by othe growths than plague.

26,434C. Lastly, will you determine whether th virulence of the plague bacillus can be increased b making a series of passages through guinea-pig by the method which was successfully employed b Mr. Haffkine in exalting the virulence of the choler bacillus?—Yes. (Witness afterwards submitted th following statement):—

I have to report that I have performed five series o inoculations with a view to testing the possibility o increasing the virulence of the plague bacillus b making a series of intra-peritoneal passages throug guinea-pigs.

The method which was adopted was the following:—

A pure culture of plague was obtained and emulsifie in broth. A few minims of this broth emulsion wer inoculated with aseptic precautions into a guinea-pig peritoneum. The aseptic precautions referred to wer (*a*) the use of a sterile capillary pipette, (*b*) burning small area in the abdomen of the guinea-pig with re hot glass rod, and (*c*) the careful introduction withi that area of the filled capillary pipette. After death *post-mortem* was made with aseptic precautions. Th peritoneal exudation was removed by a capillary pipette It was then introduced into a test tube which was place in a slanting position in the incubator. It was allowe to remain there for 24 hours, when a few minims o the purulent exudation were inoculated into the secon guinea-pig. When this guinea-pig died the same pro cedure was adopted with a series of successive guinea pigs. The exudation was always examined microscopi cally before it was introduced into a healthy guinea pig. When the fluid was found to be contaminated i was rejected. The purity of the peritoneal fluid wa also controlled by cultivation.

Five series of guinea-pigs were inoculated in th manner which has just been described.

In each case the virulence was found to be increased

The following is the longest series of inoculation which was successfully carried out:—

Guinea-pig No. 1.—Inoculated intraperitoneally wit a 15 days old agar cultivation of plague (source fro which the culture was derived was not noted). *Died i 5 days.*

Peritoneal fluid removed and incubated for 24 hours.

Guinea-pig No. 2.—Inoculated with a few drops o above peritoneal fluid. *Died in 52 hours.*

Peritoneal fluid withdrawn after death was incubate for 24 hours.

Guinea-pig No. 3.—Inoculated intraperitoneally wit a few drops of above peritoneal fluid. *Died in 24 hours*

Peritoneal fluid withdrawn after death and incubate for 24 hours.

Guinea-pig No. 4.—Inoculated intraperitoneally wit a few drops of the above peritoneal fluid. *Died in 2 hours.*

Peritoneal fluid found to be contaminated by a thic long bacillus.

It will thus be seen that by the above method th virulence of the plague bacillus for guinea-pigs can b very rapidly increased.

In other series of experiments a degree of virulenc was obtained, such that when two or three drops of th exudation were introduced intraperitoneally the guinea pigs died some time during the night.

(Witness withdrew.)

(Adjourned till to-morrow.)

At The Secretariat, Bombay.

SEVENTIETH DAY.

Wednesday, 22nd March, 1899.

PRESENT:

PROF. T. R. FRASER, M.D., LL.D., F.R.S. (*President*).

Mr. J. P. HEWETT.
Prof. A. E. WRIGHT, M.D.
Mr. A. CUMINE.
Dr. M. A. RUFFER.
Mr. C. J. HALLIFAX (*Secretary*).

Surgeon-General R. HARVEY, I.M.S., recalled and further examined.

Surg.-Gen. R. Harvey, I.M.S.

22 Mar. 1899.

26,435. (*The President.*) I understand that you have been making detailed inquiries with regard to inoculation in the Khoja community in Bombay; are you prepared to make a statement of the result of your inquiry to this Commission?—Yes, I am prepared to make a statement, but I am afraid that I cannot as yet give you any results. I find that the inquiry is a very much larger and longer one than I have had time to undertake. I have been hard at work ever since I got to Bombay and I have only touched the fringe of the subject. I have not been able to draw any inferences or conclusions or to get any definite information to enable me to draw such; for instance, we found that it is extremely difficult to ascertain the cause of death in the case of all the people who have been seen by medical men. We have taken the names and addresses of these medical men, and we intend to write to each individual so as to endeavour and get accurately the cause of death. We found the Jamaat books, the records of the Khoja community, are exceedingly accurate as regards the great majority of details, but they are extremely inaccurate with reference to the cause of death because a very large number of people are not seen by medical men and anything they are told is put down as the cause of death. Some of the entries are absolutely ridiculous. You find headache, you find asthma in a child of two years, and consumption in children of two years, and all sorts of impossible things of that kind. The inquiry, therefore, will take a very considerable time; but it is going to be continued, and the results will eventually be communicated. I estimate that it will take several weeks before they can get anything definite. I find that the original investigation of Professor Haffkine was one which involved an enormous amount of energy and thought. Although it is a very short report,* the amount of work involved in preparing it was enormous. I think that the records of the community are kept in a much more full and proper way than those of any average people; and therefore we ought to be able to get something definite out of them. At the same time you must remember that this is a community of from 10,000 to 13,000 persons spread about in the middle of a large city; so that it is not at all easy to follow the individuals; the results, therefore, will probably not be scientifically accurate. It will, however, enable us to form conclusions which are in the main correct; but they will not have the same value as those obtained under proper conditions from a smaller community. Several points have come out in addition to the fact that the records are correct. One is that the community as a whole have a most extraordinary belief in the efficacy of inoculation, though probably half of them do not accept it; there is a large number who have told me that the reason why they have not been inoculated was that they preferred to leave it to God,—that they were in God's hands, and that they thought it was not right to interfere with his decrees. So that a very large number of them, although they do not object to it and have nothing urge against it, do not actually accept it. I think it will probably be found that about half of the community are inoculated, and that half of them are not inoculated. We know, I should think, with practical accuracy from the Jamaat books those who have been inoculated and those who have not, so that we can get a very fair index as to the mortality among the two classes. Of course the difficulty is to get the actual cause of death. Another point is this. I have been asking them whether they know of any evil results arising from inoculation. Some of them have said that they heard it produced impotence and leprosy after variable times, but none of them have been able to tell me anything beyond hearsay evidence. None of them had had any experience of evil resulting from it. Most people think so little of it that a great many of them have been inoculated and re-inoculated three, four and five times. I was in hopes that one result of the investigation would be to give some indication as to the length of the protection afforded by the prophylactic. But in consequence of these constant re-inoculations, it will be extremely difficult and probably impossible to get any definite information with regard to it.

26,436. That was the main object of your inquiries?—Yes, and I am afraid they have been frustrated by the eagerness of the people to be inoculated over and over again.

26,437. Does any other point suggest itself to you which you wish to mention?—I do not think so.

26,438. Have you anything to say as to the selection of inoculated persons?—I think that in inoculations there is always a process of unconscious selection. Inoculations are performed mainly on those who come up for inoculation. People who are weak or who are sickly in any way do not come up. I have found repeated instances during the last few days of families in which the only person who has died (I have only been investigating the families in which death has occurred) has been the only uninoculated member of the family. On making inquiries as to why the individual had not been inoculated when all the other members of the family had been inoculated, I was told that the medical officers thought the individual was not fit for it or that he himself did not like to be inoculated because he was sick or old. The consequence of that has been that a considerable number of people, who ought by rights to be among the inoculated, because all the members of their families are inoculated, have died without being inoculated and have gone to swell the tale of the deaths among the uninoculated. I think that is the explanation. It brings the results of Prof. Haffkine's inquiries into the Khoja community out of the region of impossibility and makes his inferences much more likely to be true. The rates worked out in that inquiry seemed to me to be impossible. The deaths from all other causes than plague in the inoculated work out to only 3·33 per thousand per annum. That seemed to me to be an impossible death-rate in a community which included children and old people. I thought there must be some fallacy about it. I think that is the explanation with regard to it—that a considerable number of people who ought really to have been, and would have been inoculated if they had not been unwell, were included in the uninoculated, and went to swell the death-rate among them.

* *See* App. No. IV. in Vol. I. of these Proceedings.

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26,439. Did you find any evidence of age selection?—In that report a considerable number of children and old people had been inoculated, but the number of inoculated was not nearly in proportion to the numbers in the population, and therefore Prof. Haffkine left out of calculation among the uninoculated children and old people.

26,440. (*Prof. Wright.*) Have you seen any facts which would lead you to suppose that the plague prophylactic confers protection against any other diseases?—At first I was very much staggered by that statement, because although one knows that the revulsive effect of one disease occasionally drives out another, it is not a common occurrence, and I certainly do not think it could account for the enormous difference in the death rate between the inoculated and uninoculated; but I find that Prof. Haffkine has been going (it is not a random assertion) on a large number of instances where people have believed—I do not say rightly or wrongly—that the effect of inoculation has been to stop other diseases from which they have been suffering for a long time. I think the effect can only be small, but it may possibly have occurred in a certain number of cases.

26,441. It is not forced upon you by statistical evidence which you have collected?—Not by statistical evidence.

26,442. When you compare the mortality among those inoculated Khojas, what will you take as your control? Will you take the mortality among the uninoculated members of the same family, or will you take the mortality among the other Khoja families who have not been inoculated at all, or will you take the total mortality of Bombay?—I think the fairest way would be take the mean mortality of the Khoja community.

(Witness withdrew.)

Mons. W. M. Haffkine, C.I.E., recalled and further examined.

Mons. W. M. Haffkine, C.I.E.

26,443. (*The President.*) Referring to the evidence you gave us when you appeared before the Commission on a previous occasion, I find that you undertook, if possible, to supplement the information you gave us with regard to one or two subjects. You told us that you had some exact data relative to the animals which had been treated with serum, which you thought you would be able to put in afterwards (*see* Question No. 133)?—The materials regarding the treatment of plague by serum of immunized animals could, unfortunately, not be analysed and put in order, owing to pressure of work.

26,444. (*Prof. Wright.*) I believe that before you proceeded to inoculate against plague you did a series of inoculations against cholera?—Yes.

26,445. Certain inoculations against cholera had been previously done by Dr. Ferran in Spain?—Yes.

26,446. In what respect is your method of cholera inoculation an improvement on that of Dr. Ferran?—I have little information as to how Dr. Ferran did his inoculations against cholera. A large part of the publications he made at the time were in Spanish and I am not acquainted with them. My information as to the manner in which he did his inoculations comes from the inquiry of the French Government Commission, deputed during the cholera epidemic in Spain to investigate Dr. Ferran's operations. Their report has been embodied in a paper by Prof. Brouardel, President of the Commission, and Dean of the Medical Faculty in Paris, but I am not aware whether Dr. Ferran put at the disposal of the Commission the details of the method he employed.

26,447. I was under the impression that the faults which you find in Ferran's system were that he used vaccine which was not sterile, that he used a culture which was of unknown virulence, and that he did not employ a standardised vaccine. Is that so?—The fixing of the strength of the vaccine is an extremely important point in all inoculations, though the urgency of that procedure differs with regard to different microbes and different vaccines, living and dead ones.

26,448. I am asking you with regard to cholera inoculation. I understand that your improvement on Ferran is that you used a perfectly sterile vaccine, and a vaccine the strength of which was actually known to you. Is that so?—Yes.

26,449. You think these two points—of employing a perfectly sterile vaccine and of employing a vaccine in definitely known doses—are the most important points in every vaccination. Is not that so?—Yes; they are exceedingly important.

26,450. How far have you been able to carry out, in the plague inoculations, these principles which you carried out so successfully in the cholera inoculation?—In the case of cholera, the method employed for having the vaccine of a fixed strength consists in keeping the pathogenic properties of the microbes at a given height by passages through animals. It is known that very great variation takes place in the power of the cholera microbes in causing the disease. When one compares cholera cultures of different origins, one finds a variation in the proportion of about 1 to 100; that is, the amount which is necessary for killing an animal by cholera may vary from 1 given quantity of virus to 100.

26,451. In the case of plague, have you taken any steps similar to those which you took in the case of cholera to ensure that your vaccines were made with a microbe of known virulence?—No, not systematically. The operations were conducted in a place where plague existed, and we were, in the majority of cases, getting for our cultures microbes from patients, thus obviating only the variations occurring in the laboratory. Apart from our knowing that it was a microbe which had produced a fatal effect upon men, we did not fix further its virulence.

26,452. Do you think you can assume that a microbe which is taken from a plague patient is always more virulent?—No; but we sterilized the vaccine, and measured the dose.

26,453. You say in your evidence that the virulence of the plague microbe can be easily kept up by passages through animals; have you made those passages through animals?—Yes; but we were unable to do so regularly. In the inoculation against cholera we pass the vaccine through guinea-pigs daily. So far as we can get guinea-pigs, the rule is that every day fresh specimens of microbes should be taken from a new animal. The work, however, always suffers from our not being able to do so regularly.

26,454. Would it be possible to do that in the case of plague?—I was not able to do so.

26,455. Would it be possible to do so if you had time at your disposal?—Yes; not only is it possible, but, as stated, in a large number of cases we do so. In the preparation of our vaccine we employed, whenever possible, microbes passed through rats and monkeys.

26,456. We may take it from you that it has not been possible to do it always, but that, theoretically, it would be possible if you had time enough at your disposal?—Yes, it is so.

26,457. You say that the virulence of the plague microbe is kept up by passages through animals; will you kindly specify the animals?—The microbes that we used were passed through rats and monkeys. At one time a number of experiments were done in the laboratory on monkeys, and the virus was transferred from monkey to monkey for other purposes, not specially for the inoculation against plague. Those microbes acquired a virulence higher than that which they had in the beginning.

26,458. Did you find the virulence to increase when you made passages through monkeys?—Yes.

26,459. Did you find the virulence increased when you made passages through rats?—Yes.

26,460. Did you find that to hold true invariably? We had it in evidence yesterday that, in the three series of experiments which were done in the Plague Commission laboratory here, the microbes died out instead of becoming more virulent?—In our case we employed only microbes that showed themselves virulent. In the experiments which were conducted in the laboratory we had specimens of microbes which, at the beginning, were less effective, and which, after they had been passed through five, six, or a dozen animals, acquired an increased virulence.

26,461. Do you think that it is feasible to obtain virulent cultures by this means?—Yes; not, however, by passing the microbe through monkeys.

26,462. Through what animals?—Most probably rats; it may require special precautions to maintain the power of the microbe if interruption of the series occurs.

26,463. Similar to those you took in the case of cholera?—Yes, precisely.

26,464. (*Dr. Ruffer.*) Have you been able to do that in rats yet?—Yes. As I stated, I did not do that continuously, but we had microbes passed through series of rats, and which maintained a high degree of virulence. But I never had sufficient leisure for that myself, and I never had an officer who could be put on that special task of daily passing microbes through animals so as to have the microbes always passed freshly through a rat or a monkey, or any other animal which we might select finally.

26,465. (*Prof. Wright.*) In addition to laying down that a microbe of a fixed and known virulence ought to be employed in making vaccines, you also lay down that a fixed quantity of microbes ought to be employed?—Yes.

26,466. How far have you been able to carry that out so as to estimate what quantity of microbes were contained in the doses which you prescribed?—In the inoculations against plague I never relied upon the measurement of the dose itself. At the time when I gave my previous evidence I said that we always relied upon the effect which the vaccine produced upon men. Our estimate as to the doses necessary for inoculation were always obtained from the reaction observed in inoculated people.

26,467. We have had it in the evidence of Dr. Milne that you hold up your bottles to the light so as to judge the opacity, and that in conformity with this you write the doses on your bottles?—That is so. The cultivation flasks do not grow uniformly. In a 100 flasks prepared from materials obtained on the same day, passed through the same processes, and inseminated with the same microbes, there will be a variation, and often a very considerable one, in the crops obtained. In this case there is an unequal degree of modification produced in the medium also. For determining the dose one has, therefore, to take into additional account the abundance of the growth also.

26,468. You do estimate the dose by holding up the bottle to the light and by judging of its opacity?—Yes.

26,469. Do you think that is a good method of judging the number of bacteria present?—It is possible that this is the most practical method.

26,470. Do you think it is a good method?—Yes.

26,471. Do you think it is a satisfactory method?—I have to estimate to what extent the method is satisfactory by the results produced.

26,472. We had it in evidence yesterday that a series of weighings of the sediment contained in individual doses brought out the fact that some of your doses contain between seven and eight times more sediment than others; in view of these weighings, do you think that your method is a satisfactory one?—Not in view of that, but in view of the results obtained from estimating the febrile reaction and comparing the richness of growth, it was satisfactory.

26,473. Is that a satisfactory method of standardizing?—I do not believe that the weighing of the sediment is a satisfactory method.

26,474. I asked you whether, in view of the results that were obtained by weighing, you think that one can arrive at a good estimate of the amount of sediment by holding the bottle up to the light?—I do not believe that, by weighing the growth, or otherwise estimating the sediment alone, you will obtain a satisfactory method of standardizing, and I never had recourse to that method. When I say I hold up the bottle to estimate the abundance of the growth I do not mean that that is the only circumstance which I take into account. I estimate the doses by inoculation in men and by observing the reaction produced.

26,475. I understand that method of standardization, but I do not understand whether you think it is a good method of standardizing, to judge the quantity of microbes by holding the bottle up to the light and noting the opacity?—I do not say that that is the only method I employ.

26,476. But you do consider that that method is satisfactory?—No.

26,477. We had it in evidence from Dr. Milne that the dose put on the bottle was judged by holding the bottle up to the light?—We shall not be able to analyse the matter satisfactorily unless what I say is taken into account fully. I stated that we estimated the required dose by the effect produced upon man. I must repeat that, when I have a batch of 500 flasks inoculated at one time on one day, or, say, in the course of three days, with the same cultivation of microbes, and I find that it produces the required amount of fever when injected in a dose of 2½ c.c., I still watch the abundance of the growth which takes place in the different flasks, and that abundance of growth is judged by inspecting the flasks and comparing the density of their growth. If, therefore, you wanted to standardize the vaccine simply by weighing the amount of sediment, it is most probable that you would never have any definite results.

26,478. That is not suggested to you. I asked you whether it is a good way of estimating the quantity of sediment to judge the opacity by holding the bottle up to the light?—It is a sufficient method of estimating when you compare additionally the result of inoculation in men.

26,479. Do you mean that your results on man are so constant that your method of standardizing must be perfect?—In all the inoculations which were done experimentally, under conditions deliberately chosen for testing the efficacy of inoculation, we obtained a fixity of results which probably is unequalled by any other series of experiments of the kind. In my evidence on the former occasion I enumerated the observations which we made in attacked communities. You noticed how little the protection afforded by inoculation varied. As long as it was so I consider the method selected as satisfactory, being at the same time adapted to the conditions of our practice.

26,480. (*The President.*) The question is not settled. We have not gone into the point as to whether the proofs you gave of the efficacy were, to our mind, satisfactory or not; you must not assume that?—In selecting my method I had, however, to judge by my results. In continuing to apply the method of estimating the doses which I followed, I had to watch the effect produced, and as long as the results answered my expectations I was justified in employing what I found to be practicable under the circumstances of our work.

26,481. (*Prof. Wright.*) I asked you if you were shaken in your impression that your method of standardization is a satisfactory method by the fact that sometimes the sediment is eight times larger than in other cases?—Not in the least; this most probably referred to microbes of different strength.

26,482. You said you have not tested the virulence of the microbes?—No; I tested the strength of the vaccine, not by the virulence of the microbes when injected alive into animals, but by the effect which the prophylactic produced on men, as is done by the vaccine lymph for small-pox.

26,483. We have been informed that, in a great number of cases, the vaccine was not tested on men in the laboratory before it was sent out; therefore, it was in those cases tested only by this opacity of the fluid, and if that is shown to be a defective method, I ask you whether it is not true that sometimes the standardization of the vaccine was insufficient?—The method was tested on men whenever it was possible. We made the best of the existing conditions.

26,484. I am speaking apart from the question of what was feasible. I ask you, as a mere scientific fact, whether the vaccine was always correctly standardized, and whether you would recommend this method of holding the bottle up to the light as an effectual method of standardization?—As long as we have no other practical method we have to follow this.

26,485. That means it is not a good method, because it follows that you must be looking for a better one; is that so?—I do not think that any attempts to find a better method will be successful as long as the inoculations themselves continue. (Note by witness on correcting proof of his evidence:—The answer in this form has no sense, and contains surely a mistake of the reporter. It should be as follows: "I do not think that our attempts to find better methods will ever cease as long as the inoculations themselves continue.") I never maintained that the methods we employed were all that I could desire.

26,486. (*Dr. Ruffer.*) What you mean is that you are satisfied there is no better method at present?—I, personally, had no better method applicable at the time.

26,487. (*Prof. Wright.*) A further question arises as to the sterility of the vaccine; have you anything to

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say on this matter? We have had it in evidence that a good number of bottles were found to be contaminated. Do you think these contaminations are important, that is, did they affect the vaccine prejudicially?—These contaminations are a most undesirable factor, but I knew that the conditions of our work were not such as to permit of completely avoiding that factor. I was satisfied, however, that these contaminations, under the conditions under which they were likely to occur, were not a source of danger. They were mainly undesirable as endangering the efficacy of the prophylactic, and we took all possible precautions to avoid them. Our conditions of work, however, were not such as to secure this object fully. We continued to employ the means at our command because of the urgency of the time, and because I knew that the amount of risk which we ran was unimportant.

26,488. And you have come to the conclusion, with regard to the basis of your previous cholera inoculations, that it is not as necessary as you then thought it to be to employ a microbe of known virulence, and have absolutely aseptic vaccine? You think that certain divergencies from that rule may be permitted in the case of plague?—It is certain that even in plague, where we use dead vaccines, better results will be obtained if the above basis is strictly adhered to.

26,489. It has been suggested that an effect is produced upon gonorrhœa by injections of the prophylactic; do you know anything about that?—Not from personal observation. I had a verbal report from the medical officer, Captain Thomson, who made a series of observations to that effect, but have no personal knowledge on the subject.

26,490. Do you know what effect was produced by the injections?—No. I have no details. I understood that he was going to put this information before the Plague Commission.

26,491. (*Dr. Ruffer.*) I understood you to say that you had some experiments showing that there was some relation between the effect on man and the opacity in the bottle; can you put in those experiments?—Yes.

26,492. I suppose you have notes of those experiments, have you not?—We have notes in the shape of the reports of the inoculated people.

26,493. Can you add these notes to your evidence?—I would have to select them, but I will state at once what kind of information we have. The amount of growth which you obtain in a cultivation flask in one week is less than what you obtain in two, three, or four weeks. If you inoculate to-day a given number of flasks, and use some of them in a fortnight, while the others are permitted to stay on longer, there is a larger quantity of growth in the latter. The same dose inoculated from those different flasks gives a different reaction; the inoculation from the flasks which have been left to grow longer, and have a greater opacity, produces a higher reaction.

26,494. You are still of opinion that the fever is really the standard of the efficacy of the inoculation?—In the series of experimental inoculations, which was the basis of our work, we used that standard as an indication of the effectiveness of the material.

26,495. You stated, in the evidence which you gave before us three or four months ago, that you had a method by which a non-carbolized bottle could not possibly get into circulation. If you will remember, I asked you how you were sure that the bottle which had not been sterilized could not by mistake get into general circulation, and be used on patients, and you said that you had an elaborate method by which you could prevent that with certainty?—I did not mean a method, but we had arrangements made in the laboratory which would secure that.

26,496. Will you tell us these arrangements?—They have been enumerated in the rules for sterilization, which I have supplied to the Commission; since then we have introduced a controlling procedure which consists in the syphoning officers inoculating an agar tube with a sample from each flask.

26,497. To show the sterility?—Yes.

26,498. Have you any evidence to show that the prophylactic inoculation has any effect on leprosy?—No.

26,499. Or epilepsy?—No, I have no personal information on this point.

26,500. With regard to the statistical results of your inoculations, I have noticed in several places that the mortality from all causes, not from plague, among the inoculated is very much smaller than among the uninoculated; I think you found that out yourself, and put it in your* statistics of the Khoja community in Bombay. It was the same with the Khoja community in Karachi, and in the various communities which were inoculated in the State of Baroda. Have you any explanation of that fact?—In my Khoja* report I enumerated three or four circumstances which all probably took part in producing that result. One factor which influences the difference to a great extent has been mentioned by the Director-General of the Indian Medical Service in his evidence this morning. In a central station where people are inoculated as they come there is an unconscious selection, by weak individuals abstaining from inoculation. This tends to produce a difference in the general mortality, when one compares afterwards the inoculated and the uninoculated groups. This factor did not exist in the inoculations such as we have done in Undhera or in the Umdrkhadi Common Jail, where all the inmates agreed to be inoculated, but where the authorities only permitted a half of them to be done. In the instance of Umdrkhadi all the prisoners were paraded in 9 or 10 rows, and every second individual was inoculated. In the Undhera village we inoculated again a population agreeing to be inoculated in a body; the committee appointed to carry out the inoculations deliberately inoculated only half the members of each family, having their attention directed to making the groups of inoculated and uninoculated as comparable as possible. In these cases the unconscious selection is eliminated. Whereas at an inoculation station one will observe that a certain number of weaker individuals abstain from inoculation, mothers do not bring up very small babies, or old people do not come to be inoculated. This, however, is only one factor. Another is this. The mortality from general causes is generally low in the whole population during a period immediately following a severe outbreak, a large number of susceptible individuals having been killed off during the preceding period. It is certain that a number of deaths that are referred to general causes amongst the uninoculated are really deaths from plague, preventable by a specific treatment. This circumstance certainly renders the comparison comparison between the inoculated and uninoculated far less paradoxical than it looks at first. The last factor to be mentioned is the possibility of the inoculation against plague, or most probably inoculation of any pathogenic microbes, influencing also the course of diseases due to other infections.

26,501. I will put the question in another form. In Karachi there were 1,200 Khojas inoculated?—Yes, I believe, about that.

26,502. At Karachi we had the figures given to us of these 1,200 Khojas, and I find that not a single one had died from general causes in six months; do you think the explanation which you have given us just now is sufficient to account for that?—I am not able to estimate the extent to which it is applicable to that case.

26,503. You have no personal knowledge of the statistics of Karachi?—No. Moreover, in enumerating the three factors above mentioned, I am unable, and it is probably impossible, to estimate to what extent they are at work in each individual case. In the inoculations among the Khojas here in Bombay the observations referred to a period below four months, following a year when the mortality in that community han been just double the usual one, that is, it referred to a relatively short period following a severe epidemic. The mortality from general causes amongst the inoculated was exceedingly small, and I am able to take upon myself the responsibility of stating that no more deaths than those reported after my personal minute of inquiry occurred in that community. That is a circumstance which the Director-General, I believe, has now satisfied himself about completely in his present investigation. But even here I cannot distribute the result among the different factors that have effected it.

26,504. (*Prof. Wright.*) Do you think the presence of the supernatant fluid in the prophylactic is essential?—I have no information upon which to modify my views on the subject.

26,505. Upon what experimental evidence did you base the opinion that the supernatant fluid had an

* See App. No. IV., Vol. I. of these Proceedings.

important influence in conferring protection?—That was a circumstance which, in my first evidence, I asked permission not to discuss. I should like to avoid the consideration of my plan of work from a general or theoretical point of view.

26,506. I am not asking you from a theoretical point of view; I am asking if you have made any experiments which support that opinion?—I cannot support it except by the consideration of the results actually obtained.

26,507. (*The President.*) Have you any experimental data?—The results as observed on men are the only ones. I introduced the supernatant fluid in the hope of reducing the fatality of attacks in inoculated.

26,508. (*Prof. Wright.*) But you have never tried to inoculate some men with the sediment alone and others with the liquid alone?—No, not yet. In a letter, No. 260*a*,* which I addressed to the Government nine months ago, I sketched out a plan by which this could be ascertained, but I was unable to carry it out yet

26,509. When you judge by the intensity of the fever produced by the inoculation in cases where you inoculated as large a quantity as 15 c.c., do you not think the peptone contained in the prophylactic itself might account for some of the fever?—The experiments were not made in these conditions. We have the best febrile reaction with small doses of prophylactic.

26,510. In the particular case where you inject a large quantity, do you think there is that possibility?—Yes, but when we inject large doses it is because we observe little fever.

26,511. Have you made any experiments upon men with peptone?—No. One of the officers in my laboratory, Dr. Mayr, is to make those experiments, but they have not been made yet.

26,512. As far as your present information goes, you would object to eliminate the supernatant fluid?—I would decidedly.

26,513. On *à priori* grounds?—Yes.

26,514. But for no other reasons?—As long as no other treatment has been shown to be as effective as the one employed, it is inadmissible to modify it. All alterations must be justified by results actually obtained.

26,515. If experiments were brought before you showing that no protection was afforded to animals by the supernatant fluid in your prophylactic, if, for instance, the filtrate from your prophylactic proved to be no more toxic to animals than the original nutrient medium which is employed as a culture medium for those plague bacilli, would you then think there was a reason for eliminating the supernatant fluid from your injections?—I would not dare to eliminate it in the treatment of human communities until restricted observations, as mentioned in my letter to Government, would show that admissible.

26,516. But you have no experimental evidence to show that it is useful. If you had experimental evidence of animals to show that it was useless, would you still persist in using it on men?—I would not adopt a modification in the general practice until exact observations on men showed such a modification to be admissible.

26,517. Pending observations on men, if the experimental evidence of animals seemed to show that the supernatant fluid conferred no benefits on animals, would you still approve of employing the filtrate on men?—Yes.

26,518. Why?—When you try the effect of the treatment on animals under special conditions you may fail to detect in the whole of the prophylactic fluid any elements justifying its application to men. Such was the case with the experiments carried out in London, in St. Bartholomew's Hospital, in an attempt to immunize guinea pigs against the plague. *A fortiori*, when you examine the material, not as a whole, but from one particular standpoint, and fail to detect reasons for a particular component of the prophylactic, that would not make me less diffident to adopt the conclusion based on your experiment. In the inoculation against cholera, many experienced investigators, after prolonged and careful experiments on animals, came to an absolutely negative result as to the efficacy of the method, whereas the inoculation in men proved to themselves and to others the striking efficacy of the method.

26,519. In your experiments with regard to cholera inoculations you were instigated to do your experiments on men because the experiments on animals had proved successful?—Yes, and with regard to the inoculations against the plague that was the case also.

26,520. If experiments, often repeated on different animals, showed that the supernatant fluid conferred no protection, and was not more toxic than the original nutrient fluid, would you then abandon it?—No. At the present stage of our work I would admit only a guarded attempt on an isolated group of men, and restricted experiments made and repeated with regard to that particular point.

26,521. Do you prefer to do your experiments on men to doing them on animals?—For establishing final results, for proving a method completely, it is impossible to rely upon any other experiments than experiments made directly on the species of animals which is expected to benefit by them.

26,522. But if all experiments on animals showed that the thing was noxious, or no use, would you still recommend it for men?—I would first distinguish between useless and noxious. It is, however, evident that if I myself had made a series of experiments on animals and found a method useless, I would have no inducement to try it upon men.

26,523. Pending the information, obtained from results of animals, that the supernatant fluid is of benefit, why do you still say it should be used on men?—That brought us unfortunately to a kind of discussion which I was trying so much to avoid, viz., as to what extent my considerations were sufficiently sound to induce me to apply the method I devised. I stated in my first evidence, that in acting as I did I made use of all my previous experience and knowledge, and might have been as easily mistaken as be in the right. It so happened that the method I employed answered entirely, in all particulars, to my expectations. I had, therefore, up to now, no reason to be shaken in my reasoning, but should like to avoid discussions on this point.

26,524. You think that your own opinion, independently of any backing up by experiments on animals, is sufficient for you to use the supernatant fluid?—I have backed up my opinion by the results on men.

26,525. Are the grounds you refer to, to be found in your experiments, or are they to be found in your imagination?—Mr. President, I beg your kind permission to withdraw now from this discussion.

26,526. (*Dr. Ruffer.*) Have you any facts, from experiments made on animals, in connection with plague, to show that the supernatant fluid is of use in producing immunity?—No, not on animals, and I should add, such experiments are impossible.

26,527. (*The President.*) I think you said you still believe that the pyrexia was a good gauge of the preventing influence of a preventive such as the fluid you have been using?—Yes.

26,528. You think that the pyrexia as distinguished from the quantity of the preventive substance which you have introduced is really a true indication, or a true reason, of the protection?—It seems to be a satisfactory practical guide in using the plague prophylactic.

26,529. Is it a question of fever or a question of the introduction of a certain quantity of antidote, or is the fever a mere unavoidable accident because it is an effect which that thing will always produce?—I have no grounds for believing that the immunity is produced by that rise of temperature, I mean I have no grounds for determining the actual part which the rise of temperature plays in producing the final resistance. But I have adopted the rise of temperature as an indication of the activity of the vaccine; and this indication guided us satisfactorily in our operations. In the inoculations where we observed that the prophylactic failed to produce a sufficient febrile reaction the results were lower. On the other hand, whenever we had a prophylactic which, when injected in moderate quantities, produced a high degree of fever, the results were more satisfactory. It is on that account that we accepted and followed the above indication. In the first experiments in the Mufassil, in Lanauli, and in Kirki, I had a material which I had to inject in increased doses in order to obtain any marked degree of fever. I had finally to content myself with a lower temperature, and the results were less satisfactory. In Kirki for instance, where the difference in the mortality between the inoculated and the uninoculated was only 77·9 per cent., I had a material which failed to produce a sufficient febrile reaction when injected even at a dose of 10 c.c.

26,530. You have no idea of how much of any special chemical substance was present in the fluid?—No.

26,531. (*Prof. Wright.*) Have you any special knowledge of the results in Kirki? We were told in

* *See* App. No. LXXXII. in this Volume.

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Poona that the records in Kirki were kept so badly that nothing could be made out of them. Did you have a special report?—I have not embodied my observations in a special report, but I have all the information and documents to show that the statement which has been made to you in Poona is incorrect. I have in my laboratory all the papers referring to the composition of the families and all particulars as to the occurrences of plague in the inoculated and the uninoculated.

26,532. There must then be two reports, one inaccurate, which was the one which Col. Fawcett was speaking about, and another, an accurate one, which you have. Has your report been communicated to the Commission?—The results have.*

26,533. I understand there was a report kept of results in Kirki locally, and that that report on examination was found to be absolutely unreliable. In addition to that it was suggested that you must have had a special report made to you by a special reporting officer?—I was myself on the spot and collected the information personally with two assistants.

26,534. Have you communicated that information to us?—I have communicated the final results, the number of the inoculated and uninoculated, and the occurrences of plague among them. But the documents which I have in my laboratory have not yet been embodied in a report like that made out with regard to the Daman or Undhera outbreaks.

26,535. Your figures were based on personal inquiries?—I was in Kirki during nearly the whole of the epidemic, and was visiting the infected barracks and the Plague Hospital twice a day; and all the information I have is in my own handwriting, or in that of the assistants who were with me.

(Witness subsequently put in certain statements A., B., and C., regarding Lanauli, Kirki, and Undhera, which are published below; and he also put in a statement which, at his desire, is published as Appendix No. LXXXIII. in this Volume).

A.

OCCURRENCES of PLAGUE in C. and D. Wards of LANAULI, (put together), between the 24th July, 1897, and the end of September, 1897.

Date.	Number of uninoculated present.	Attacks.	Deaths.	Number of inoculated present.	Attacks.	Deaths
24.7.97 -	711	4	4	45	—	—
25.7.97 -	636	5	5	116	—	—
26 7.97 -	621	4	3	126	—	—
27.7.97 -	568	2	2	175	—	—
28.7.97 -	544	3	3	197	—	—
29.7.97 -	472	2	1	266	—	—
30.7.97 -	460	6	4	276	—	—
31.7.97 -	430	3	2	300	1	1
1.8.97 -	398	8	6	328	3	2
2.8.97 -	373	8	6	342	3	1
3.8.97 -	341	1	1	363	1	—
4.8.97 -	336	3	2	336	1	—
5.8.97 -	331	1	—	368	1	—
6.8.97 -	329	3	1	367	—	—
8.8.97 -	323	1	—	370	—	—
9.8.97 -	322	1	1	370	—	—
10.8.97 -	320	1	—	371	1	—
11.8.97 -	319	1	1	370	—	—
12.8.97 -	318	1	—	370	1	1
13.8.97 -	316	1	1	370	—	—
14.8.97 -	315	1	—	370	—	—
17.8.97 -	314	1	—	370	—	—
19.8.97 -	313	1	1	370	1	1
20.8.97 -	312	1	—	369	—	—
22.8.97 -	311	6	5	369	—	—
23.8.97 -	305	—	—	369	1	1
26.8.97 -	305	1	1	368	—	—
3.9.97 -	304	1	1	368	—	—
4.9.97 -	303	1	—	368	—	—
6.9.97 -	302	1	1	368	—	—
7.9.97 -	301	3	3	368	—	—
13.9.97 -	298	1	1	368	—	—
23.9.97 -	297	1	1	368	—	—
Total -	—	78	57	—	14	7
Average daily strength.	377	—	—	323	—	—

* *See* Questions No. 68, and 69, of these Proceedings.

Calculating upon the death rate among the uninoculated, the inoculated inhabitants of C. and D. Wards of Lanauli should have had 49 deaths instead of 7, which is a difference of 85·7 per cent.

B.

OCCURRENCES of PLAGUE in the FOLLOWERS of the FOUR BATTERIES at KIRKI (put together), between the 16th August, 1897, and the end of September, 1897.

Date.	Number of uninoculated present.	Attacks.	Deaths.	Number of inoculated present.	Attacks.	Deaths.
16-17.8.97	1,542	11	10	98	—	—
17-18.8.97	1,307	4	4	322	—	—
18-19.8.97	1,268	2	1	357	1	1
19-20.8.97	1,239	6	4	383	—	—
20-21.8.97	1,105	7	7	511	1	1
21-22.8.97	1,074	5	5	534	2	—
22-23.8.97	1,047	3	3	554	2	1
23-24.8.97	1,044	9	7	552	5	3
24-25.8.97	926	6	4	656	—	—
25-26.8.97	827	6	6	749	3	2
26-27.8.97	812	3	1	755	4	3
27-28.8.97	809	5	3	751	2	1
28-29.8.97	804	4	2	749	—	—
29-30.8.97	800	3	3	749	1	1
30-31.8.97	797	2	1	748	1	1
31.8-1.9.97	795	11	6	747	1	1
1-2.9.97 -	784	9	4	746	1	1
2-3.9.97 -	775	3	2	745	1	—
3-4.9.97 -	772	3	2	744	—	—
4-5.9.97 -	769	5	3	744	1	—
5-6.9.97 -	764	6	3	743	2	—
6-7.9.97 -	758	3	3	741	—	—
7-8.9.97 -	755	1	—	741	1	—
8-9.9.97 -	754	4	2	740	—	—
9-10.9.97 -	750	1	1	740	—	—
10-11.9.97	749	1	—	740	1	—
11-12.9.97	748	3	2	739	—	—
12-13.9.97	745	1	1	739	—	—
13-14.9.97	744	4	3	739	1	1
14-15.9.97	740	1	—	738	—	—
15-16.9.97	739	2	—	738	—	—
16-17.9.97	737	2	2	738	—	—
17-18.9.97	735	1	1	738	—	—
19-20.9.97	734	1	—	738	1	—
20-21.9.97	733	2	—	737	—	—
23-24.9.97	731	1	1	737	—	—
25-26.9.97	730	1	1	737	—	—
28-29.9.97	729	1	—	737	—	—
Total -	—	143	98	—	32	17
Average daily strength.	859	—	—	671	—	—

C. (i)

COMPARATIVE ANALYSIS of the 28 FAMILIES of UNDHERA, where CASES of PLAGUE have occurred, from the date of INOCULATION on the 12th February, 1898, up to end of March, 1898.

Caste.	Inoculated.		Uninoculated.	
	Total.	Deaths.	Total.	Deaths.
Hindus:—				
Carpenter - -	1	—	1	1
Pattidar - -	30	1	27	7
Baria - -	16	2	16	8
Patan wadia -	4	—	3	3
Khalpa - -	4	—	4	1
Sweeper - -	6	—	4	2
Dher - -	10	—	9	4
Total - -	71	3	64	26

C. (ii.)

CLASSIFIED according to AGES.

Ages.	Inoculated.		Uninoculated.	
	Total.	Deaths.	Total.	Deaths.
Under or of 5 years -	13	1	10	3
Over 5 and under 60 years.	56	2	53	22
Over 60 years - -	2	—	1	1
Total - -	71	3	64	26

C. (iii.)

CLASSIFIED according to SEXES.

	Inoculated.		Uninoculated.	
	Total.	Deaths.	Total.	Deaths.
Male - - -	41	3	24	7
Female - -	30	—	40	19
Total - -	71	3	64	26

Mons. W. M. Haffkine, C.I.E.

22 Mar. 1899.

(Witness withdrew.)

The Hon. Sir ANDREW WINGATE, I.C.S., K.C.I.E., recalled and further examined.

Hon. Sir A. Wingate, I.C.S., K.C.I.E.

26,536. (*The President.*) I understand that you are prepared to continue some statements with regard to plague which you favoured us with up to a certain date when we last had the pleasure of seeing you. Will you kindly continue those statements?—When last I had the honour of appearing before the Commission, I was asked if I would prepare some maps showing the results of plague infection. The points in these district maps I now put in,* to which I would invite attention, are as follows:—The date of the first case in each of the three epidemics is given under each village. The three epidemics are distinguished by colours; the first is shown in red, the second in blue, and the third in green. This map (*pointing on map*) being of the Dharwar district, the infected villages are all in green. If you take the total plague recorded deaths up to the week ending the 3rd of March 1899, you will see that they are 94,000 approximately.

26,537. Is that in the Bombay Presidency?—Yes, in the Bombay Presidency. Those are the figures from the published weekly list. During the current year in the Belgaum and Dharwar districts, there have been 17,000 and 30,000 plague deaths respectively up to the end of the same week, the 3rd of March; that is 47,000 deaths in those two districts, which is half of 94,000, the total plague mortality of the Presidency, including Bombay City. One-half of the mortality up to date in the current year has thus occurred in these two practically newly infected districts. That seems to indicate, as compared with the 61,000 recorded plague deaths of the previous year ending May 1898, that, over the very large area of the Presidency generally, the present epidemic has been of a more restricted character—I cannot use the term milder where the percentage of deaths to cases continues so high. I should like to invite the attention of the Commission to a fact that they will see in nearly all the maps, that although, for example, there have been 30,000 deaths in the Dharwar district, these deaths are nearly all confined to this part of the district (*indicating on the map*). This localization is well emphasised in Hubli taluka. Hubli town is *here*. These green marks, as you will see, cluster round first, the town, and then the taluka, though, as you will see, a more distant village is here and there infected. The point is that the spread of infection appears to me to be the same in the country as you find it to be in a town, that is very localised at first, and spreading from a given centre. It is precisely the same if you extend your glance over the whole of India. The Bombay Presidency is the infected area, while the rest of India remains practically free. It is very striking. In every map you will see huge areas—highly populated areas—without a single case, or perhaps one or two only over a whole district. That seems to me to indicate that the outflow from an infected area is probably more dangerous close to the centre of infection than at any distance from it, that as the outflow gets further away from the centre, so the danger seems to lessen. Take the case of a man, starting from an infected house: if he goes 10 miles in two hours, he appears to be more dangerous than if he has gone 20 miles in four hours. Or it may be that the local spread of infection is due to entry into an infected area. It may be a market town which is infected, and where you have a market town the people from all the villages round go into the infected area: and, therefore, you get this localised infection. Into Ahmedabad or Broach the exodus from Bombay I presume has been as constant and as great in the past three successive years, but it is only this year that North Gujarat has been attacked. Take the town of Broach. You will find that it was not until Ankleshwar village, within 10 or 12 miles of Broach was attacked, that Broach was infected. That seems to me to point to the fact that the danger to Broach was not the incoming of people from infected areas at a distance, but either the outgoing of the people from Broach to an infected place within their easy reach, or the entry of people from a neighbouring infected village into Broach. You may say that the people of Broach went and came also to and from Bombay. That is so, but there you introduce two fresh elements—the one of distance and exposure to sun and air when going back to Broach, and the other of the medical inspection and the disinfecting arrangements, whether they involve detention or merely inspection. The people who go from Broach to Bombay by rail, and go back, come under those restrictions; and, therefore, are to a certain extent not so liable to introduce infection, as when the infected area approaches close to a town. And there is the further danger that it is by the people of the town visiting that infected area that plague is also carried. All I wish to point out is, that as the Commission will examine our maps they will almost invariably find that the infected places are grouped; that when they hear of a district with 30,000 deaths in it, and expect to find the

* Not printed with the Commission's Proceedings.

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whole district a mass of plague, it is nothing of the sort. The plague is absolutely localised in certain parts of the district. It is just the same when going into a plague infected town; you will be taken to one or two infected streets, and it is there where you have the chief mortality.

Turning now to the point that was aimed at last year, trying to detain the outgoing population of an infected place, I have a few figures which I brought with me which I should like to show you of the needlessness, after experience, of putting the people to a vast amount of inconvenience. Last year at 12 stations during a period of 5·14 months, 131,074 people were detained for periods varying from 5 to 10 days. Out of those 131,000 people, 201 cases of plague were detected, and nearly 131,000 people were disinfected. At 14 stations this year, during a period of 3·8 months—a shorter period—116,000 people have been disinfected, against 131,000 in the previous year; and 160,000 people haven been subjected to the clinical thermometer test. Out of these nearly 12,000 people have been detained on account of a rise of temperature, and 266 cases of plague have been detected. It is too soon to say what the results in the districts may be of the re-infection from Bombay. You cannot tell that until the next two months have expired. Up to date, at all events, the proportion of the exodus of the two years is not very dissimilar, and 266 cases of plague have been caught, against 201 in the previous year; and only some 12,000 people have been detained instead of 131,000. From what I know, I think that the medical examination has been still more thorough than the detention, and has probably led to fewer cases; I say fewer cases, because if you get a case into a camp, there have been a good many instances of cases after 9 days, 11 days, and 12 days, which seems to me to point to infection within the camp from one to another. On the other hand, by the use of the clinical thermometer, and the detention of all people who show the least temperature, 266 cases of plague have been detected, and apparently now the cases all develop in the first few days. These statistics seem to me to indicate that there is a very small amount of risk from the exodus of the population of an infected town under precautions for carefully medically examining the people. The major part of the exodus from Bombay has gone by sea. Our infected ports up to date this year have been two small places in the Kolaba district, and one, or perhaps now two, in the Ratnagiri district, comprising a very small number of cases of plague. These are considerations which seem to me to indicate that under proper precautions, plague is not a thing to terrify people who are at a safe distance, and who are not likely to go into the infected place itself.

26,538. Do I understand that you are of opinion that this railway examination is valuable?—Most valuable. It is not only valuable because of the cases which it detects, but it is probably still more valuable in preventing sick people from leaving the infected areas. If you look at the weekly list for the Thana district, where the restrictions this year were specially relaxed compared with the year before, you will see that there were 756 imported cases in the first year, and 163 imported cases last year, when there were detention camps; whereas this year the number has increased to 212 up to date. These figures are not confined to cases imported from Bombay, but indicate generally the variations in the number of cases imported from Bombay. In the Thana district, last year, there were less than 1,200 deaths, whereas this year there have already been over 3,000. The medical examination was purposely taken off between Bombay and one part of the Thana district, in order to give a certain amount of play to the Bombay population, and not to tie them up too tight. The medical examination acts in those two ways—that you catch the case that does go out, and you prevent from going out a great number of cases which would otherwise go out.

26,539. Do you think the great trouble involved is quite justified even by stopping 266 infected persons?—Do you mean trouble to the people?

26,540. Everyone; to the officers employed, and the people subjected to examination?—I do not think that there is much annoyance from a simple medical examination.

26,541. I do not mean annoyance—trouble or expense?—There is a certain amount of trouble, and there is a certain amount of expense; but if the examination were taken off, I think that the country would be flooded with plague cases immediately.

26,542. I quite expected you to say that, it is obvious?—I will prove it. Take the town of Hubli. How were all these places round the town infected? Had it been possible to have a medical examination, by the thermometer, of everyone going out of the town, and to detain everyone showing any excess temperature, the chances are that spread of plague would have been less in that part. The other danger, which I indicated to you, of people going into Hubli, would, perhaps, have remained. Persons who go into the infected house, or into the infected area, are susceptible, and may come out infected.

26,543. Have you anything to tell us about evacuation?—This map of the Dharwar district probably represents the state of things as they existed at the end of the rains. Perhaps another 50 or 60 infected villages have been added since; but had you seen the map at the end of the rains, it would practically have been something like it is now. The extent of infection represented makes it look like a hopeless task to anybody, unless you put in an army of people to deal with it. By simply putting in a few English officers—about 10 or 12—and working with the people, not doing anything by force, but working with the people on our side, the results I venture to think have been very remarkable. There was a percentage of deaths on the total infected population of 6·8 before evacuation, and after evacuation the percentage fell to 3·2. There were 17,000 odd deaths before evacuation, and there were a little over 7,000 after evacuation. If I had been able—but I regret to say I have not been able yet—to find out how many of the 7,000 deaths occurred in the first ten days after evacuation of each particular village, I have no doubt we could show our results in a far more striking light. Still, taking it broadly, there were 114 infected villages, with 17,467 deaths, up to the 11th of November 1898. I should like to show you this statement, showing the total plague mortality of the Dharwar district at the same time, as follows:—

DHARWAR DISTRICT.

Plague Progress Statement.

1.

For Period previous to Evacuation, *i.e.*, from 5th August to 11th November 1898.

1. Week ending	2. Total in District Indigenous. Cases.*	Deaths.*	3. Number of Villages infected Week by Week. Progressive Total.	New.	4. Total Number of Villages which had Indigenous Plague during Period stated.	5. Aggregate Population of Villages in Column 4.	6. Percentages. Total Cases and Deaths on Total Population. Cases.	Deaths.	Deaths on Cases.	7. Averages. Of Cases and Deaths per Village through Epidemic. Cases.	Deaths.
1898.											
August 5th	191	162	2†	1‡							
„ 12th	400	316	8	4							
„ 19th	809	671	13	5							
„ 26th	879	657	23	10							
September 2nd	765	589	25	2							
„ 9th	1,038	885	35	7							
„ 16th	1,642	1,300	39	4							
„ 23rd	1,083	916	46	10	115§	255,228	8·6	6·8	80·2	189	151
„ 30th	2,074	1,683	53	5							
October 7th	1,671	1,424	66	10							
„ 14th	1,539	1,237	76	20							
„ 21st	2,368	1,914	78	8							
„ 28th	2,286	1,397	84	11							
November 4th	2,128	1,558	91	5							
„ 11th	2,924	2,358	94	12							
Total	21,757	17,467	—	114							

For Period Evacuation progressing, *i.e.*, from 12th November to 2nd December 1898.

1898.											
November 18th	2,194	1,857	94	12							
„ 25th	2,344	1,876	97	6	23	36,896	4·8	4	82·9	78·1	65·3
December 2nd	1,846	1,562	100	5							
					Cases, 1,799	Deaths, 1,503					
Total	6,384	5,295	—	23							

For Period subsequent to Evacuation, *i.e.*, 2nd December 1898 to 3rd March 1899.

1898.											
December 9th	1,412	1,143	92	13							
„ 16th	1,200	1,035	98	13							
„ 23rd	1,241	1,061	111	13							
„ 30th	1,080	860	107	26							
1899.											
January 6th	806	662	94	12							
„ 13th	792	641	94	12	143	224,055	3·9	3·2	82·3	61	50·2
„ 20th	495	467	84	9							
„ 27th	285	238	81	7							
February 3rd	195	139	66	5							
„ 10th	360	255	59	10							
„ 17th	344	253	54	9							
„ 24th	292	235	46	7							
March 3rd	118	87	37	4							
„ 10th	104	89	44	3							
Total	8,724	7,185	—	143							

* The figures are taken from the daily reports.
† Hubli and Dhárwár.
‡ Dhárwár.
§ Hubli has been added to complete the total.

Note.—Villages in which the figures and facts are not reliable and those which had imported cases only are omitted.

You will see that from the middle of November, when evacuation began to take effect, the death rate immediately began to decline. After evacuation, there were 143 freshly infected villages (excluding the 114 I mentioned before), and there were 7,185 deaths after evacuation. That gives, previous to evacuation, an average of 151 plague deaths per village, and after evacuation, of 50 deaths per village. From the figures, you will see that the Dharwar district has declined from a weekly mortality of over 2,000 to a weekly mortality of about 74. This improvement has been gained, I think, almost without a single prosecution, without almost a complaint, and practically by the people themselves, under the guidance of a few

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officers. The evacuation of villages is now so well understood, that we are continually hearing of villages where a few dead rats have been discovered, and the villagers have, of their own accord, immediately vacated their village. They are continually vacating villages now, of their own free will. Therefore, I think, that in future the expenditure is capable of great reduction, because we shall be able to rely more and more upon the voluntary intelligent action of the people. The people are coming forward in a most splendid way, not only to bear the great death rate when it attacks a particular village, but in the way of self-help. I think, considering how very exposed they are to infection, and how very liable they are to take it, that the way the people are coming forward to carry out the measures which they are convinced are necessary is most creditable to them. Up country, I think I am right in saying that, in small towns and villages, particularly during the fair season, there need be very little difficulty in checking an epidemic. Infection was very quick here (*indicating on the plan*). It has thrown out a great deal of plague in various directions, but from the moment evacuation came in the spread of the disease has been checked. Evacuation acts in this way. First it checks the epidemic. If it is done early in the epidemic, it will check the epidemic absolutely; if it is done after an epidemic has become pretty severe, it will bring down the mortality very rapidly. Evacuation will therefore lessen mortality; and it will decrease the duration of the period. If information is got early, and evacuation is prompt, your epidemic may only last a few days, or a few weeks, and thus curtailed, the spread to new villages is arrested. The longer an epidemic lasts in any particular town, the greater the danger of the infection spreading. On the other hand, if you have turned out the population of the infected village to live in their fields, the villagers from villages all round do not visit the infected area, and there is no danger from visits to the evacuated population.

26,544. In the favourable figures which you give us, I suppose you include cases in which evacuation was carried out in circumstances which were not the best for the evacuation—where it was done late in the epidemic, and so on?—Yes. All villages are included. Evacuation was begun late because of the rains. It might very reasonably be said: "The epidemic has "stopped of itself; it is an accident that your evacua- "tion measures began in November, when the thing "was going out of itself; it is a declining epidemic." To that I would invite your attention to the figures in the Bijapur district, which is in all soil and climatic conditions very akin to parts of the Dharwar district which were infected, and where evacuation was able to begin immediately. If you compare the villages, they are few in the Bijapur district, though a certain number of new villages are always being attacked. If the mortality be compared, in the Bijapur district there are seldom more than 25 to 50 cases per village, frequently much less; whereas in the Dharwar district, during the rains, there are few villages which did not suffer severely. I will take one village of 1,280 people. There were 424 deaths—that is 33 per cent. of the population—was carried off. In another village of 4,222 souls, 922 persons died, or nearly 22 per cent. That just shows what plague can do if circumstances are such that the people cannot help themselves, nor can the Government help them. The problem now before us, I think, is solved, with regard to the method of dealing with plague up-country during the cold season—which is the dry season for up-country. I think plague is always capable of being controlled under these circumstances inexpensively and comparatively without compulsion or coercive methods which are very unadvisable to resort to. The problem still unsolved is: What are we to do in the next rains, when a town or village has been infected? Our experience up-country has been that the villages which have been attacked during the rains have mostly occasioned a great spread of infection. Take Igatpuri, Sirur, Belgaum, and Hubli; in every case the attack has been during the rains, and in every case the spread has been very serious. There is another point, perhaps, to consider, upon which I can offer no explanation, although it seems to me to be somewhat borne out by the maps, namely, the fact that in the Deccan the spread does not seem to be so rapid or violent as in the black soil districts of Satara, Kolhapur, Belgaum, and Dharwar. The seizures appear to be very rapid in those districts. There is no apparent reason why a town like Poona, or say Sirur, when attacked, should not have produced the same tremendous spread as has been the case further south. There seems to be something which I cannot venture to explain. I only offer it as a fact which I have observed.

26,545. With regard to the black soil districts in the rainy weather, how do you propose to deal with an epidemic?—If evacuation is considered to be impracticable, the only thing that I think is feasible in the rains would be to prevent the population from leaving save under restriction. To confine a population has always appeared impossible, as being inhuman. But given inoculation, I think it would be quite feasible to say to the population—"You can be inoculated or you "can leave the town after temporary residence in a "camp or you may stay in the town." We might offer a protective prophylactic, but leave an open door under certain precautions. Probably medical examination by the clinical thermometer and disinfection of all articles of clothing and bedding would prove sufficient without temporary residence in a camp, and in any case the inoculated must be subjected to these precautions. Then it might be necessary to control the entry into the town, because although the residents may be all inoculated, it seems to me that the town itself may be infected. This surmise is due to what appears to be the difference between plague and every other disease, namely, that plague attacks animals, particularly the domestic creature, the rat. The rat in India runs at night about everybody's house and over and about the sleepers on the ground, and therefore although you may have an absolutely protected because inoculated population, yet if plague were imported into the town, and the rats caught the infection, it is quite possible that there might be a certain amount of local infection independent of people. To what extent that may be the case I am not prepared to say, but there is this fact which is very difficult to get over, and which all our officers have had experience of over and over again,—that when an evacuated population re-occupies the village site and the villagers open up and clean the houses (the rats naturally go into the roofs and other places), a certain number of plague cases sometimes result—perhaps very few—even after the population has been free from plague for weeks before they go back to their houses. That seems to be explainable, not by the fact that the whole place has remained infected—for if it had been, everyone would have got plague when they came back—but by the probability that, a day or two before, some diseased rat had infected the particular house in which the fresh plague case suddenly occurs. I do not think the infection lasts long. Localization of the disease appears to imply that the infection cannot be a permanent one, since it is rarely carried to a distance. I hope in the next two months the Dharwar district will be absolutely clear, because there are no big towns now infected. Therefore the situation there, I think, is a very hopeful one.

26,546. Have you arrived at any conclusion as to the maximum population of a town that might be evacuated under ordinary favourable average conditions?—I would not resort to total evacuation in any town—in which the people did not themselves desire it—in which the population was over, say, 50,000 or 60,000. I daresay evacuation could be done quite easily up to 100,000. Even in the case of a town of 50,000 or 60,000, I would hesitate if the people objected. Take an example of Broach, which has just been infected. The Collector there, with only partial evacuation, seems to be carrying the town through what would be a very severe epidemic, with a comparatively small mortality. I do not think there are more than 5 or 6 cases a day, whereas there ought to be 20 or 30 according to previous experience, and the time the epidemic has lasted. The mills are all working, and the trade but little interrupted, and everything is done with the goodwill of the people. There is no great spread of infection to the surrounding villages. I do not attach very great importance to a certain spread from the town, because from experience in Sholapur, or Admednagar or Malegaon or Jalgaon, all places which have been completely evacuated, the infection has been checked directly the townsfolk have gone out, and the surrounding country has been cleared very rapidly of any infection which has followed the evacuation. I do not think that the villages are of much importance, except near towns; but when a village gets infected near a town, it ought to be absolutely evacuated at once, under precautions that the villagers cannot enter the town.

26,547. You do not, therefore, think that the mere ocess of evacuation is likely to extend the plague to e surrounding country especially?—No; I think that evacuation is done in the early stages it certainly will ot. The only time it does extend the plague is when ou get a very badly infected place, and turn all the eople out. The result is that you stop a dangerous idemic, and immediately infect 10 or 20 villages; but ou turn these villages out at once. It takes a few weeks get the place clear, but it is done, and the epidemic over. I mean there is much less danger, as it seems me, in evacuating a town like Sholapur, at the risk f 10 or 20 villages being infected, and having two or ree cases in each village, than in keeping a central lace like Sholapur infected for, perhaps, eight or ten onths, during which time the infection may be going ll over the country.

26,548. In either case there is a necessary risk of xtension from the infected area, but on the whole you hink that that the risk is almost smaller, where perfect vacuation has been adopted, than where the disease is llowed to run its own course?—Quite so, and the uicker the evacuation the less the risk.

26,549. Is there any other point you would like o bring before us?—I present this chart,* exhibiting he weekly total mortality of Bombay City to the Commission made up to date. I think it is a very emarkable chart. It is what we have had to guide us n Bombay City; it is the expectation we have followed oth before and since the present epidemic re-appeared. It seems to me to point to certain conditions of the epidemic which are, perhaps, controllable—I will not ay they are controllable, because I cannot say how far what we do may influence or not influence an epidemic. I hear statements that the epidemic is infinitely worse this year, and that we are going from bad to worse, but I do not think so at all. The total mortality last year in Bombay was over 55,000; we are now up to 40,000 deaths from all causes, and we have only two more months to run. I do not think we shall reach the 55,000 of the previous year in this present epidemic. Of course, I cannot tell what is in the future, but if the epidemic goes on as it has done, we ought to have only another week or two before it falls. We do not know how long the high mortality may continue up and down, forming what I may term a truncated cone on this chart, but the turn appears to have come just as it did last year.

26,550. Could you very briefly tell us what are the measures which are being adopted at the present moment in Bombay?—The two principal measures which are being adopted at the present moment are those which have been in force all along. Where a house is unsuitable for the patient to be treated at home, he is taken to hospital, and I am glad to say that I think in almost every instance the patient goes perfectly willingly, and with the concurrence also of his friends, some or all of whom accompany the patient, the reason being that the hospitals now are as attractive and comfortable as probably it is possible to make them. The only difficulty that I can see in the way of hospitals being thoroughly popular in this particular epidemic is the terrible death rate. As an exception, I may mention, the Plague Hospitals in Dharwar and Gadag, where I think there was a very considerable number of inoculated patients. The returns from these two hospitals are certainly remarkable. There are no two hospitals in the whole of the Presidency, so far as I am aware, which can show the same results in cures, and I take it that that is due to the proportion of inoculated patients who passed through the hospitals. I do not see any other reasons, because the death rate outside the hospitals was pretty stiff in the Dharwar district. There was no less virulent type of disease there.

26,551. From the evidence we have, we understand that very few inoculated persons have had plague at all in Dharwar?—In the hospital statistics of Dharwar and Gadag, you will see that there is a certain number of inoculated people who were admitted into the hospitals, of whom comparatively few died, and that, I think, must tell on the general percentages of recovery.

26,552. What further measures are being adopted now in Bombay?—There is evacuation of the infected house, if the house is not of a nature to be disinfected immediately, and the people allowed to go back immediately. If, for instance, it is a crowded and extremely badly constructed house, then there is evacuation, but it is a short evacuation. That, again, is a difficulty in Bombay. The people would go out a great deal more readily if they had not to go back again, but they do not like going back to the houses again—they would prefer to remain in the camp.

26,553. Because they are frightened of plague?—No.

26,554. Because they like the camp?—Yes. Directly they are out they infinitely prefer the camp. The great trouble is to get the people out. That, I have found, was the same in England. When I went to see a segregation building in London, I asked the Health Officer if he had much trouble in getting the people there. He said that there was a great deal of trouble in getting them there, but that they had sometimes to smoke them out. It is the same here. You give them a very comfortable, light, airy place, compared with the den from which they came. It is, of course, only the very poorest who go to these camps.

26,555. They appear to appreciate the surroundings?—Yes, and the absolute safety from the plague.

26,556. Are these the two great measures which are now being followed?—Yes.

26,557. Putting it generally, they are not quite so stringent as they formerly were in other epidemics in Bombay, are they?—The difference now is that perhaps there is not the house searching, but I do not know that that led to any more cases being discovered than now. Some people think that there were more cases found last year, and others think that there were less, but in all statistics you require to look very carefully to see how the figures are compiled before you accept comparisons.

26,558. House searching was not favourably regarded by the people, I think?—I think if measures have any effect at all upon the progress of the epidemic—if we assume that the present epidemic will begin to fall in the course of a short time, a few days or a week—if, that is to say, the present epidemic is going to draw the same pattern upon that chart which it has drawn in the two previous years, and if the measures have any effect at all upon the drawing of that line,—it stands to reason that the measures in the three epidemics have been very nearly equal in their effects on the mortality in proportion to the population. The measures may have some cumulative influence, or the epidemic may be waning, because the death-rate, in proportion to the population, seems to become rather less each year. The death-rate was highest in the first year, rather lower in the second, and I think will be lowest in the third; therefore showing that stringency does not lead to any better results than relying on the co-operation of the people, and not advancing in front of their intelligence, or rather driving them against their intelligence, but waiting until you can take their intelligence along with you, in which latter case you get far better results every year. I look forward to the intelligent co-operation of the people in Bombay with great hopefulness. I think, if the epidemic is to be conquered, it will be conquered through and by the people, as every other great reform in India always has been.

26,559. I understand there is a considerable exodus from Bombay at the present time?—There is some exodus, and perhaps you are right at the moment in calling it considerable, but that is only true of the exodus during the last week or two. During December of 1897, and January and February of 1898, 94,893 people left Bombay in excess of arrivals. In the same three months of 1898–9 the excess was 115, 863, a difference of some 2,000. Considering that this year the population has been very much larger than it was in the previous year, and that trade and the going backwards and forwards have been much brisker, the difference of 21,000 in the two years is a mere bagatelle—it is not worth talking about in a great population of 900,000. No doubt, if we had the figures before us of 10 years back, we should find that in one year there were 20,000 who left the town; in another year 30,000, in another year 5,000, and so on. But undoubtedly there is a difference this year. If you take the individual months, you will find that in January 1898, 52,430 people left Bombay, and in January 1899, 40,444. Up to that date there is no difference. This year, in fact, fewer left in January in proportion to the population than in the previous year. This deficit is made up by only 38,489 going out in February 1898, against

* *See* App. No. LXXXV. in this Volume. The chart was later completed up to May, 1899.

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79,481 going out in 1899. You may say this exodus in February is due to the increase of plague, or you may say it is due to very much fewer people having gone out in the month of January, compared to the month of January in the previous year, but taking the whole three months together there has not been anything unusual. Then at this time of the year the harvest is going on up-country, and there are perhaps two or three hundred thousand people who have come temporarily into Bombay, many of whom, after earning their bread here for two or three months, go back to their harvesting, and therefore, if there is an exodus now, I do not think it is very much in excess of the exodus which would take place in an ordinary year. The difference this year is, perhaps, that people are not coming in so freely to replace those who are going out, which very often happens. In the first 17 days of March last year, 18,234 people left, and this year, in 17 days, 53,980 have left. Last year the epidemic was going down during that period, or at all events was not very bad. There is the difference between 18,000 and 53,000.

26,560. Assuming there is no important difference in these two years, there is at the same time some slight difference at any rate?—There is some slight difference, but I should like to say that it is very much less than than we expected. Last year we tied up the town, we had the gates locked, so to speak. This year they have been opened absolutely, and we expected an enormous exodus, which, however, has not taken place. The reason we expected it was that last year the natives all said we would not let them go out, and that they were dying here.

26,561. Assuming there is no important difference between the exodus of this year and the exodus of last year, at the same time there is a slight difference, and I understand there is a slight difference in the degree of stringency of the plague measures, to the advantage of the present period; the measures are not quite so stringent now?—They are not carried on in the same way. There were certain measures adopted last year which which were very objectionable to the people.

26,562. How do you account for the exodus continuing, notwithstanding that the measures are not quite so irksome to the people? Do you think that the plague measures, or the plague itself, has had chiefly to do with it?—I think the best informants in Bombay will tell you that the exodus, so far as it extends to the lower classes, is very largely due to the reason I have given, namely, the annual reason—the harvesting, which calls for the return of a great number of people, who are cartmen, and porters, and mill hands, and so on, who have all got their agricultural interests up-country. Then the other reason which has come under my own notice to a certain extent, is that the higher class of people, among whom there is very little plague, but who are no doubt extremely sensitive—people who read the statistics every week in the paper, and get sensitive when a case comes near them—think that it is time to go, although they are not the class which gets plague. There is also some exodus due to the approaching hot weather.

26,563. Having regard to the facts which you have laid before us, do you think the exodus is chiefly to be accounted for by people wishing to avoid plague measures, or by people wishing to avoid the plague itself?—I do not think there is an exodus from Bombay now due to the people avoiding plague measures. There may be individual instances, but certainly not generally.

26,564. (*Mr. Hewett.*) Last year there was quarantine?—Yes.

26,565. You cannot compare the two years, because there was quarantine against Bombay?—Yes.

26,566. The people were not allowed to leave this place, unless they were segregated for 10 days?—Exactly. This year, although the gates are open, the exodus is extremely nominal. I do not think it is exceptional, except during the last week or two, and with regard to that, as I say, we have no statistics of previous years to set against it, but there is always a hot season exodus. I said the other day to some of the high class families in Bombay who were leaving here, and taking their wives and children to the extent of 22 or 23 members each, when they came to me to get letters of introduction—"Why are you going away?" and they said—"We go every year as the hot season comes on." They would not admit that they were running away from plague. They said, "If we had "been running away from plague, we should have "gone two months ago, or at all events a month ago, "but we have stayed right through the epidemic. We "believe the epidemic is now nearly over, but the hot "season is approaching and we are off." Whether that is true or not, I do not know, but I do not attach much importance to the exodus. I thought it would have been four or five times what we can show from our statistics.

26,567. (*The President.*) Then there is a reduction in the number of people who are entering Bombay?—Yes.

26,568. What do you think is the probable cause of that reduction?—The fear of plague. It is natural that people would not go to a town where there was an epidemic. That fear did not exist so much in the early days. I think that fear is growing on the people—the fear of going to an infected town.

(Witness withdrew.)

(Adjourned to London, *sine die*.)

At The India Office, London.

SEVENTY-FIRST DAY.

Saturday, 6th May 1899.

PRESENT:

Prof. T. R. FRASER, M.D., LL.D., F.R.S. (*President*).
Prof. A. E. Wright, M.D. | Mr. A. Cumine.
Mr. C. J. Hallifax (*Secretary*).

Surg.-Gen. C. Planck.

6 May 1899.

Surgeon-General C. Planck called and examined.

26,569. (*The President.*) You have retired, I think, from your appointment of Superintendent-General of Vaccination and Sanitary Commissioner in the North-Western Provinces and Oudh?—Yes, I served for about 20 years as Sanitary Commissioner and Superintendent-General of Vaccination in the North-Western Provinces and Oudh and retired in 1886—13 years ago.

26,570. You have prepared an interesting Report upon Mahamari, which we propose to print in the

idence*?—Most assuredly it is at your service. My sire in writing that Report was to record the facts ted at the time of inspection, unprejudiced by ieory.

26,571. We will assume that that is taken in, but ere are a few questions we wish to ask you in ldition.

26,572. (*Prof. Wright.*) I would like to know more the conditions of the people who infect themselves visiting plague houses, as I believe they do?—So has seemed to me.

26,573. I am anxious to know whether these people ear shoes or stockings?—The heads of the villages, uch as the Padhan (Headman) do wear shoes, but the oor people have no shoes. The poor people go about their naked feet. I never saw a poor man with shoes, o the best of my recollection; they would not dream f shoes—they have no men to make shoes. My expeience of the disease was gained in isolated villages far way from civilisation. The value of my report, to my ind, lies in its proof that plague was in India long efore the existing great trouble arose. The same isease was in Kamaun and Garhwal as now prevails in he plains, without apparent differences. Some medical uthorities inclined to the opinion that the disease was severe form of typhus, but that, I think, was a mistake. It was true plague and could not be otherwise amed in English. With reference to the present outreak in the plains, of course there seems to be a uspicion that it may have been imported to the plains y some person or goods coming down from the Hills. But there is a considerable traffic by ships and junks etween Hong Kong, where plague prevails, and Bombay, and that seems to me the more likely route f plague introduction to the plains of India.

26,574. Is there any traffic between Garhwal and Thibet?—In the spring and summer people are passing o and from Hundes, a country in the neighbourhood f Thibet; they travel across the passes when the snow melts, especially many flocks of sheep laden with borax n small bags accompanied by men and women (*see* para. 222 of the Report)*; most assuredly they would be in contact with people on the other side of the Himalayas. I do not know whether there was a prevalence of plague in the Himalaya mountains beyond British territory, but there was in China, in Yunnan itself, as mentioned in the last paragraph of Dr. Watson's Report.* Yunnan is a country of great extent, but part of it is on the China side of the Himalaya mountains.

26,575. Do you think the outbreak of plague in Garhwal might be due to this cause, or do you think the germ lives in the country?—I think the germ lives in the country of Garhwal and Kamaun. I think from the statements of my report, small in degree as they are and, perhaps, unimportant from the present extending view of the disease that plague broke out in the villages which I inspected in this way. It was generally a child or a woman living almost continually in the house who first suffered, and the cause of the disease seemed to be in the house itself. With regard to preventing the people from remaining in their habitations after plague has attacked a member of the family, I think you will find that when plague becomes well-known in the plains of India the people will flee out of their houses of their own accord. I have been 32 years in India and I never heard of a case of plague in the plains of India in all that time, so that the plains people know comparatively little about the disease yet. I believe you will find that if plague goes on for two or three years, the relatives of the sick person will flee away and leave him in the house. They will vacate their habitations. Plague has existed in Garhwal and Kumaun probably for centuries. By the present native generation it was supposed to have been commenced as a manifestation of God's displeasure, owing to the incorrect, or new, method of performing a certain religious ceremony by the then head priest (*see* para. 45 of Report).* Of course these unusual ceremonies had nothing to do with the outbreak of plague. It must be due to a specific microbe or some hurtful influence of that character (para. 248).*

26,576. Do you find that people may go on having plague when they do move out of their houses?—Yes, they would carry away the disease with them to the hut in which they lived on the hill side. That was not a frequent occurrence, but it did so happen occasionally, as the histories of the Report* show. As a general rule, the re-appearance of the disease amongst the people who had vacated the family home, arose out of the return of a number of the family to the home to fetch grain or other food, or to house cattle at night. The people, when they vacated their homes, lived in temporary huts, high up on the mountain side, or in caves. The disease seemed to be in the houses, and the people knew it, for in some instances, they tried to persuade me to burn their houses, and even burnt them of their own accord (para. 262).*

26,577. Are there many rats in this part of the world?—There are many delicate grey rats, but different in appearance to the rats of Europe.

26,578. Was the mortality among rats always noticeable in plague epidemics?—Not always. The rats I saw were grey in colour, rather squirrel-like, and about as long as a man's hand. As a rule, the death of rats in a house seemed to presage the appearance of plague disease *de novo* there, where rats had not died the outbreak of plague disease in the house appeared to have resulted from contagion. The rats did not seem to me to have died from poison, but from suffocation (para. 254).*

26,579. Did you see any rats dying?—No.

26,580. You did not observe that they died a very long lingering death?—No, they were found dead on the floor at night among the sleeping people.

26,581. Did you find many mild cases of plague occurring in association with these severe cases?—I do not think there were many mild cases. I had a history of 240 cases, and only 17 cases recovered, so that they must have been severe, as a rule.

26,582. Do you think that buboes are familiar objects to the people of these parts?—Syphilitic buboes do you mean?

26,583. No, I mean generally, are the people accustomed to suffer from mild buboes, or is a bubo a rarity in these parts?—I should say that syphilis is rather prevalent among them, and I should suppose buboes would be, but Medical Officers of Dispensaries know more about buboes of that character.

26,534. You have never been up in these Hills except in plague times?—I have been there for 12 years on and off.

26,585. If cases of mild bubo occurred there, would they have come under your observation?—No. I know that syphilis is very prevalent in the Himalayas, but I do not think there can be any doubt about the plague disease I saw, because I examined patients, and they had a temperature of 105 to 110, the pulse at 120 and 130, and some died in 12 hours after the attack commenced, and then no buboes were visible.

26,586. I do not think you noticed in your report congestion of conjunctivæ as a sign of plague?—No, it is so long ago, that I do not remember it. It was not likely that I should have noticed it if it had existed. At para. 117 of my report, the eyes were seen to be "suffused." I think that points to congestion of the conjunctivæ. But I cannot be sure now. I certainly did not make a point of looking for conjunctivitis. Generally, the patients eyes were closed.

26,587. Have you noticed that in their delirium, the people were inclined to wander about?—Yes, and fall down, or throw themselves down into the ravines. There are one or two cases recorded in my report that so ended in death.

26,588. I understand you to say that the grain did not seem to cause any infection?—No, I think not, Dr. Watson thinks it did, as recorded in his report appended to my own report.* I do not think you will find plague disease is due to any hurtful character of the grain consumed, but to something much more serious than any unwholesome food. I am quite sure the people will flee away from their homes if the plague continues to prevail in the plains of India. They will leave their sick friends and flee away as from certain death. It was so with regard to this plague, or Mahamari, in Garhwal and Kamaun. The prevalence of the disease was not known to the authorities for some time, until at last the Commissioner of the Province noticed that the people suddenly left their homes, and inquired as to the reason for this unusual flight. "Mahamari" is not the native name for the disease to avoid which they fled away. Mahamari means any deadly form of

* *See* App. No. XXV. in Vol. II. of these Proceedings.

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disease. "Gola-Rog" is the correct local name for plague, its exact translation being "bubo-disease." "Rog" is the Sanskrit word for disease, and "Gola" for bubo or boil. Any epidemic form of disease, for example, cholera, is called Mahamari during its prevalence (*see* para. 245).* The Civil Officers of the Government, recorded the term "Mahamari" as the native name of the disease, hearing it so spoken of. But the true native name of the disease is as good a definition of plague disease as any yet recorded, I think. Anyway, its fairly accurate translation as "Plague of Boils" is strangely in accord with the sacred record of the disease from which Egyptians had to suffer in the time of Moses.

26,589. Did any epidemic come under your observation where the mortality was comparatively mild?—I have seen no other cases, except those which are mentioned in my report. There were comparatively few deaths in any one village—about, at most, 14 or 15. This I attribute to the fact that the people vacated their homes quickly, taught by experience. Absolutely all my knowledge in regard to plague disease in Garhwal and Kumaun stands recorded in my report* which is to be printed in evidence.

* *See* App. No. XXV. in Vol. II. of these Proceedings.

(Witness withdrew.)

Surg.-Col. J. Richardson.

Surgeon-Colonel J. Richardson called and examined.

26,590. (*The President.*) I believe you have served in the Vaccination and Sanitary Departments of the North-Western Provinces of India?—Yes. I have served for over 30 years in India.

26,591. Will you tell us between what dates you served?—I entered the Service on the 27th July 1859, and left on the 19th May 1894.

26,592. And you have had some opportunity of observing the disease which resembles plague in Kumaun?—I have.

26,593. (*Mr. Cumine.*) On how many occasions had you that opportunity?—Only two occasions that I can distinctly remember.

26,594. Did you see many cases or few?—I saw few.

26,595. Had you any opportunity of watching a case from its beginning to its close, or making a *post mortem* examination on the body of anyone who had died of this disease?—No, I had not.

26,596. In what years did you visit Kumaun?—In 1884 and 1886; but I visited Chaudans in 1876. I did not see plague there, but I was in a village from which the people had just fled on account of the plague, and I saw dead rats there. That is the only material fact I can remember in connection with it.

26,597. Will you describe your first visit in 1884?—When I reached the village of Liti near Shama, on the way to Tejam, I found the place deserted and partly burnt down. The inhabitants were living in shelters at some distance from the village. I could get none of them to enter it with me. In one house I discovered the dead body of a young man, of about 20 years of age, in a sitting posture, resting against the wall. Death had taken place too long previously to make it possible for me to come to any conclusion from the examination of the body as to the cause of death. The mother of the lad came to within about 100 yards of the village; and, with tears, told me of her son's illness, and of his death from gola or phutkia. She said she was not allowed to stay with him or to go and see to the disposal of his body. None of these people spoke of mahamari, only of gola or phutkia.

26,598. How did they describe the disease to you?—As a fever which began like an ordinary fever, but which speedily became violent, depriving the patient of sense and proving fatal in from two to three days.

26,599. Did the people who developed swellings in the groin or armpit seem to have a better chance of surviving, or a worse chance?—They did better because they lived longer. They did not develop swellings until some days had passed. Those of the sick who survived for a longer time showed swellings in their groins and armpits.

26,600. In this case was there any history of the death of rats in the village before or during the outbreak?—There may have been, but I cannot remember.

26,601. Will you describe to us your visit in April 1886 to the village of Bargaon?—I remember all the salient features of my visit there, but I do not remember the details. In April 1886, I visited the village of Bargaon in Kumaun, near the road after it crosses the Ramganga leading into Garhwal. The village had been evacuated by the inhabitants excepting by a man who was ill with gola, and too weak to move. This man had swellings in his groins, and was suffering from fever, but was quite sensible, and was in hopes of getting well. The swellings, he said, were getting smaller; there certainly seemed no tendency to suppurate. A relation came and placed food and drink by him, and he was attended to by a Hospital Assistant from the Ganai Dispensary. Some of the other villagers had been ill but were recovering; they were living in the vicinity of their village, although they did not enter it.

26,602. How did the villagers say the disease began in this place?—They said it began as the ordinary kind of fever—they called it sanjar—which is not uncommon in the Hills. It becomes contagious, I believe; anyway, it prevails in an epidemic form in villages, but it is unaccompanied, so far as I know, by bubonic enlargements.

26,603. In this case, after the sanjar had gone on for a little time, did they notice that anybody had a swelling in the groins?—They did. The account they gave me was that when they were carrying one of their number down to the little stream below, to burn the body, they noticed swellings in the groins, and they hastily threw down the body. I think the Patwari told me it had been eaten by jackals. Anyway, it was thrown down and the people cleared out of the village.

26,604. When they discovered the swelling in the groin did they say: "We were wrong in supposing it "to be sanjar; it turns out to be something else"?—That was their belief, that the disease which they had in the first instance thought to be sanjar was actually gola. They did not imply that the disease grew from sanjar to gola; that was my own idea, not theirs. They said that at first they believed it was fever, and seemed to think it was ordinary sanjar, severe form of fever, something of the nature of typhus. Then when they were taking this body down to be burned, after several had died of the other disease, they discovered these swellings, and said they had made a mistake and that it was a more severe form of the disease. I will not say they said that—I made a mistake—because they did not connect the two. They said it was gola. I never heard them make use of the word "mahamari." They threw the body down and cleared out of the place. They were not afraid of the first disease, but with regard to the other one they said: "This is the real "'golarog' which will kill us all," so they left.

26,605. Did you understand that golarog and sanjar were distinct diseases, or that golarog is merely a severe form of sanjar?—I did not gather from their account that they believed the diseases to have any connection. At least they did not express it to me, and I never heard them say that the two diseases were identical.

26,606. Did you observe anything which showed that a house remained infected after the people had abandoned it?—They told me that one man had died after the village had been evacuated by all the people, and that this was a man who went back to fetch some grain, or flour, out of his house. After the visit to his house, he contracted the disease and died. That is the account they gave to me; it is not what I saw.

26,607. In this village of Bargaon did the people tell you anything about the death of rats?—I cannot remember that. Probably they did, but I could not say they did. I am speaking from memory.

26,608. Ten years before that—in 1876—when you visited Chaudans in Eastern Kumaun, had you been shown any dead rats?—The village at the time was empty; but a man who belonged to the village came round the village with me, and showed me some dead rats. He said: "These have been dying about here; "they died before the outbreak of this phutkia."

26,609. You saw several villages in which golarog had been prevailing in 1876?—Yes.

26,610. Did you see any cases yourself?—No, I did not.

26,611. What is the character of the dwelling-houses in that part of the country?—The dwelling-houses are generally dark and ill-ventilated. One had almost to enter on hands and knees, and could scarcely stand upright after entering. These rooms, or rather garrets, were crowded with baskets of grain, baskets of wool, and goatskin bags of flour, wooden vessels filled with ghi and other domestic belongings; usually the cattle occupied the ground floor underneath these garrets or lofts.

26,612. Is that general?—That is pretty common, I believe; that is not confined to Chandans alone.

26,613. Have you seen any cases of the plague which is now prevailing in the plains of India?—Yes, I saw them in a cursory way in a hospital at Sholapur—about 18 or 20, so far as I remember.

26,614. From the cursory examination which you were able to make, did the disease appear to you to be similar to the one in Kumaun?—Most distinctly. I would not say it was identical. I did not enter into an examination of the cases sufficiently to entitle me to express a decided opinion.

26,615. (*Prof. Wright.*) Was the plague of mahamari spoken of by the people there as a familiar thing, not quite a new thing?—As a familiar thing. They seemed to be quite accustomed to visits, if not in their own villages at any rate in other villages about. They did not speak of it at all as a new thing. They were very much afraid of it; but it was not a thing that came upon them as an unexpected calamity.

26,616. They are familiar with both sanjar and golarog?—Yes, they are familiar with both.

26,617. Do you think the outbreaks in this district might be caused by importation from the far side of the border?—I would not say what is possible or impossible; but it struck me that it might be that the disease may have originated in this way. I suppose you must have germs before they can live, but the conditions were such in these houses as certainly to encourage the development of contagious fever of the nature of typhus. That was the idea we all had for many a day.

26,618. You thought the local conditions were sufficiently bad to account for the disease without any theory of importation?—It might have been imported from time immemorial, and the germs may have remained there in a latent condition. They may have become excited by the excess of those conditions favourable to their development.

26,619. Do you know if the people in this district wear shoes on their feet?—The wearing of shoes is, I think, chiefly determined by the part of Kumaun dealt with—low down in the warmer parts they are, as a rule, not worn; high up near the snows both shoes and socks are often worn. The people do not always wear shoes, I allow; they do not in the summer time any way.

26,620. Cases came under your observation where people got plague from re-visiting their houses after the houses had been evacuated?—Yes, I have heard repeatedly of that.

26,621. In cases of that sort, did the people who returned to their houses wear shoes, or did they go about barefooted?—I cannot say, because I do not remember.

26,622. You do not know what the habit of the country is?—I know the habit of the country well.

26,623. Is it the habit to wear shoes, or to do without them?—The majority of them do not wear shoes. The coolie people and the labouring classes, who form the bulk of the poor people of the community, do not wear shoes as a rule.

26,624. Not even in winter?—That I do not know; I have not been there in winter, but I think not. The better classes nearly always wear shoes with a sort of woven woollen uppers and leather soles. The "uppers" are usually made of woven woollen bands.

26,625. Do you know what their sleeping quarters are like, do they sleep on the ground?—They sleep in the lofts; in the upper storeys.

26,626. Do they sleep on the floor?—Yes, they do not have beds. I have never seen anything in the way of beds in most of the places I have been in. The people sleep just on the boards on the loft or garret in which they live. Some live in the lower storeys.

26,627. What are the floors made of in the upper storeys? Is the upper floor a boarded floor?—Yes.

26,628. Is the floor bare boards, or is it covered with cow dung?—I do not think I saw that; it was very dark. In the lower storey, probably, they have cow-dung floors.

26,629. (*The President.*) There is much cow-dung in the lower floors?—Yes; they were generally covered with it; the cattle lived there.

26,630. A foot or two of cow-dung?—Perhaps so; until the season came when they carried it out in baskets and put it in the fields.

26,631. In the upper floors, what were the means for ventilating the rooms?—The ventilation found its way between the slates. They were mostly covered with big, coarse, thick slabs. There were bits of holes now and again, in which they stuffed something, as a rule. They were very ill-ventilated and dark places. I remember particularly about the darkness—you knocked your head against the baskets and things when you went in. They were pent-roofed, as opposed to flat-roofed, affairs.

26,632. (*Mr. Cumine.*) I understood you to say that they regard the two diseases, sanjar and gola, as separate?—They did not seem to do that.

26,633. Therefore, they might regard one as a familiar thing, and the other as a new thing?—They were familiar with both.

26,634. (*The President.*) Perhaps the difference they particularly attended to was that one was a dangerous disease and the other was not?—The other was not so dangerous. I think, that is the feeling.

(Witness withdrew.)

Surgeon-General James Cleghorn, I.M.S., called and examined.

Surg.-Gen. J. Cleghorn, I.M.S.

26,635. (*The President.*) You are a retired Surgeon-General?—Yes.

26,636. You have lately acted as Director-General?—Yes.

26,637. You are able to give us an account of the measures which were adopted in connection with plague throughout India, which came under your official knowledge, from 1896, until you left India?—Yes.

26,638. That evidence is in your printed statement which you put in?—Yes, it is as follows:—

[*This Note must necessarily be incomplete, as some time has elapsed since my departure from India, and I have not now access to the office records.*]

In August 1896 a peculiar form of fever, of a fatal type, was observed by some of the medical practitioners in the City of Bombay. In some instances it was accompanied with glandular swellings, and in others with pneumonic symptoms; but none of the observers had apparently seen a similar disease before, and they naturally hesitated regarding the diagnosis. A death was said to have been registered on the 31st August as having been due to "bubonic fever," and on the 23rd September, Dr. Viegas, I think it was, who reported to the Municipal Commissioner that plague was present in the City. Others, however, doubted the correctness of the diagnosis; but on the 29th September the Government of Bombay telegraphed to the Government of India, that the Surgeon-General of Bombay had reported having seen 20 cases of plague, all of a mild type. On that date the Government of India telegraphed to Mr. Haffkine, then on cholera inoculation duty, to proceed at once to Bombay, and report to the Government of India his opinion, after having made a thorough bacteriological examination of cases He reported, on the 13th October, that the disease was undoubtedly plague.

Although no deaths from plague appear in the mortuary returns for September, yet, on the recognition of the disease as plague, its presence was discovered in nearly all the quarters of the City. The disease at first spreads slowly and irregularly, and its

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diffusion in October showed, I think, that cases of the disease must have existed for some time previously.

In the latter end of September the Government of Bombay appointed a Committee, composed of official and non-official medical members, with Mr. Snow, the Municipal Commissioner, as Chairman, to advise the Municipality as to the measures which should be taken towards stamping out the disease, and improving the sanitary condition of the City.

Acting on the advice of this Committee, Mr. Snow, on the 6th October, issued a notice to the inhabitants, making provision for the removal to hospital of all plague patients, and for the evacuation—compulsory if necessary—and disinfection of all houses in which cases had occurred. A hospital was opened in Arthur Road for the reception of cases, and huts were erected for the isolation of individuals who were liable to infection.

These proposed measures gave rise to great excitement among the population; large numbers left the City, and crowds assembled round the hospital, threatening to destroy it and assault the staff. The conservancy staff of the Municipality became disturbed, fears were entertained that they also would leave the City, and thus put a stop to the ordinary cleansing of the town and to all sanitary work in connection with the plague.

The cases of plague being at the time few in number, and the disease showing little or no tendency to increase, it was considered advisable, in view of the popular excitement, to modify the measures published on the 6th October. The Health Officer was accordingly instructed, on the 16th October, not to remove patients living in houses where they could be isolated and properly attended to; and on the 30th October rules, modified in this direction, were published for general information. Hospitals were at the same time established by private individuals for the treatment of cases belonging to different religions and sects; but they apparently failed to attract patients.

In the meantime, the large staff of officers and subordinates employed by the Municipality worked most vigorously in cleaning and disinfecting houses, improving and extending the drainage, and in attending generally to the sanitary wants of the City.

All their efforts, however, failed to check the progress of the disease, and, as the Government of India could not, from the details received, clearly appreciate the actual state of affairs, or satisfy themselves as to the extent and efficiency of the measures adopted, I was directed to proceed to Bombay and make myself acquainted with the facts. I accordingly left Calcutta on the 4th January, arriving in Bombay on the 7th. In company with various Municipal officials I visited different localities infected and inspected a large number of the houses and rooms in which cases had occurred. These houses had distinctive marks on the front wall, showing the number of deaths and cases which had occurred in each, so that there was no difficulty in recognising them.

I was much struck with the enormous amount of work which had already been done by the officers serving under the Municipality, but I at the same time realised that the situation was, as regards the prevention of the disease, a hopeless one, unless the scheme was made more comprehensive. The incidence of the disease showed that it had chiefly appeared in the worst houses, and that the better ones, including the small hotels situate in the infected quarters, had practically escaped.

I had, of course, a considerable experience of the conditions under which the poorer classes in other parts of India lived, but I had never seen anything so bad as existed in the chawls of Bombay.

After I had made myself fully acquainted with the condition of affairs, I invited the leading medical practitioners, both official and private, and the various experts, to meet me and discuss the question at the office of the Surgeon-General. Their views, as published on page 288, Vol. II. of "The Plague in India" compiled by R. Nathan Esq., C.S., I submitted to the Government of India, and strongly recommended that they should be adopted and acted upon. This was done and the measures enforced by the Government of India were in future based on the following lines:—

1. Early detection of cases.
2. Segregation of the sick and their treatment in suitable hospitals.
3. Removal from infected localities of those liable to infection, and their segregation.
4. Disinfection of clothes and other articles liable to infection.
5. Disinfection of houses situated in an infected locality.
6. Enforcement of general sanitary measures.

The authorities in Bombay were, however, so impressed with the local difficulties attending compulsory evacuation and providing temporary accommodation for the segregation of the people removed, that little was done in this direction.

I left Bombay on the 13th January, arriving in Calcutta on the 16th. After clearing off the more important arrears in the office, I left for Karachi on the 22nd January, arriving there on the 26th, but was only able to remain there until the following evening, as I was ordered by the Government of India to proceed to Venice, as delegate to the Conference on plague about to assemble there.

The steps being taken at Karachi were in accordance with the views of the Government of India, and under the direction of Mr. (now Sir) A. Wingate, they proved ultimately successful. No force was employed to move the people into huts. It was accomplished in time by the personal influence of the officers, assisted, no doubt, by the fear created by the presence of plague.

I arrived in Calcutta on 31st January, and left for Venice on 4th February, and reached Venice on 20th February, some days after the opening of the Conference.

Quarantine was in force both at Brindisi and Marseilles. As a delegate, I was allowed to land at the former port, but the steamer with the other passengers had to proceed direct to England. While in Venice I succeeded in obtaining the intervention of the delegates from Italy and France towards removing the quarantine.

I had, while in transit, prepared a note on the characteristics of the disease as presented in the outbreaks at Bombay and Karachi, its progress up to date, and the measures taken by the Government for controlling its spread both by sea and land. The note was read at the Conference, and the information furnished appeared to satisfy the delegates that the Government of India were taking every precaution to arrest the progress of the disease, and in preventing its spread to other countries. The results of the Conference were, from the Government of India point of view, satisfactory.

After reporting to the Secretary of State for India, I returned to Bombay in April. On arrival there I found that plague operations in the City of Bombay had been entrusted to a Committee, of which General Gatacre was President. I had the advantage of being taken over several of the hospitals by him, and of learning from him the organisation in force.

The objects the Committee had in view were:—

1. The discovery of all cases of plague.
2. The treatment of all cases in hospitals.
3. The gradual segregation, as far as possible, of the probably affected, that is, of those living in the same room with, or in close attendance on, persons suffering from plague.

The work of the Committee also included all sanitary measures, the disinfection of houses, and the destruction or alteration of those considered unfit for habitation.

The removal and segregation of those liable to infection were not carried out on any extensive scale, or in the manner desired by the Government of India; but the organisation, and the methods employed for carrying out the other objects, were of the most complete and perfect character. Except at first, little or no opposition was experienced, and General Gatacre, by his tact and great personality, carried the whole population with him.

On leaving Bombay I took over charge of my duties at Simla. All papers and questions regarding plague were passed on to me for information and remarks, and the necessary orders were issued by the Government of India. With further experience the rules were elaborated, and, in some instances, modified to suit local conditions; but the main principles, as previously laid down, were invariably adhered to.

The reserve of officers on special plague duties was fully utilised. Many officers were removed from their ordinary appointments, and placed on plague duty. In the case of outgoing steamers and craft from Bombay harbour, all the crews and passengers were inspected by

ne Port Health Officer, who was assisted by a staff consisting of an additional Port Health Officer, three Commissioned Medical Officers, three Military Assistant Surgeons, one Hospital Assistant, and two lady doctors. When the stress of work was great, or when time was of importance, the Port Health Officer obtained further help from the medical staff engaged in other duties in the city. The work thrown on these officers was exceptionally heavy, and the thorough manner in which it was performed was proved by the results.

Posts of inspection, numbering 221, were gradually established on the different lines of railway, at the principal stations and junctions. Forty-six of them were under the charge of Commissioned Medical Officers. Huts were erected at a large number of the stations for the treatment of the sick, and for the segregation of suspects. There were altogether employed entirely on plague duty 64 Commissioned Medical Officers, 41 of whom belonged to the Indian Service, and 21 to the Army Medical Staff; 20 private practitioners; 31 Military Assistant Surgeons; 36 Civil Assistant Surgeons; and 211 Hospital Assistants; besides a number of lady doctors and nurses. Other officers and subordinates, holding regular appointments, always assisted when their services were required.

So far as I can remember, no complaints were received by the Government of India as to the manner in which these officers discharged their duties, except in two instances. One was with regard to a native of India, who was a member of the Indian Medical Service; the other concerned a Military Assistant Surgeon.

In addition to the above, there were employed on sanitary duties 45 British officers, 16 native officers, and 1,617 non-commissioned officers and men.

The additional requirements for medical officers for famine and war proved too great a strain on the two medical services, and in July or August private practitioners were obtained from England for plague duties.

In October, a Commission, consisting of Sir A. Wingate, Surgeon-Colonel Hay, and myself, was appointed by the Government of India to visit the places in the Bombay Presidency attacked with plague and to see how the operations were being carried on. I left Simla on 1st November, and visited alone, or with the other members, nearly all the infected localities. The objects of the measures and the manner of execution were carefully explained to all the officers concerned, and our visits were much appreciated. It was difficult for local officers to grasp the object of written orders on such a complicated subject. Inspection of travellers at the different stations was rendered more complete, and means of disinfecting clothing by steam were introduced at appropriate stations. Local measures were introduced on a systematic plan. Our recommendations with regard to each place were forwarded to the Governments of India and Bombay.

On my return to Calcutta on 24th December, I drew up fresh instructions regarding plague administration for the general guidance of district and other officers, which embodied the results of the experience gained on the tour. This note was circulated to all local governments and administrations for information and guidance, with the Government of India Resolution, No. 227—240, dated 3rd February 1898.*

The large amount of extra work, both clerical and inspectional, thrown on me by the plague, proved too much for my constitution, and on my return from the Bombay tour, I felt unable to continue in office. With the consent of his Excellency the Viceroy I left India in March.

26,639. I think there are only a few questions which we have to ask you?—I gave everything I could think of in connection with the administration of plague, and the different measures which were adopted.

26,640. You often visited Bombay and Karachi and the other important towns and districts which were affected with plague?—Yes.

26,641. And you were impressed with the insanitary condition of some of these towns and districts?—Yes, the condition is common to all Indian towns.

26,642. You thought that insanitary conditions were specially prevalent in Bombay, I think?—Yes, in the houses.

26,643. You do not refer so much to the general conservancy as to the houses?—That is so.

26,644. You thought that these houses were, in that respect, worse than those of Calcutta?—Worse than any place I had seen, owing to the enormous number of people who lived under the same roof. I have never seen anything like such overcrowding. In a place, probably, the length of this room—6 feet by 12 feet—I have seen the floors covered with people lying down, and in addition, beds hanging from the roofs on which other people slept at night. There was absolutely not one ray of light or any means of getting air to these rooms. We had to strike a match to see the opposite walls, because it was so dark, and that was in the middle of the day.

26,645. I suppose the ceilings from which they suspended their beds were not very high?—No, they were not. I have never seen anything like such overcrowding in rooms absolutely without any means of ventilation. All the windows, when there were windows, were closed with rags and remnants of bedding.

26,646. Where these windows existed, I suppose they were not large?—No, not in all cases.

26,647. In many cases they opened into a narrow space?—Yes, on to the wall of another house.

26,648. Where the air was necessarily foul?—Yes, the drains in the alley between the houses were open, and the passages were just wide enough to allow the sweepers to get up to sweep the drains, therefore, to ensure privacy from the inmates of the next house, all the windows were covered up with clothing, &c.

26,649. Is it within your knowledge that plague was specially prevalent in these places?—Yes, especially in these insanitary houses. I gave a description of these chawls in a note I submitted to the Plague Conference in Venice. I have not quoted it in my evidence. It is in Mr. Nathan's compilation. I have given a précis of it in my evidence.

26,650. You have given a précis of it in your evidence, but a fuller description will be found in Mr. Nathan's compilation?—Yes.

26,651. Your refer to several alterations which took place in the measures which were adopted to meet plague at different periods and times?—Yes, as our experience increased, and as the difficulties presented themselves to us, the rules were elaborated—the complete rules which were ultimately accepted were those issued by the Government of India to local governments.

26,652. Numbers 227–240?—Yes. That gives the complete rules. We acted on those rules previously, but they were elaborated so as to meet the work which the district officers had to contend against.

26,653. These were issued in February 1898?—Yes.

26,654. But there were also modifications which were not so much dependent on the reasons which you have given, but upon the difficulties in carrying out proposed regulations?—Yes, but it was after my trip with Sir Andrew Wingate through the plague-stricken districts that we thoroughly realised what these difficulties were. It was impossible sometimes to carry out certain rules.

26,655. What was the nature of the difficulties?—There were difficulties with regard to preventing people in infected districts getting out of the districts, not by railway, but by country roads. You could not have possibly controlled all traffic. The local officers knew exactly the local conditions and were advised to act according to circumstances.

26,656. Were not there difficulties also due to the opposition of the people themselves to certain steps which you wished to be carried out?—We never had serious opposition after the plague was fully established, so far as I can remember. With a good local officer there would be few difficulties. If a local officer took time to carry out the measures and got the leaders of the people with him, there was little difficulty. In Surat, for example, there were 20,000 people in huts in different camps, 4,000 or 5,000 in each camp, and there was not the slightest difficulty there. The officer in charge was possessed of tact, and had great influence with the people, and he carried them with him.

26,657. It was the personality of the man?—Yes. I heard there was opposition on the Great India Peninsula Railway line in the villages, and there was opposition in Bombay City at first.

26,658. In what way did that opposition show itself?—They absolutely refused to allow the removal of the sick, or to evacuate the houses in which the sick were Mr. Snow then modified the rules.

* Not reprinted with the Proceedings of the Commission.

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26,659. To what extent?—He did not remove the people from the infected localities.

26,660. Was it made optional with the Health Officer?—Yes.

26,661. After proper isolation?—If proper isolation could be made and the patients could be properly attended to in their own houses they were allowed to remain there.

26,662. And with regard to contacts and the people in the neighbouring houses, they did not attempt to do anything with them at that time?—No; I do not think so. It was never completely done in Bombay. Evacuation was never carried out on the scale necessary to make the measure thoroughly effective.

26,663. Do you think it was an impossibility?—The local officers and the local government said it was an impossibility.

26,664. They have now, however, established a very large number of camps in Bombay to which people are removed?—They did, for a time, before I left—there were camps then. It was said there was not enough space, but there are open spaces in Bombay. There were places in the immediate neighbourhood where people could be taken by train. Of course it means a great organisation and expense, but the point was that here was a disease in Bombay City, and that the whole of India was imperilled, and the commercial relations of the city seriously affected for the time being, unless the plague could be exterminated in Bombay. The feeling was that, with so much at stake, it was really worth while acting on the rules which were considered advisable, notwithstanding the great expense which would necessarily be incurred.

26,665. At first, in several large towns, not so much, of course, as in Bombay, there was, undoubtedly, a good deal of opposition, I think?—As for instance?

26,666. I mean in every large town?—There was passive opposition.

26,667. Against segregation?—It was always, I think, opposition in a passive form.

26,668. Has that been got over?—That has been got over, to a large extent, by the tact, firmness and individuality of the officers concerned. Little or nothing was known about the plague in Sholapur until it was nearly all over the town. The early recognition of the disease is one of the difficulties, but eventually the whole place was evacuated.

26,669. But they had a difficulty at first?—Yes, a great difficulty. The inhabitants concealed the existence of cases.

26,670. Was there any fear of active opposition?—There is always that danger, as a very slight matter might develop it. In carrying out measures affecting the habits and customs of the natives there is always the feeling that opposition may be aroused, unless the local officer is conversant with the people and has gained their confidence.

26,671. From what you stated a moment ago, I assume that you think evacuation is a very valuable measure?—Yes, evacuation of a locality, that I am perfectly clear about.

26,672. You think it is the most effective measure?—Yes, it is I think, the most effective remedy, because the plague as I saw it was absolutely a disease of locality. What struck me going through Bombay was that none of the good houses were attacked, not even the small hotels where sailors and others lodged.

26,673. There was not plague even in those quarters?—No. The sanitary arrangements there were fairly good. I do not think that a single case occurred in those small hotels.

26,674. Was there plenty of light and no overcrowding in these houses?—That is so. They were like the dak bungalow arrangements in India, and the places were built according to European ideas. There was no overcrowding, of course.

26,675. Have you any instances in which evacuation was very successful? I suppose you have many?—Yes; when taken in time, and we have early intimation of the cases. Under those circumstances evacuation almost invariably arrested the disease. It was arrested at Chandausi, but it took some time, and also in Khandraoni, Dr. Crofts stopped the disease there at once. Its progress was also arrested at Surat and in Karachi. It took Sir A. Wingate weeks to get the people to leave the town.

26,676. What is the population of Karachi?—I think about 100,000. The evacuation of the town was a most difficult undertaking, and was carried out by sections. Sir A. Wingate worked very carefully. He built huts, acting, I think, under instructions from the Government of India, and he got one section of the town evacuated. Gradually all the people were located in huts, and the disease was arrested there for a long time. It was a very great undertaking indeed.

26,677. In a large place you think a great deal might be done by evacuation in sections?—Yes, it was always found that when the disease was reported in a village or a town, there were invariably cases found in the vicinity of those reported, so that it was not only necessary to evacuate the houses in which a case was reported, but also the houses in a certain area round about.

26,678. When you are dealing with a very large population, much, if not all, the benefits of complete evacuation may be obtained by evacuating the people in sections?—Yes.

26,679. There is almost no limit so far as population is concerned if they are treated in sections?—Except as to the accommodation which you can provide.

26,680. Supposing you can dispose of 40,000 or 50,000 people at one time, that practically comes to almost no limit, so far as the population is concerned?—Not if you have places in which to accommodate them and you have the organisation which is necessary. Of course, even then it is not so effective as complete evacuation would be when carried out in the case of a small community. But the only practical way to limit the disease is to get the people out of the quarter.

26,681. You may, at any rate, much lessen it, even if you do not altogether stop it?—Absolutely; that, I think, is perfectly clear.

26,682. In some of these cases referred to, such as Karachi and many other places where there has been complete success, the disease has again broken out on the people returning, and there have been two, three or even four outbreaks afterwards; how is that to be accounted for?—I should say it is on account of the construction of the houses, either by re-infection of new arrivals, or by the measures not having been thorough in regard to the cleansing and disinfection of the house. It was intended to destroy the worst houses but this was found to be impracticable.

26,683. After all this trouble in large towns—as in Poona, for example—has been taken and much expense incurred resulting in a successful suppression of the outbreak, if an outbreak occurs again because of infection from some other place, that shows that in order to eradicate plague altogether, it is very necessary that the other infected places should be treated in the same way, does it not?—Certainly.

26,684. If there be one infected place within probable communication, which is not dealt with in this—in your opinion—the only effective manner, then re-infection may at any time occur?—Yes.

26,685. And the work that has been done will be thrown away?—Yes.

26,686. With regard to this question of evacuation, I suppose you appreciate that the process of evacuation may itself sometimes extend plague by the people not being entirely in hand and running away to non-infected places?—That was one of the difficulties, and it was met by surrounding the section of the town to be evacuated, either with troops or police.

26,687. That may not be absolutely effective because people break the cordon?—They have done so, undoubtedly.

26,688. But even if they have done so, it is much better that the plague should have been reduced by the measures that have been taken than that it should have been left to itself or only inadequate measures adopted?—Yes. Evacuation of the locality was the first measure to be carried out. If those evacuated could not for any reason be properly segregated, the disease would doubtless in many instances be communicated elsewhere. Evacuation and segregation would, if possible, be enforced in the new centres. Latterly, when the disease first appeared in towns, the people were allowed to leave the place.

26,689. Your views are clear and decided, but have they been entirely carried out?—I think they have.

26,690. Did you make any special inquiry as to how plague was introduced into India?—Yes, but the results were not satisfactory; nothing authentic could be ascertained.

26,691. It was first introduced into Bombay?—Yes, the idea was that pilgrims from Garhwal had introduced it into the city. They came from Garhwal where mahamari was believed to be present.

26,692. How long does it take to travel between the two places?—It depends upon what part they come from. By the railway it would take three days and nights.

26,693. Do many pilgrims or other people travel from that district by railway?—They prefer to travel by rail. Any people who were going to Bombay would go by rail.

26,694. And you say they do go to Bombay?—Yes.

26,695. Therefore, an infected person might arrive in Bombay three days after leaving Garhwal?—No; because he has to travel down by road from Garhwal to the train. There was absolutely no clear evidence of its introduction from Garhwal.

26,696. How long would they take from the part nearest the railway to get to the railway?—A week or 10 days.

26,697. Before they reached the railway from the nearest part?—Not the nearest part, in Garhwal.

26,698. A week or so?—About a week. I do not know much about Garhwal. There were no reported cases of plague in Garhwal at the time.

26,699. It would take seven days to go to the railway alone?—Yes.

26,700. And another three days to go to Bombay?—Yes, it would probably take more than three days to go to Bombay, because the travellers would probably break the journey to prepare their meals, and, perhaps, visit certain holy places *en route*.

26,701. What do you think of the possible importation from China?—I think that it is a very probable source.

26,702. Have you any suspicion with regard to the Caspian Sea?—No, there is no proof of it coming in that way. We heard of no plague at the time, and there are no people coming from the Caspian Sea to Bombay, except Mecca pilgrims. There is constant traffic between China and Bombay.

26,703. Is there any direct communication between Calcutta and China?—Yes.

26,704. Assuming that China was the source of the infection, how do you account for Calcutta having been almost free from plague?—I do not know.

26,705. Have you had much experience of the effects of inoculation?—Only the returns sent by Monsieur Haffkine.

26,706. You have studied these?—I have. They were satisfactory so far as they went. They were considered so satisfactory that the Government of India directed the Government of Bombay to give Monsieur Haffkine every facility for carrying out voluntary inoculation.

26,707. Your experience may also have allowed you to come to some conclusion as to the efficiency of inoculation as a plague measure in stopping the progress of plague or extinguishing it?—It was hoped that these inoculations would, for a time at least, secure immunity to the individuals, and, if so, the progress of plague would, under certain circumstances, be arrested, or its virulence materially modified. Inoculations would then greatly simplify the measures adopted by the Government of India.

26,708. Supposing that inoculation failed as an actual measure for eradicating plague, what, then, would you fall back upon now—have you formed any opinion upon that?—With early information regarding the presence of the disease, evacuation, segregation, and the other measures adopted will prove effective: without such information, difficulty will always be experienced in the case of large towns.

26,709. You state that it seems to prevail very largely, if not chiefly, in the most insanitary quarters of the town?—Yes.

26,710. To what extent do you think the removal of these insanitary conditions might make plague impossible?—I believe that plague cannot appear and spread if the locality is in a proper sanitary condition.

26,711. Therefore, it would be the most effective of all measures?—Yes, the most effective. The Bombay Municipal Bill, if carried, should place that city in a good sanitary condition.

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26,712. That Bill would have to be extended to a large number of other towns?—Yes, to the whole of India, undoubtedly. I believe that the extinction of plague and the prevention of recurrence depends upon sanitation.

26,713. Therefore, you would hail, no doubt, with pleasure, any steps which could be adopted to permanently—not temporarily in a panic merely—but permanently and continuously uphold the sanitary condition of India?—Certainly.

26,714. (*Prof. Wright.*) Were you impressed with the infectivity of the plague in Bombay when you made this inquiry?—No.

26,715. Did you form an impression that the plague was not an infective disease?—I formed the opinion from personal observation and from the other sources of information that plague was not a very infectious disease, and that there was, practically, very little danger in visiting an infected locality or in attending on the sick.

26,716. You mean that the bacillus cannot pass from one person to another?—That is so under ordinary circumstances.

26,717. Then do you regard plague as infective only in the sense that tape-worm is infective? In other words, do you think that the plague bacillus becomes infectious only after it has lived for some time outside the body?—No, I do not go to that extent. I say there was no danger in going into an infected house—practically, no danger.

26,718. Then why do you propose to isolate the people and get the people out of their houses?—There is danger in the locality.

26,719. Do you approve of the sick man being taken out of his house?—Yes.

26,720. Then are you of opinion that the sick person is a danger to his neighbours?—There is a danger to other people in his expectorations.

26,721. Then a man may be directly the cause of infection to other people?—Yes, but not in the same sense.

26,722. I want to understand the distinction that you draw. Many are of opinion that the plague bacillus passes directly from one man to another?—I do not know much about that. We found that if a man scratched himself, or had an abrasion, and happened to get the bacilli there by handling the corpse at *post-mortem* examinations, or by opening the bubo, there was the possibility of his getting the plague in that way, or from the expectorations of the patient.

26,723. Does not that bring plague into the category of infectious diseases?—Yes, it is an infectious disease because it may be conveyed by the clothing. I never denied that it was infectious.

26,724. I understood that you differed from the general opinion in respect to that. In other words, I understood that you thought a man could not be directly a source of infection to other people?—Not in the same way as scarlet fever.

26,725. Not in the same degree you mean?—Yes. I said, practically, it was not infectious in that way, that is, you can handle the patients, but if you have a patient lying in a room expectorating there is danger in the locality. His friends must attend upon him. At one time I said, "All right, if there are these "obstructions to the measures, let the patient and his "friends live in the house and die there if necessary, "but get the other people out." Every thing has proved that it is infectious to a certain extent.

26,726. Do you think you have any means in India for getting information of the introduction of an infectious disease into a town?—It is absolutely impossible to get early information.

26,727. Do you think any measures ought to be taken for getting this early information?—Yes.

26,728. What measures? Do you recommend the use of search parties?—I recommend employing the natives.

26,729. Do you mean that the natives are to be paid as spies?—No.

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26,730. You mean that they are to be employed as voluntary agents?—Yes, we divide each town into sections and put them under natives.

26,731. Is that an efficacious manner of obtaining information of infectious disease?—It was the only method we could adopt at the time.

26,732. Do you think that infective disease will never be successfully combated until you get a system of death registration?—That is so.

26,733. And until you ascertain the causes of death?—Yes, but that is practically impossible.

26,734. Ought not that to be put in the fore-front of all plague measures?—Yes, but look what an enormous expense it would be; how could you do it? Where would you get the medical officers?

26,735. Do you think there would be any difficulty in ascertaining the cause of death in Bombay in a period when there was no epidemic—say, when the plague mortality declines in summer?—Yes.

26,736. Do you think there would be any difficulty in examining 60 corpses a day?—No, I do not think so, but the difficulty is in getting the report of the deaths; they are not always reported.

26,737. Do you think it would be possible to pass laws and make it penal to move or to dispose of corpses before they had been examined?—I think it would be very difficult to do that.

26,738. Do you think it is only playing with plague to attempt to deal with it without introducing corpse inspection? I am right, am I not, when I say that in Bombay they have been fighting plague for years and that they have not yet introduced that measure?—That is the cardinal measure, before sanitary and preventive measures if you can get it. Of course our difficulty was how we could do it.

26,739. Have you seen the system of corpse inspection worked in Poona?—No.

26,740. Have you seen it worked anywhere?—No.

26,741. In Karachi?—No.

26,742. Was it after your day that they had corpse inspection there?—Yes.

26,743. You do not recommend corpse inspection?—No.

26,744. Do you know whether corpse inspection has led to disturbances?—I think it was only applied in Poona as far as I remember, and the natives were utterly disheartened in Poona. The danger was in extending a measure of that kind to the North West Provinces and the Punjab. Up-country people would not stand it. Then, again, what would you do with a pneumonic case if you had corpse inspection?

26,745. Do you think corpse inspection might be assisted by bacteriological observations in cases of that sort?—Yes, but you are not supposed to touch corpses after the death ceremonies are performed. Before the washing process takes place you can do it probably, but you would certainly now and again get a person who would not allow you to look at the corpse, especially if it were the corpse of a woman.

26,746. Do you think that a provision for ascertaining the cause of death should be put in the fore-front of all regulations when it is a question of ascertaining when an infectious disease comes to a new town?—We could not do that. That would not be part of the scheme for ascertaining the cause of death.

26,747. You think that corpse-inspection ought not to be included in your scheme of discovery?—No.

26,748. You would not recommend any system of corpse-inspection or death registration as a plague measure?—I would have no inspection of corpses as a method of detecting the cause of death, knowing, as I do, the feelings of the people in regard to any such measure, but a system of death registration would be valuable.

26,749. You would rather go on under the present system?—Yes, with medical officers to certify to each death.

26,750. How do you propose to work the death registration without corpse inspection? Whose certificate would you take?—The certificate of a medical officer.

26,751. Do you mean a Commissioned Medical Officer?—Of any medical officer.

26,752. Would you take a native's certificate?—I would take the certificates of natives of a certain class.

26,753. Would you take the certificates of unqualified natives?—Not in that way, not for a perfect death registration.

26,754. Would you recommend, as a measure for fighting plague that a system should now be introduced which would ensure efficient death registration?—Yes. That is the principal thing, if you can show a practical way of doing it.

26,755. From your knowledge of India, do you think it would be impracticable to introduce a system of registration?—Our great difficulty was to obtain a report of deaths. Death registration is possible but hardly practicable in a country like India. It could be introduced into towns.

26,756. Do you recommend this as one of the essential things?—Yes, to get early information of the deaths.

26,757. There are many ways. Search parties is one way. Do you like that system?—No, I do not like that.

26,758. Do you think death registration is better?—Yes, if you can get it.

26,759. And in default of death registration you believe in inspection of the body?—Yes, certainly.

26,760. What certificates would you take as to the cause of death?—That of a duly qualified medical practitioner, either with an Indian or an English qualification.

26,761. Do you think you can trust the certificates of native practitioners?—I think you could trust the Assistant Surgeon class.

26,762. You would trust the medical certificate of a native?—I have always depended on them thoroughly.

26,763. Is it inconsistent, with what you know of the native practitioner, to suppose that cases of plague would be reported as measles, whooping cough, or something of that sort?—We get individual cases of that kind.

26,764. Do you think that that would be largely done?—I am inclined to place perfect confidence in the certificates of the Assistant Surgeon class.

26,765. You, then, would accept certificates only from the Assistant Surgeon class?—Yes, that is a very good class.

26,766. You would take their certificates?—Yes.

26,767. You would have to make some hard and fast rule, would you not?—Yes, but you get very good men amongst that class of local officers, and I think you could take their certificates.

26,768. Would you then propose to leave it to the discretion of the Medical Officer of Health to determine in each case what certificates he would take?—Yes, I would do that; I would make him responsible as he is the responsible officer, but I certainly would accept every certificate from the Assistant Surgeon class.

26,769. You would take every certificate from the Assistant Surgeon class as unquestionable?—Yes, until I found it was wrong.

26,770. Supposing, under those circumstances, you found that plague existed in a town and was not reported to you?—I should say, therefore, that the certificates must be wrong.

26,771. What would you do then?—I should put it into other agency, if I could get it.

26,772. Would it not be well to commence with a superior agency at first?—No, I would not do that. It would cast a slur on the native practitioner.

26,773. Would you give a Health Officer the power to inspect the corpse if he doubted the correctness of a certificate?—No.

26,774. You would not use corpse inspection in any form?—No, I would not. I would not do it as a Government measure. If he chose to do it on his own responsibility he could do it.

26,775. Do you adhere to the view in full knowledge of the fact that corpse inspection has been accepted quite peaceably in Poona?—In Poona the people were so frightened that you could do anything you liked with them.

26,776. (*The President.*) Why?—On account of the plague.

26,777. They were frightened?—Yes. If you tried to do anything like that in the Punjab—in Lahore, for instance—you would very soon get a disturbance.

26,778. (*Mr. Cumine.*) Poona is a large military cantonment, is it not?—Yes

26,779. When people have been evacuated, and they return secretly and sleep in their houses, do they not frequently catch plague?—I believe so.

26,780. Does not that look as if the infection lay in the cow-dung floor which they sleep upon?—My idea is that it is local infection. I do not know whether it is cow-dung or what it is, but it is something in the locality.

26,781. (*The President.*) You mean partly so, because you have said that it might be in the human being?—Yes, partly. Once it gets into a locality that becomes a fresh centre. I could not say about the cow-dung floor.

26,782. (*Mr. Cumine.*) My point was, whether it does not appear to be in the floor of the house?—I am not certain; we have no information on that point; we do not know anything about the natural history of the bacillus of plague.

26,783. Assuming, for a moment, that the infection does lie in the floor, and that the natives get it from going in with bare feet or from sleeping on the actual floor without bedsteads, would the sanitary improvements which you think are required to eradicate plague from India touch the floors?—Sanitary measures will prevent the floor and anything else becoming a source of infection, because the bacillus could not live in sanitary conditions.

26,784. Would these sanitary conditions interfere with the floor of the house?—Not necessarily.

26,785. (*The President.*) I thought you said they would, because of the fresh air and light?—Yes, in that way, but I thought you meant with regard to the floors being made of stone, lime, etc.

26,786. (*Mr. Cumine.*) Would you prescribe any particular kind of floors?—Nothing of the kind. All I would prescribe would be that the house should not be overcrowded, and that it should be built under ordinary sanitary conditions.

26,787. You would not prescribe certain sizes of windows?—No, certainly not.

26,788. (*The President.*) You would not say that there must be a certain sized window and a certain air space, but you would say they must not be smaller than a certain standard?—A decent size.

26,789. (*Mr. Cumine.*) Would you do that?—That is going into such minutiæ that it would worry the life out of the people. What I say is, have proper ventilation in the room, and then you can afford to do what you like if you have proper ventilation and proper light.

26,790. Who is going to see that that is carried out?—The Government of India or anyone else.

26,791. (*The President.*) You must have a new organisation in the whole of the Sanitary Department?—Yes, we want a new Sanitary Department.

26,792. The present Sanitary Department is scarcely able to undertake it?—It is impossible; we are merely advisers. I was merely adviser to the Government of India. I could issue no rules without their sanction, nor could I do anything without their orders.

26,793. Having regard to the vastness of the country and of the population, the present organisation is insufficient to maintain India in a sanitary condition?—Quite insufficient and ineffective. It is a defective organisation entirely.

26,794. I understand, from some answers you have have given, that you think that a satisfactory method of obtaining early information would be to accept the medical certificate of a man whose qualifications shall be afterwards defined, and alternately if that is not obtained, by corpse inspection?—Not corpse inspection.

26,795. You altogether disapprove of corpse inspection?—Yes.

26,796. Even though it has not caused much opposition, not only in Poona, but in other parts of India?—There are only one or two parts.

26,797. There are several parts of India in which it has been done without opposition?—Yes.

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26,798. Karachi certainly, and Belgaum, and two or three other places where they are carrying it out: the people have got accustomed to it, and now that they know what it means they do not oppose it so much. At all events, if corpse inspection could be done in a manner that would not cause serious opposition, you would not object to it?—Then I say, what is the object of corpse inspection?

26,799. Supposing you cannot get a medical certificate from a man whose qualifications you would be willing to accept?—I say that corpse inspection is useless to discover the disease, because you cannot find out the pneumonic form of the disease by corpse inspection.

26,800. But, supposing the number of pneumonic cases is small relatively to the bubonic cases, as it generally is, and the bubonic cases are discoverable, is it not something to discover part of the cases, if not all?—Of course, if you can do it.

26,801. (*Mr. Cumine.*) The President asked you to suppose that corpse inspection could be done in a manner that would not cause serious opposition, and then asked if you would approve of it. But that supposition is one that you find it impossible to make, I understand?—With regard to up-country, yes. I think it would give rise to great opposition. I have have always held that opinion. My idea is that if that measure were imposed on the people up-country, it would certainly not be worth the opposition it would give rise to.

26,802. You said that where the Collector was a good officer he would manage to get all the people out of an infected town very readily?—I think so. I do not mean a good officer in the sense of being a good Civil officer; I mean a man with a certain amount of individuality and tact, and a man who knew the natives. I think he could do anything he liked with them if he has their confidence.

26,803. I was going to ask you whether, though he would gladly do it one year, and ask all his people to go out one year, and they would go out one year, would he be ready to keep on asking them to go out in two or three successive years, and do you think they would continue to be willing to go out two or three years in succession?—Certainly, if the arrangements were good for housing them; but with such repetitions, the question would naturally arise as to providing new and permanent accommodation for the people.

26,804. (*Prof. Wright.*) How would you propose to find out the cause of death by medical certificate in the case of people who are not seen by a medical man?—That is difficult.

26,805. Would you have corpse inspection then?—No.

26,806. Would you let these people be buried without diagnosis?—There would always be a way of getting over it. The Medical Officer of the district in which the death had occurred would know.

26,807. You would take his certificate?—Yes; he would manage to find out by asking the people: he could see the body and see if there were signs of plague.

26,808. Would you take a relative's opinion?—No.

26,809. Would you not sooner have corpse inspection?—No, it is so distasteful to the people.

26,810. Supposing it can be done, would you not think it would then be advisable to try it in cases where you get no medical certificates?—I am not in favour of it; it is so utterly repugnant to the people when they are in a normal condition. They would not allow me to touch a corpse in the ordinary course of practice. I could not go near the body, and especially near the body of a woman—they would not allow it. I have asked all classes of Muhammadans and Hindus about it, and they all say it is utterly repugnant to their system of religion.

26,811. (*The President.*) But in some parts of India the Muhammadans have submitted to corpse inspection?—I daresay they have, either from being cowed, or seeing the hopelessness of opposition.

(Witness withdrew.)

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Dr. W. J. Simpson, F.R.C.S., called and examined.

26,812. (*The President.*) You are a Doctor of Medicine?—Yes.

26,813. And you have lately been Health Officer in Calcutta?—Yes.

26,814. Within what time were you Health Officer in Calcutta?—From May 1896 to September 1897.

26,815. During that period did you see in Calcutta any cases of plague?—Yes.

26,816. When?—About the beginning of October 1896.

26,817. Would you give us a description of the cases you considered to be cases of plague?—The cases of fever and glandular enlargement in Calcutta, and which were suspected to be cases of plague, that came under my observation were as follows:

(1.) On 8th October, one case at Howrah, that of a Goanese youth, aged 17, named Cotta. He had arrived from Bombay 12 days previously. I was asked to see him by Surgeon Lieut.-Colonel Tomes. He had enlarged glands on each groin, which were tender. There was no sign of syphilitic or venereal affection. There was no history of injury. The full history of the case is given in my Annual Report for 1896, p. 18, from which I extract the following:—

"In October a case of glandular sickness with fever occurred in Howrah, in a patient who had arrived from Bombay. Surgeon Lieut.-Colonel Tomes asked me to see the case, which I did, with Dr. James, on October 8th, 1896. The patient, without any history or sign of syphilis or venereal affection, was suffering from tender buboes on each groin, and had a temperature of 102°; though perfectly sensible and intelligent, he seemed to require to be aroused before he answered any question. His eyes were somewhat suffused, and tongue furred in the centre, and red at the tip and edges, he had on his legs, arms, and body an urticarial-looking eruption, and distinct from this, on his back and shoulders, was, here and there, a reddish rash varying in size from less than a split pea to that of a bean. He had started from Bombay on the 23rd September, arriving in Howrah on the 26th, and was attacked with fever on the 28th. With careful antiseptic precautions the patient's finger was pricked, and cover-glass preparations were made. The needle of a syringe was inserted under his skin, and a small quantity of blood drawn off and inoculated into two culture tubes of nutrient agar and bouillon. With similar precautions blood and serum were taken from the glands and groin, and culture tubes inoculated. The microscopical specimens were examined the same night and found to contain diplo-bacteria.

"The next morning the patient was seen by Dr. Cobb, Dr. Tomes, and myself, and after careful examination the following notes were taken:—

"*History of Patient.*—J. C., aged 17 years, who had been living with his father, a telegraph master, residing at Bellasis Road, Byculla, Bombay, came with his sister to Howrah on a visit, and took up his residence in Ghose's Lane, Howrah.

"In Bombay, he attended the Byculla School. He left Bombay with his sister on the 23rd of September 1896, and arrived in Howrah on the 26th September. About 15 days before leaving Bombay, he noticed the gland of the left groin to be enlarged and painful, then the gland in the right groin began to enlarge, but he felt quite well until he reached Nagpur, on the 24th September, when he began to experience symptoms of malaise, loss of appetite, and the glands in the groin grew larger, and four small rounded pimples appeared on the penis. These pimples lasted for four days. He remained much about the same until his arrival in Howrah, when, on the 28th September, he was attacked with fever, which was of a remittent character, being worse at night than in the morning. He perspired profusely and suffered from severe itching, due to urticaria. The pain in the groin was of a dragging character, but did not interfere with his walking about until the last few days, when, on account of the discomfort in the groin, and the heaviness in his head, he was obliged to take to his bed. The sister of the patient stated that she was well acquainted with the plague, as she had seen several cases while in Bombay.

"On examination of the patient on October 9th, his countenance appeared heavy, and there were dark hollows around the eyes. His tongue was covered with a whitish fur in the centre, the edges being red. His conjunctivæ were slightly congested. He seemed dull and apathetic in answering questions. His head was hot to the touch. Patches of urticaria, varying in size from a pea to a bean, were scattered over the back, forearm, and legs. There were also some small reddish patches which disappeared on pressure, and which varied in size from a pea to that of a four anna piece or larger, on the skin of back and shoulders. These were not observed on any other part of the body. In the right groin was an enlarged gland, the size of a hen's egg. It was situated above Poupart's ligament. It was hard and slightly tender. In the left groin, there were two enlarged glands, one similar to that on the right side, but smaller; another, the size of a small marble, was situated below Poupart's ligament, and was more tender than the others. The penis was carefully examined and found to be free from sores. On inquiry, the patient denied that he had ever had syphilis. The spleen, liver, heart, and lungs were examined and found to be healthy. There was no sore throat. There were no other enlarged glands in the body. The patient stated that the bowels were regular and urine natural. Pulse 110. Temperature 99·6 in mouth. Blood was taken from the finger and examined at the laboratory.

"After a prolonged consultation, Dr. Tomes, Dr. Cobb, and myself, came to the conclusion it was a case of *pestis ambulans*.

"The Medical Board,* investigating the case later on, came to the conclusion that there was no evidence to consider this a case of true bubonic plague."

26,818. Of whom did the Medical Board consist?—Mr. H. H. Risley, Secretary to the Government of Bengal, in the Municipal and Medical Department; Mr. J. G. H. Glass, Secretary to the Government Public Works Department; Dr. G. C. Ross, Inspector-General of Civil Hospitals; Mr. P. Playfair, Sheriff of Calcutta; Dr. D. D. Cunningham, Professor of Physiology, Medical College; Dr. H. J. Dyson, Sanitary Commissioner, Bengal.

26,819. Your microscopical examination was restricted to a determination of the morphological characteristics?—Yes, at that time.

26,820. And at any other time?—Later cultures were made from some of the blood which was taken from the groin. In slide specimens prepared from the blood of Cotta, some of the Shropshire cases, Tincauri Pal, Grace Hodges, Heman Shah, Surendra Nandi, Giga, Bepin, Golab, and the rats suffering from glandular sickness, there were bacteria of a diplo-bacterial character, and morphologically similar to the specimens of plague in man and rats from Bombay.

26,821. I understand you did more than that?—From Cotta a comparatively pure specimen was obtained, which afterwards became contaminated. Sub-cultures were made from Cotta, which were taken from the blood.

26,822. While he was alive?—Yes.

26,823. At what period in the illness?—It must have been about three weeks after the glands began to enlarge.

26,824. Late in the disease therefore?—Yes.

26,825. Was he convalescent?—He was not convalescent.

26,826. Was he pyrexic?—Yes, he had a temperature of 102° at that time.

26,827. Your cultures became contaminated, I understand?—Yes.

26,828. Could you draw any conclusions from these contaminated cultures?—No.

26,829. Did you have other cultures from this man which were not contaminated?—The first cultures appeared to be contaminated, and then later, the microbe, or what appeared to be the microbe, was isolated, but after passing this through several generations this microbe, which seemed to be pure at first, ultimately became contaminated.

26,830. Did you do that in a culture medium?—Yes.

26,831. Will you describe the character of the culture, its appearances, and in what you cultivated the microbe?—I cultivated it first of all in bouillon and agar. It was a bouillon culture which ultimately became contaminated.

* *See* App. No. XXI. in Vol. I. of these Proceedings.

I had, as standard specimens, cultures from Bombay brought to me by Dr. Dutt. I sent my assistant, Dr. Dutt, to Bombay, at the time of the outbreak of the plague there. He worked with M. Haffkine for a short time, and M. Haffkine was kind enough to give him cultures obtained and prepared from plague patients. Dr. Dutt also brought cultures from plague patients which he had himself prepared in Bombay. Then he brought me also Dr. Surveyor's cultures from plague affected rats in Bombay. Cotta's cultures appeared in many respects very much like the cultures that Dr. Surveyor had made from the rats.

26,832. What do you regard as the distinguishing character of the cultures in the case of the true plague microbe?—At that time we did not know so much as we do now. Haffkine's test method was not in existence, and we had simply to go by the morphological appearance under the microscope, and the bluish whitish appearance of the growth on the agar. We had nothing further to go by at the time.

26,833. Did you make any experiments on animals with regard to the toxicity of any of the substances or products obtained from Cotta?—Yes. I am confining my observations to the case of Cotta. I inoculated a guinea-pig with the cultures.

26,834. That is when you got it in a pure form differentiated from the other organisms?—Yes, I inoculated it into a guinea-pig, which was about 500 grammes in weight.

26,835. How much did you inoculate?—Into the guinea-pig 1 c.c. was injected into the peritoneum. 1 c.c. was also injected into the peritoneum of a mouse. The guinea-pig died in four days time, and the mouse died in 30 hours. Similar doses were injected subcutaneously into a guinea-pig and a rat of the same weight, and the only result was loss of weight.

26,836. In all these experiments?—In Cotta's case.

26,837. Do I understand that you injected the products from Cotta into several guinea-pigs and several rats?—Yes.

26,838. And in each case they recovered?—Yes, when injected subcutaneously.

26,839. Did you make any intra-peritoneal injections also?—None, other than those mentioned.

26,840. Only subcutaneously?—Yes.

26,841. The experiments appear to show that it was not an active virus?—It showed it was not an active virus.

26,842. And 1 c.c. is a large quantity, is it not?—Yes, it is an enormous quantity.

26,843. Will you proceed to the next case?—The next case brought to my attention was on the 10th October. Surgeon-Lieutenant Cobb showed me a letter from Surgeon-Major Skinner, which was as follows:—"The case of *pestis ambulans* published in "to-day's paper has aroused our curiosity to know "more about it. Among the soldiers, for certainly "a year past, we have had cases of fever, followed, or "preceded, or accompanied by enlargement of glands "in the groin. There has been no suspicion of "venereal origin. A lad will sometimes come into "hospital with malarial fever, which will run its "course, leaving him cachectic; while being treated "for the cachexia he will grow a bubo. Sometimes a "a man comes in with a non-venereal and non-infected "(*i.e.*, by sepsis) bubo with fever. These buboes will "suppurate in a sluggish manner, and take a long "time to get well. We call them malarial buboes. "When I had dysentery in July last I got small "buboes on both groins. The glands here are still "perceptible, as also in both axillæ. How is one to "diagnose between these cases and a case such as "that of Cotta, reported in to-day's paper? I may "add that, before I came to Calcutta, the above cases "were looked upon as venereal, though without vene-"real origin. It was only when I drew particular "attention to them that they were recognised as non-"venereal." A few days after this I visited the Military Hospital with Dr. Cobb and saw 10 cases belonging to the Shropshire Regiment. The Shropshire Regiment arrived in Calcutta in January 1895 from Hong Kong.

26,844. You say the cases were in October 1896?—Yes.

26,845. The Shropshire Regiment arrived in Calcutta in 1895?—Yes, in January 1895, Dr. Cobb had visited these cases previously and taken from a number of cases specimens of blood from the finger. He came to my laboratory. The specimens were stained immediately, and in a number of them these were diplobacteria of a kind. For example, I have here a drawing (No. 1.) of one which was taken from the

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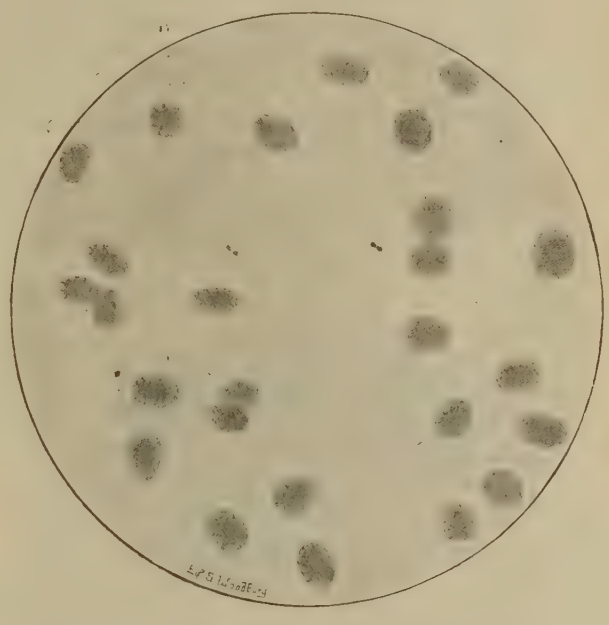
I.



microscopical specimen; it was drawn by Professor Cunningham's draughtsman. I have some others also (*handed to the President*).

26,846. Was anything further done in the microscopical examination besides determining the morphological characteristics?—That is the result of Dr. Cobb taking these specimens. Then I visited the hospital with Dr. Cobb.

26,847. (*Prof. Wright.*) These were specimens taken by Dr. Cobb?—Yes.

26,848. Were they stained by you or stained by him?—I could not exactly state whether they were stained by him or by myself.

26,849. Do you remember how they were stained? That is the point?—They were stained in my laboratory; and I was with him.

26,850. What dye were they stained with?—I think they were stained with methylene blue. Some were stained with eosin.

26,851. Dr. Cobb stained several of them, and you stained the others?—Yes.

26,852. Were the morphological characteristics the same in both?—Yes, they were the same.

26,853. Was there any other examination?—Yes. A few days afterwards I went with Dr. Cobb, to the Military Hospital, and, with the permission of Major Skinner, took some blood from two of the patients there. The method adopted was as follows:—I used a hypodermic syringe. I took out the ram of the hypodermic syringe, and attached some gutta-percha tubing to the barrel of the syringe. I thoroughly boiled this apparatus, and then inserted it into the basilic vein, and drew blood into some tubes containing bouillon and into others containing nutrient agar. I took about 12 tubes from each of the two cases, and in a few days—about three days—there were growths in some of them; and in others there were no growths. I isolated these growths, one from the man Rabbid and another from Finney. Those were the names of the soldiers. The one from Rabbid grew very slowly, in single colonies, in different parts of the agar when I transferred it. The growth was exceedingly slow, but morphologically it showed diplobacteria under the microscope. Finney's, at times, morphologically showed diplo-bacteria, which at other times developed into chains, but at other times grew

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very irregularly, and I cannot say whether they were involution forms or contaminations.

26,854. You got these cultures from the second man, Finney?—Yes.

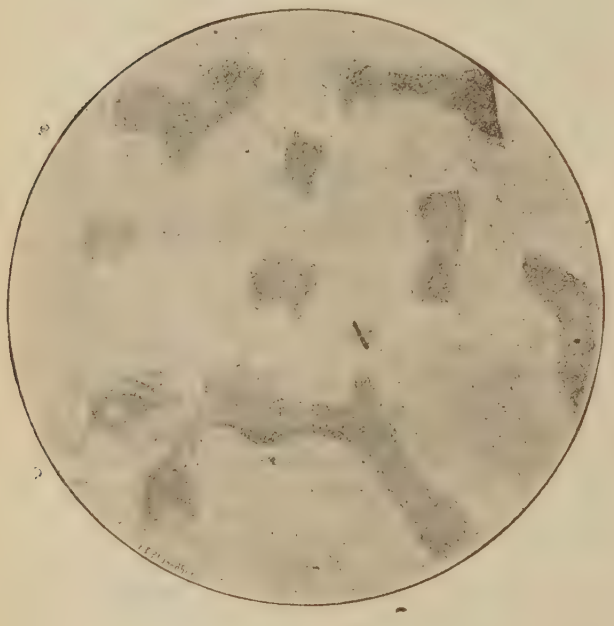
26,855. Did you only draw off blood on one occasion?—Yes.

26,856. (*The President.*) What do you mean by saying that at times such and such a thing occurred, and at other times something else? Do you mean in different or in the same cultures?—Yes, they did not seem to act always in the same way; so much so that I thought there must be contaminations. Sometimes the irregular growths reverted to this diplo-bacterial character again.

26,857. Do you mean in the same specimen?—Yes, after passing through several generations.

26,858. (*Prof. Wright*) Have you any drawings to illustrate this?—That (No. 2) is only the slide specimen

II.



I have. The reason I have not a drawing of Finney's case was that I sent the specimen then, along with others, to Dr. Havelock Charles, who was going home through Japan, and who was kind enough to take them to show to Kitasato, to get his opinion upon them.

26,859. Dr. Charles told us in Calcutta that he had submitted the specimen to Kitasato, and Kitasato said they were diplo-bacilli?—I did not know that Dr. Charles had given evidence on this point. I injected the cultures from Finney and Rabbid subcutaneously into guinea-pigs and rats, but they produced no effect whatever.

26,860. (*The President.*) And in large quantities?—Sometimes as much as 2 c.c.

26,861. Do you recollect in what period of the disease the blood of these specimens had been drawn from these two soldiers?—I do not know how long they had been in hospital. I suppose they might have been in hospital three weeks.

26,862. Do you know if they were suffering from any acute symptoms at the time?—They were in bed.

26,863. Do you know if they had a temperature above the normal?—No, I do not.

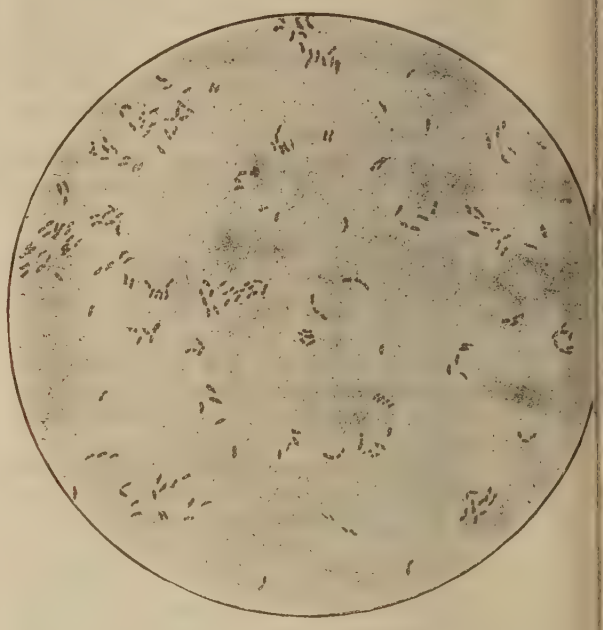
26,864. You have noted the glands which were affected, have you not?—They were in the groin.

26,865. In both groins in each case?—I am not certain.

26,866. But at any rate in the groin?—Yes.

26,867. Is there anything more you wish to say about these cases?—This (No. 3) is a drawing of diplo-

III.



bacteria from the man Rabbid (*handed to President*). It was drawn by the same man who drew the others.

26,868. Would you go on to the next case?—In the second week of October three cases of fever with glandular enlargement were reported to me among children. The names of the children were Tincauri Pal, aged 14, who worked at a jeweller's in Kansaripara Bhawanipur, and who had a considerable dealing with the Marwaris; Grace Hodges, aged seven, whose father was a Custom-house officer, and who lived in Chattawal Gali, Bowbazar, the case was reported by the authorities at the Medical College; and Heman Shah, aged 12, by trade a hawker, living in Jaunbazar Busti, Fenwick Ward. The history of these cases is as follows:—Tincauri Pal, aged 14, lived at Elgin Road, Calcutta. He was apprenticed to a jeweller living at Kansaripara, Bhawanipur. He began to be ill on 5th October with fever. For four or five days previous to this he had had ulcers on his tongue. I described the case at the time with great minuteness, because there was no certainty as to what they were, and I thought it desirable to record exactly what was seen.

26,869. There was no suspicion of these ulcers being syphilitic, was there?—Oh, no. On the 6th October he attended the Bhawanipur Hospital. He had no pain then. On the same day he noticed a swelling in the left groin, and it was painful and tender on pressure. On the 8th, the temperature was 103°, on the 9th, 104, on the 10th, 104°. When the fever rose he had headache in the temples. On the night of October 9th he was delirious.

"*Present Condition.*—On 10th October, afternoon, temperature 104°, pulse 98, tongue furred in centre, a portion of the epithelial lining of the front of palate denuded in patches. A purple coloured rash, varying in size from that of a split pea to that of an eight anna piece, thickly studded on back. Patient's friends state that urine first passed was yellow, and that last passed, colourless. Bowels said to be natural. A bulla on left leg about the size of a two-anna piece. On left groin were three glandular enlargements, one the size of an egg, the second slightly smaller, and just below Poupart's ligament, and the third below these, and the size of a marble; on the right side one of the glands below Poupart's ligament is slightly enlarged. Face somewhat pale, eyes slightly congested, perspires profusely." Blood was taken from the finger, and on examination was found to contain diplo-bacteria. I say 'found to contain diplo-bacteria.' I might have to search half-a-dozen slides before I found them. Diplo-bacteria were not there in numbers. My method of preparing the slides was as follows:—I always had slides prepared in the laboratory, and then put into alcohol. In this case

I washed the finger thoroughly first with soap and water.

26,870. And nothing more?—No. After that I swabbed it with a solution of carbolic acid, and then with a little alcohol, and then pricked the finger, and with a sterilised needle spread the blood over the whole slide, dried the specimens thus made, and then put them into a box and took them to the laboratory at once and examined them.

26,871. Then there was the chance of contamination, was there not? The actual blood was not in any way covered except by being put into the box?—Not in this case. I stained it in the manner already described, and found a similar organism to that I had previously found in the other.

26,872. Do you know what the after-history of this case was?—He went to the hospital and got rapidly well. The Medical Board saw him after a few days, and came to the conclusion that it was not a suspicious case.

26,873. Did the enlarged glands suppurate, or did the enlargements simply become absorbed?—They became absorbed.

26,874. Did you do anything else in the examination of the blood?—No, not in this or the next three cases; they were merely slide specimens.

26,875. Will you give us the next case?—The next case was Grace Hodges. The history was as follows:—On 8th of October she complained of feeling ill, with pains all over the body. On the 9th October she complained of pains in the groins and fever. On examination her mother found the glands in the groin enlarged, and also those of the neck. On the same day the mother took her to Chandni Hospital, where the doctor found the glands in the axilla enlarged, and the temperature of the child was 99°. The Chandni Hospital doctor told the mother it was a suspicious case. Afterwards the doctor wrote a letter stating that he thought this was a syphilitic case. On the evening of the 9th the temperature rose to 102°, but the next day (10th October) after a dose of castor oil, the child seemed better. On the morning of the 11th October she was taken to the Medical College Hospital, where she was also told that the case was a suspicious one of plague, and the Medical College authorities officially notified the case as one of suspicion to the Health Officer. On this notification Dr. Cobb visited the house, in company with Dr. Vaughan, and found the patient in a small house in a crowded locality. The child walked to an upper room, where we examined her and found the following:—

"She looked pale and weakly. Her tongue was furred in the centre and red at the edges. The breath was foul, the bowels said to be regular, eyes clear. There was no rash on the skin, there were no sores on the feet, or signs of irritation about the genitals. There were three small boils on the back and one on the side of the head behind the ear; the skin was dry, and the mother stated she had not noticed any excessive perspiration. There were three enlarged glands under the anterior folds of the right axilla, each about the size of a small marble; there were two similar ones under the anterior folds of the left axilla, and another at the apex of the left axilla. There were three enlarged glands in the left groin above Poupart's ligament, and there was a vertical chain of small enlarged femoral glands on the right side. The enlargements of the glands were more marked on the left side. The cervical glands on both sides of the neck were felt to be enlarged. Blood taken from the finger showed diplobacteria."

26,876. Why was it supposed to be a case of syphilis? What information did you receive?—I think it is stated in the letter of Dr. Panioty that he had attended the sister who had had syphilis, and the child herself had teeth which indicated hereditary syphilis.

26,877. Did you see the father or mother?—Yes.

26,878. Did you make any inquiry as to the possibility of syphilis?—No, we did not. We examined the child thoroughly.

26,879. I mean did you examine the parents?—No.

26,880. Will you give us the next case?—The fourth case was Heman Shah, a boy, aged 12 years, living at 134, Jaunbazar Street, Fenwick Bazar, who came from Monghyr four months ago. On the 10th of October he was attacked with fever, and for two days he passed loose stools, and was delirious at night. On the 12th he was seen by us; his temperature was 103. He was prostrated and complained of severe headache. He had a peculiar, apathetic, and dazed appearance, his eyes were suffused, his tongue was much coated in the centre and red at the edges, pulse was small and very soft. There were no eruptions on the skin. The inguinal glands were slightly enlarged, but not particularly tender to pressure. Blood was taken from the finger, and found to contain diplo-bacteria. On the morning of October 15th his fever was less, and he seemed to be more sensible. He was removed to hospital and, on admission, his temperature was 99. His skin was dry. There was no rash, no head symptoms, tongue moist and reddish. No pain or tenderness in the abdomen. Bowels were constipated. Slight cough, moist râles here and there in both lungs, specially at left base. Heart normal; pulse moderately full and regular; no splenic or hepatic enlargement. Lymphatic glands of neck, right axilla, and both groins were enlarged. On October 14th blood showed diplobacteria.

26,881. Will you describe the enlargements of the glands in this case?—Some were very similar to the others. They were not very large. They were about the size of a small marble.

26,882. Were the margins of the enlargements quite definite?—No; they were somewhat suffused.

26,883. Was there any swelling surrounding the glands?—There was a slight swelling surrounding.

26,884. Where is Monghyr, where the patient came from?—It is in North-Eastern Bengal.

26,885. I ask you because I want to know whether there is any possibility of there having been plague cases in that district?—No, I do not think so. There was not much communication.

26,886. You have no knowledge of any source of infection in Monghyr?—None whatever.

26,887. Will you proceed with the fifth case?—The fifth case was S. M. N., aged 9 years, living in Old Chinsura, who was examined on the 19th October. Dr. Dutt, of Chinsura, who attended the patient, reported the case to me by mistake. It ought to have been reported to the Medical Board. The Medical Board asked that all cases of a suspicious character should be reported to them wherever they might occur, but Dr. Dutt made a mistake, thinking the report ought to be made to me. On receiving the report Dr. Cobb and I met Dr. Dutt and examined the case.

26,888. Is Chinsura a part of Calcutta?—No it is a little north of Calcutta, on the other side of the river. It is about 20 miles north of Calcutta. This is the history which Dr Dutt gave me. The boy took ill on the 5th October with strong fever and severe pains in the forehead and temples; his temperature in the axilla varying from 102 and upwards, rising on the 8th and 9th to 106. After this it fluctuated between 102 and 104. On the 10th October he became delirious; no rash was observed on the body, nor had he any attack of shivering. On the 11th October the glands in his neck, groin, and axilla, were noticed to be enlarged and tender, and they were painful. On the 13th October, Dr. Dutt found the boy's temperature to be 102·6. He noticed a dull and stupid expression on his face; the tongue covered with a whitish fur in the middle, with red tip and edges. There was no sore throat. The glands of the neck were swollen on the right side specially, particularly the chain of glands behind the sterno-mastoid muscle. At night the fever rose to 104·6, and then to 105. He was delirious, with a pulse of 128—small, weak, but regular. The heart sounds were weak, the first sound being almost inaudible. There were a few râles on the bronchi, and the breath sounds at the bases of the lung were weak. There was congestion at bases. There was no enlargement of the spleen. The liver dulness was normal. The urine was high coloured, sp. gr. 1,009, and it contained a small quantity of albumen.

"*14th October.*—Temperature 102·6 in the morning, evening temperature 103·5. The boy complained of pain in the left axilla, but no swelling was perceptible; he was much worse in every way. Pulse 130 to 135,

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and respiration 55 to 60. Dr. Dutt, at this period, feared a fatal issue.

"*15th October.*—The glands in the right axilla became painful on this day. Dr. Vaughan saw the patient, and took some blood from the finger. Morning temperature was 102·6, the evening temperature 104.

"From the 15th to the 19th October there has been a gradual improvement, the fever and the glandular swelling gradually subsiding, but the patient remains in a very weak state. Present condition, as recorded by Dr. Cobb, who made the medical examination: The child was lying on his back in a weak and exhausted condition, with a peculiar dull and apathetic look on his face. The eyes were sunken with dark hollows round them. The conjunctivæ were not congested. Tongue covered with a white fur in the centre with red tip and edges. Pulse was 120. Temperature 100·6 in the mouth. Respiration, 50 per minute.

"*Lungs on Percussion.*—The sounds were normal except at the right base, where there was some loss of resonance.

"*On Auscultation*, bronchial and crepitant râles were heard at both bases, and there was bronchial breathing at the right. Liver and spleen were normal in size.

"*The abdomen* was somewhat distended; there was no sore on the penis or prepuce.

"*Right Groin.*—One inguinal gland was found to be enlarged to the size of a betel nut; it was surrounded by three smaller glands, one being femoral.

"*Left Groin.*—There were four enlarged inguinal, and one femoral gland in this situation.

"The deep *lumbar* glands were felt to be enlarged.

"*Neck.*—The chain of lymphatic glands at the back of the stern-mastoid muscle on the right side were felt to be enlarged, as also were some of the glands in the front of that muscle.

"In the *left axilla*, a gland of the size of a large bean was felt in front of the posterior fold.

"In the *right axilla*, a small gland was felt in the inner side behind the anterior fold."

Blood was taken from the median-basilic vein, and found to contain diplo-bacteria.

26,889. Was that on the first occasion or on the second occasion?—On the second occasion. Blood was also taken by Dr. Vaughan.

26,890. (*Prof. Wright.*) Was that only a cover-glass specimen?—Yes; I took blood from the median-basilic vein; but, unfortunately, I used a syringe, which had been sterilised, and had been used for the anti-cholera inoculine. After filling the syringe and when putting some of the blood into the culture media, it was distinctly noticed that the syringe was defective, and that air was being injected into the tubes as well as blood. I had half-a-dozen cultures, and one of those I sent to Dr. Cunningham at once; the other five were retained. In a few days I discovered the whole of these were contaminated. They certainly had aërial organisms in them. I informed Dr. Cunningham of the matter, and it is one of those cases which he mentions in his report where there was contamination. These were the circumstances.

26,891. (*The President.*) That examination was not satisfactory?—It was not satisfactory.

26,892. Will you proceed to the next case?—On the 20th October, the death of a woman, named Golab, was reported to me by one of my inspectors and by the police. I went to inspect the body before it was burnt, and was accompanied by Captain Vaughan and two of my medical assistants. On inquiry, we found that she had suffered from high fever, and from swelling of the glands on one side of the neck. These were not parotid glands. On inspection, we found no swelling in the groins, but a swelling of the glands on one side of the neck. Blood was drawn by a sterilised hypodermic syringe from the heart, which was found to contain large numbers of diplo-bacteria, but as all the cultures were much contaminated, no decided opinion was formed at the time. After seeing similar cases in Poona, I am satisfied that this was a case of plague.

26,893. What was the cause of the contamination?—The body had been lying there for a few hours before we reached the place.

26,894. So far as the bacteriological examination was concerned the result was nil?—Yes, it was nil.

26,895. Then will you give us the seventh case?—On the 1st November, a child named Giga, aged about 4 years, the son of a Marwari, living at 13, Armenian Street, was reported to me by Major Charles, Professor of Anatomy at the Medical College, and Second Surgeon to the Hospital, as being a suspicious case of plague. The following history was given me by Dr. Charles:—"Ill about 10 days; fever and slight "bronchial catarrh; glands of left axilla enlarged to "the size of small marbles; those of right axilla "enlarged, but less so. Glands of right groin palpable; "one gland in left groin inflamed, and will suppurate, "sub-occipital glands can be felt, but do not count, as "child has had eczema of the auricles." I thought I had his letter, but this is a quotation from the letter.

The gland was opened and pus and blood specimens taken, and diplo-bacteria were found in the groin. The microbe could not, however, be isolated, owing to contamination; the reason being that the abscess was opened in the room before any precaution had been taken. The people in the house were Marwaris, who received goods, clothing, and yarns from Bombay. On inspection of the warehouse below, in which the child Giga was accustomed to play, a rat was found, sitting on the step, looking very ill. It did not attempt to escape, and seemed to be in a dazed condition. The rat was taken to the laboratory, killed there, and immediately examined. Its glands in the left groin were found to be enlarged, congested, and agglutinated together. The internal organs were generally congested; diplo-bacteria were found in the blood and internal organs, and cultures from these gave growths resembling specimens of plague bacilli in rats received from Bombay. Healthy rats were examined, but no bacilli were found in their organs. I examined at that time seven healthy rats from other parts of the town. The following are notes made at the time on the condition of the rat suffering from glandular sickness in November 1896 in Calcutta:—

"*1st November* 1896.—Rat from 13, Armenian Street, found ill, furred coat, eyes dull and watery, seemed to be unable to move freely, did not run away when approached by people. Killed at 1 p.m. Blood, when examined, found to contain diplo-bacteria and large coccoid bacteria. On deflecting the skin, the glands, in both inguinal and axillary regions, much enlarged and inflamed. Spleen enlarged, liver enlarged and soft, heart dark in colour, kidneys enlarged and congested, blood very watery, lungs light grey with darker patches. Bouillon and agar inoculated with blood from heart, spleen and liver. In two days bouillon and agar showed diplo-bacteria similar to those in the specimens in the rat.

"*3rd and 4th November.*—Seven healthy rats examined, and microscopic specimens made from heart, liver, and spleen. No microbes observed in the specimens. Cultures from five show no growths, cultures from two show micrococci and bacillus subtilis." It was found that the enlarged glands and the organs were filled with diplo-bacteria.

26,896. Could you give us any particulars of the appearance of the growths in the agar and bouillon cultures?—The appearance of the growths was in bouillon slightly granular, and on agar bluish white colonies. At first they grew much more rapidly, apparently, than those which I have been speaking of, and much more luxuriantly. Under the microscope a number of them had a coccoid form, but were much smaller than in the specimens I had received at Bombay—M. Haffkine's specimens. They were smaller, but that was the only difference I could see.

26,897. Did you take any steps to ascertain whether any of these cocci were pathogenic?—No, unfortunately, I did not. I did cut off part of the organs of this rat and let other rats eat the pieces, but no result followed.

26,898. Will you give us the eighth case?—A week later there was an outbreak of glandular sickness, with great mortality, among rats in two houses in the Marwari quarter. It was reported to me by the inspector of the special cleansing gang working in Jorabagan that within a few days there had been an unusual mortality among rats in two grain shops in Burtolla Street.

26,899. Is this quarter near Armenian Street?—Yes, just a little north of Armenian Street.

26,900. Therefore, these houses were near 13, Armenian Street?—They were not near; they were separated by at least three streets.

26,901. Do you know if these people had any intercommunication with the people in Armenian Street?—I do not think so.

26,902. Did you inquire into that?—I did not.

26,903. Will you proceed?—Dr. Cobb and I visited the two grain depôts, 81 and 14, Burtolla Street, and saw nine sick rats and eight dead in 81, Burtolla Street, and one sick rat in 14, Burtolla Street. 14, Burtolla Street is nearly opposite No. 81. Some sick rats were taken to the laboratory and examined, with the same result as that obtained from the rat in 13, Armenian Street. This (No. 4) is the drawing of the

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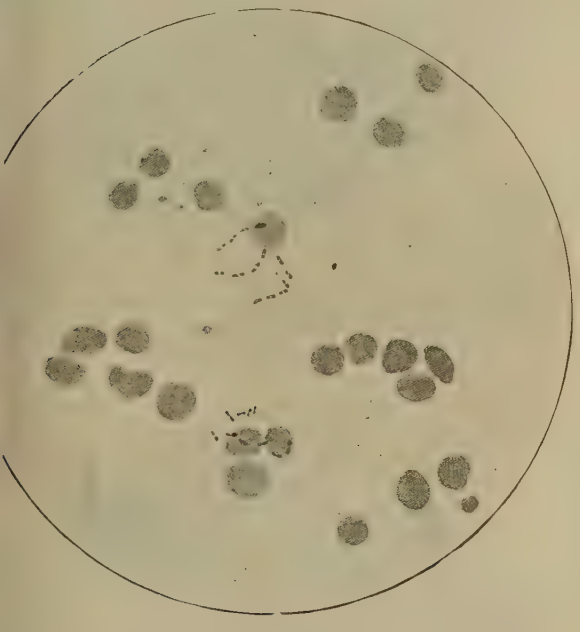

plo-bacteria in the spleen of one of them; this (No. 5)

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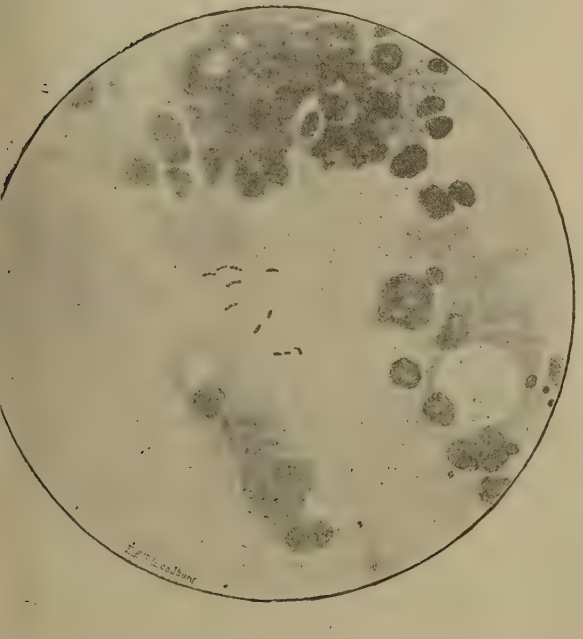
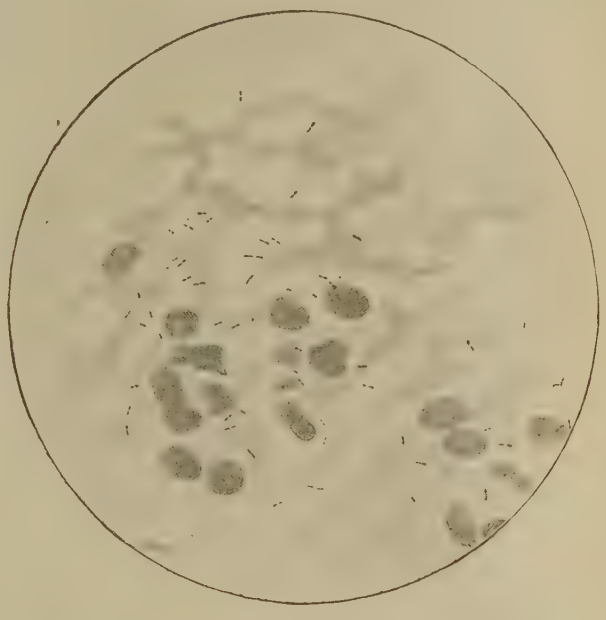


of the diplo-bacteria in the liver of another; and here (No. 6) is the drawing of the plague bacillus in the

VI.



gland of a rat in Bombay which was sent to me. Steps were taken to have No. 81, Burtolla Street closed. Meanwhile, the number of deaths among the rats was recorded, the mortality in 81, Burtolla Street reaching in one day to 100. The floors were dug up; the place flooded with disinfectants, and any rats found killed; after which, no further cases occurred. This history was repeated in No. 14, Burtolla Street, somewhat later, with a similar result. A few sick rats were found in adjoining houses. These houses were also thoroughly cleansed and disinfected, and the rats destroyed. After this, it became evident that the epidemic among rats was checked, as no other sick rats could be found. Examination of healthy rats showed them to be free of diplo-bacteria.

26,904. What disinfectants did you use?—Crude carbolic acid. I will give you the extract from my notes on the *post-mortem* :—

"*9th November.*—Three rats, No. 1, No. 2, No. 3, brought from 81, Burtolla Street. One died on the way to the laboratory, and was, therefore, not examined; another died in the laboratory, and a *post-mortem* was made at once. The axillary glands on the right side were enlarged, the liver was enlarged, and there was a greyish deposit on the surface. Specimen of spleen pulp showed diplo-bacteria in pure culture. The liver pulp showed diplo-bacteria, and the blood from heart showed diplo-bacteria. Cultures in bouillon next day showed diplo-bacteria in chains, and large cocciform bacteria single and in chains.

"*11th November.*—Rat No. 4, sent by Dr. Banks from Burtolla Street. The rat died a short time after arrival. *Post-mortem* held at once. The glands of both groins found to be enlarged. Liver congested with greyish white deposit on surface; spleen large and congested; the lungs and the auricles of heart congested; spleen and liver pulp showed diplo-bacteria varying in shape from the coccoid to the diplo-bacterial form.

"*16th November.*—Rat No. 5. Killed one of two rats brought yesterday from 81, Burtolla Street, the other having died during the night. *Post-mortem* showed no enlargement of glands. Intestines not inflamed. Liver not congested, but has a few whitish granular spots on the surface. Spleen congested, dark coloured, and enlarged. Heart normal. Specimen of heart blood, liver, spleen, show numerous diplo-bacteria, many of which were in chains.

"*19th November.*—Rat 6, sent by Mr. Scott, the inspector, from 81, Burtolla Street. Died at 2.40 p.m.

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Post-mortem at once Has nearly the whole of its hair denuded. Has a large swelling on the neck about the size of a marble, another swelling of the same size under the left axilla; another swelling oblong in shape about three-quarters of an inch in length and half an inch in breadth on left groin. On deflecting the skin on abdomen and chest, the inguinal and axillary glands on right side slightly enlarged, those on left much swollen. The liver is enormously enlarged, occupying nearly half the abdominal cavity; the surface is spotted with white caseous matter; the pus from the glands teems with diplo-bacteria.

"20*th November*.—Cultures from glands show diplo-bacteria mixed with straight bacilli; cultures from heart blood show diplo-bacteria and coccoid bodies single and in chains, and a few long bacilli."

26,905. Were they inoculated into other animals?—No; I, unfortunately, missed doing so. I was extremely busy at the time, and was unable to carry on the investigation as I should have liked.

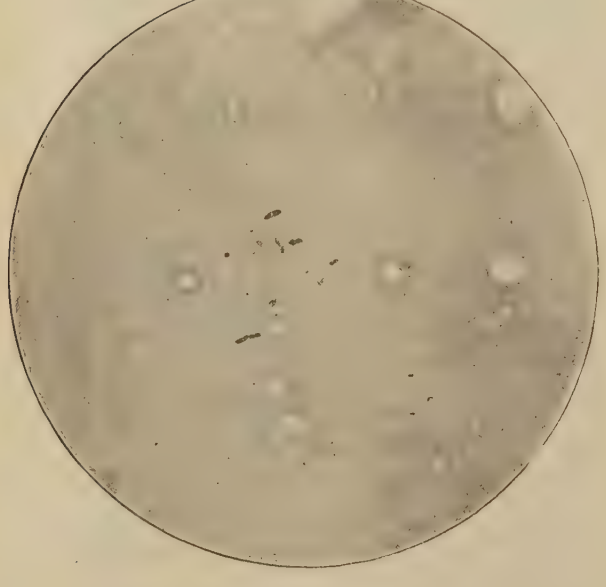
26,906. I understand that there were no cases among human beings associated with this rat-mortality?—No. One of the circumstances against the likelihood of cases occurring amongst the inhabitants was that in No. 14, Burtolla Street there were no inhabitants. We had turned them out previously. It was only a grain depôt, and in No. 81, Burtolla Street we turned the people out immediately from the lower floor, and the rest remained in the upper floor until we turned them out from there.

26,907. The only case of suspicious plague in that neighbourhood is the one you have described as number 7?—The only suspicious case was that of the child.

26,908. Will you proceed with the next case?—On the 3rd November, Bepin, an adult, working at a sugar factory, at No. 1 Raja Rajbullub Street, was reported to me by a native practitioner, Dr. Mukerji, as suffering from plague. Bepin had been attacked suddenly with fever, accompanied with severe head-ache pain, tenderness, and enlargement of the glands in the right groin. He soon became insensible, and died on the evening of 3rd November. Blood taken before death from vein in the arm, and from groin, showed diplo-bacteria, which were afterwards cultivated.

26,909. That is immediately before death?—Immediately before death. I have specimens of these cases. (*Handed to the President* (A, B, C)).

A.



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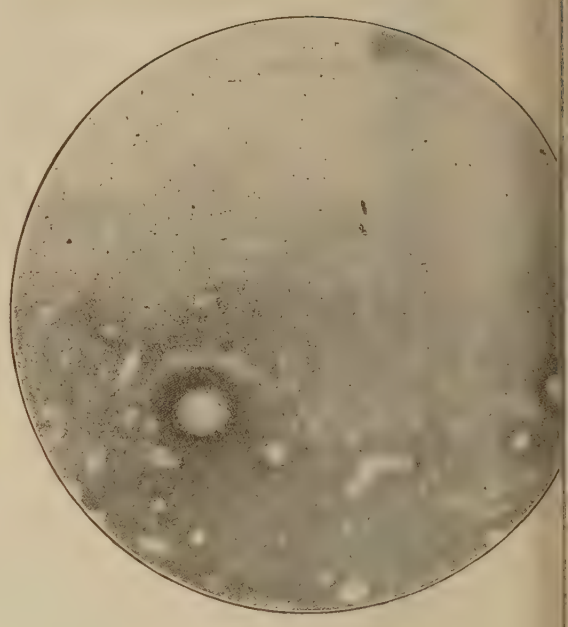

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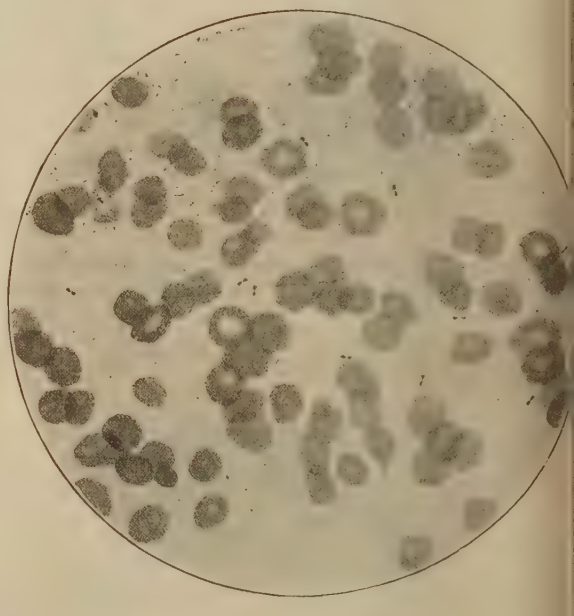


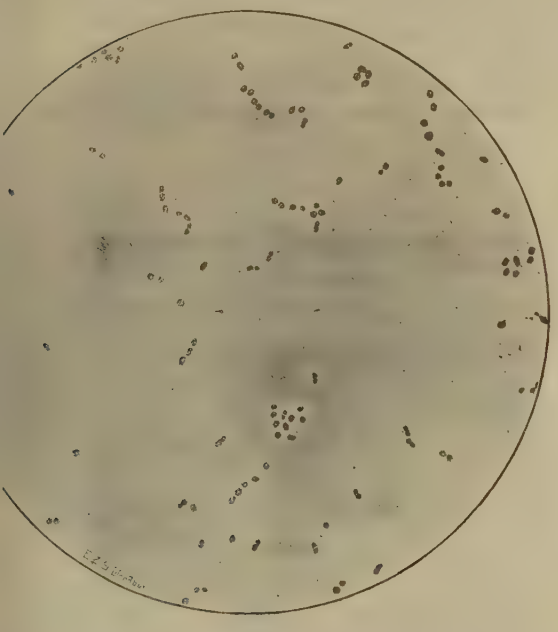
26,910. (*Prof. Wright.*) Are these drawings of cover glass specimens derived from that case?—No, not cover glass specimens in the case of the blood, but slide specimens.

26,911. Did you draw off the blood with a syringe?—No, not with a syringe. I had one of the sterilised pipettes which are used in the laboratory.

26,912. (*The President.*) Are these all magnified to the same extent?—Yes, one-fifteenth (Reichert). This

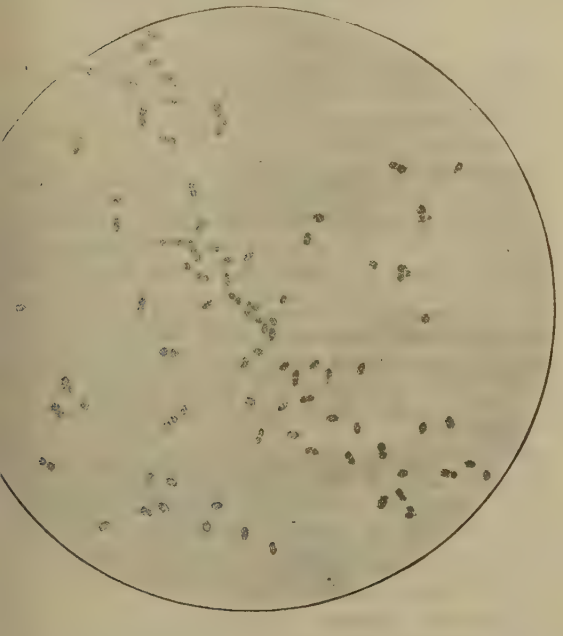
the appearance in the serum culture. (*Handing drawing to the President* (D)). They vary at times. At

D.



times they formed chains, while at other times they did not. This (E) is from a rat which was killed by 1 c.c.

E.



In another case 2 c.c. injected under the skin killed the rat.

26,913. (*Prof. Wright.*) Are these drawings of preparations which were made from rats killed with blood from Bepin?—Yes.

26,914. (*The President.*) Two c.c. is an enormous quantity, is it not?—Yes, it is; on the other hand 2 c.c. from the others already mentioned did not kill. My best specimens were, unfortunately, sent to Dr. Charles, and I did not get them back, so that I was not able to draw the specimens that were most characteristic.

26,915. Will you proceed with the next case?—On the 12th November I examined, along with Dr. Cobb, the dead body of a man named Marcellies, who lived in Bowbazar, and who was reported as having died of fever and glandular swelling. We found the axillary glands on one side much congested and swollen. I found organisms in that, but they were such that I could not give any opinion upon them. At one time they appeared to be of a similar kind, while actually later they came to be more like the streptococci of septicæmia. In December the Medical Board reported that none of the cases which had come before them were plague, but due to other causes.

26,916. (*Prof. Wright.*) All these cases were looked into by the Medical Board, I presume?—Yes.

26,917. (*The President.*) I want to make that perfectly clear. Each of these cases in human beings, which you have hitherto narrated came under the investigation of the Medical Board?—Yes.

26,918. Other cases investigated by the Medical Board did not come under your observation?—No, I do not think they were reported to me.

26,919. Will you state the next case?—In December 1896 a young girl, aged 16, living in a lane in Bowbazar, was treated for fever and glandular enlargements; the glands in both groins and in one of the axillæ enlarged, and after about a fortnight's illness suppurated. I saw the case with the medical attendant.

26,920. Did the case recover?—Yes, the case recovered, but it was tedious.

26,921. How near to each other were these two cases in Bowbazar; were the houses near to each other?—The two cases, Nos. 9 and 10, were not very far off from one another.

26,922. Have you any reason to suppose that there was any intercommunication?—No, I think not.

26,923. Did you ascertain that there could not have been?—No, but I may say that there had been a suspicious case of fever and glandular enlargement next door to No. 10. But I did not see the case, because the man had bolted by the time I visited the house.

26,924. Then will you give us the eleventh case?—About the same time two cases of fever, with glandular enlargement in one groin occurred in two boys, of one family, living in the southern suburbs of Calcutta. They were young boys about seven or eight years of age. The glands in the groin went on to suppuration.

26,925. Did you see the cases?—Yes.

26,926. What is the next case?—In January 1897 I saw a case of fever with glandular enlargement in the groin in a man who had arrived, a few days previously, from Singapore. He appears to have become unwell on the voyage; the disease corresponded in character with that in the Shropshire Regiment.

26,927. How long does it take to travel from Singapore to Calcutta?—A fortnight.

26,928. The man was already convalescent on arriving at Calcutta?—No, I do not think he was convalescent.

26,929. Did you see him?—Yes.

26,930. He had fever when you saw him?—Yes. The information I am about to give, I may say, is second hand. In January 1897 a large number of cases of fever with glandular swelling and buboes, some of which were fatal, occurred at Singapore.

26,931. What was your source of information? I understand it is the 21st Appendix to the First Volume of the Army Medical Reports, 1897?—That may be so, but I think I got it from the Singapore Reports. No satisfactory diagnosis appears to have been made. Active measures, however, for the segregation of the sick and the disinfection of the infected houses, were carried out. In connection with the occurrence of these suspicious cases it is to be noted that the Hong Kong authorities reported that undoubted cases of plague were brought to Hong Kong from Singapore. Some time in the early part of 1897, I was asked by the medical officer in charge of the Military Hospital at Alipore to see a native soldier who was ill with fever and glandular swelling of the parotid; also a boy in the same hospital, suffering from fever and a glandular

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swelling in groin. Both were well isolated, and every care was taken of them. The soldier died.

26,932. Will you proceed?—A medical man in practice in Calcutta informed me that he had, in the early part of 1897, come across several cases of fever with glandular swellings, all of which got well.

26,933. What was his name?—Dr. Coulter. He is one of the European medical men in private practice in Calcutta.

26,934. Will you proceed with Number 14?—In February, according to the report of Dr M. J. Rosenau, the Quarantine Medical Officer to the Supervising Surgeon-General of the United States Marine Hospital Service, the British ship *Annie Maude* left Calcutta with three suspicious cases amongst the crew. The report is dated 16th July, and is as follows:—

" National Quarantine Station,
" Angel Island, California,
" SIR, " July 16, 1897.

" I HAVE the honour to report the British ship *Annie Maude*, 143 days from Calcutta, was placed in quarantine to-day for disinfection. A short while after leaving Calcutta one of the crew was taken ill with swellings in the axilla, groin, and elbow, and died. Two more of the crew suffered with buboes, from which they recovered.

" Very respectfully,
" M. J. ROSENAU,
" Passed Assistant Surgeon,
" U.S., M.H.S."

26,935. Can you tell me with regard to all these cases whether you have had any evidence to suggest infectivity or contagiousness among them?—No, I have had none.

26,936. You have had no evidence to show that they emanated from any previous cases of plague or disease of a similar kind?—None.

26,937. You have had no evidence to show that any of the rats which died communicated the disease, or that the disease had been communicated to the rats from human cases?—None.

26,938. With regard to your blood examinations, did you make any similar examination, following, as nearly as possible, the same procedure, in cases altogether free from suspicion of plague?—Yes. Dr. Cobb and I examined a number of healthy men and took blood from them, following the same procedure, using the the same apparatus, and the results were entirely negative.

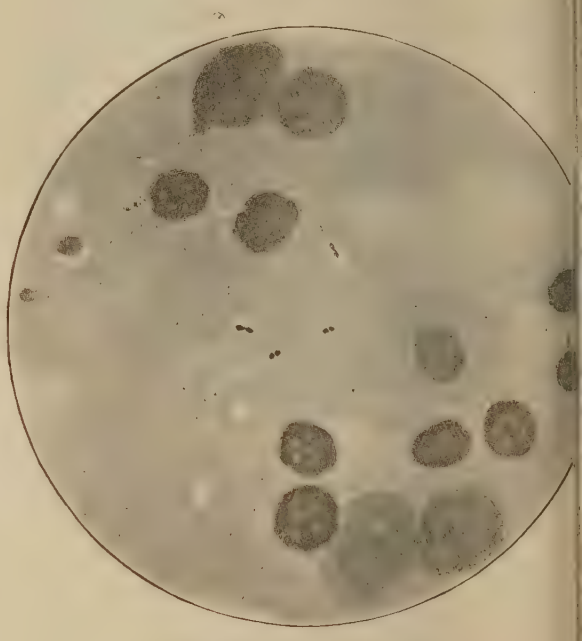
26,939. You found no organisms?—No organisms at all. I may mention that for years I have been examining blood in Calcutta. I am very much interested in vaccination, and also in the possible cultivation or discovery of an organism. During the process of trying several experiments, I have examined hundreds of cases of blood from children who have been vaccinated. I have also examined cases of patients who have had small-pox, and I have never seen any micro-organisms of this kind.

26,940. The procedure adopted in all these instances was the same as you have described?—Yes, very similar to that. It came to me as perfectly new to find that on the slide specimens there were organisms in these cases. It is supposed that the cultivations are all contaminations, but you cannot get an organism of that description which I have shown you in the slide specimens from the air.

26,941. What did you do to test that?—That is my opinion from a fair amount of experience. I have had slides specimens made in my laboratory over and over again, and had them exposed, and examined them afterwards, but have never found organisms. There is another point which I may, perhaps, mention to the Commission. I have been examining the blood of animals since 1894, with the object of seeing whether there were any organisms in the blood of those suffering from cattle plague. It was only in January 1897 that I found organisms in some cattle similar to those I have shown. There was an outbreak, very near the locality where the rats were dying. The cattle did not altogether suffer from the same symptoms which I had observed before in rinderpest. Some of the cattle evidently suffered from emphysema, with air passing into the tissues under the skin which made them look like stuffed animals. On examining two of these I found in the blood of these cattle, which were supposed to be suffering from rinderpest, organisms of a very similar character. On the *post-mortem* examination I found that the mesenteric glands were exceedingly congested and enlarged to a degree I had never previously seen.

26,942. You mean you found similar organisms to those in the rats—diplo-cocci?—Apparently diplo-bacteria (F). I have examined the blood of hundreds

F.



of cattle, both *post-mortem* and alive, and in the rinderpestic animals I have never found anything.

26,943. When did the cases occur in which you did find them?—In 1897.

26,944. At the same time?—A little later. They had the peculiarity which I have never found in rinderpestic animals, of the mesenteric glands being perfectly black, congested, and enormously enlarged.

26,945. Where had these cattle been kept?—They were kept in Calcutta, in some of the cattle-sheds which, I daresay, the Commission have seen, just a little north of the grain depôts.

26,946. You suggest that these cattle had also this disease, which you suppose to be plague—*pestis ambulans?*—I put that forward as a suggestion, but nothing more.

26,947. How many cattle did you see showing these symptoms?—There was an outbreak of the disease; I only examined, *post-mortem*, about three or four, and I examined the blood of only two in this way. Of course, these things only gradually dawn on one. I would not put it forward as more than an observation which, I think, is worthy of notice.

26,948. (*Prof. Wright.*) I understand you take each of these cases as being a probable case of plague. You have some grounds, in each case, for thinking it may gave been a case of plague?—Yes, I looked at it in this light; seeing there was plague in Bombay, and that these cases had cropped up in Calcutta, it was being on the right side to take precautions and endeavour to isolate them. That was the position I took.

26,949. I quite see the practical necessity for doing so. In the case of Cotta, the boy had buboes in the groin, had he not?—Yes.

26,950. And we may assume from that, may we not, that these bubos were due to some absorption of bacteria, either from the genital tract or from some wound on the surface of the skin of the leg?—Yes.

26,951. And in this particular case there is a history of sexual contamination, is not there? There is a definite history on that point, is there not?—It was not definite.

26,952. I thought you reported the presence of a rtain number of pimples on the *glans penis?*—Yes, it the glands had enlarged before that time.

26,953. I noticed that there was a disparity in that spect between your account and the account of the edical Board. You think you have a history of the ands having enlarged first, *i.e.*, on the 8th September. ow the sexual intercourse is said to have taken place the 13th, according to the Report of the Board?—es.

26,954. Further, if we take the history which you ve, it appears that these glands got worse simulneously with the appearance of "pimples" on the nis?—Yes.

26,955. Therefore, it is probable that some of the andular trouble was due to the absorption of septic atter through the penis?—It might be that.

26,956. Therefore, if this was a suspicious case of ague, it was at any rate a case which was complicated y septic infection from the penis?—Yes.

26,957. And further, unless it is absolutely clear om the history that the glandular swelling appeared efore the sores on the penis, it would be more natural regard the case simply as a case of sexual bubo?—es; the only thing is this, that our examination larmed people, and I think they naturally looked to omething else.

26,958. As an explanation, you think they might have nvented the venereal history to account for the preence of the bubo?—It is rather a hard thing to say hat, but I think it is possible. Three medical men nquired into the case, and the first thing we naturally hought of was syphilis, and both Dr. Tomes and Dr. Jobb made very minute inquiries concerning this aspect f the case. We discussed the matter thoroughly, and aving it in view that these glands were possibly due o syphilis, we were very particular in the history we ot from the patient. There was no mention whatever t that time of this sexual connection on the 13th September.

26,959. In the history, which was obtained by the Medical Board which examined the cases, it was recorded that Cotta was treated twice for soft chancres by Mr. Mitchell, the Assistant Surgeon?—Yes, but hey must have healed very rapidly, because I understand they were there only four days.

26,960. They are reported to have been better on the 4th, and you saw Cotta on the 9th. I do not know whether that account is correct?—He went on the 2nd and had an application, and they were better on the 4th. That is a very short time for them to get well.

26,961. But the principal point which induces you to believe that this was plague is the fact that you found bacteria in the cover-glass preparations of blood?—Yes, and the glands having enlarged before there was any venereal history.

26,962. Do you not think that general experience shows when you find bacteria in the blood in plague the case is a very serious one?—I know that is the general experience, but on the other hand, there is the experience of Kitasato. Kitasato found in 25 cases out of 28 the bacterium in the blood, and he states that it is found in cases of recovery. I have here a note from the "Twentieth Century," Vol. XV., which was written by Kitasato and another Japanese, Nakagawa. They described the two bacilli, and they say Yersin's bacillus is much larger than Kitasato's bacillus, and does not possess the distinctly diplo-coccus-like appearance of the latter. Owing to its polymorphism exact measurements are difficult to give. It does not possess a capsule. Yersin's bacillus is not mobile, and is decolorised by Gram's method. Cultural differences are equally conspicuous, on agar the growth of Yersin's bacillus is extremely luxuriant, and though the growth is rather slow at first, it continues for a week forming creamy white opaque colonies, projecting a good deal above the level of the surface of the media. Very young colonies are small and quite translucent. On the other hand, the colonies formed by Kitasato's bacillus are extremely delicate, transparent, small discs which attain the size of a pin's head and cease growing, and then disappear on the fourth day of incubation, presenting in all these respects a close resemblance to the growth of diplo-coccus pneumonia. Bouillon is not made uniformly turbid by the development of Yersin's bacillus; further, milk is not coagulated by the growth of Yersin's bacillus. The above-mentioned differential points ought to be enough to distinguish the one from the other. It is curious to observe that Yersin states that his plague bacilli (Y.B.) are found in the blood only of foudroyant cases, while Kitasato maintains that the bacilli (K.B.) can be found constantly in the blood; and further, that they may be found in the blood of convalescents for from three to four weeks after the symptoms have subsided.

26,963. At any rate it is the common opinion of bacteriologists that plague bacilli are only present in the blood in grave cases of plague, is that not so?—Yes, I admit that.

26,964. With regard to these other cases which occurred in the Shropshire Regiment, what opportunities do you think these men had for infecting themselves with plague? Did you bring the cases in any relation with the fact that these men had been in Hong Kong in 1895?—I only got my information second-hand.

26,965. And you learned, as a matter of fact, they arrived from Hong Kong on the 3rd January 1895?—Yes.

26,966. And these cases occurred in October 1890?—Yes, there were crops of them continually occurring from 1895 to 1896.

26,967. But in view of the fact that there was a continual crop of such cases recorded from the year 1892 onwards, and in view of the fact that non-venereal buboes are found all over India, do you think you can bring these cases into any relation with the fact that the regiment had served in Hong Kong?—Yes. I think Dr. Skinner's letter shows that these cases have not been continually occurring.

26,968. I have before me a record* of cases of inflammation of the lymph glands occurring in the troops in Calcutta from 1892 to 1896, and I find 29 cases recorded in the year 1892, 39 in 1893, 20 in 1894, and 42 in 1895?—I admit that, but I think possibly that these lymph cases may be similar to what occurred in England. There must have been some difference for the medical men to have taken notice of the cases that occurred in 1896 under Major Skinner, and the others.

26,969. You think these cases which occurred in 1896 were different?—I think so, otherwise why should they have been noticed at all? Why should the question arise? What are these cases? I have been looking into the Army Medical Report of 1894, and I notice that under the lymphatic and glandular system there were 11,000 admissions in England and 324 in Ireland, cases of a similar character. If these cases are so well known, how was it that it should attract the attention of the medical authorities at that time—in 1896—as being something peculiar, not knowing the cause; because first of all on the tickets of these cases they are marked "syphilitic," afterwards they are marked "malaria," and still later "unknown"? I think that very fact indicates that these cases which occurred in the Shropshire Regiment must be different from those which are commonly noticed elsewhere, and which were so common in the army under this particular classification.

26,970. We have had Major Skinner's evidence on that point, but we want to supplement his evidence by your evidence on the bacteriological facts. I understand you made cultivations from two of these cases, one, the case of the man Rabbid, and the other the case of Finney?—Yes.

26,971. You inoculated 12 tubes from each of these men?—Yes.

26,972. We have it from Major Skinner that all these tubes remained sterile except one; is that accurate or inaccurate?—It may have been so, but I could not say the exact number. I think there were one or two that gave this organism, but I would not be certain up on that.

26,973. Still, the large majority of the tubes you inoculated remained sterile?—Yes.

26,974. (*The President.*) There were 24 tubes in all and no growth appeared except, possibly, in two?—Two or three of each set, but the majority of them remained sterile.

26,975. (*Prof. Wright.*) How do you, in view of the fact that these drawings which you have put in, show 2, 2, and 5 bacteria respectively in each field, account

* *See* App. No. XXI. in Vol. I. of these Proceedings.

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for the fact that bacteria did not develop in each of the tubes you inoculated with this blood?—I mentioned that I took a large number of slide specimens, and it is only in one or two that I found this.

26,976. It was not in every field that you found bacteria, but only in a very few of the fields?—That is so.

26,977. Did you find bacteria in each cover glass?—No. By examination we found them in one or two specimens of each.

26,978. (*The President.*) Supposing you had one cover glass in which you found them, does that mean that they were found in only one field of the cover glass or several fields?—It was not a cover glass specimen, but a slide specimen.

26,979. One part only, or in many parts?—Not in many parts on one slide. I have had that drawn. That was the only one found on that particular slide.

26,980. (*Prof. Wright.*) Do not you think there it would be quite possible to find that number of contaminating micro-organisms on every slide?—I hardly think that it would repeat itself so frequently.

26,981. In view of the fact that diplo-cocci, such as you figure here, are very frequently met with in the earth in Calcutta, do you not that it is very probable these diplo-cocci which you figure may derive from contamination?—I hardly think so. Of course, if they had fallen on the ground it might be a different matter, but seeing they were put into a box and taken to the laboratory at once and examined, I do not think that could be so. I will not say it is an impossibility, though, personally, I do not think it is possible for a fully cultivated and regularly formed organism to alight from the air on to a slide. I do not wish in any way to press my views on the Commission. I want it to be understood that what is really wanted is truth. I have no bias one way or the other. Having met with these organisms, I am not attached to them.

26,982. In view of the fact that these cases occurred 18 months after the regiment came from Hong Kong and in view of the fact that so few bacteria were found in the blood I ask you whether it did not suggest itself to you that possibly these buboes had nothing whatever to do with the plague?—You must remember that although it was 18 months after the regiment came from Hong Kong there were continuous crops of cases recurring and that it was only the last ones which were ill which were examined.

26,983. Do you adhere to your opinion that these cases may have been cases of plague in view of the fact that these cases occurred not only in the Shropshire Regiment, but also in the Artillery, which has never been in Hong Kong?—I think the very fact that these cases occurred after the Hong Kong Regiment reached Calcutta shows that there is no reason why they should not also occur in the Artillery which were in close association with them.

26,984. That applies only if it can be shown that these cases among the Artillery did not occur also before the Shropshire Regiment arrived?—Yes. But what has struck me is this, that these lymph glands are so common in England and elsewhere that they must be well-known to Major Skinner and to others, and yet medical men came to the conclusion that very year that these cases might be venereal, might be malarial, or might be due to an unknown cause. They were cases that attracted attention and puzzled the medical men. It seems to me there was something different from what was usually met with.

26,985. I take it, in view of the evidence that we have already had from Major Skinner, that the facts did not present themselves to his mind in that light?—I think he says attention has been directed to it for certainly a year past.

26,986. Is there a single case among those which you have reported in which it appears to you to be absolutely established that you were dealing with a case of plague, or is your opinion that you were dealing with plague, based upon a general survey of all these cases? Are there any of these cases in which you obtained the plague bacillus in pure culture?—Yes, I think there can be little doubt that Giga, the child attended by Dr. Charles, was a case of plague; that Golab was a case of plague; and that Bepin was a case of plague; and I think, taking a general survey of the other cases, that most of them were probably mild cases of plague. I obtained the plague bacillus in a pure culture in the case of Bepin, and in 1897 I took the culture to M. Haffkine, when I was leavin India, and he examined the tube and said the growt was suspicious. He also examined the microbe micr scopically and said it was very much like the plagu bacillus. Then he subjected the bacillus to the stala tite test, but, unfortunately, it did not grow—apparent the culture was too old.

26,987. (*Mr. Cumine.*) Of the cases which you kne of, and thought to be plague in 1896, how many died —I mentioned Bepin's case and then I mentioned tha of Golab.

26,988. I mean what proportion died?—I have n worked out the proportion but they were exceedingl few. I did not go on the mortality.

26,989. If it had been really plague would not large proportion have died?—Not necessarily. I hav known instances where a large proportion have no died even in the Bombay Presidency. There was th outbreak which was reported by Dr. Dyson in Kosumba in the Bulsar Táluka, where three-fourths of the case were very mild. He reports that: "The disease was o "a mild type, characterised by slight fever of two o "three days' duration and the formation of buboes "chiefly in the groin. Fully three-fourths of the 3 "cases which occurred were of this type, and during "one visit to the village I found two boys, about 1 "years of age, with buboes in the groin, whose feve "had been so slight as to escape observation, and "they had not been recognised as cases of plague." You get different types of plague. Then there are well-known cases which were suspected to be plague in Astrachan† in 1877; there were 150 to 200 person attacked without a single death. There was also the epidemic of plague at Resht‡ in which two forms o the disease were noticed, a severe and mild form. O the former, who mostly died, there were about 170 There were about 600 mild cases which nearly all recovered.

26,990. Did any of those cases which you thought was plague produce any infection in the neighbourhood?—No, I was not able to trace any infection.

26,991. Or amongst the people who attended the sick?—No.

26,992. (*The President.*) Can you give us the reference to the statement made about Astrachan?—I think it is in the Medical Officer's Report for 1879, to the Local Government Board. I will send the exact reference to the Commission.†

26,993. (*Mr. Cumine.*) Did you attempt to see whether the bacillus, which you thought was plague bacillus, could be obtained from the blood of healthy animals, such as fowls?—No, I did not. I tried with rats. I examined 12 rats to see if they contained any bacilli.

26,994. Did you see the bacillus which Dr. Cunningham obtained from the blood of a healthy fowl?—No, I was not favoured with that.

26,995. You do not know whether that exactly resembles the one which you found?—No.

26,996. Were all the specimens of the supposed plague bacillus found by you exactly alike? Did they all resemble one another?—They seemed very much alike in character. Culturally, they did not go on alike. Sometimes they were of a more coccoid form—egg-shaped bacilli amongst them—and at other times they seemed to go into chains, and others were diplo-bacteria.

26,997. In the case of the lad Cotta, does not Dr. Dyson report that the local sores were noticed on the boy on the 17th, and that the swelling of the glands occurred on the 19th?—Yes, there is a discrepancy between the result of his inquiry, and the result of ours.

26,998. Were the symptoms of the boy Cotta such as one finds in plague? Dr. Dyson, for instance, says that his appetite was good, his tongue was clean after nearly a month's illness, that he never suffered from frontal headache, delirium, high fever, hæmorrhage, or epistaxis, or injection of the conjunctiva, that the enlarged glands have never caused severe pain, and, in

* *See* "Account of Plague Administration in Bombay Presidency; from September 1896 till May 1897," prepared by Mr. E. Couchman, I.C.S., page 244.

† *See* Report of the Medical Officer to the Local Government Board, for 1879, p. 49.

‡ *See* Tholozan, "Comptes Rendus," 1877, p. 432.

fact, that he has had none of the classical symptoms of plague. Do you admit that description of the symptoms as given by Dr. Dyson?—Hardly. I think there are varieties.

26,999. Are these symptoms of the boy Cotta correctly described?—Yes.

27,000. Are these symptoms compatible with his having had plague?—Oh, yes, I think so. For that reason I would mention what is stated by Dr. James in his report on the Hong Kong epidemic, which appears in the Army Medical Department Report for 1893. In Appendix No. 5, he states, on page 331, that the disease is endemic in the province of Yunan, and cases occur every year, those attacked having buboes, but very little constitutional disturbance, and all recovering. On page 335, he mentions that some of the soldiers employed in the working parties became affected, and some who were not so employed, and an officer, Captain Vesey, of the Shropshire Regiment, was taken ill early in June of plague and succumbed. On p. 348 he states that in abortive plague the bubo occurs without fever. Similar cases to those in Yunan occurred in Mesopotamia. I think it was Dr. Tholozan, in his work on plague, who mentions that from epidemic to epidemic the bridge is filled up by these mild cases of plague, which produce very little constitutional disturbance. Dr. Dickson, at page 55 of the Report of the Medical Officer for 1879 to the Local Government Board, describes the prevalence of *glandular swellings free from fever* two or three months previous to fatal plague in Hillah and Bagdad, and on the cessation of plague *apyretic glandular swellings* reappeared again for two months longer.

27,001. Can you give us the authorities for the statement about Yunan?—Major James mentions it in his report on Hong Kong.

27,002. Do you know who was his original authority?—No. Possibly Dr. Tholozan's work would give some of these mild cases of plague.

27,003. (*The President.*) Do you know the title of that?—May I be allowed to send the reference to you. (Note by witness on correcting proof of his evidence: "La Peste en Turquie dans les temps mo- "dernes, sa prophylaxie defectueuse, sa limitation "spontanée.")

27,004. (*Mr. Cumine.*) In the case of the boy Tincauri Pal, was there any history that before his becoming ill on this occasion he had now and then suffered from fever and cold and swelling of the inguinal glands?—I did not discover a history of that kind.

27,005. With regard to these climatic buboes, that is to say, buboes such as were seen in the Shropshire Regiment, have they been noticed to any large extent and for a long time on the East Indian station, and to a lesser extent on the Mediterranean and China station?—Yes. It is stated that cases like that have been reported. I remember that Surgeon Godding, of the *Centurion*, wrote a paper* in the *British Medical Journal* on climatic buboes, and gives instances. The peculiarity of his instances was the fact that they were seamen who were stationed at Hong Kong at the time of the plague.

27,006. He says: "I have myself seen the worst "cases on the East Indian station at Zanzibar and "adjacent coasts, the patients having huge masses of "glands in the groin often suppurating, producing "anæmia and debility"?—That is among the seamen at Zanzibar. Many of the ships that go to Zanzibar, go also up the Persian Gulf, and have communication with those places where, I believe, these buboes occur frequently. I was speaking to a medical man who had been in Bagdad for a number of years, and he stated that every year there were a few cases of glandular swellings.

27,007. (*The President.*) They were supposed to be cases of mitigated plague?—Yes, so that I do not think much importance can be attached to these cases being affected at Zanzibar, considering that those affected most likely visited the region where cases occur.

27,008. (*Mr. Cumine.*) Does he not say they occurred also, though less frequently, in the Mediterranean, Australia, and the West Coast of Africa?—I think it is very likely that they would occur, though possibly not the same disease as the glandular swelling in plague infected areas. The difficulty is to differentiate the one from the other, unless special attention is given to each, and the several likely causes investigated.

27,009. (*The President.*) I gather that your chief reason for believing these cases to be a form of plague was the microscopical examination?—Partly microscopical and partly for other reasons. In Giga and Bepin, the symptoms are, to my mind, after my experience in Bombay and Poona, typical plague, such as may be seen anywhere in a large epidemic, and, similarly, though more obscure, are some of the others. The death of the rat with swollen glands in the house of Giga and similar micro-organisms being found in the child and rat strengthened this opinion, also the rats dying to such an extent in the grain depôts, and having very similar micro-organisms in their organs.

27,010. Your cases do not illustrate any power of infection at all?—I have not noted any.

27,011. You have made inquiries in that direction?—Yes.

27,012. You did not find it in any case?—No; they seemed to be dotted about. A similar discussion took place, I believe, with reference to the appearance of an illness with glandular enlargements when the Pali plague broke out in 1836. Doctor Forbes, who investigated the cases, came to the conclusion that they were a mild form of plague. It was stated at the time that cases of a similar nature were common and due to malaria, but his investigation indicated that the illness appeared some time before plague broke out, and the cases were connected with plague and not malaria. He made very particular inquiries of native medical men as to the prevalence of buboes after malarial fever, and they did not know of cases at that time; subsequently they met with them.

27,013. In regard to the cases in which cattle appeared to have suffered from something like plague, are you aware that in India, generally, cattle often live in the same houses as human beings?—Yes, that is so.

27,014. In many instances these houses contain plague-affected human beings?—Yes, in the hills.

27,015. Not only in the hills, but, in the villages in the plains?—Yes.

27,016. Do you know if anything similar to plague has occurred among these cattle in India in any city outside of Calcutta?—No, I have not read of any. I have watched the cases with that view, but nothing has come to my notice. Dr. Weir records that there was a great mortality among cattle in Bombay in the middle of 1896.

27,017. Very virulent forms of plague have occurred in human beings in houses in which cattle have been kept?—Yes.

27,018. There appears to be no evidence that these cattle have ever taken the plague?—None has been collected.

* *See* App. No. XXI. in Vol. I. of these Proceedings.

(Witness withdrew.)

(Adjourned till Saturday, 20th May.)

At The India Office, Whitehall.

SEVENTY-SECOND DAY.

Saturday, 20th May, 1899.

PRESENT:

PROF. T. R. FRASER, M.D. LL.D., F.R.S. (*President.*)

Mr. J. P. HEWETT, C.I.E.
Professor A. M. WRIGHT, M.D.

Mr. A. CUMINE.

Mr. C. J. HALLIFAX (*Secretary*).

Captain H. D. MASON, R.A.M.C., called and examined.

Capt. H. D. Mason. R.A.M.C.

20 May 1899.

27,019. (*The President.*) What are your medical qualifications?—I am M.R.C.S.

27,020. What was the appointment which you held in India?—I was on plague duty under the Government of India, in the Cutch State.

27,021. You were in the Cutch State during the time of the plague epidemic?—Yes.

27,022. And you had an opportunity of seeing many cases treated by ordinary methods?—Yes.

27,023. And many also treated with serum?—Yes.

27,024. I think of each group you have tabulated 100 cases?—The cases given in my précis were one typical hundred of each—one hundred taken out of many. Each group of cases was taken under exactly the same conditions, 100 being treated by ordinary methods, and 100 with the serum antipesteux. Both series were taken between the first and the fourth day, and the two have been contrasted with each other.

27,025. These cases were not selected for any special reason?—Not the least in the world, they were treated exactly as they were admitted.

27,026. And the gravity of the illness?—Had nothing whatever to do with it. In some cases we did make a distinction at first. Those who died in the first 24 hours were eliminated. In these lists we thought that the cases which died in the first 24 hours should be included, and that has been done.

27,027. You have included them all in your tables?—Yes.

27,028. Will you put the tables in?—Yes, they are as follows:—

ONE HUNDRED CASES of PLAGUE TREATED by ORDINARY METHODS in the BRAHMAPURI HOSPITAL, CUTCH MANDVI.

Cases admitted between first and fourth days (four after or unknown).

No.	Name.	Sex.	Age.	How long Ill.	Admitted to Hospital.	Buboes.	Result.
					1897.		
1	Mohanji	Male	30	2 days	25th May	Nil	Died.
2	Lalji	,,	27	Very old case	,,	,,	,,
3	Cooverji	,,	35	4 days	,,	Left groin	,,
4	Khimji	,,	50	2 ,,	,,	Right groin	,,
5	Shambai	Female	18	2 ,,	,,	Nil	,,
6	Panabai	,,	18	Not known	,,	,,	,,
7	Vishnubai	,,	50	3 days	,,	,,	,,
8	Moonjibai	,,	30	Nil	,,	,,	,,
9	Premjibai	,,	18	,,	,,	,,	,,
10	Ratanshi	Male	20	2 days	,,	,,	,,
11	Soonderbai	Female	70	2 ,,	,,	,,	,,
12	Dhanbai	,,	32	2 ,,	,,	Left axilla	,,
13	Nanbai	,,	30	3 ,,	26th May	,, groin	,,
14	Dharamshi	Male	70	3 ,,	,,	Nil	,,
15	Sambai	Female	35	4 ,,	,,	,,	,,
16	Dahya	Male	25	3 ,,	,,	,,	,,
17	Harjiwan	,,	55	3 ,,	,,	,,	,,
18	Pritambai	Female	60	3 ,,	,,	,,	,,
19	Kashibai	,,	23	2 ,,	,,	Left groin	Discharged.
20	Gangabai	,,	40	3 ,,	,,	,, axilla	Died.
21	Joshi Manji	Male	50	2 ,,	,,	,, ,,	,,
22	Gombai	Female	10	2 ,,	,,	,, parotid	Discharged.
23	Poonja	Male	85	3 ,,	,,		Died.
24	Sungsi	,,	20	2 ,,	,,	Left axilla	,,
25	Velji	,,	80	4 ,,	,,	Right axilla	,,
26	Lakanbai	Female	32	3 ,,	,,		,,
27	Buddibai	,,	20	2 ,,	,,	Right inguinal	Discharged.
28	Lalji	Male	19	3 ,,	,,		,,
29	Govan Khimji	,,	60	2 ,,	27th May	Left groin, small	Died.
30	Meghji Premji	,,	30	2 ,,	,,	Right axilla	,,
31	Premabai	Female	22	4 ,,	,,	,, groin	,,
32	Lohsajbai	,,	16	3 ,,	,,	Nil	,,
33	Velbai Rambai	,,	13	2 ,,	,,	Right axilla	Discharged
34	Vhalibai	,,	14	2 ,,	,,	,, parotid	Died
35	Mithoobai	,,	30	2 ,,	,,	Nil	,,
36	Pragji	Male	10	2 ,,	,,	Right groin	,,

No.	Name.	Sex.	Age.	How long Ill.	Admitted to Hospital.	Buboes.	Result.
					1897.		
37	Mooli	Female	15	4 days	27th May	Right groin	Discharged.
38	Meethi	„	20	4 „	„	„ axilla	Died.
39	Painbai	„	50	2 „	„	Left parotid	„
40	Velibai	„	38	1 day	„	Right groin	Discharged.
41	Gomtibai	„	40	2 days	28th May	„ „	Died.
42	Vasanji	Male	80	2 „	„	„ axilla	„
43	Manekbai	Female	25	2 „	„	„ groin	„
44	Ramanbai	„	30	2 „	„	„ „	„
45	Hiroobai	„	6	2 „	„	„ „	„
46	Nanji	Male	6	2 „	„	„ „	„
47	Dharsi	„	14	6 „	„	Pneumonia	„
48	Manbai	Female	4	2 „	„	Right groin	„
49	Veljji	Male	10	3 „	„	-	„
50	Panoo	„	25	4 „	„	-	„
51	Jetha	„	35	2 „	„	-	„
52	Virbai	Female	16	1 day	„	Left groin	„
53	Gomatibai	„	50	1 „	„	Nil	„
54	Damodar	Male	11	4 days	„	Right axilla	„
55	Loolibai	„	35	1 day	„	„ groin	Discharged.
56	Naran	„	45	2 days	„	Nil	Died.
57	Nathibai	Female	30	2 „	„	Right axilla	„
58	Charupanbai	„	25	4 „	„	„ inguinal	Discharged.
59	Khimji	Male	60	4 „	„	„ groin	Died.
60	Hansibai	Female	18	6 „	„	-	„
61	Kalianji	Male	21	3 „	„	Left groin	„
62	Panchibai	Female	12	1 day	„	„ „	„
63	Gangabai	„	25	2 days	„	Right groin	Discharged.
64	Kanji	Male	10	1 day	29th May	„ axilla	Died.
65	Pralimanbai	Female	5	4 days	„	„ groin	„
66	Ramdass	Male	35	1 day	„	Maxillary	„
67	Parvatibai	Female	30	1 „	„	Left groin	„
68	Kanji	Male	13	1 „	„	Right „	„
69	Wandubai	Female	20	2 days	„	Left „	„
70	Manekbai	„	8	4 „	„	Right „	„
71	Champsi	Male	22	3 „	„	„ „	„
72	Devkorebai	Female	15	2 „	„	Left „	„
73	Dhyanbai	„	28	3 „	„	-	„
74	Virji	Male	9	3 „	„	Right cervical	„
75	Manjibai	Female	20	1 day	„	Left groin	„
76	Kalidas	Male	35	2 days	30th May	-	„
77	Amratbai	Female	50	4 „	„	Right and left groins	Discharged.
78	Gangabai	„	—	1 day	„	Nil	Died.
79	Ratanbai	„	42	2 days	„	-	„
80	Nursey Jiva	Male	9	4 „	„	Left groin, left axilla, œdema of left foot and hand.	Discharged.
81	Cooverbai	Female	20	1 day	„	Left groin	Died.
82	Motibai	„	7	4 days	„	Tenderness, left groin	„
83	Ladibai	„	40	2 „	„	Left groin	Discharged.
84	Manekbai	„	48	3 „	„	„ axilla	Died.
85	Ramji	Male	41	2 „	„	„ groin	Discharged.
86	Govindji	„	40	1 day	„	„ „	Died.
87	Samji	„	40	2 days	31st May	„ „	„
88	Moolibai	Female	50	2 „	„	Nil	„
89	Hansraj	Male	25	1 day	„	Left groin	„
90	Lakhmibai	Female	14	2 days	„	„ axilla	„
91	Motio	Male	6	2 „	„	„ cervical	Discharged.
92	Ramji	„	8	1 day	„	„ inguinal	„
93	Sivji	„	18	1 „	„	„ „	Died.
94	Motibai	Female	35	3 days	„	„ groin	„
95	Lilbai	„	20	1 day	„	Right „	„
96	Panbai	„	40	3 days	1st June	-	„
97	Kara	Male	20	3 „	„	-	„
98	Kamabai	Female	70	2 „	„	Right cervical	Discharged.
99	Hirabai	„	25	3 „	„	„ „	Died.
100	Hadhubai	„	35	2 „	2nd June	„ groin	„

In cases treated by ordinary methods the prognosis is better in children than men and in men than women.

Of 260 deaths in the Brahmapuri Hospital (Hindus) there were :—

Children under 10	25
Males	89
Females	146
Total	260

Whilst in 3,516 deaths from all hospitals, there were :—

Children	563
Males	1,226
Females	1,727
Total	3,516

The healthy and robust appear not only prone to contract the disease, but to suffer from the most virulent forms, and die in as great a proportion as those of weaklier frame and less sthenic constitution.

The epidemic at Mandvi was at its height in May and June. The non-inoculated cases were collected in May and June. The inoculated cases treated in the Mandvi Brahmapuri Hospital in May and June are comparable.

The epidemic at Gundiali town was at its height in August and September. The inoculated cases treated there during that time are comparable to the non-inoculated cases treated in the Brahmapuri Hospital in Mandvi when the epidemic was at its height there. The same applies to Merau village.

The cases treated in the various hospitals are bracketed.

Capt. H. D. Mason, R.A.M.C.

20 May 1899.

CASES of PLAGUE treated with SERUM ANTIPESTEUX in CUTCH MANDVI.

	No.	Name.	Sex.	Age.	Admitted to Hospital.	How long ill when injected.	Total Quantity Injected.	Result.	Bubo.	Remarks.
Brahmapuri Mandvi.					1897.			1897.		
	1	Nursey Jiva	M.	9	30.5	4 days	90 c.c.	Discharged	Parotid, left groin, left cervical.	
	2	Jayanak	„	35	„	„	105 „	Died 4.6	Right axilla.	Not injected till 4th May.
	3	Ramaubai	F.	15	1.6	1 day	120 „	„ 3.6	„ „	
	4	Sivjee	M.	8	„	2 days	210 „	„ 11.6	Right inguinal and femoral.	
	5	Godavribai	F.	32	2.6	„	220 „	Discharged, 11.6	Right groin, plague pustule left foot.	
	6	Bhababai	„	13	„	4 „	190 „	Died 6.6	Left femoral, right and left feet, swollen right and left cervical.	
	7	Hirjee	M.	8	„	1 day	50 „	„ 4.6	Left axilla.	
	8	Matoobai	F.	40	3.6	2 days	140 „	„ 5.6	Left femoral.	
	9	Rattanbai	„	45	4.6	„	170 „	„ 11.6	Right groin.	
	10	Rattanbai	„	29	„	„	200 „	„ 23.6	Left femoral (pneumonia).	
	11	Godavribai	„	30	„	1 day	160 „	Discharged, 11.6	Left groin.	
	12	Kankoobai	„	20	„	„	100 „	„	Nil.	
	13	Mambai	„	40	„	„	140 „	Died 9.6	Left femoral.	
	14	Karoo	M.	40	„	2 days	230 „	„ 8.6	Nil.	
	15	Dhaneshwar	„	45	5.6	„	170 „	„ 9.6	Right inguinal, suppression of urine.	
	16	Sambai	F.	28	6.6	„	230 „	„ 13.6	Right groin (pneumonia).	
	17	Janki	„	25	7.6	3 „	140 „	„ 11.6	Left groin.	
	18	Bambabai	„	60	9.6	„	120 „	„ 12.6	Right axilla.	
	19	Sumar (Khoja Hospital).	M.	60	„	4 „	100 „	Discharged, 19·6	Left groin.	
	20	Rattanbai	F.	21	11.6	3 „	40 „	„	Right groin.	
	21	Rasambai	„	30	15.6	„	90 „	„	—	
	22	Padmabai	„	10	„	„	100 „	„	—	
	23	Jane Pereira	„	33	„	1 day	150 „	Died	—	
	24	Koomabai	„	45	22.6	2 days	120 „	„	Left submaxillary.	
	25	Aboo	M.	55	23.6	1 day	150 „	„	Right groin.	
	26	Samji	„	50	„	1 „	90 „	Discharged	Left axilla.	
	27	Hazarabai	F.	18	19.6	2 days	110 „	„	Left groin.	
	28	Havabai	„	17	„	„	10 „	„	„ „	Severe central intoxication. Bilious vomiting, sputa typical.
	29	Monji Jiva	M.	20	3.7	„	80 „	Died	„ „	
	30	Devkanbai	F.	18	5.7	„	75 „	„	Right axilla.	
	31	Manekbai	„	12	„	„	100 „	„	Tenderness both groins.	
	32	Setambai	„	105	14.7	4 days	70 „	„	Left groin.	
	33	Sackerbai	F.	7	4.8	1 day	45 „	Discharged, 6.8	Nil	Swelling and tenderness in right groin.
	34	Bachibai	„	10	„	„	50 „	„ 6.8	„	
	35	Daibai	„	40	6.8	„	100 „	Died 8.8	„	
	36	Ghelo Ajramal	M.	11	„	„	30 „	„ 7.8	„	Passed round worms.
	37	Kalimanbai	F.	11	„	„	120 „	„ 9.8	Left inguinal.	
	38	Rhamabai	„	25	4.8	4 days	95 „	Discharged, 13.8	Right axilla.	
	39	Manabia	„	7	10.8	3 „	70 „	„ 13.8	Left inguinal.	
	40	Monji Kersondas	M.	14	16.8	1 day	80 „	Died 18.8	Nil.	
	41	Mena Tello	F.	11	„	„	100 „	„ 13.8	Left and right groin, right axilla.	
	42	Koorbai Vello	„	9	„	„	30 „	„ 16.8	Right groin.	
	43	Menbai Feloo	„	—	17.8	2 days	110 „	„ 18.8	Nil	Pemphigoid rash on face and back.
	44	Velji Ladha	M.	30	19.8	„	130 „	„ 22.8	Left inguinal.	
	45	Dayal Chamsay	„	50	„	3 days	180 „	„ 24.8	Right groin.	
	46	Harbai Vello	F.	—	17.8	2 „	80 „	„ 20.8	„ „	
	47	Dosa Vishram	M.	28	20.8	„	130 „	„ 24.8	Left inguinal.	
	48	Derkanbai Dhauso.	F.	9	17.8	3 days	100 „	„ 22.8	Right axilla, right cervical, left cervical, left axilla, right and left inguinal.	
	49	Nursee Osmal	M.	—	18.8	2 „	140 „	„ 21.8	Right groin.	
	50	Premji	„	14	13.8	1 day	120 „	Discharged, 30.8	Right axilla.	
	51	Lakhmibai	F.	10	15.8	—	100 „	„ 2.9	Left groin.	
	52	Raoji Jivan	M.	11	10.8	—	100 „	„ 30.8	Right and left groin.	
	53	Soonderji	„	—	15.8	—	130 „	Died 19.8	Left axilla and right groin.	
	54	Gangabai	F.	22	16.8	4 days	160 „	„ 28.8	Nil	Muscular rheumatism, secondary pneumonia.

	No.	Name.	Sex.	Age.	Admitted to Hospital.	How long ill when injected.	Total Quantity injected.	Result.	Bubo.	Remarks.
					1897.			1897.		
Gutdiala.	55	Nagai Bharu	F.	10	17.8	2 days	130 c.c.	Discharged, 2.9	Right axilla, right inguinal.	Secondary, right leg. and synovicle.
	56	Kanji Devji	M.	7	19.8	3 „	100 „	„ 2.9	Left groin and left cervical.	Secondary.
	57	Bhanji Jiva	„	12	20.8	2 „	100 „	Died 25.8	Left inguinal.	
	58	Hila Mona	„	9	23.8	1 day	120 „	Discharged, 2.9	Left cervical and tenderness, both groins.	
	59	Jethibai Raoji	F.	9	24.8	3 days	100 „	„ 1.9	Right inguinal	Absorbed.
	60	Kanji Monji	M.	4	26·8	2 „	40 „	„ 2.9	Nil.	
	61	Korsibai	F.	13	„	„	100 „	„ 2.9	Left axilla.	
	62	Monbai	„	40	„	„	120 „	„ 2.9	Right inguinal.	
	63	Velo Versi	M.	7	28.8	„	50 „	„ 2.9	Right femoral.	
	64	Vejabai Serang	F.	40	„	„	130 „	Died 31.8	Right inguinal.	
	65	Doongarsi	M.	12	28.8	2 days	45 „	„ 30.8	Left groin.	
	66	Noorbai Jakoo	F.	—	17.8	—	120 „	Discharged 27.8	„ „	
	67	Lakhabai	„	—	—	1 day	130 „	„ 28.8	Right groin.	
	68	Valji Monsee	M.	12	18.8	2 days	100 „	„ 5.9	Left axilla.	
	69	Kamilia Baunuhall.	F.	—	25.8	1 day	160 „	Died 29.8	Nil.	
	70	Mensee Lakha	M.	—	28.8	3 days	160 „	„ 29.8	„	
	71	Menbai Koorarji	F.	20	27.8	2 „	120 „	„ 29.8	Tenderness, right groin.	
	72	Ladoo Passor	„	12	28.8	2 „	30 „	„ 30.8	Right axilla, with effusion.	
	73	Villa Khoda	M.	35	„	2 „	160 „	„ 2.9	Nil.	
	74	Khetbai Napoo	F.	45	„	2 „	160 „	„ 6.9	Left groin, L.	Popliteal bubo, acute thrombosis of left femoral and popliteal and posterior of tibial.
	75	Devrajookede	M.	40	2.9	2 days	150 „	„ 6.9	Left groin.	
	76	Rhanabai	F.	10	6.9	6 „	150 „	„ 6.9	Primary plague pneumonia.	
	77	Monabai Lalji	„	24	2.9	4 „	120 „	Discharged, 14.9	Effusion, right axilla.	
	78	Demotbai	„	—	„	2 „	180 „	„ 13.9	Nil.	
	79	Mamibai	„	8	„	3 „	60 „	„ 14.9	Left inguinal.	
	80	Premabai Jerat	„	60	5.9	2 „	160 „	„ 10.9	Nil.	
	81	Rhanbai Amod	„	40	„	3 „	150 „	„ 15.9	Left groin.	
	82	Haji Hoosain	M.	50	„	2 „	220 „	Died 10.9	Right axilla.	
	83	Sonabia Joose	F.	25	„	3 „	150 „	„ 8.9	Nil	Primary plague pneumonia.
	84	Poonjibai Jeraij	„	18	6.9	1 day	130 „	„ 8.9	Left Inguinal.	
	85	Virbai Dongarsi	„	14	7.9	1 „	60 „	„ 11.9	Right groin	Left sub-maxillary and popliteal bubo developed 9.9.97.
	86	Ratibai Dersi	„	10	8.9	2 days	45 „	Discharged, 17.9	Sub-clavicular	Bubo, left side, developed.
	87	Phaphoobai Joones.	„	19	„	3 „	90 „	Died 9.9	Left groin.	Eyes injected.
	88	Panchabai Jirat	„	30	„	2 „	110 „	„ 13.9	Left axilla.	
	89	Ajbai Raghaiji	„	40	2.9	3 „	120 „	„ 8.9	Left inguinal	Primary plague pneumonia.
Gundiali.Meran.	90	Sagiban	„	—	5.9	1 day	90 „	Discharged, 18.9	Left groin.	
	91	Manbai Harangar.	„	35	„	2 days	140 „	„ 9.9	Right inguinal.	
	92	Manabai Ragoji	„	9	6.9	2 „	60 „	Died 9.9	Right axilla	With effusion.
	93	Logakhelley	M.	75	„	2 „	160 „	„ 8.9	Nil.	
Meran.	94	Bhanji	„	25	„	2 „	150 „	„ 9.9	„	
	95	Malibai	F.	50	7.9	2 „	120 „	„ 9.9	Right groin.	
	96	Pababai	„	24	8.9	1 day	170 „	„ 12.9	Glands parotid, left.	
	97	Mansi	M.	30	11.9	3 days	80 „	Discharged, 20.9	Left groin.	
Gundiali.	98	Monji Morarji	„	12	4.9	2 „	100 „	„ 23.9	Right cervical, right inguinal.	Right and left groins developed, 9.9.97.
	99	Gangbeir	F.	16	5·9	2 „	150 „	„ 23.9	Right inguinal.	
	100	Meghaibai Hunsary.	„	25	10.9	2 „	120 „	„ 21.9	Right Abdominal.	

27,029. Will you briefly state the results?—In contrasting the cases treated by serum and those treated by ordinary methods it will be seen that out of 100 cases of the former 59 died and 41 recovered; of the latter 83 died and 17 recovered.

Capt. H. D. Mason, R.A.M.C.

20 May 1899.

27,030. What is your opinion as to the value of the serum antipesteux which you used?—I think it is of the greatest value in all except the very gravest cases. On those, apparently, it had very little effect—those who came with the most marked symptoms of plague, marked delirium, high temperature, continuous vomiting, &c. On those, as I say, it seemed to have little or no effect, but in the milder cases, undoubtedly, it produced a favourable effect.

27,031. As contrasted with the ordinary treatment?—Yes, I do not think the ordinary methods had any effect at all—not the slightest effect in the world.

27,032. Your percentage of recoveries is considerably better with serum than without it?—Yes.

27,033. What serum did you employ?—The serum prepared by Dr. Roux, of the Pasteur Institute.

27,034. Could you state generally what were the doses that you used?—They varied. Of course, with us it was quite an experiment. It had never been used before, except a little in Bombay, and when it came to us we used it in small doses at first—10 c.c. and 20 c.c.—but afterwards in very much larger doses, up to 60 c.c.

27,035. Are you able to contrast the effects of the smaller doses with those of the larger doses?—It is quite possible that I could, but I cannot from these tables. I had every case which I treated entered in books, but without going through these books I do not think I can contrast the effect of the smaller doses with the larger. But, roughly, I should say that the larger doses were more successful than the smaller ones, because it was that fact which led us to use the larger doses.

27,036. The smaller dose was not so satisfactory as the larger one?—No.

27,037. Perhaps, as you have the data, you could put them into some statistical form for us?—I will endeavour to do so when I go back.

27,038. We want you to contrast as nearly as you can the larger with the smaller doses?—The following figures give the required comparison :—

	Cases.	Deaths.	Recoveries.
Small doses (60 c.c. and under) -	14	7	7
Large doses (over 60 c.c.) - -	86	52	34

In the recrudescence I have a certain number of cases which were treated by very large doses in certain caste hospitals, and those were very curious. Amongst the Muhammadans 9 out of 18 recovered, but among the Bhatias we only injected the serum in six cases, and every one of them died.

27,039. Were the latter treated by larger doses?—Yes.

27,040. The result was not altogether satisfactory even with the larger doses?—It was among the Muhammadan caste, but not amongst the Bhatias.

27,041. Have you any explanation of that?—Nothing more than the general explanation, that the Hindus are a much feebler class of men; they are vegetable eaters. The Muhammadans have much greater vitality and finer physique altogether. Whether that is any explanation or not I am not prepared to say.

27,042. On the whole, the doses with which you treated these several groups were much the same?—Yes. You will see in the following table, of the 18 Muhammadans inoculated 9 recovered, and that every one of the Bhatias died. The following is the table I refer to :—

Cases inoculated with Serum Antipesteux at Caste Hospitals during Recrudescence of 1898.

Name of Hospital.	Admissions.	Inoculated.	Discharged.	Died.
General - - -	—	35	10	25
Banniah - - -	—	1	0	1
Bhatia - - -	—	6	0	6
Muhammadan - -	—	18	9	9

I remember distinctly that all these cases were very serious indeed, and which would certainly not have got well with any ordinary treatment.

27,043. Do you mean in the four groups?—No, in the Bhatia group.

27,044. Then it would be a mere question of individual resistance to the severity of the illness?—Perhaps so. The reason I remembered these cases was that we had great difficulty in getting the Bhatias to be inoculated. We were very confident of the result after inoculating the Mahommedans, and we did what we could to get the Bhatias inoculated. But, unfortunately, every one of them died, and, therefore, we did not think it wise to go on after that on account of the feeling it produced in the caste.

27,045. At what stage in the illness did you commence the treatment?—As soon as ever we could get them. The earlier the inoculations the better the results.

27,046. You commenced between the first and the fourth day?—Yes, after the fourth day it was hopeless. The patient either got well in that time or it was hopeless.

27,047. How many doses did you inject in each case?—They also varied according to the results of the temperature charts. The temperature charts were all taken, and with a big fall the indication was not to continue the treatment, but directly the temperature began to rise we gave another large dose. As soon as the temperature went up we gave three or four inoculations.

27,048. Taking the cases generally, how many injections did you make?—That I cannot tell you without the temperature charts. I much regret that I did not bring them with me.

27,049. Perhaps you will send them?—Yes.* You will see that on each fall of the temperature the amount of the injection is given above the fall, that is to say, if the temperature went up to 104°, on the top of the 104° the amount of the inoculation would be registered, and if it came down again—wherever it was given on the temperature chart—the amount was registered on it.

27,050. What was the most conspicuous beneficial effect which you observed?—The fall in the temperature in the first place, and the general condition altogether. For instance, the patient who came in delirious became calmer, the temperature fell, the vomiting stopped, and there was general amelioration of the condition.

27,051. Were those good results obtained in cases which afterwards died as well as in cases which recovered?—Yes.

27,052. It was an almost invariable consequence of the injection of the serum that the temperature fell?—No, I could not go so far as to say that. I had cases which were treated with ordinary methods where the temperature also fell. It is certain that in ordinary cases the temperature falls under the action of the first injection, and it is equally certain in many cases it never re-ascends. This fall in temperature is obtained whatever the day of the disease on which the treatment is commenced. The most characteristic signs of the beneficial influence exerted by the serum are: fall in temperature; amelioration of the general condition; diminution of stupor and improvement in prostration. The improvement in the general condition, like the fall of temperature, is, in the majority of the gravest cases, apt to be transitory; the symptoms again become serious, and the disease appears to run its course as if the serum had not been given.

27,053. Did you find that the beneficial effects were most likely to be obtained if you commenced the treatment early?—Yes, the earlier the cases are treated the more is the intensity of the symptoms and the progress of the disease diminished. In cases which appear certain to die, the course of the disease seems to be stayed, and even if recovery does not take place, it nevertheless provides a most powerful resistance to the disease, and one which may last a considerable time. That remark was made on account of a very serious case which came in and which seemed to be kept alive by the administration of serum, and which eventually died. That was one case among others.

* *See* App. No. LXXXV. in this Volume.

27,054. What is your general conclusion?—I submit at if the treatment by serum is ineffective in a cer-in number of the worst cases it has an enormous ower over those where the toxine poisoning is not of e gravest, and where the disease does not run its ost rapid course. In these cases we are bound to mit that the recovery is due to the serum.

27,055. Is it your opinion that if you could have ven larger doses the results would have been still ore satisfactory, or do you think you attained the aximum dose which it was requisite to administer?— at is a difficult question to answer. The reason we d not give any larger doses was because in certain ses a kind of synovitis developed in various joints— the elbows and wrists, and it was doubtful whether is synovitis was part of the plague itself or whether was produced by the large doses of serum adminis-red. That is why the doses were not increased.

27,056. Did these large doses have much effect on e heart so far as you observed?—No, I have no servation to that effect.

27,057. You have taken the pulse rate?—Yes, they e all recorded—the pulse, the temperature, and spirations.

27,058. These do not show that any effect was pro-uced by the large doses of serum?—As far as I member, no.

27,059. Was there much local effect produced at the at of injection?—None whatever.

27,060. Even by the maximum dose?—None what-ver.

27,061. What was the maximum single dose?—I went p to 60 c.c.

27,062. Even 60 c.c. produced no local disorder?— ot the slightest.

27,063. No complaint of pain?—There was a certain mount of pain, of course.

27,064. The swelling subsided spontaneously and pidly?—Yes.

27,065. I see you have also made some experiments n animals. That is with the same serum, I presume?—Yes.

27,066. What animals did you use?—Mice, rats, nd two monkeys.

27,067. Will you put those experiments in?—Yes.

EXPERIMENTS ON ANIMALS made with SERUM ANTIPESTEUX (No. 31).

(I.)

Mouse No. 1.—Inoculated with plague culture under ot of tail, 5 minutes after received $\frac{1}{50}$ c.c. serum.— esult, died in 3 days.

Mouse No. 2.—Inoculated with plague culture, 5 inutes after received $\frac{1}{40}$ c.c.—Recovered.

Mouse No. 3.—Inoculated in same manner, 20 hours fter received $\frac{1}{10}$ c.c.—Died in 90 hours.

Mouse No. 4.—Inoculated in same manner, 20 hours fter received $\frac{1}{3}$ c.c.—Recovered.

Mouse No. 5.—Inoculated in same manner.—Received o serum, died in 42 hours.

(II.)

Mouse No. 1.—Inoculated with blood of a rat dead of lague, 20 hours after received $\frac{1}{4}$ c.c.—Died in 7 days. Neither examination of organs nor cultures showed lague bacillus, nevertheless there was reason to believe eath due to plague.

Mouse No. 2.—Inoculated in same manner. Received o serum.—Died in 60 hours.

(III.)

Mouse No. 1.—Inoculated with lymph from a human lague pustule containing enormous numbers of acilli, 24 hours after received $\frac{1}{3}$ c.c. serum.—Recovered.

Mouse No. 2.—Inoculated with same lymph. Re-eived no serum.—Died in 76 hours.

SERIES made with SERUM (No. 88).

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(I.)

Mouse No. 1.—Inoculated with culture from human bubo, 20 hours after received $\frac{1}{4}$ c.c.—Recovered.

Mouse No. 2.—Inoculated in same manner. Received no serum.—Died in 72 hours.

(II.)

Rat No. 1.—Inoculated by subcutaneous injection with $\frac{1}{20}$ c.c. of the blood from the heart of a mouse dead of plague, 24 hours after received 2 c.c. serum.—Recovered.

Rat No. 2.—Inoculated in same manner. Received 36 hours after 2 c.c. serum.—Died in 96 hours.

Rat No. 3.—Inoculated in same manner. Received no serum.—Died in 71 hours.

(III.)

Monkey No. 1.—Received by subcutaneous injection the half of a mixture made by crushing up in 2 c.c. of sterile water the spleen, heart, and liver of a mouse which had died of very virulent plague, into the right thigh:—

After 12 hours.		An enormous femoral bubo appeared.
,,	18 ,,	High fever, walked with difficulty, and refused food.
,,	24 ,,	Received 2 c.c. serum.
,,	49 ,,	Not much change, difficulty in walking increased.
,,	,, ,,	Second injection of 20 c.c. serum.
,,	66 ,,	General improvement.

On the 4th day began to eat again, bubo increased in size, commencing to break down.—Complete recovery, 10th day.

Monkey No. 2.—Received same quantity of same preparation at same time:—

After 12 hours.		Bubo in right thigh.
,,	24 ,,	Fever less marked than No. 1.
,,	36 ,,	Difficulty of walking increased.
,,	48 ,,	Could scarcely move.
,,	66 ,,	Died.

27,068. In the first group of experiments you gave the serum first?—Yes.

27,069. How many experiments did you make?—There were five.

27,070. How many died, and how many recovered?—Two of them died, and three recovered.

27,071. Did you have any control experiments to show the quantity of the plague virus which would probably have killed each of these animals, without the serum?—Yes; Nos. I 5, II 2, III 2.

27,072. Did you satisfy yourself in each of these cases that a lethal dose of plague virus had been administered?—Yes, certainly.

27,073. Did you give much above the minimum lethal dose?—That I cannot say.

27,074. You have no idea what the minimum lethal dose of the virus may be?—No.

27,075. Then how can you say that it was above the minimum lethal dose?—It was sufficient to kill those that received no serum.

27,076. It might be that some of these recoveries were not due to the serum at all, but to the fact that the dose had not been a lethal dose?—Not probable.

27,077. It seems a fundamental requirement that that should be shown?—Yes, it does.

27,078. The next series you made was where the serum followed the administration of the virus?—Yes.

27,079. They were actually therapeutic experiments, I think; that is to say, the treatment followed the reception of the virus?—Yes.

27,080. Did you make many of that description?—No, not many.

27,081. How many did you make in the case of mice?—Three.

27,082. What were the results of these experiments?—The greatest result was that the administration of the

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serum either prolonged the existence of the animals, or that they recovered.

27,083. The first two died?—Yes.

27,084. Only one recovered?—Yes.

27,085. You do not state how much of the virus you gave in these cases?—No.

27,086. You did make an attempt, no doubt, to give a similar quantity. How did you weigh the virus? How much did you give of the culture, for example?—At the time these were done, we had no notion what the lethal dose was by actual measurement.

27,087. I thought that for any given virus you might have roughly ascertained what the lethal dose was. You did not do that?—No.

27,088. In the next series, you made some experiments on rats; three experiments?—Yes.

27,089. There were actually two experiments with serum?—Yes.

27,090. One died, and one recovered?—Yes.

27,091. You state that one-twentieth of a c.c. of blood was used in the first instance?—Yes.

27,092. How much did you use in the second instance? Was it done in the same manner, with the same quantity?—Yes.

27,093. And the same blood?—Not exactly the same manner.

27,094. But was it exactly the same blood as the first of these experiments?—Yes.

27,095. And the same dose?—Yes.

27,096. Perhaps you had better put that more definitely when you correct your evidence. The "same manner" does not quite cover the whole of the conditions?—I will do so. (Note by witness on correcting proof of his evidence:—All three rats were inoculated by subcutaneous injection with one-twentieth c.c. of the blood from the heart of a mouse died of plague.
No. 1 received 2 c.c. serum 24 hours after inoculation.
No. 2 „ „ „ 36 „ „ „
No. 3 „ no serum).

27,097. In the third experiment, I presume you gave the same blood?—The same dose and the same blood.

27,098. And the animal died?—Yes.

27,099. How do you explain the death in the one instance, and the recovery in the other, if the conditions of the experiments were absolutely the same?—Because one of them received the injection 24 hours afterwards, and the other 36 hours afterwards.

27,100. That certainly makes a considerable difference?—Yes.

27,101. They were not exactly comparable?—The time in which the one received the serum was earlier than the other.

27,102. How many experiments on monkeys did you make?—Only two.

27,103. What were the experiments?—The mixture was divided into two, and an equal part was given underneath the thigh. The mixture was made by crushing up the spleen, heart, and liver, of a mouse which died of virulent plague.

27,104. By subcutaneous injection?—Yes, injected into the thigh.

27,105. In the first experiment, you gave 2 c.c. of serum 24 hours afterwards?—Yes.

27,106. What was the result?—The results are all stated in the details which I have put in, and the symptoms which were produced on each inoculation are given.

27,107. In the second experiment the monkey received the same dose of virus?—Yes, without any serum at all; he got gradually worse.

27,108. The monkey died in 66 hours?—Yes.

27,109. What were the weights of these monkeys?—I cannot remember that, they were the ordinary "Guzarati" monkey.

27,110. They did not differ much in weight?—No, they are both exactly the same size.

27,111. You think they were about the same size?—Yes.

27,112. That question of weight would make a considerable difference. If there was much difference in the weight of the animals, the results can hardly be compared?—The monkeys were not weighed, but they were of the same class.

27,113. You know, roughly, they were about the same weight?—Yes.

27,114. Therefore, they are probably comparable?—Yes.

27,115. So far as they go, these experiments seem to indicate some curative power?—Yes.

27,116. You gave, in all, 22 c.c. of serum?—Yes, a small dose at first, and then a big one when he began to get worse.

27,117. You have had some opportunities of observing the effects of segregation, I believe?—Yes.

27,118. Will you describe what the segregation consisted of?—All the ports of Cutch were closed to passengers, except Cutch Mandvi and Khari-Rohar. At Mandvi no steamer was allowed to disembark between 6 p.m. and 6 a.m. All passengers from Bombay and Karachi were taken direct to the segregation camp on the Bandar through an alley way specially railed off. The age, caste, father's name, place of departure and destination of each passenger was taken and registered. The register was called over at uncertain times to see that all passengers were present. The method for the disinfection of clothing consisted in boiling in large copper vessels, and, in the absence of any special dry air or steam apparatus, was found very efficacious. Perishable articles likely to be destroyed by boiling were exposed to the sun daily. It was only at Cutch Mandvi that the people were segregated, that is where they were thoroughly segregated. Khari-Rohar was not under my supervision, and I am not aware what was done there. I believe the people were segregated in a rough manner, but there was no stringency whatever about the rules.

27,119. All the passengers were examined?—Yes, they were all examined by English nurses, and then from the examination on the quay they were taken through the Custom-house—where all their things had to be examined by the Customs authorities—into the segregation camps which were all carefully railed off. There were four or five large "dharamsalas,"—stone buildings—and every accommodation was given there for providing food and everything that they wanted.

27,120. Were all passengers segregated, or did you select certain of them for segregation?—They were all segregated.

27,121. For what period of time?—That varied. When the plague was bad they segregated them for 20 days, and later, when it got less virulent, 10 days. It was 10 and 20 days.

27,122. How many people did you deal with in that way?—The segregation camp opened on October 7th, 1897, and closed on the 22nd July 1898, during which time 15,958 passengers passed through; 178 were sent to the observation hospital, and 22 to the Plague Hospital.

27,123. Plague developed in 22?—Yes.

27,124. But 178 had symptoms suggestive of plague?—Yes.

27,125. You say that these 178 were sent to a separate place?—Yes, they were sent to the observation hospital away from the rest to see whether they developed plague or not.

27,126. And 22 of them developed plague?—Yes; cases of illness were sent originally to the observation hospital, and then from there they were drafted into a separate Plague Hospital which was nowhere near the segregation camp.

27,127. Of these 22, how many died, and how many recovered?—19 died, and 3 were discharged.

27,128. Have you had any experience of Haffkine's prophylactic fluid in Cutch?—None at all.

27,129. Did you use the serum from Paris as a prophylactic?—Yes, a certain amount of it was used.

27,130. How many persons were treated with it?—Owing to the objections raised by the Cutchis, serum, as a preventitive, was not used to any considerable extent. 1,044 persons of all ages were inoculated, of

whom five were afterwards attacked with plague. I put in a table showing those who were attacked :—

		Inoculated.	Attacked.	Result.
*(1.) Aisanbar -	Mundra Borah Caste.	14.9.97	26.9.97	Died 28.9.97.
*(2.) Kharmji -		„	„	Died 1.10.97.
(3.) Esmailyi -		„	25.10.27	Recovered
(4.) Isayi -		„	16.10.97	„
(5.) Mahomed -		„	„	„

* Brother and sister both attacked 12 days after inoculation. Their mother was attacked with plague two days after the children were inoculated.

27,131. How many of the plague cases died in which serum had been used as a preventive?—Two died and three recovered.

27,132. In the fatal cases was there any beneficial effect, as far as you could observe, from the injection of the serum? — Not that I could see. The only point was that the brother and sister were both attacked 12 days after the inoculation, and the mother was attacked with plague two days after the children were inoculated. It is quite possible that they may have had plague if they had not been inoculated with this preventitive serum.

27,133. Do you know the interval of time in each of these five cases between the injection of the serum and the occurrence of the plague?—Yes, they were all inoculated on the same day, and the dates are given.

27,134. What dose of serum was given?—Ten c.c.

27,135. One injection?—Yes.

27,136. (*Mr. Hewett.*) Did you find in the first epidemic that many cases of plague occurred in the same house?—Yes.

27,137. I mean when people were left alone in their houses?—In the earlier part of the epidemic there were more cases in one house than there were later.

27,138. Did you find any difficulty in recognizing the disease?—Roughly, I say not.

27,139. Do you think that many cases escaped detection by reason of their not being recognized?—Most of the cases which I saw were brought to me in hospital. Whether cases escaped the search parties I do not know at all. I do not think that many cases which were brought to hospital were missed because they were not plague. One was constantly going to houses to decide whether cases were plague or not, and I should be inclined to think that very few of them were missed, because if there was any doubt about it they were either removed to the observation bospital or to the Plague Hospital.

27,140. (*Prof. Wright.*) Were there many cases which had no buboes which were diagnosed as plague?—Yes, there were a great many.

27,141. Had you any difficulty in differentiating these non-bubonic cases from other diseases?—One had a certain amount of difficulty in some cases. Some of them were easy enough. The pneumonic cases were easy enough, but, of course, one did get a certain number of cases in hospital which were undoubtedly malarial, and then there was a difficulty in differentiating them from plague during the early stages. One had fevers of various kinds brought into hospital, and those, in some cases, were treated by serum and afterwards discovered to be malaria, and then they were eliminated from the list.

27,142. (*Mr. Hewett.*) And any inexperienced medical man might very easily think that cases were plague which were not plague?—Yes, many of the native assistants sent all kinds of cases into hospital which were not plague, and which had to be sent out again.

27,143. In the course of your service with the British troops in India did you come across cases in which soldiers got these buboes?—Yes. I treated two of them in Kolaba Hospital. That was where I got it myself. I got it from one of those men.

27,144. You had plague yourself?—Yes.

27,145. And these men also had plague?—Yes.

27,146. (*Prof. Wright.*) When was the epidemic worst in Cutch Mandvi?—I cannot remember the dates. It was worst when I first went up there in May and June.

27,147. Was it getting better by August?—Yes.

27,148. Your first 100 cases which you took as controls to the serum cases were cases which you took in May?—Yes.

27,149. Would there not have been a greater mortality in May and June, when your control cases came under observation, than in July and August when your serum cases were observed?—Yes.

27,150. If these 100 cases had not been treated with serum would you not expect to get a smaller mortality among them than among the 100 cases which you take as controls?—Yes, but not so small as after the use of the serum. If you like I will send you up 100 cases which we got later on still, and 100 of those which were inoculated later. Perhaps they will be more favourable for the purpose of comparison than those two groups. These were the first 100 cases which were admitted into the hospital.

27,151. Can you give us cases which were not treated with serum which occurred within the same dates as your serum cases?—Yes, that is what I will endeavour to do. (The witness intimated later that the list was not available.)

27,152. Do you know whether it will make much difference in the results? You have not worked that out, have you?—A certain amount of difference, possibly, but not much. The mortality is always heavier at the beginning of the epidemic than at the end.

27,153. Did you take every case and inject it with serum, or did you leave a certain number in the hospital un-injected with serum during those months of June, July, and August?—There were possibly some who objected to be inoculated, but, as far as I remember, all were inoculated. Of course a certain number were bound to be in the hospital which were not treated, because they might have been brought in after four days, or moribund, or brought in for diseases which were not known for certain to be plague. A certain number of that class were bound to be in the hospital.

27,154. How, then, can you furnish control cases which are strictly comparable to these serum cases?—Only by giving, as you suggest, those who were admitted about the same dates.

27,155. Did you get out your serum direct from the Pasteur Institute?—Yes, it came direct from the Pasteur Institute.

27,156. Was it sent to you direct?—Some was sent to me and some of the serum was that which Yersin left when he went away. Yersin come up for a few days and did some experiments, and then he was recalled.

27,157. Do you think you can give such particulars as will suffice to identify the particular serum which was used? We can then inquire at the Pasteur Institute as to how the serum which you employed was prepared?—That is so. (Witness intimated later that no particulars were obtainable.)

27,158. Were a whole series of sera employed for your injection?—Yes.

27,159. Do you know whether they all came from horse No. 31 whose serum you tested on animals (*see* Question No. 27,067)?—Oh no, they came from various horses.

27,160. Then the serum which was employed for your animal experiments was not the only serum which was employed for your therapeutic inoculations on man?—No.

27,161. It was only one of the sera which you employed?—Yes.

27,162. Can you particularise the cases which were treated with this particular serum?—No.

27,163. They were all mixed up?—Yes, as they came out they were used.

27,164. (*The President.*) Have you not a special note on the record of the case indicating what serum it was?—No.

27,165. (*Prof. Wright.*) I notice that in the third series of experiments which you put in you say: "One "mouse was inoculated with material derived from a "human plague pustule"?—Yes.

27,166. Could you tell us in connection with that, what eruptions you have seen in connection with

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plague? Have you seen a pustule eruption?—No, I mean the plague pustule which starts with a large black blob, and which goes on to necrosis.

27,167. With regard to these carbuncles, do they, in your experience, invariably occur at the very beginning of the plague attack, or do they occur also during the course of the plague attack?—As far as I remember, I have seen them on patients when they were admitted into hospital, on the body at various places, and others where they developed after the patients came into hospital.

27,168. You do not adopt the view that one of these phlyctenules indicates the original site of infection?—No, they are part and parcel of the plague itself.

27,169. Was the synovitis which you speak of ever complicated with urticaria?—No.

27,170. Did synovitis ever occur when you gave as little as 10 c.c. of serum then?—No. I am not at all inclined to say that it was due to the serum at all, except that it was an observation made after big doses in a few cases that this synovitis resulted, and we thought that possibly it was due to those large doses.

27,171. You speak in your précis of pyæmic plague occurring in association with abscesses all over the body: did these abscesses occur in association with buboes?—No; I called to mind one case of necrosis of the scalp—three or four places in the scalp where abscesses developed and left a large necrosis as big as a five-shilling piece.

27,172. Is this not similar to the carbuncle you have spoken of previously?—No.

27,173. It was a different kind of abscess?—Yes.

27,174. Was corpse inspection resorted to in Cutch Mandvi?—Yes. We went to see cases of people who had died to verify whether it was plague or not.

27,175. Was an objection ever made to your inspecting the corpse?—None whatever.

27,176. Were objections not raised among the Muhammadans?—No.

27,177. Have you ever noticed any œdema on the chest of people who died of plague?—Do you mean œdema extending from buboes?

27,178. That is a moot point. I understand that in cases of axillary buboes the œdema may extend right over the front of the chest?—Yes.

27,179. I am inquiring, however, with regard to cases of pneumonic plague and inguinal plague. Œdema of the chest has been asserted to occur in such cases; has that come under your observation?—Never with pneumonic plague, but in many cases of inguinal and axillary bubonic plague.

27,180. Do you think much importance is to be attributed to the œdema as a sign of plague?—No, because so far as my experience goes you get a bul first, and then the œdema extends from the bubo.

27,181. You have not found this œdema in septicæm cases of plague?—No.

27,182. With regard to pneumonic plague have y ever seen complete solidification of the lungs?—I ha had very little experience of *post-mortems*.

27,183. But have you ever seen dulness extendi all over the chest?—As far as I remember there we only a few cases. One side was completely dull and r breath sounds at all.

27,184. Those cases are of very rare occurrence a they not?—Yes, the great majority are of the lobul form.

27,185. Is it difficult to distinguish between t septicæmic form of plague and the pneumonic?—Yes

27,186. Very difficult?—Yes.

27,187. Do you find that nearly every case septicæmic plague is complicated with some pneumon symptoms?—Yes.

27,188. You would not make a special category pneumonic cases of plague, would you?—No.

27,189. You do not see your way to make thes different categories of bubonic plague and septicæmi plague and pneumonic plague?—No, it is all plague.

27,190. (*Mr. Cumine.*) Were there private cas hospitals at Mandvi when you were there?—Yes.

27,191. How did the willingness of the people to b taken to these non-official hospitals compare with the willingness to be taken to the Government hospitals?- Most favourable for the caste hospitals. We had n difficulty in getting them to the caste hospitals, b very considerable difficulty in getting them to th Government hospitals.

27,192. (*The President.*) I want to make one poi clear. Did the synovitis which you observed in plagu cases occur only in those who had received injection of serum?—Oh no: one gets synovitis in cases treate with ordinary methods as well. That is why I mad it a point that it is not certain that it is due to th large doses of serum. But it was curious that o found synovitis when large doses were given, and on also found it when there was no serum given.

27,193. Can you go so far as to say that the synoviti was more frequently seen in those cases where yo gave large doses of serum?—I cannot say that.

27,194. It might be a coincidence?—Yes. Whethe it was due to the serum or not I am not prepared t say.

27,195. In the cases treated with serum in whic synovitis appeared, did the synovitis appear only whe large doses were given, or also with small doses?—A far as I remember, only where large doses were given

(Witness withdrew.)

Major R. W. S. Lyons, I.M.S. called and examined.

27,196. (*The President.*) Are you in the Indian Medical Service?—Yes.

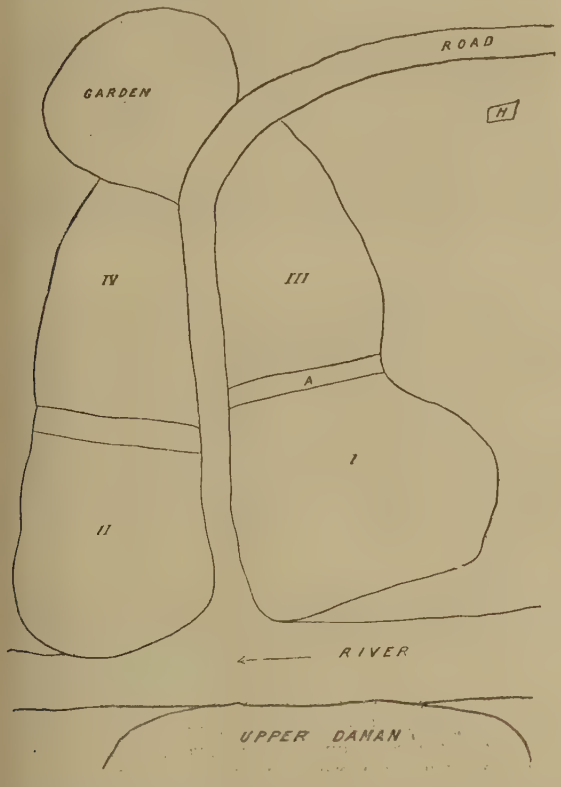
27,197. What are your medical qualifications?—I am M.D. of Queen's University, Ireland.

27,198. (*Mr. Cumine.*) Did you go to Daman?—Yes.

27,199. To inquire into inoculations there?—Yes.

27,200. Will you tell us how you conducted the inquiry?—Yes.

27,201. Did you go to every house in the town?—Perhaps I had better begin at the beginning. You have been to Daman, and you know that it is an ordinary native town with one large street down the centre, and a cross street dividing it, somewhat unequally into four parts. The road to Daman curves round in this way—



I started by taking *this* (I) section. With a few exceptions, the people are very poor and live in small houses, generally of one room each. Sometimes there is a room behind the front room, and running along the front is a verandah. I went to the first house and inquired if there were any inoculated people there, and, if so, had any attacks of plague occurred among the members of the family. They told me whether there had or not, and then, if attacks had occurred, I found out whether they had occurred before or after the inoculation, and whether among the inoculated or uninoculated. Then I inquired if they knew if there were any inoculated in the next or adjoining houses. I did this at each house, so as to make as certain as possible that I did not allow any to escape me. I went through each section of the town in this way. *These* streets divided them very fairly. *This* (IV.) is the smallest division, because this garden contained a large number of people whom I could get hold of at the same time. They were people employed in the garden, and who lived in little huts all around, as gardeners do in India. *This* (IV.) was my last day's work. I finished early and went to some shelter evacuation huts which had been put up in the fields about *here* (H), not far from the road.

27,202. Did you go to every house in the town?—So far as I am aware I visited every house in the town, with the exception of that in which Dr. Fernandez, the man who assisted me, lived. I got information from a person in each house as to what the results were there. They might have deceived me if they wished to do so, but they had no reason to, and Dr. Fernandez had no reason to deceive me with regard to his own house.

27,203. What I wished to get clear was the extent to which you went to all the houses. You say you went to every house in the town?—Yes.

27,204. Not merely to those houses in which, there being both inoculated and uninoculated persons, you were informed that an inoculated or an uninoculated person had been attacked with plague?—No. My first inquiry was: had any inoculations been done in the house?

27,205. (*Prof. Wright.*) Does that refer to Upper Daman or to Lower Daman?—Lower Daman.

27,206. Is Upper Daman part and parcel of the same town?—No. It is separated by the river from Lower Daman. Upper Daman is quite a small place, composed of a fort, with a small number of houses outside the fort.

27,207. Were any inoculations done in Upper Daman?—I do not know. Our inoculations were done only in Lower Daman. When they were started the Governor had placed a cordon along the river to cut off all communication between Upper Daman and Lower Daman.

27,208. There were no inoculations in Upper Daman?—I cannot say. We sent the Governor a certain amount of serum for inoculation; but whether any were done or not in Upper Daman we were not informed.

27,209. Is the population of Upper Daman included in your estimate of the population?—No, it is excluded.

27,210. (*Mr. Cumine.*) Did you make out investigation sheets?—Yes.

27,211. Did you bring into these investigation sheets all the houses in which inoculated people lived?—No, only those families in which plague cases had occurred, and in which inoculations had been done. *This* particular street (A) had the worst outbreak of plague of any that I found. In *this* street there were two or three houses, in each of which all the inhabitants had been inoculated; and all had escaped plague entirely. They were the only houses in the street which escaped plague entirely. It was a most convincing example of the benefit of inoculation to anyone who saw the surrounding circumstances; but this cannot very well put it on paper statistically.

27,212. To what extent did you see other inoculated persons who are not brought into your investigation sheets, and to what extent did you find out, by actual investigation, what had happened to them?—I merely inquired at each house if plague cases had occurred. I did not examine each individual who was inoculated.

27,213. When we see in your investigation sheets that in a certain house in Daman three inoculated persons were living, and that one had plague and recovered, are we to understand that you made these entries because you saw these three persons in the house, and actually saw the person alleged to have recovered, or merely that you were told that these people were living there, and that one had had plague and recovered?—In each house I obtained from one person, who was fairly intelligent, all the information.

27,214. (*Prof. Wright.*) How was the population arrived at?—That was a difficulty, as there was no census. The population was obtained from the Governor. The Governor told us there were 12,000 people, as far as my memory goes, in Lower Daman. (Note by witness on correcting proof of his evidence:—This statement is wrong. The Governor told me that the population of Daman was about 11,000, not 12,000.)

27,215. I think you put it down at 10,900?—Yes; we took the liberty of cutting it down. (Note by witness on correcting proof of his evidence:—We believed that the population of the town was reduced by 1,000 deaths at least, from plague, during the month of February.)

27,216. But that improves your statistics?—No, it brings them down.

27,217. The larger the population the smaller is your percentage of mortality?—On the total population, yes.

27,218. So that, if you cut down the population from 12,000 to 10,000, you improve the statistics, since your uninoculated population is got by subtracting your inoculated population from your total population?—We cut off 1,100. I think, if you had gone through Daman,

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would have thought that we estimated the population fairly correctly.

27,219. I am not calling that in question, but we want to get at the exact facts. I want to know how the population was arrived at. Your statement as to the number of the inhabitants is worth more to me than the Governor's if you tell us how you arrived at your estimate?—I personally thought, and Mr. Sorabjee Damaun Wallah, who knows Daman, ultimately also thought, that 12,000 was too high.

27,220. I have an intimate knowledge of certain towns, and yet I have no idea as to what is the exact population of those towns?—That was our difficulty in this case. There is no census.

27,221. I want to know how these figures, which you put in, were arrived at?—There was no census so far as I am aware. I tried to get a census.

27,222. What did you substitute for a census?—We substituted that 10,900.

27,223. How did you arrive at that figure?—We arrived at it partly from the Governor's statement, which was 12,000 (*see* note by witness to Question No. 27,214 above) and from the estimate of Mr. Damaun Wallah, which was 11,000. (Note by witness on correcting proof of his evidence:—This statement is wrong. Mr. Damaun Wallah estimated the population at about 10,000.)

27,224. You have estimated it at a little less than either?—Yes.

27,225. What principle did you take as your guide in cutting the population down?—None.

27,226. Is then your estimate of the population anything more than a guess?—It was quite a guess.

27,227. Then what is the meaning of your working out your percentages to the decimal point?—I think there is. One must take some standard to go by, one must have a standard whether right or wrong, and even if one argues out statistics from a false standard one gets an impression not exactly right, but one does get an impression in which there is an uniform error all through.

27,228. But it may be evenly wrong in favour of the inoculations as well as against them. Your statistics are worked out here to decimal places?—With regard to the inoculations, as far as I know, the results are absolutely correct.

27,229. But, first I want to get the facts about the population. You say 10,900 is the population. That is got at by guess?—That is so.

27,230. Then you subtract from this number 670 persons who you say died of plague before that time, how did you get at that figure?—I do not remember the figures now, it is two years since I worked them out.

27,231. As far as I understand from your printed report,* the Governor told you that the death-rate might be reckoned as averaging 30 a day?—Yes.

27,232. Further, the Governor told you that you might neglect all deaths before the 1st March?—We did.

27,233. The plague began on the 1st February, but, the Governor told you, you might neglect the deaths in February, and that you might estimate the deaths in March as averaging 30 a day?—Yes.

27,234. But when I take that estimate, and when I multiply by the number of days up to the 24th March, when the inoculation were begun, I make the deaths to be 720. You, on the other hand, arrive at the figures 670?—We lowered the estimate of the death rate to 26 a day, because Mr. Damaun Wallah——

27,235. Who is the Damaun Wallah?—He is the owner of about three-fourths of Lower Daman. He made a census of the families in which deaths occured, which gave the total number of deaths at 2,352.

27,236. Do you know how he made the census?—I believe he made it by collecting from house to house the reports of all the deaths.

27,237. We have it that the people emigrated in large numbers, were there not lots of quite empty houses in Daman?—Yes.

27,238. How then could the Damaun Wallah get at the correct number of deaths?—After the emigration had occurred, he could not obtain statistics. He made his census after the plague cordon was removed and when the people had come back to their houses, when there were no deaths in Daman, in fact, while it was plague-free, and the towns around were infected, so that he would probably have the greater number of his people back again. But, in any case, taking into consideration the fact that he may have made mistakes, he reduced the average to 26 deaths a day instead of 30 a day, that we estimated it at.

27,239. Then, you say 2,000 people left the place, was that a guess too?—Yes, although it is a pretty accurate guess, for the reason that it was so estimated by Mr. Judge, the Salt Commissioner, who was on the frontier, and who knew about the number of people who passed out each day.

27,240. He did not draw his cordon round Daman till later, did he?—Yes, there was always a salt cordon. The cordon was not closed with regard to people until he settled down there himself on a certain day.

27,241. Do you know how this estimate of 2,000 emigrants is arrived at? You say Mr. Judge knew. Did he give the information to the Governor or did the Governor guess?—As far as I remember I had the number from Mr. Judge, not from the Governor.

27,242. Did the people emigrate by sea as well as by land?—Almost all by land. I should think all by land, because with the exception of the fishing population they have no boats, and they could not get into Bombay or any harbour along the coast, coming from a plague-infected place.

27,243. Do you know anything about the strength of the vaccine which was employed at Daman?—No.

27,244. Was the strength of the vaccine experimentally tested?—I do not know, I am not a bacteriologist.

27,245. Are you able, in your statistics, to separate the persons who were vaccinated in the first series of inoculations from those in the second and third series? When you say that 1,372 persons were inoculated and that the mortality among them was 13 per cent., is it not true that these 1,372 are made up of two different lots, *i.e.*, of some who had been inoculated a month before, and of some two months before?—Quite so.

27,246. We have no data given here to enable us to determine whether the effect of the inoculation was less in the second month as compared with what it was in the first?—That is a thing which I would like to have done, but I will show you the difficulty. If you take any inoculation sheet you will see that some of the members of the family were inoculated once and some twice. If they had all only been inoculated once we could have followed out each series inoculated from beginning to end separately, that is to say, taking the total number inoculated on the first occasion we could have followed out their statistics separately and kept them apart from the number inoculated in the second series of inoculations. But a number were inoculated both times, both first and second. Supposing, in a house, every one wished to be inoculated, they would not want to have it done at the same time, because the inoculated were laid up for some days, and there would have been no one to cook or do the housework. Therefore, a certain number were inoculated, say this month, and next month some of the first series were in many instances inoculated again, and the remaining members of the family were inoculated for the first time.

27,247. With regard to these inoculations you say that Dr. So-and-so did so many on a particular date; I do not know whether the figures which are given refer to new people, or whether they include also second inoculations?—All the inoculations so referred to in these statistics are first inoculations.

27,248. And you have no reference as to second inoculations?—Oh, yes.

27,249. You have not put it into this statement?—No, you will find it in these sheets.*

27,250. But you have no statistical summing-up of the results attained by second inoculation?—No.

27,251. Then these results apply sometimes to people inoculated once and sometimes twice?—Yes.

27,252. Your report shows the effect of single, and in some cases of double, inoculations for a period of six weeks after inoculation?—Yes.

* *See* App. No. II. in Vol. I. of these Proceedings.

27,253. You did not make any subsequent inquiry as to the fate of these people?—Daman was plague-free in June 1897, when this report* was drawn up and remained plague-free till I left in November. This investigation was made up to the 1st June, I think. Of course, I do not know what was done after I left India.

27,254. You say in your report that, although you have not determined the duration of immunity, the facts you have collected show that the question has lost its urgent interest. What is the meaning of that?—My idea is, that if a way has been found of preventing plague from spreading, that is all that is required.

27,255. But in the case of Bombay, where you have plague lasting for a long time, do you think that an immunity, which lasted for six weeks and no longer than six weeks, would be of any avail?—We do not know that protection lasts only for six weeks, but even so, why should they not get re-inoculated every six weeks when plague is prevalent around them?

27,256. Cannot you give us any data with regard so the duration of the immunity?—No. That could have been done at Tarapur. I inoculated about 600 people there myself in 1897, and they had a second outbreak at Tarapur in the following year.

27,257. You say a weaker vaccine was used for the second and third inoculations, and you say that, in consequence of this, much larger doses were given?—Yes. It did not give as favourable results as the more powerful inoculation.

27,258. It did not give as great a clinical reaction?—That is so.

27,259. Do you know why the dose was not increased until it did give as satisfactory a clinical reaction as the first and stronger vaccine?—Yes, I do partly. The inoculations were being performed by native doctors, and we did not trust them to investigate the clinical effect of increased doses.

27,260. I do not quite see the bearing of that. Do you mean you would not take their word that the reaction which was obtained was slighter?—No, we did not trust them to increase the doses. I know from the inoculations I did myself that the reactions were not equal.

27,261. But, if you had known that at the time, you would, would you not, have increased the dose?—I did afterwards, but I never got the same re-action.

27,262. Why did you not increase until you got the reaction you desired?—I was not sufficiently acquainted with the action of these toxines to do that.

27,263. I understand that at first the standard dose of 2½ c.c. was given?—Yes.

27,264. In the second and third inoculations you gave two or three times as much?—Three, four, five, or six times as much.

27,265. That is, you gave 12½ c.c.?—Yes, more than that. I have given 15 c.c.

27,266. You did not give 30 c.c. because you thought there might be risk?—Yes.

27,267. Did you take the temperatures of these people when you inoculated them?—Yes, I took a certain number. With the weaker serum, frequently, I did not get a temperature more than 99°, rarely over 100°, whereas with the stronger serum one got a temperature rarely under 101°.

27,268. I do not see specified here what inoculations you did—the second, third, and fourth series?—I never did any of those inoculations at Damaun.

27,269. Then you inoculated elsewhere with some of this same vaccine?—Yes, I did, in Bombay and on Europeans, where I had every chance of knowing exactly how the temperature was ranging.

27,270. Had you any opportunity of finding whether the Europeans you inoculated there did receive benefit from it?—No cases of plague occurred among them. Europeans seemed to be very little subject to plague in Bombay.

27,271. Have you any notion what the limits of error are in the estimates of population which are arrived at by guesses?—No.

27,272. We have had in evidence different estimates of the uninoculated population of Hubli, and these estimates varied between 500 and 8,000. Do you think that your estimates of population may be as wrong as some of these?—I would have a very poor opinion of the intelligence of a witness who could not distinguish between 500 and 8,000 persons.

27,273. You do not think there may be errors of this order of magnitude in your statistics?—No.

27,274. How many houses did you visit?—I have no idea. Purely as a guess, I should think I visited between 500 and 600 houses.

27,275. What would the population of those houses be?—I cannot tell you that.

27,276. You, practically, made a census of the town if you went round to every house?—That is not so easy as you appear to think. If one inquires at a house whether inoculations have been done there, and they say "No," one goes on to the next house at once. If one had to write down the name of every individual in the house, their ages, and so forth, one would spend a quarter of an hour, or half an hour, according to the number of people there.

27,277. You could average the number of inhabitants in the houses?—You will see by the sheets that they varied enormously.

27,278. I say you could strike an average?—Some were 21, and some four, and so on. If one rejected the highest and the lowest number, and struck a mean for the remainder, one could have got an average; but I was not looking for that.

27,279. You cannot supply us, can you, with the number of houses which you visited, so as to enable us to construct some sort of census by multiplying the number of houses by the average number of inhabitants as shown in your investigation sheets?—No.

27,280. (*Mr. Cumine.*) Can you tell us, from this Report* (*handed to witness*), the percentage mortality during the time they were under observation, amongst the total uninoculated and the total inoculated, separately?—24·6 per cent. for the uninoculated, and 1·6 for the inoculated.

27,281. Can you tell us what is the mortality of the uninoculated and the inoculated in your investigation sheets?—For the uninoculated it was 43·5 per cent., and 20 per cent. for the inoculated.

27,282. That is the percentage of attacks; will you mention the percentage of deaths?—29·8 per cent. deaths in the uninoculated, and 8 per cent. in the inoculated.

27,283. The mortality rate among the inoculated in your investigation sheets would be some five or six times higher than that among all the inoculated? It is 8 per cent. among the people in your investigation sheets, and only 1·6 per cent. among the total inoculated; so it is five or six times higher among the former, is it not?—Yes.

27,284. Therefore, it becomes important to see to what extent you ascertained what had happened to the inoculated people who did not come into your investigation. Were there a certain number of inoculated people not in your investigation sheets about whom you did not manage to find out anything at all, whether they were alive or dead?—No, I inquired at each house if inoculations had been done, and whether cases of plague had occurred.

27,285. At some of the houses to which you refer, were you unable to get any information about a certain number of the inoculated people?—No; I asked whether any cases of plague had occurred, and if no plague cases occurred I did not inquire further.

27,286. How did you know whether any plague cases and occurred or not?—I had to depend on the statement of the people in the house. There is a reason, of course, why the death-rate should be higher in the 84 houses to which these sheets refer, because they were plague infected. Where a case of plague occurs in a house it is generally found that plague spreads right through the house; where a case of plague has not occurred the house may escape scot-free, although plague is all round it.

27,287. (*Prof. Wright.*) Cannot you also test your results in another way? What is the relative mortality of the inoculated and the uninoculated as recorded in your investigation sheets, and how does this compare with the relative mortality among inoculated and uninoculated in the general population?—Are you referring to the inoculated and uninoculated as a whole,

* *See* App. No. II. in Vol. I. of these Proceedings.

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or those in the houses where plague cases have occurred, or where inoculations were done?

27,288. I understand that the mortality from plague will be greatest both among the inoculated and among the uninoculated when there is a case of plague in the house?—Yes.

27,289. And the inoculated and uninoculated alike will have a better chance of escaping plague if there are no cases of plague in the house where they are living?—Yes.

27,290. Therefore, let us determine the number of the plague deaths among the inoculated and uninoculated in the plague houses, and let us compare that with the number of deaths among inoculated and uninoculated who were not living in plague houses. Can you supply me with these figures? You have stated in your tables that there was a death-rate of 8 per cent. among the inoculated; what was the death-rate among the uninoculated in these houses which you have tabulated?—29·8 per cent.

27,291. That is practically 30 per cent. The percentage among the inoculated for the whole town was 1·6 per cent; what was the percentage of the uninoculated for the whole population?—24·6 per cent.

27,292. Then the relation of the mortality of inoculated to the mortality of uninoculated in plague houses is 1 to 4, while the same relation all over the town was 1 to 15. These figures are, surely, not comparable?—I think you will see that 24·6 per cent. is the death-rate among the general uninoculated, which is lower than 30 per cent. among the uninoculated occurring in plague-stricken houses.

27,293. I am quite prepared to take that, but these cases of death among the inoculated which appear in your investigation sheet were brought to your knowledge by the fact that you made a census of the houses. But the cases of death among the inoculated in the town outside were not brought to your knowledge in the same accurate manner?—You must remember there are a great number of persons in the town who never come in contact with plague.

27,294. That applies to the inoculated as well as the uninoculated?—Quite so.

27,295. I take it that these investigation sheets are correct, and this being so, they ought to be a picture in small of what occurred in the whole of the town, ought they not?—Yes.

27,296. We find that they are not a picture in small of what occurred in the whole town?—I think I can show you the reason. In the plague-infected houses almost every person catches plague, in the town every person does not catch plague. The reason probably is that a great number of the inhabitants of the town have not come in contact with plague.

27,297. I do not see that this meets the point?—In order to get comparative statistics for the whole town I should have taken in a street say, four houses, in which inoculations had been done and compare them with four on the opposite side of the street in which, perhaps, every member had been attacked by plague, but I did not do this. I neglected those inoculated houses altogether in which plague did not occur. My object in selecting only plague-stricken houses in which inoculations had been done was not to find evidence in favour of inoculation, but all the evidence there was against it. I did the same in Bombay; I investigated every plague case among the inoculated there, and advertised for them in order to obtain all the evidence against inoculation.

27,298. I have had experience of the futility of the effect of advertising for information?—In Bombay I had particular advantages. I knew the majority of the medical practitioners, and anyone who was called in to a case of plague in which inoculation had been done always told me about it.

27,299. (*Mr. Cumine.*) There are four investigation sheets?—Yes.

27,300. Will you look at that Report* ?—I have the Report here. There are four sheets.

27,301. When the proof of your evidence is sent to you for correction will you put down with regard to each of these four tables separately the attacks and deaths amongst the inoculated and uninoculated respectively?—May I explain what these sheets are? In the first division all the families are shown in which inoculations were done on the first occasion in Daman. Through a printer's error two families which should have appeared in the first division are shown in the second. In this first series there are a considerable number of inoculations done on the second occasion—not the majority, but a certain number. In the second series there are no inoculations done on the first occasion, they are all inoculations done on the second occasion. In the third series they are inoculations done on the third, and in the fourth series on the fourth occasion, after the plague had stopped in Daman altogether, for a number of people were inoculated even after the plague ceased.

27,302. Do you think that any inoculated people could have emigrated? Were there any privileges given to inoculated persons?—Not from Daman.

27,303. (*Mr. Hewett.*) They could have emigrated, I suppose?—No, I do not think so.

27,304. Why not?—Mr. Judge kept a strict look out.

27,305. Do you think that a cordon can keep people in a place?—I do not think it can keep individuals in a place. But I do not believe that families escaped, although individuals may have done so.

27,306. (*Prof. Wright.*) Do you think that the inoculated in your investigation sheets are comparable, in the matter of age and health, with the uninoculated; or do you think that you have a greater number of the feeble and old people among the uninoculated?—If you look through these sheets you will see exactly. I have put down their ages in each case.

27,307. I only wanted to know whether, when you saw these people, you thought the uninoculated were comparable as a whole with the inoculated people. You probably saw them?—Yes.

27,308. Why were these people not all inoculated?—It not unfrequently happened that, when the members of a family went to be inoculated, one or more were left at home to attend to household matters.

27,309. (*The President.*) Why did they not go?—It is on account of family life in India.

27,310. (*Mr. Hewett.*) I think you did over 8,000 inoculations in Bombay City?—No, I did not do them. I did a certain number there. I think there were about 18, or perhaps 20, cases of plague which occurred among the inoculated in Bombay, and I found out from the people who treated them all about these.

27,311. You say that these 20 cases occurred; how do you gather that other cases did not occur?—That I cannot be sure of.

27,312. So that the facts would not bear so much upon the relative immunity of the inoculated?—Not on the relative immunity of the inoculated; but it did show a result which Daman did not show.

27,313. What was that?—In the 18 or 20, as far as I remember, there was no death.

27,314. At what period of the epidemic was that?—I came to Bombay on the 25th of January, and the plague was at its height in Bombay about the end of March or the beginning of April 1897. They occurred during that time.

27,315. Have you any security that the practitioners always gave you the cases which were unfavourable to inoculation?—They knew that I was looking for them. Of course, I have no guarantee.

27,316. These were native practitioners, were they not?—Yes.

27,317. Assuming that benefits were given to the inoculated persons, it would be to their advantage to let you see that the results of the inoculations were good?—There was no benefit to be given to inoculated persons.

27,318. There was subsequently, and there was some idea that there might eventually be?—My impression is—it is merely an impression—that the very opposite was the case—that the native practitioners sought out every possible thing they could against inoculation.

(Witness withdrew.)

* *See* App. II. to Vol. I. of these Proceedings.

Dr. KLEIN, F.R.S., called and examined.

27,319. (*The President.*) You have been for years employed by the Local Government Board in biological research?—Yes.

27,320. (*Prof. Wright.*) Perhaps you will begin by telling us something of the morphological characteristics of the plague bacillus. I see* you lay great stress upon the fact that certain of the colonies which are produced on agar are thready colonies?—That was in gelatine plates.

27,321. Your experience was gained, was it not, by a study of two different cultures of plague?—Yes.

27,322. One of them was obtained from a plague case which occurred in London?—Yes, and one was sent from Hong Kong.

27,323. Have you verified that your observation holds true also in other plague cultures?—I have not had any others.

27,324. I do not think that in your Report to the Local Government Board you referred to the formation of stalactites in broth?—No.

27,325. Have you confirmed M. Haffkine's statement that a stalactitic growth is invariably obtained with cultures of plague?—I have not worked on the same lines. Owing to the Vienna scare, we were put in a very awkward position.

27,326. Have you verified Mr. S. Mervyn Gordon's statement that the plague bacillus possesses flagella?—Yes, but very few bacilli have a flagellum.

27,327. You do not lay any stress in your Report to the Local Government Board upon involution forms; do you think these involution forms are very common in cultures and very characteristic?—They are very common where the media is not absolutely suitable; for instance, in Hankin's cultures where there is an excess of salt.

27,328. Have you found that to be a good medium upon which to obtain involution forms?—Yes, but I have not had a sufficient number of human cases to make a series of observations upon. In those I have had before me there were no involution forms in the active cultures.

27,329. Do you know whether your cultures still give involution forms or not, or whether they have ceased to do so?—I have not tried that.

27,330. Do you know whether bipolar staining can be got in bacteria grown on artificial media?—Yes, constantly.

27,331. I ask because there is an impression that bipolar staining can only be obtained in the case of bacilli which are obtained directly from the animal?—No; I think bipolar staining is a very characteristic feature.

27,332. It is said that when the plague bacillus is transferred from agar to agar for a number of generations it no longer shows bipolar staining?—I doubt that. With regard to these bipolar stainings, it depends so much on the operator himself as to the way it is stained and the way it is washed afterwards. You can get from the same culture preparations which show these bipolar stainings, and in others slightly different you would not have it.

27,333. We had it in evidence that bipolar bacteria are often found in the earth; do you think that the fact that these bacteria showed bipolar staining would, by itself, justify the diagnosis of plague?—I should be very slow to do that.

27,334. Do you think that the clear space in the centre corresponds to a vacuole?—I think that it is due to the segregation of their protoplasm, and to the collection of this at the ends of the rod.

27,335. Could you see a clear space in the unstained bacillus?—I must say I have not done so.

27,336. You say the plague bacilli find their way into the intestines of guinea-pigs inoculatod with plague?—Yes; we have made several experiments on that point with animals dying from plague after inoculation. In some instances the small intestine was found distended with some mucous matter, and we have made not only cultures successfully from it, but in several instances injecting a guinea-pig from this we produced definite plague.

27,337. Have you ever found that the plague bacillus escapes in the fæces?—We have not looked for it there. Only in the animals which died we took out matter from the intestinal canal.

27,338. Have found many plague bacilli in the intestinal canal? Were they in sufficient quantities to be seen under the microscope?—There are a great many other bacteria; but the best and easiest way of showing that they are the plague bacilli is to inject a little of the mucus into the subcutaneous tissue of a fresh guinea-pig.

27,339. Have you ever isolated plague from contaminated matter of that kind; is it easy to do so?—I think that when sufficient care is used there is no difficulty.

27,340. You found bacteria in the bronchial secretions of the guinea-pig?—Yes.

27,341. Did you find them in any of the guinea-pigs before they died of plague?—Only after death. Even in those cases in which guinea-pigs were dying within 48 or 72 hours, and were showing very little change in the lung, except a slight congestion—even in those cases—and those are the cases which are particularly referred to in my Report*—did the mucus in the trachea and the larynx yield cultures of the plague bacillus; where the lungs were greatly involved (inflamed) it is easy to demonstrate the plague bacilli in the bronchial contents.

27,342. You surmised, from your observations on guinea-pigs, that plague bacilli might be found in the human sputum?—Yes.

27,343. I understand that no example of a transference of plague from one animal to another came under your observation?—No.

27,344. Did you take any great precautions to prevent such a spread?—The usual precautions were taken. There is a room with hutches one above the other, in which the animals are placed.

27,345. Have you left infected animals in contact with non-infected animals?—Sometimes there are animals which are infected with material which proves to be non-virulent, and they are put together with animals which have been infected with material which proves virulent. When I say "non-virulent" I mean a material which I have tested and found to be non-virulent. The animals are put together, and the one with the virulent material dies, and the other does not become affected at all with plague.

27,346. But you have not kept quite normal animals in contact with plague-infected animals?—They are in those hutches above and below in the same room. There are animals which we call fresh animals which have not been touched, and we never had any of these becoming infected with plague.

27,347. Have you ever demonstrated the presence of agglutinins in case of plague in animals?—No.

27,348. Have you seen agglutinins developed in men?—No; I have never studied a case of plague in man.

27,349. Have you tested any of the sera sent out from the Pasteur Institute?—I have not seen any. I have myself had sera from guinea-pigs and rabbits.

27,350. Have you ever noticed that animals which received serum which had been drawn off from an animal which had previously been inoculated died faster than control animals? You refer to a case here, in page 296, of your Local Government Report,* Guinea-pigs Nos. 24 and 26. You say, "The result was "rather unexpected, inasmuch as both guinea-pigs "developed the typical disease. The serum-plus-"plague-guinea-pig No. 26 was indeed more severely "affected than the control animal; it died in 24 hours, "whereas the control animal died in 60 hours." You give an instance there of an animal in which the course of plague was apparently hastened by the introduction of a therapeutic serum obtained from an animal which you had endeavoured to protect?—Yes.

27,351. Have you seen such a result from the introduction of other ostensibly therapeutic sera?—I am sorry to say I know of cases where it does occur. There is on organism which I have worked with for the last three or four years, which is present in the intestine of acute diarrhœa, in fact, in all diarrhœa affections you find this organism abundantly present. It is an

* *See* App. B. No. 7 to the Supplement to the Report of the Local Government Board for 1896-7 containing the Report of the Medical Officer.

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anærobe and is spore-forming; we call it the bacillus enteritidis sporogenes. It is found in sewage, in street-dust contaminated with horse dung, and in human fæcal matter. Some of it is derived from a diseased alimentary canal. If a small part of such material is put into milk and the milk heated to 80° C., so as to destroy everything else but the spores themselves, it yields on anærobic incubation at 37° C. in 24 hours a pure culture of bacillus enteritidis sporogenes. This culture when injected into guinea-pigs invariably produced a fatal result within 20 hours. We can tone down the amount injected into the guinea-pigs so as not to produce a fatal result. The animal becomes ill, has a tumour at the seat of inoculation, the tumour ulcerates, and the animal ultimately recovers. We have had a series of animals and tried to protect them by sub-fatal doses, but we always found that they became much more susceptible to a second infection when they have once passed through the disease.

27,352. Have you inoculated animals with this micro-organism, and have you tested the effect of these animals' sera upon other animals?—Yes, we have done that also.

27,353. And you also found that you got prejudicial results?—Yes.

27,354. So that you think it is possible that the "anti-plague" serum might do harm as well as good?—I should not like to say that, because I should not like to generalize from one instance.

27,355. With regard to vaccination against plague, you have done some experiments—you have inoculated animals by living cultures?—Yes.

27,356. And you find that they do not thereby become noticeably less susceptible to plague?—Slightly less susceptible; after several inoculations with sub-fatal doses they still re-acted. They still re-acted on a fresh inoculation by showing the characteristic tumour and becoming ill.

27,357. Do you know how long this partial immunity lasts?—No, but there is only very slight immunity conferred after several inoculations; the animals are still susceptible.

27,358. Do you find you get better results by using living cultures than by using dead cultures?—I made a large number of experiments with both, particularly with sterilized cultures, and the sterilized cultures certainly have not given those results which one might have expected from similar experiments with other microbes, *e.g.*, typhoid and cholera.

27,359. How did you sterilize those cultures?—By heat—60° to 70° C. for five minutes, 60° to 63° for ten minutes is quite sufficient, but in order to make sure we generally used 62° to 65° for ten minutes or 70° C. for ten minutes.

27,360. Have you determined whether there is a diffusible toxine produced by the plague bacillus?—No, I have not.

27,361. Have you found that the dead bacteria are very toxic?—Apparently not. Large doses injected subcutaneously or intraperitoneally produced no disease.

27,362. You did not get a necrosis of the skin afterwards?—No.

27,363. In the case of animals which have been partially protected against plague, are the *post-mortem* appearances which are found when they succumb to plague in any way different from the *post-mortem* appearances which are found in animals which die rapidly from plague without offering any resistance?—Apparently not; when they die they have the same kind of lesions, that is to say, the spleen is enlarged and full of bacilli, the lung may be affected, and may have necrotic patches; the inguinal tumour at the seat of inoculation shows exactly the same characteristics. I mention here one experiment where an animal, after the fourth inoculation, developed a tumour which suppurated and which contained a great many other organisms besides the plague bacilli, so that these animals do not seem to have offered the same symptoms as an animal which is inoculated for the first time. On page 295 of my Report, I say:—"On the "9th day, the general condition of the animal remain-"ing normal, the tumour, which was still very large, "was examined, and found to be filled with fluctuating "contents: the skin above it, however, was unbroken." The animal had evidently some immunity conferred upon it.

27,364. Have you any experiments to show whether animals can be infected with plague by putting plague bacilli on their mucous membranes?—No, I have not.

27,365. (*The President.*) Have you ever fed animals with virus?—I do not think we have done any systematic experiments in that direction.

27,366. You have nothing to show whether the admission of the virus into the intestinal canal is followed by plague symptoms or not?—No; I could not say anything about that. The only thing I mention here is that animals have died from plague, having the proper bacilli in their alimentary canals. I have not made any systematic experiments by feeding fresh animals with these contents, only by inoculating animals.

27,367. In the alimentary canal were they restricted to the small intestine?—Yes.

27,368. Not in the stomach or higher up?—No. The small intestine was congested and generally distended by mucus.

27,369. These were cases in which the virus had been introduced by the skin?—Yes.

27,370. You think the serum of animals which have recovered from plague is destitute of any immunizing quality?—The serum of guinea-pigs which have been repeatedly infected with sub-fatal doses has certainly very little protective power with other guinea-pigs; that is all I can say. I could not go one step further. After these experiments, which I made on these guinea-pigs myself, I should be the last person to apply them to the human subject. I should like to state that very emphatically. Whatever is to be applied to the human subject has to be found out in the human subject. Whatever I found out on the guinea-pig applies to the guinea-pig. I should like to say that it applied to the guinea-pig only.

27,371. Have you tested the bactericidal power of the serum obtained from an animal which has recovered?—I have made several experiments, but the result was practically nil. One-tenth of $\frac{1}{4}$ c.c. of the serum of an animal which had been injected several times previously with plague culture, and which has recovered from these repeated injections, has very little protective power or germicidal power for another animal.

27,372. Have you made any experiments upon the anti-toxine as distinguished from the bactericidal power?—No, I have not.

27,373. I think I heard you say a moment ago that animals may have had even four attacks of plague and yet are still susceptible to fresh inoculation?—Yes, they re-act; that is to say, they show the tumour, the bacilli develop there, and the animals become ill generally. In fact, if you use a sufficiently large dose they may succumb.

27,374. That is a condition which applies to all immunizations, is it not?—Yes.

27,375. There is no such thing as complete immunity?—No.

27,376. Are the doses necessary to produce the initial re-action required to be increased in order to produce the same re-action subsequently?—Yes.

27,377. Have you made any experiments as to the influence of various external conditions upon the vitality of the bacillus?—Only heat, no other.

27,378. What are the results of your experiments on the effects of temperature?—The temperature, I think, was 62°—upwards of 62° for five minutes—but to make quite sure we heated up to 65° or 70°.

27,379. Sixty-two degrees is sufficient to kill the bacilli?—Yes, 62° or 63° C. for 5–10 minutes.

27,380. Have you made any experiments on the influence of sunlight on the vitality of the bacillus?—No.

27,381. Or of access of air?—I have made recently some experiments—and they will be published in the next Blue Book of the Local Government Board—with reference to the vitality of the plague bacilli in the dead body.

27,382. Can you give us some of the results of these experiments?—Yes. At 14 days they are still recoverable. I am giving you now the results which are still in the printer's hands. I should say that 17 or 19 days

was the extreme length in the guinea-pig. I am speaking of the guinea-pig. That is to say, I inoculate the guinea-pigs with living plague culture subcutaneously, and the animals die between 48 and 72 hours. One set of animals are put in a wooden coffin and another set in a tin coffin, and a third are buried in the earth wrapped up in cloth, and another set are wrapped up in cloth and buried direct in the sand. The coffins are also put in the earth. Then they are exhumed after a given time. After 14 days and 17 days the plague bacilli are still recoverable both from the subcutaneous tumour and the spleen, but after 21 days there are no more living plague bacilli to be recovered from the dead body. Speaking from memory, I am not quite sure that 21 days is not the limit, but certainly beyond 21 days they cannot be recovered. (Note by witness on correcting proof of his evidence:—On looking at my paper on the subject, I find that in no case were the plague bacilli recoverable 21 days after burial.)

27,383. What was the season of the year?—These experiments with the plague bacilli are one of a whole series of experiments with typhoid, cholera, tubercle, bacillus prodigiosus, and staphylococcus aureus. Amongst the series are also experiments with the plague bacillus. That was done between May of last year and March of this year. The experiments extended over a considerable time.

27,384. Therefore the living bacillus exists actively for a long time in the dead body?—17 days, but certainly not so long as the popular belief would have it. I must say I do not quite understand the experiments which have been published with reference to the survival of the plague bacillus in earth. I should like to see those things done again.

27,385. (*Prof. Wright.*) Whose experiments are you referring to?—Hankin, I think, has published such, and Yersin himself made some statements with regard to this matter.

27,386. (*The President.*) You have not made any experiments?—No; Hankin could not recover it from earth.

27,387. Have you made any experiments with any other animals?—Rabbits and guinea-pigs only.

27,388. You have not investigated fleas or bugs?—No.

27,389. (*Mr. Hewett.*) Did I understand that you put the plague bacillus on earth, and that you were not able to recover it?—I placed it in a dead body in earth. We have made in the laboratory a large number of experiments with reference to many of these organisms in earth—bacillus prodigiosus, cholera, and typhoid—and the results are very remarkable in this way, that it is extremely difficult to recover them. They are very marked organisms, easily recoverable if present. You know how much more marked typhoid or prodigiosus is than the plague bacillus.

27,390. This applies to non-sterilized earth?—Yes. They were put directly into certain areas of soil of different kinds, and in a few days, or a few weeks, it is a matter of extreme difficulty to recover them.

27,391. (*The President.*) You have not made any definite experiments yourself?—No.

27,392. (*Prof. Wright.*) You have not made any experiments on clothing to see how long the infective material would remain in clothing?—No.

27,393. Do you kind that the plague bacillus is soon killed by desiccation?—I have made a few experiments with the plague bacillus, and, being a non-sporing bacillus, if it is desiccated in a thin film it dies immediately, just like many other non-sporing bacilli. Of course, it entirely depends on the thickness of the film. If you have a thick layer or a large lump—a piece of spleen—for instance, the possibility is that the centre remains undried.

27,394. (*The President.*) Have you found the bacillus in the blood of animals?—Yes.

27,395. In all parts of the circulation?—We generally use the heart's blood, and then we make sections of the organs and find them also in the blood vessels there.

27,396. Have you found them in the living animal?—Yes, in the blood.

27,397. Have you had much difficulty in finding the bacillus in living animals?—In the early stages, before 48 hours, I do not think we have succeeded in obtaining the plague bacilli from the blood, but on the last day of the animals' existence there was not very much difficulty in demonstrating them in the blood of the general circulation. I am speaking of cultivation.

27,398. Have you found the bacilli in any animals which recovered?—No.

27,399. Only in those which died, and shortly before death?—Yes, in those which were really afflicted, that is, animals which had a tumour and were quiet.

27,400. What is the longest interval before death that you have found the bacillus?—If an animal is injected subcutaneously in the groin with plague bacilli, there is generally a swelling already after 24 hours, and in this tumour plague bacilli can be easily demonstrated in large numbers, but in the blood of the general circulation of such an animal plague bacilli have not been demonstrated before 48 hours are over.

27,401. Have you tried the effect of chemical disinfectants?—Yes, I have tried Izal and carbolic acid, 2 per cent. carbolic and 1 in 300 Izal, and in three minutes they both are capable of disinfecting completely.

27,402. Killing entirely?—Yes. It was the ordinary pure carbolic acid.

27,403. You kept them under observation for some considerable time?—Yes.

27,404. For how long?—The cultures were kept 10 days to a fortnight.

27,405. Will you describe the experiments with disinfectants?—We generally use pure cultures of the material—24-hour agar culture, grown at 37° C. in order to ensure having active microbes. A particle of it is taken off and put in the disinfecting solution, which had been put previously into a sterile test-tube; a particle of the growth is put in and well shaken up so as to distribute it uniformly. It generally distributes very rapidly. It is then kept in this condition for five minutes; after this a considerable amount of it is taken out, two or three loops—which would, if the solution were water or salt, yield an innumerable number of colonies—and with it nutritive media (agar or broth, or both) are infected. These are then put into the incubator at 37° C., and the result watched. If only a few colonies come up, we should say that a great many of the microbes had succumbed, but that some few had survived the action of the disinfectants; but if no colonies at all come up, then we say there were no living microbes left. If you make such an experiment that is to have scientific value, you have to use definite pure cultures and not merely material which contains these microbes. The only disinfectant experiments on the plague bacilli that I made were with carbolic acid 2 per cent. and Izal 1 in 300.

27,406. In your experiments on the two species of animal you found that the susceptibility of a rabbit was different from that of a guinea-pig?—Yes, a rabbit is more susceptible than a guinea-pig.

27,407. How did you dose them?—We gave them a good dose so as to ensure a positive result.

27,408. You mean you gave the rabbit the same dose as you gave the guinea-pig?—We give them always a much bigger dose than would be required to kill them.

27,409. Then you do not know what the amount which is required to kill them is?—No, only the duration of the disease. I could not put my hand on the rabbit experiments, but I could look them up in my note-book. I know, in order to ensure a fatal result, we always gave them a dose which was larger than was exactly the fatal dose.

27,410. You made no attempt to determine the smallest quantity?—No; we do not know what is the highest or the lowest quantity.

27,411. How did you come to any conclusion as to the difference of susceptibility? By weight?—Yes. We weighed the animals and accordingly gave the amount of dose.

27,412. What amount of dose?—$\frac{1}{10}$th, $\frac{1}{12}$th, or $\frac{1}{20}$th of a culture.

27,413. And you varied that amount according to the weight?—Yes.

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27,414. And you found that the variation which held with the guinea-pig did not hold with the rabbits?—Yes.

27,415. (*Mr. Hewett.*) Can you give us any facts as to the case of plague alleged to have been landed at Plymouth from the *Golconda*?—On December 29th, 1898, I received four capillary tubes with blood taken from the finger of a patient landed at Plymouth from the *Golconda*. The material was brought to me by Dr. Bulstrode, Inspector to the Local Government Board, and was to be examined for the Medical Department of the Local Government Board, to which Department my report was sent early in January. Neither by microscopic examination, nor by culture, nor by experiment on animals, were any plague bacilli discovered in the above blood samples.

(Witness withdrew.)

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APPENDIX No. LV.

(*See* Question No. 17,798.)

NOMINAL ROLL

of INOCULATED and UNINOCULATED KHOJAS in BOMBAY, who died from PLAGUE and other causes from 20th April, 1898, to 21st December, 1898, compiled by Mr. ISMAIL JAN MAHOMAD.

INOCULATED. DIED OF PLAGUE.

28.4.98. Karmally Lootafally, age 10 years, died of plague at hospital.

5.8.98. Nanjee Taya, age 22, died of plague at Moondagali. *Inoculated in* 1897.

12.8.98. Mahomed Varin, age 37, died of plague at Bhendibazar. *Was twice inoculated this year, and twice inoculated last year,* 1897.

28.9.98. Valli Mahomed Karim, age 12, died of plague at hospital, residing at Diva off Thana. *Once inoculated.*

INOCULATED. DIED OF DISEASES.

17.5.98. Motibai, wife of Kasam Jan Mahomed, age 20, died of delivery at Durga Street.

25.5.98. Loopli Jivan Soorditha's daughter Viroo, age 2 years, died of diarrhœa at Mazagaon.

30.5.98. ? Kasam Karmaly Soojis' wife Khainsabai, died of consumption at Khoja Street. *Twice inoculated.*

9.6.98. Jan Mahomed Visram, age 45, died of (?), Khoja Street; died at Hospital of Jamsetjee Jeejeebhoy. *Once inoculated.*

14.6.98. Jaffer Lakha, age 32, died of paralysis at Durga Street. *Once inoculated.*

15.6.98. ? Avalbai, wife of Remtala Mitha, age 30, died of consumption at Dougri. *Once inoculated.*

3.7.98. ? Jainab, daughter-in-law of Jagga Visram, age 20, died of consumption and delivery at Khoja Street. *Was inoculated.*

9.7.98. ? Sakina Meetha Rahimtula, age 11, died of consumption at Dongri Street. *Once inoculated in* 1897.

16.7.08. Pirbhai Devan, age 50, died of asthma at Duncan Road. *Was inoculated.*

23.7.98. ? Janbai, wife of Neuji Jiva, age 65, died of asthma at Dongri. *Was inoculated.*

28.7.98. Ali Mahomad Karsan Bava, age 2, died of fever and cold at Pala Bazar. *Once inoculated.*

5.8.98. Hoosen, son of Abdula Nensi, age 8 months, died of diarrhœa at Durga Muhalla.

7.8.98. ? Foolbai, wife of Hoosen Taya, age 56, died of brain fever at Moondagali.

7.8.98. Ranjee Bhanjee, age 54, died of asthma at Khojagali.

7.8.98. ? Issa Ibraham Pardhan, age 2½ years, died of consumption at Kharak.

7.8.98. Kasam Poonja's granddaughter, Sakina, age 1½ months, died of convulsions at Furniture Bazar.

14.8.98. Kara Kanji, age 35, died of (?) at Kandi Muhalla. *Once inoculated this year.*

18.8.98. ? Shubanali Kammoo Kurmali, age 24, died of consumption at Mazagaon H. A. compound yard. *Inoculated.*

20.8.98. ? Kamarbai, wife of Kasam Nathu Habooani, died of consumption at Pala Lane. *Once inoculated.*

23.8.98. Motibai, wife of Harji Nanji, age 18, died of (?) at Kandi Muhalla. *Twice inoculated.*

23.8.98. Hirbai, wife of Kanji Bhimji, age 56, died of brain fever at Kandi Muhalla *Inoculated.*

24.8.98. Aladina Bhahdina's wife Jaithbai, age 65, died of fever at Umarkhadi. *Once inoculated.*

3.9.98. Suntokbai, wife of Moona Nathu, died of (?) at Bhendibazar. *Inoculated twice.*

10.9.98. Nagindas Mathuradas, age 34, died of brain fever at Bhuleshwar. *Twice inoculated.*

12.9.98. Hassan Rahimtula, age 28, died of prolonged illness of fever at Dongri. *Twice inoculated.*

15.9.98. Siribai, daughter of Soomar Bharmal, age 9, died of other fever at Pala Lane. *Twice inoculated.*

16.9.98. ? Sonbai, wife of Kasam Manji, age 47, died of fever at Kharak. *Once inoculated.*

21.9.98. ? Ismail Rattansi, grandson Kasam Hassan, age 15, died of brain fever at Kandi Muhalla. *Twice inoculated.*

25.9.98. Dhanji Rooga, age 75, died of asthma at Kandi Muhalla. *Twice inoculated.*

29.9.98. Fatma, wife of Mahomed Nanjee, age 17, died of consumption. *Twice inoculated.*

14.10.98. ? Jaffer Lila Devji's daughter Fatma, age 10, died of fever at Kandi Muhalla. *Thrice inoculated.*

18.10.98. ? Daughter of Pundit Siv Dayal, Sakina, age 5 years, died of fever at Mazagaon at H. H. *Once inoculated.*

26.10.98. ? Sakina, daughter of Khanji Lalji, age 10, died of brain fever at Pala Bazar. *Once inoculated.*

4.11.98. Jaina, daughter of Mahomad Karim, age 5 years, died of diarrhœa at Dongri. *Twice inoculated.*

4.11.98. Jaina, daughter of Samsoodin Bhajin, age 4, died of diarrhœa at Dongri Bazar. *Once inoculated.*

6.11.98. ? Vally Jamal, age 32, died of fever at Durga Muhalla. *Once inoculated.*

8.11.98. ? Kajbai, wife of Hirji Fakir, age 25, died of delivery at Durga Street. *Twice inoculated.*

15.11.98. ? Karim Nathu Shivji's wife, Mariambai, age 25, died of consumption at Durga Muhalla. *Once inoculated.*

18.11.98. Mariambai, wife of Jan Mahomed Gangji, died of (?) at Moonda Gali. *Once inoculated.*

18.11.98. Khatza, daughter of Rahimtoola Khimji, age 22, died of asthma at Palki Muhalla. *Twice inoculated.*

22.11.98. ? Sakinabai, wife of Mahomed Alloo, age 24 years, died of delivery at Durga Street. *Once inoculated.*

23.11.98. Sonbai, mother of Goolam Hoosen Moolji, age 72 years, died of asthma at Durga Street. *Once inoculated.*

27.11.98. Sakinabai, wife of Allarakhia Gangji, age 25, died of consumption at Durga Street. *Once inoculated.*

28.11.98. Sakinabai, wife of Canji Lalji, age 27, died of Pala Lane. *Once inoculated.*

29.11.98. Minbai, wife of Hassan Megji, age 45 died of consumption at Furniture Bazar. *Once inoculated.*

NON-INOCULATED. DIED OF PLAGUE.

28.4.98. Jaffer Poonja, age 50, died of plague at hospital. Had come from Bandra.

1.5.98. Goolam Hoosen Alarakhia, age 20, died of plague at hospital. Had come from Bandra.

12.5.98. Manji Khimji, age 30, died of plague at hospital. From Bandra.

28.8.98. Navroj Ladha Sachoo, age 22, died of plague at hospital. Resident of Mandvi. Plague caught at Bombay.

31.5.98. Mahomad Rahim Manji, age 16, died of plague at Dongri Street.

2.6.98. Ladha Lachoo's wife, age 50, died of plague at hospital. Residence of Manori.

24.7.98. Kasam Mahomad Hassan, age 14, died of plague at Hospital. Came from Danda with plague.

9.8.98. Sherbanoo, granddaughter of Haji Lalji age 12, died of plague at Khoja Street.

19.8.98. Remtala Sajan, age 56, died of plague at Durga Street.

19.8.98. Kesarbai, wife of Goolansan Abbhai, age 35, died of plague at Durga Street.

20.8.98. Roopsy Virjee, age 38, died of plague at Pala Lane.

21.8.98. Lonbai, wife of Mahomed Premji, age 55, died of plague at Pala Lane.

23.8.98. Hoosen Jaffer Ibrahim, age 23, died of plague at Kharak.

25.8.98. Jaffer Mariambai, wife of Hoosen, age 15, died of plague at Kharak.

27.8.98. Cooverbai, wife of Khan Mahomed Simji, age 25, died of plague at Kharak.

28.8.98. Kasam Ramji Boga, age 18, died of plague at Durga Muhalla.

1.9.98. Alladin Hassan's mother, age 60, died of plague at Dongri.

1.9.98. Jamabai, wife of Haji Nathoo, age 40, died of plague at hospital.

2.9.98. Pirbai Karmali, age 23, died of plague at Pala Lane.

4.9.98. Babool Ramji Canji, age 18, died of plague at Pala Bazar.

4.9.98. Mithibai, wife of Jooma Kara, age 30, died of plague at Dongri.

6.9.98. Ibrahim Shariff, age 64, died of plague at Durga Street.

7.9.98. Alli Ghiga, age 50, died of plague at Kharak.

7.9.98. Kasam Nathoo, Manji, age 29, died of plague at Furniture Bazar.

7.9.98. Virji Khimji, age 65, died of plague at Kharak.

9.9.98. Virji Khimji's daughter Sakina, age 15, died of plague at Kharak.

10.9.98. Abdoola Karim, age 40, died of plague at Dongri.

12.9.98. Lalbai, wife of Hassan Karim, age 40, died of plague at Dongri Street.

12.9.98. Manji Dhanji, age 19, died of plague at Pala Bazar.

13.9.98. Meheralli Rattansi, age 16, died of plague at Kharak.

22.9.98. Karmali Hassan Megji, age 12, died of plague at Dongri.

25.9.98. Mahomed Hasan Shivji, age 23, died of plague at Durga Muhalla.

24.9.98. Jaffer Ibrahim, age 38, died of plague at Durga Muhalla.

28.9.98. Ramjanalli Karmali, age 19, died of plague in hospital at Dongri.

28.9.98. Mooraj Alloo, age 25, died of plague.

28.9.98. Karmali Rahim, age 12, died of plague at hospital.

28.9.98. Daughter of Bundealli Allarakhia, age 10, died of plague at Durga Street.

29.9.98. Moosa Kasam Karim, age 39, died of plague at Dongri.

3.10.98. Bhanji Hasanaly Sowcar, age 18, died of plague at Pala Bazar.

22.10.98. Ismail Jaffer Nathoo, age 28, died of plague at Dongri.

12.11.98. Ali Ibrahim Mahomad, age 20, died of plague at Dongri.

17.11.98. Jan Mahomed Ramji, died of plague at Bhendibazar (died at hospital.)

15.12.98. Shivji Nanji Visram, age 25, died of plague at Pala Lane.

NON-INOCULATED. DIED OF DISEASES.

23.4.98. Fatma, wife of Abdala Ismail, age 28, died of consumption at Durga Muhalla.

24.4.98. Alibhai Hassan Khimji, age 20, died of consumption at Kandi Muhalla.

25.4.98. Aloo Meraly's son, age six months, died of fever at Mazagaon.

25.4.98. Lale Soonar's daughter, age 4 months, died of convulsion at Dongri Street.

26.4.98. Virbai, wife of Virji Haboo, age 70, died of consumption and sore throat at Palla Gali.

27.4.98. Minbai, wife of Ravji Sanga, age 55, died of heart disease at Durga Muhalla.

27.4.98. Wala Devji's son, age 1½ years, died of fever at Kharak.

— Dhala Lachoo's son, age 1 month, died of convulsion at Durga Muhalla.

28.4.98. Piarally Abdala's son, age 1½ years, died of consumption at Kharak.

28.4.98. Mitha Premji's brother's daughter, age 1 year, died of convulsion at Durga Street.

29.4.98. Mirza Ali Khan Mahomad, age 30, died of consumption at Durga Muhalla.

29.4.98. Ismal Meraly's son, age 4 months, died of convulsion at Durga Street.

30.4.98. Goolam Hoosen Mahomad Versi, age 4 months, died of fever at Dongri.

1.5.98. Mahomadbhoy Ravji, age 40, died of heart disease at Khoja Molla.

3.5.98. Kasam Jan Mahomad's daughter, age 1 month, died of convulsion at Durga Street.

4.5.98. Fazal Noor Mahomad's grand-daughter Mariam, age 4 months, died of convulsion at Kharak.

4.5.98. ? Sherally Pirbhais' son Jainatabdin, age 3 years, died of convulsion at Durga Muhalla.

— Alibhai Karmally Soorji, age 46, died of consumption at Khoja Muhalla.

5.5.98. Sonbai, wife of Walji Ramji, age 60, died of consumption at Durga Muhalla.

— Khatijabai, daughter of Ladha Remtala, age about 6 months, died of cough at Dongri.

7.5.98. Lootafally Karmally's daughter, age about a month, died of diarrhœa at Durga Street.

7.5.98. Karmally Fazal, age 98, died of asthma at Israel Street.

10.5.98. Kasam Nanji, age 45, died of consumption at Durga Muhalla.

10.5.98. Suleman Rahim's wife, age 35, died of consumption at Durgadevi.

11.5.98. Abdala Ladha Chatoo's daughter Sakina, age 12 months, died of convulsion at Pala Lane.

12.5.98. Motibai Kanji, age 22, died of consumption at Kandi Muhalla.

13.5.98. ? Goolam Hoosen Ebram Walji, age 1½ years, died of fever at Mazagaon.

14.5.98. Aladin Kasam Khimji, age 40, died of liver complaint at Dongri.

14.5.98. Karim Panchan's wife Aval, age 32, died of consumption at Mazagaon. *Once inoculated.*

14.5.98. Chatoo Visram, age 70, died of consumption at Durga Street.

14.5.98. Abdala Fazal, age 15, died of fever at Durga Street.

15.5.98. Mahomad Bhagbaria, age 35, died of paralysis at Palagali.

16.5.98. Fatma, daughter of Ragooji Jamal, died of convulsion at Durga Street, age 72.

16.5.98. Dost Mahomad Hassan Noor Mahomad, age 5, died of fever at Moonda Gali.

18.5.98. Sakinabai, grand-daughter of Jessa Premji, age 14, died of brain fever at Durga Street.

18.5.98. Sakina, wife of Fazal Noor Mahomad, age 24, died of delivery at Kharak.

20.5.98. Mother of Noor Mahomad Khatri, age 75, died of fever at Darga Muhalla.

23.5.98. Aba Moloo, age 54, died of asthma at Durga Street.

23.5.98. Fazal Ladha Lalji, age 40, kidney disease at Khoja Muhalla.

24.5.98. Lalbai, wife of Ibram Sheriff, age 62, died of consumption at Durga Street.

24.5.98. Ibram, son of Remtula Koorji, age 9, died of asthma at Duncan Road.

25.5.98. Alarakhia Thaver, age 54, died of consumption at Kharak.

25.5.98. Mahomad Vally Jessa, age 2 years, died of convulsion at Durga Street.

26.5.98. Abdala grandson of Haji Sivji, age 12 months, died of convulsion at Durga Street.

26.5.98. Alarakhia Kanji's grandson, age 6 months, died of convulsion at Durga Street.

27.5.98. Hoosen Ali Lutafally of Kamria Mahomad Chotti, age 26 died of consumption at Dongri Street.

29.8.98. Wife of Premji Gangji Soolbai, age 60, died of diarrhœa at Pala Gali.

31.5.98. Grand-daughter of Khaki Radamsi, age 2 years, died of convulsion at Kharak.

3.6.98. Daughter of Fatehally Thaver, Bai Sherbanoo, age 12, died of consumption at Furniture Bazar.

3.6.98. Hoosen Alarakhia Jivray, age 7 months, died of convulsion at Khoja Street.

4.6.98. Bacheebai, daughter of Ismal Suji, age 4 months, died of diarrhœa at Dongri Street.

4.6.98. Jafferaly Aladin Kassam, age 2½ months, died of convulsion at Dongri Street.

6.6.98. Fazal Ibram's wife Jainab, age 39, died of fever at Khoja Street.

7.6.98. Fatma, wife, daughter-in-law of Premji Paroo, age 15, died of consumption at Khoja Street.

7.6.98. Nanbai, wife of Soomar Rajpar, age 60, died of diarrhœa at Dongri.

10.6.98. Bachoo, grandson of Mahomed Karim, age 8 months, died of convulsion at Byculla.

11.6.98. Kasam Hassan Mawjee, age 1 year, died of fever and convulsion at Umarkhadi.

12.6.98. Lowjee Canjee, age 22, died of ? at Kolaba.

14.6.98. Abdul Husen Mahomed Ibrahim Bhuloo age 1 year, died of convulsion at Khoja Street.

15.6.98. Mariam, daughter of Nowroj Lakha, age 7 months, died of convulsion at Dongri Street.

18.6.98. Jaffer Ismail Mahomed, age 8 months, died of fever at Durga Street.

18.6.98. Goolamalli Hoosen Ladak, age 5 months, died of convulsion at Dongri Street.

20.6.98. ? Dhanbai, daughter of Mahomed Jootha, age 12, died of consumption at Kandi Muhalla.

20.6.98. ? Moloo Mahomed Ladak, age 30, died of consumption at Kharak.

23.6.98. ? Fatma, wife of Lale Mahomed Ibrahim, age 28, died of consumption at Khoja Street.

23.6.98. ? Sakina, daughter of Lowji Daya, age 12, died of consumption at Dongri.

24.6.98. Nanjee Valji's son, age 8 months, died of fever at Umarkhadi.

24.6.98. Sheriff Dossa's daughter, age 3 months, died of convulsion at Kandi Muhalla.

27.6.98. Sherbano Rahim Assar, age 8 years, died of diarrhœa at Dongri Bazar.

28.6.98. ? Nanbai, wife of Jiva Soomar, age 48, died of consumption at Durga Street.

28.6.98. Joosab Karmali Haji Boga, age 10 months, died of convulsion at Khoja Street.

30.6.98. ? Daughter of Rajabally Jan Mahomed, age one week, died of ? at Kandi Muhalla.

3.7.98. Ahmed Gangji, age 25, died of consumption at Old Nagpada.

3.7.98. ? Fatma, grand-daughter-in-law of Simji, age 25, died of consumption at Market.

4.7.98. Khakoo Peerbhoy, age 65, died of liver complaint at Khoja Street.

6.7.98. ? Mahomed Khulphan's wife Walbai, age, age 28, died of consumption.

6.7.98. Mahomed Lowjee Visram's grandson, age 4 months, died of ? at Dongri.

6.7.98. ? Grand-daughter of Khakhoo Laljee, age 3 years, died of convulsion at Durga Street.

9.7.98. ? Rhematbai, wife of Dhanji Jairaj, age 60, died of consumption at Fhosabazar.

10.7.98. Daughter of Mahomed Jaitha, age 4 months, died of convulsion at Kandi Muhalla.

12.7.98. Goolam Hoosen Dharamsi Damji, age 15, died of ? at Pala Lane.

12.7.98. Bachoo Dhala Jivraj, age 15 days, died of convulsion at Kharak.

13.7.98. Jamal Jiva, age 45, died of ? at Dongri Street.

13.7.98. ? Koorbai, mother of Jan Mahomad Magji, age 85, died of fever at Khoja Street.

13.7.98. ? Fatma, grand-daughter-in-law of Bana Lila, age 24, died of fever at Kharak.

13.7.98. Bachi Allarakhia Virjee, age 2 months, died of purge at Byculla.

13.7.98. ? Mahomed Rowjee Aloo, age 2½ years, died of asthma at Durga Street.

14.7.98. Serenbai, daughter of Ladha Thaver, age 12, died of ? at Pala Lane.

15.7.98. ? Mongibai, wife of Karmali Jamal, age 16, died of consumption at Durga Street.

16.7.98. ? Haji Ramjanalli, age 14 months, died of fever at Bhendibazar.

16.7.98. Bachoo Fazal Alidina, age 6 months, died of convulsion at Umarkhadi.

18.7.98. Janiab, grand-daughter of Nanji Bhanji, age 6 months, died of fever at Dongri.

19.7.98. ? Fatma, daughter-in-law of Ladak Rahim, age 17, died of consumption at Khoja Street.

19.7.98. Mahomad Khimji's son, age 23 days, died of diarrhœa at Pala Bazar.

20.7.98. Daughter of Gangji Walji, age 3 months, died of convulsion at Dongri.

21.7.98. ? Nanbai, mother of Hassanally Nanji, age 45, died of asthma at Kharak.

25.7.98. ? Rambai, wife of Hoosenally Nanji, age 16, died of delivery at Kharak.

26.7.98. Manbai, wife of Remtula Kadar, age 60, died of sore and asthma at Durga Street.

26.7.98. ? Sonbai, wife of Haji Visram, age 30, died of consumption at Khoja Street.

26.7.98. ? Fazal Khaki, age 5, died of asthma at Kharak.

29.7.98. Sakar, daughter of Hoosen Mamuk, age 1 year, died of diarrhœa at Durgamolla.

27.7.98. ? Fazal Noormahomed Jan Mahomed, age 21, died of brain fever at Pala Lane.

27.7.98. ? Fatma, daughter of Kanji Jivraj, age 10, died of asthma at Kharak.

1.8.98. ? Nanji Khakoo, age 65, died of fever at Dongri.

2.8.98. ? Nasar Manji's daughter, age 2 years, died of convulsion at Durga Muhalla.

3.8.88. ? Ismail Meraly's wife, Jainab, age 21, died of delivery and consumption at Durga Street.

3.8.98. Moosa Hassan, age 62, died of prolonged illness of asthma and consumption at Dongri.

3.8.98. Ahmadally Goolamally Goolamhusen, age 1 year, died of asthma at Dongri.

3.8.98. Virji Goolamsen's daughter, age 6 months, died of convulsion at Dongri.

3.8.98. Sakina, daughter of Ladba Daraj, age 12 months, died of fever at Kharak.

3.8.98. Sakina, daughter of Daya Kara, age 1 year, at Dadar.

4.8.98. Nensi Nathoo's daughter, age 20 days, died of diarrhœa at Kharak.

8.8.98. Visram Ooka's son, Jooma, age 2½ years, died of convulsion at Kandi Muhalla. *Once inoculated.*

9.8.98. Dhala Vanya's wife, age 42, died of Nasove at Dongri.

9.8.98. Moolji Ravji, age 70, died of fever at Furniture Bazar.

11.8.98. Kasam Bhimji, age 56, died of (?) at Dongri.

11.8.98. ? Dewji Jaitha, age 65, died of fever at Kharak.

13.8.98. ? Kasam Bhimji's grandson, age 1½ years, died of ? Durga.

13.8 98. ? Jooma Merali's son, age 5 years, died of fever at Durga Muhalla.

16.8.98. ? Sonbai, wife of Versi Merali, age 60 years, died of fever and asthma at Durga Muhalla.

21.8.98. ? Hasan Hemraj, age 30, died of ? at Jacob Circle.

22.8.98. ? Sajan Goolam Hoosen's mother, Rataribai, age 70, died of asthma at Pala Lane.

23.8.98. Hasan Alloo Ibrahim, age 37, died of consumption at Dongri.

24.8.98. Hasan Meheralli's son, age , died at Mazagaon.

24.8.98. Ismail Rhemoo's son, age 8, died at Grant Road.

24.8.98. ? Hoosen Moolji Rahim, age 14 months, died of asthma at Durga Street.

24.8.98. Sonbai Karim Nanjee's grand-daughter-in-law, age 16, died of fever at Kharak.

27.8.98. ? Jainab, daughter of Noormahomed Valji, age 8, died of convulsion at Furniture Bazar.

28.8.98. Manick Virji, age 60, died of diarrhœa at Furniture Bazar.

30.8.98. Jooma Dharamsi, age 40, died of sore at Durga Muhalla.

31.8.98. ? Sakina Mahomed Anund, age 2 years, died of fever at Durga Muhalla.

31.8.98. Bachoo, son of Bakhsi Shariff, age 1½ months, died of diarrhœa at Kharak.

1.9.98. ? Kaloo, father of Kala, age 75, died of consumption at Jaamat Khana.

1.9.98. Wife of Jan Mahomed Versi, age 25, died of consumption at Kharak.

1.9.98. Mariambai, wife of Kasam Goolam Hoosen Dinani, age 34, died of delivery at Dongri.

4.9.98. Sakina Kasam Somji, age 1½ years, died of convulsion at Kharak.

5.9.98. Koolsan Jairaj Lakhamsi, age 5 months, died of indigeston at Market.

6.9.98. ? Rajabally Sharriff Moloo, age 23, died of consumption at Kharak.

8.9.98. Pirbai Versi, age 42, died of consumption at Durga Street.

9.9.98. Hoosen Khimji Goolam Hoosen, age 35, died of consumption at Dongri (residing at Vasur).

9.9.98. ? Meherali Karim, age 56, died of asthma at Dongri.

9.9.98. ? Fatma Kasam Thaver, age 3 years, died of consumption at Kandi Muhalla.

10.9.98. Gangbai, wife of Hassan Bhaloo, age 64 years, died of (?) at Khoja Street.

11.9.98. Issa Kasam Bhimji, age 14 years, died of consumption at Dongri.

11.9.98. ? Virji Kala, age 65, died of consumption at Kandi Muhalla.

13.9.98. Siri, grand-daughter of Pira Bana, age 10 months, died of diarrhœa at Kharak.

15.9.98. Sathbai, daughter of Meheralli Dewsi, age 12, died of consumption at Old Nagpada.

18.9.98. ? Jainab, wife of Somji Lalji, age 34 years, died of consumption at Pala Lane.

21.9.98. Alli Jairaj, age 52, died of fall from accident on board of steamer, residing at Kharak.

19.9.98. ? Kara Koreji, age 80, died of asthma at Byculla.

25.9.98. Ramji Dada Danji, age 3, died of diarrhœa at Palki Muhalla.

25.9.98. Sakina, grand-daughter of Rahimtula Dewji, age 3 years, died of ? at Lalwadi.

25.9.98. ? Fatma, daughter of Pardhan Haji, age 16, died of consumption at Dongri.

28.9.98. ? Sakina, daughter of Alla Jivraj, age 14, died of consumption at Umarkhadi.

29.9.98. Fatma, daughter of Sachoo Valli, age 11, died of consumption at Kharak.

1.10.98. Fatma, wife of Ali Lakhamsi, age 32, died of delivery at Furniture Bazar.

1.10.98. Sakina, daughter of Jan Mahomad Harji age 8, died of diarrhœa at Pala Lane.

5.10.98. ? Gagan Nenji, age 72, died of asthma at Durga Street.

5.10.98. ? Lalji, age 2 years, died of convulsion at Durga Street.

6.10.98. ? Jainabai, daughter of Goolamhusen Mooraj, age 14, died of asthma at Dongri.

6.10.98. Bagbai, wife of Meraly Premji Jivraj, age 25, died of delivery at Kharak.

6.10.98. Sivinbai, daughter of Magji Taija, age 10 years, died of fever at Kharak.

6.10.98. Noor Mahomad Chagle, age 60, died of paralysis at Kharak.

7.10.98. Ali Rahim, age 48, died of consumption at Kharak.

7.10.98. Nathibai, age 65, died of fall from window at Dongri.

12.10.98. Hassan Goolam Hoosani Mitha, age 18, died of ? at Durga Street.

13.10.98. Hoosani Sachoo Habib, age 1 year, died of fever at Kharak.

14.10.98. Megbai, wife of Bhanji Bhunji, age 18, died of asthma at Durga Street.

17.10.98. Abdoola Mahomad Khoja, age 28, died of diarrhœa at Durga Street.

19.10.98. ?Sakina, daughter of Khimji Sajan, age 11, died of fever at Kharak.

19.10.88. ?Remat, wife of Mahomad Bhanji, age 24, died of consumption at Darga Mohola.

19.10.98. ?Remat, wife of Nanji Kalian, age 42, died of ? at Durga Street.

20.10.98. Nathoo Karnu's son, age 2, died of fever at Palagali.

21.10.98. ?Suleman Pirbhai Sadoo, age 25, died of fever at Dongri.

21.10.98. Kachra Rhemu Premji, age 2½ years, died of fever at Furniture Bazar.

22.10.98. ?Nanbai, wife of Hassan Jaffer, age 17, died of delivery at Dongri Street.

26.10.98. Jaina, daughter of Kara Ratansy, age 6 months, died of fever at Durga Street.

26.10.98. Sakina, daughter of Walji Punju, age 1½, died of consumption at Kandi Muhalla.

28.10.98. ?Fatma, wife of Fazal Rhemtala Walji, age 24, died of fever at Durga Muhalla.

29.10.98. ?Jairaj Ladha, age 65, died of brain fever and asthma at Kharak.

29.10.98. Kulsam, daughter of Fazal Alibhoy, age 6 months, died of consumption at Dongri, residing at Koorla.

30.10.98. Satbai, wife of Jaffer Soomar, age 25, died of suicide by drowning herself in a fountain, residing at Durga Muhalla.

1.11.98. Fatma, daughter of Gella Bhaiji, age 2 years, died of fever at Dongri.

5.11.98. Goolamally Mahomad Mirza Bhaloo, age 1¼ years died of diarrhœa at Khoja Muhalla.

8.11.98. Kulsam, grand-daughter of Suji Hassan, age 3 months, died of convulsion at Furniture Bazar.

9.11.98. ?Merally Hassan Kallian, age 23, died of consumption at Umarkhadi.

9.11.98. Pir bhai Noor Mahomad Virji of Mahim, age 35, died of liver at Falkland Road.

13.11.98. ?Goolamhusen Merally, age 12 months, died of fever at Dongri.

14.11.98. ?Sakinabai, daughter of Anund Moorji, 2¼ years, died of fever at Mazagaon.

14.11.98. ?Ramjan Goolam Hoosen Mahomed, age 35, died of fever at Durga Street.

16.11.98. ?Janbai, wife of Shivji Manek, age 73, died of asthma at Khoja Muhalla.

19.11.98. ?Rattanbai, mother of Canji Coorji, age 65, died of asthma at Kharak.

21.11.98. Nagji Valji's son, Ahmed, age 6 months, died of fever at Umarkhadi.

21.11.98. ?Virbai, wife of Nanji Moolji, age 58, died of consumption at Durga Street.

22.11.98. Hassam Nathoo Moolji's son, Bachoo, age 7 months, died of fever at Palagali.

23.11.98. Ahmeed Allimahomed Shariff, age 17, died of convulsions at Market.

27.11.98. Ahmedalli, son of Goolam Hoosen Ismail, age 9 months, died of convulsion at Durga Street.

28.11.98. Ladha Munji, age 25, died of wound on foot at J. J. Hospital (he came from Mahim).

29.11.98. Dawood Janmahomed's son, Suleman, age 1¼ years, died of fever at Kharak.

30.11.98. Minbai, wife of Juvan Munji, age 85, died of asthma at Dongri.

30.11.98. Hoosen Nensi Kheraj's son, Suleman, age 1¼ years, died of fever at Kharak.

3.12.98. Ladak Megji's son, Aboo Mahomed, age 5 months, died of vomit and diarrhœa at Kharak.

6.12.98. Bai Lalbai, wife of Somar Nensi, age 65, died of fever and diarrhœa at Furniture Bazar.

8.12.98. Sherally, son of Valji Soomar, age 4 months, died of fever and diarrhœa at Kandi Muhalla.

8.12.98. Jivraj, son of Alli Mahomed Virji, age 2 years, died of fever at Khoja Muhalla.

8.12.98. A maid servant of Bibi Gohar, died and buried in our Durga.

12.12.98. ?Ahmed Lakha, age 65 years, died of asthma at Durga Street.

12.12.98. Bibi Gohar, age 75, died of asthma at His Highness Aga Khan Mazaz, *not* buried in our Durga.

13.12.98. ?Meralli Mahomed Gangji, age 30 years, died of asthma at Durga Street.

15.12.98. Jan Mahomed Gangji's daughter, age 1 month, died of convulsions at Kharak.

16.12.98. Visrani Moolji's daughter, age 1 day, died of ? at Dongri.

17.12.98. Alarakhia Nathoo, age 62, died of asthma at Mahalakshmi (his city address, Pala Bazar).

18.12.98. ?Sonbai, wife of Haji Jiva, age 40 years, died of consumption at Furniture Bazar.

18.12.98. Dost Mahomed Pirbhai's son, age 22 days, died of convulsions at Dongri Street.

20.12.98. ?Sonbai, wife of Fazal Virji, age 18 years, died of delivery at Durga Muhalla.

20.12.98. ?Visram Allana, age 62 years, died of fever at Palki Muhalla.

20.12.98. Daughter of Fazal Virji was delivered, *still-born.*

21.12.98. Jooma Shamji's son, Ranjoo, age 14 months, died of fever at Kandi Muhalla.

21.12.98. Bachooli, daughter of Fazal Dharamsi Poonja, age 3 years, died of cold at Khoja Street.

APPENDIX No. LV. (i.).

REPORT

BY

Dr. KHAJA ABDULLA,

REGARDING

THE KHOJA HOSPITAL, BOMBAY,

WITH

A LIST OF INOCULATED PERSONS TREATED IN IT.

I.

From the Secretary to the Indian Plague Commission to Dr. Khaja Abdulla, Khoja Hospital, Bombay, dated India Office, 28th July, 1899.

Sir,

I am directed to invite your attention to my letter, No. 969, dated March 27, 1899, to which no reply has as yet been received. The Commissioners will be obliged if you will send as early as possible, and if possible by next mail, to me at the India Office, London, the particulars you were asked to supply regarding anti-plague inoculation among the Khojas, viz.:—

(1) A list of those treated at the Khoja Hospital, showing the names of the inoculated Khojas who have had plague, giving the number and date of the inoculation of each person, the date of attack and admission, and the result of the disease, whether fatal or not; and

(2) The total number of uninoculated Khojas who have had plague, together with the mortality among them.

II.

To the Secretary, Indian Plague Commission, India Office, London.

Sir, Bombay, 18th August, 1899.

I have the honour to inform you that on the 15th of April last, I handed over to the late Khan Bahadur Mahomedbhoy Ebrahim, the founder of the Khoja Plague Hospital, as usual, to be sent to you, my report, together with the statistics about the inoculated and non-inoculated patients treated in the hospital. On the death of the founder of the hospital it was discovered that the said report was not sent to you, and the lawyers of the representatives of the deceased, at my request, returned to me the same on the 14th instant, a copy of which I beg to forward to you.

I have, &c.
Khaja Abdulla.

III.

To the Secretary, Indian Plague Commission.

Mahomedbhoy Ebrahim Plague Hospital for Khojas.
Bombay, 15th April, 1899.

Sir,

In reply to your letter dated 27th March 1899, I have the honour to submit the particulars about the patients treated in this hospital, both inoculated and non-inoculated.

Since the opening of the hospital on the 26th of April 1897 to 5th April 1899, 347 plague patients were treated in the hospital, out of which 62 cases were inoculated. Some of the patients produced certificates of inoculation, others did not produce any certificate, but the relatives and guardians of the patients declared the information of the fact of their being inoculated. The list herewith annexed gives all particulars in our possession regarding cases of inoculated persons. Of the 62 inoculated cases treated for plague, 24 proved fatal, 33 recovered, and 5 remained under treatment up to the 5th April, 1899.

Since the opening of the hospital, up to the 15th January, 1898, 77 patients, both inoculated and non-inoculated, were treated with ordinary mercurial and stimulant treatment, out of which 48 died, and 29 recovered. From the 15th January 1898, to the 5th April 1899, 209 non-inoculated patients were treated with native Indian drugs and occasional doses of stimulants, out of which 84 were discharged cured, 12 were still under treatment on the 5th April, 1899, and 113 died.

Of the total number of cases treated, two were treated by Dr. Galeotti with Lustig's curative serum, both of which proved fatal.

I have, &c.
Khaja Abdulla.

IV.

List of Inoculated Patients treated in the Khan Bahadur Mahomedbhoy Ebrahim Khoja Plague Hospital Bombay, from the Date of its Opening, *i.e*, 26th April 1897, up to 5th April 1899.

No.	Name.	Age.	Sex.	Residence.	Date of Inoculation.	Date of Attack.	Date of Admission.	Date of Death.	Date of Discharge.	Remarks.
1	Goolamhosen Mherally.	Years. 7	Male	Durga Muhalla: Carmally Soorjee's house.	21.4.97	1.1.98	7.1.98	16.1.98	—	According to the statement of his guardians, he was inoculated about 1½ months ago.
2	Mahomed Vishram -	22	Male	Durga Muhalla, 255 -	7.1.98	14.1.98	15.1.98	16.1.98	—	——
*2	Tejbai Mahomed -	23	Female	Palki Muhalla: Mowjee Gheewala's house.	—	10.1.98	13.1.98	16.1.98	—	Inoculated about 1½ months ago, according to the statement of her husband.
3	Fazal Visram -	12	Male	Palki Muhalla: Piroo Mahomed's house.	—	12.1.98	14.1.98	—	29.1.98	Inoculated twice, according to the statement of his father.
4	Hosenally Munjee -	6	Male	Chibra Gali: Pakmodia Street.	5.1.98	13.1.98	15.1.98	—	1.2.98	——
5	Mahomed Visram -	20	Male	Durga Muhalla: Jooma Moosas' house.	6.1.98	14.1.98	15.1.98	16.1.98	—	Inoculated eight days ago according to the statement of his guardians.
6	Goolamally Dossa Cassum.	6	Male	Tantanpura Street: Mooljee Jeewraz's house.	17.1.98	7.2.98	8.2.98	—	21.2.98	Inoculated 20 days ago.
7	Jooma Visram -	21	Male	Palki Muhalla: Ladha Ebram's house.	—	24.2.98	26.2.98	—	31.3.98	Inoculated twice; once by Dr. Yersin's serum last year, and once by Prof. Haffkine's serum this year, about 1½ months ago.
8	Hassun Poonja -	16	Male	Parel - -	10.1.98 26.2.98	27.2.98	28.2.98	—	15.3.98	The patient has got a certificate.
9	Cassum Jeewa Premjee.	12	Male	Durga Muhalla: Carim Munjee's house	26.1.98	28.2.98	1.3.98	—	10.3.98	Inoculated twice. Has got a certificate. Last date of inoculation, 26.1.98.
10	Poorbai, wife of Poonja.	50	Female	Parel - -	5.2.98	1.3.98	7.3.98	9.3.98	—	——
11	Khadija, daughter of Barmal Poonja.	10	Female	Parel - -	5.2.98	1.3.98	8.3.98	—	15.3.98	——
12	Ally Sachoo Allarakia.	8	Male	Nishanpada Cross Lane.	16.2.98	12.3.98	13.3.98	—	22.3.98	——
13	Vally Mahomed Carim.	20	Male	Love Lane: Mazagaon: H.H., Aga Khan's bungalow.	19.1.98	16.3.98	24.3.98	—	12.4.98	——
14	Fazal Meghjee -	10	Male	Durga Muhalla: Carmally Sorjee's house.	30.12.97 7.2.98	23.3.98	27.3.98	—	23.4.98	Inoculated twice. Has got a certificate.
15	Jaffer Walljee Nathoo.	12	Male	Durga Muhalla: Carmally Soorjee's house.	6.1.98 7.2.98	26.3.98	31.3.98	—	12.4.98	Inoculated twice. Has got a certificate.
16	Soanbai, wife of Visram Hurjee.	25	Female	Dongri, Mahomed Rowjee's chawl.	1.3.98	2.4.98	4.4.98	—	18.4.98	Inoculated twice. Last date, 1.3.98.
17	Kallo Momjee -	35	Male	Nesbit's Lane, H. H. Aga Khan's bungalow.	—	6.4.98	9.4.98	—	30.4.98	According to the statement of the brother of the patient, the patient was inoculated about 1½ months previous to attack.
18	Carmally Lootafally	9	Male	Kharak: Ebram Peerbhoys' chawl.	2.2.98 16.2.98	25.4.98	26.4.98	28.4.98	—	Inoculated twice. Has got a certificate.
19	Nanjee Teja - -	19	Male	No. 13, Nishanpada, Cross Lane.	March 1898	1.8.98	4.8.98	5.8.98	—	——
20	Mahomed Warind -	35	Male	No. 416, Parel Road -	27.1.98	1.8.98	10.8.98	12.8.98	—	Inoculated twice in 1897; and twice in 1898. The first date in 1897 is 27.1.98, as per certificate.
21	Hosen Noormahamed.	18	Male	No. 13, Nishanpada Lane.	—	11.8.98	13.8.98	15.8.98	—	Inoculated once in 1898.
22	Noorbai, widow of Jina Damjee.	42	Female	Palagali: Memon's house.	—	13.8.98	18.8.98	—	4.9.98	Inoculated once four days before the sun eclipse.
23	Moloo Jamal - -	18	Male	Mahim - -	30.3.98 7.4.98	24.8.98	25.8.98	—	12.9.98	——
24	Alibhoy Veerjee Khimjee,	10	Male	Kharak, D. C., Charitable home.	.2.98	7.9.98	10.9.98	—	24.9.98	Inoculated once in Ramzan (Feb.).
25	Foolbai, widow of Canjee Jivraj.	35	Female	Do. - -	—	8.9.98	10.9.98	—	1.10.98	Inoculated twice.
26	Noormahomed Thariff.	19	Male	Hasen Califa Street -	7.9.97 18.3.98	15.9.98	18.9.98	—	8.10.98	——
27	Vallymahomed Carim.	14	Male	No. 1-6, Tandel Street	Jan. or Feb. 1898	24.9.98	27.9.98	28.9.98	—	Inoculated either in January or February 1898, according to the statement of his guardian.
28	Mongee, wife of Noormahomed Veerjee.	13	Female	No. 184, Samuel Street	Jan. 1898	28.9.98	30.9.98	—	28.10.98	Inoculated once only in about January.
29	Dhunjee Laljee Visram.	36	Male	No. 386, Samuel Street	—	28.9.98	1.10.98	—	4.11.98	Inoculated twice in 1898.
30	Esmal Jaffer Nathii	37	Male	No. 526, Dongri Street	Dec. 1897	17.10.98	21.10.98 1.30 p.m.	21.10.98 11.30 p.m.	—	According to his statement, he was inoculated once only in December 1897.
31	Hoorbai, wife of Ramjee Janmahomed.	30	Female	No. 12-20, Dongri Street.	Jan. 1898	1.11.98	3.11.98	—	15.11.98	Inoculated in January 1898.
32	Allymahomed Poonja	12	Male	No. 616, Parel - -	—	10.12.98	12.12.98	—	12.1.99	Inoculated twice in 1898.
33	Bhanjee Alibhoy -	14	Male	No. 95, Chinch Bunder Road.	—	30.12.98	1.1.99	3.1.99	—	Inoculated once only in 1898.
34	Panbai, wife of Mooradally.	28	Female	No. 109, Tandel Street	—	1.1.99	7.1.99	—	22.1.99	Inoculated once only about 12 months ago.
35	Sherbannoo Ebram Veerjee.	13	Female	No. 618, Dongri Street	30.12.97 10.1.98	16.1.99	19.1.99	—	19.2.99	——
36	Fatma, wife of Ebram Veerjee.	33	Female	Do.	—	18.1.99	20.1.99	—	3.2.99	Inoculated thrice in 1898.
37	Verbai, widow of Ebram Ahmed.	6	Female	Do.	Jan. 1898	17.1.99	20.1.99	21.1.99	—	Inoculated in January, 1898.
38	Mariam Thaver Jivroj.	48	Female	No. 8, Kitchen Garden Street.	28.2.98	30.1.99	31.1.99	—	6.3.99	Inoculated twice; last date being 28.2.98 (*vide* certificate).

* Doubtful. The husband of the deceased said she was inoculated in Khoja Muhalla by a Parsee doctor. Professor Haffkine says she was not inoculated.

No.	Name.	Age.	Sex.	Residence.	Date of Inoculation.	Date of Attack.	Date of Admission.	Date of Death.	Date of Discharge.	Remarks.
		Years.								
39	Carmally or Carim Goolamhosein.	18	Male	No. 3, Bhandori Street	—	28.1.99	1.2.99	—	6.3.99	Inoculated once last year, according to his statement.
40	Jaffer Bhanjee Peonja.	47	Male	579-81, Dongri Street	—	7.2.99	2.2.99	11.2.99	—	Inoculated twice in 1898.
41	Jainab Ebram Mahomed.	8	Female	Do.	—	8.2.99	10.2.99	13.2.99	—	Inoculated twice in 1898.
42	Mahomed Dawood Dator.	25	Male	Nil.	—	7.2.99	11.2.99	12.2.99	—	Inoculated once in 1898.
43	Allymahomed Ladha	20	Male	Tantanpura Street	—	9.2.99	12.2.99	—	6.3.99	Inoculated once last year.
44	Nanbai, widow of Jamal Warin.	55	Female	46-48, Jail Road	—	10.2.99	14.2.99	—	6.3.99	Inoculated twice about eight months ago, according to her statement.
45	Avalbai, wife of Remtoola Cassum.	20	Female	9-11, Chuckla Street	—	13.2.99	15.2.99	16.2.99	—	Inoculated once last year, according to the statement of her husband.
46	Alunjee Ramjee Lakha.	35	Male	74, Chinch Bunder	—	18.1.99	20.2.99	23.2.99	—	Inoculated once last year, according to the statement of his relations.
47	Goolamhosen Walljee.	8	Male	Mazagaon	—	24.2.99	25.2.99	—	12.3.99	Do.
48	Sakina, wife of Muradally Jooma.	20	Female	No. 29, Nishanpada Cross Road.	—	2.3.99	4.3.99	—	19.3.99	Do.
49	Goolamhosen Mahomed Dewjee.	6	Male	Kamatipura 5th Lane	—	4.3.99	8.3.99	—	22.3.99	Inoculated twice last year, according to the statement of his guardians.
50	Veerbai, widow of Ebram Sewjee.	28	Female	118, Nishanpada Road	—	7.3.99	9.3.99	12.3.99	—	Inoculated twice about five months ago.
51	Sarabai Mahomed Remtoola.	10	Female	Chinch Puckli	—	6.3.99	9.3.99	11.3.99	—	Inoculated once last year.
52	Jainabai Mahomed Veerjee.	9	Female	No. 594, Dongri Street	—	6.3.99	9.3.99	—	—	Under treatment. Inoculated three times last year, according to the statement of her guardians.
53	Ramjee Teja Dhunjee.	33	Male	No. 46-48, Jail Road	4.3.99	11.3.99	11.3.99	—	—	Under treatment. Inoculated once in 1898, and again in 1899, on 4th March.
54	Remat Bai, widow of Meherally Bhimjee.	35	Female	No. 1-2, Jail Road	—	12.3.99	15.3.99	16.3.99	—	Inoculated twice in 1898.
55	Nazarally, son of Hassum Poonja.	12	Male	Mahatar Pakhdy	—	18.3.99	20.3.99	—	4.4.99	Inoculated once only, about 1½ months ago.
56	Bachoo Jamal	16	Male	No. 1-6, Tandel Street	—	18.3.99	20.3.99	25.3.99	—	Inoculated twice in 1898, according to his statement.
57	Jetbai Pardhan Ladha.	10	Female	245-47 Nishanpada	—	19.3.99	21.3.99	—	—	Under treatment. Inoculated twice in 1898.
58	Poorbai Goolamhosen Dossa.	6	Female	185, Nishanpada Road	—	22.3.99	25.3.99	29.3.99	—	Inoculated once in 1898, according to the statement of her guardians.
59	Cachra Shariff	42	Male	Lohargali, Kharak	—	25.3.99	28.3.99	3.4.99	—	Inoculated once last year, as stated by him and his relations.
60	Jainabai Meherally Laljee.	7	Female	246, Samuel Street	—	28.3.99	29.3.99	30.3.99	—	Inoculated once last year.
61	Fatma Datoo Vally	9	Female	526, Dongri Street	—	29.3.99	1.4.99	—	—	Under treatment. Inoculated three times last year.
62	Dosan Nathoo	46	Male	No. 5, Love Lane: H. H. Aga Khan's bungalow.	28.3.99	2.4.99	4.4.99	—	—	Under treatment.

15th April, 1899.

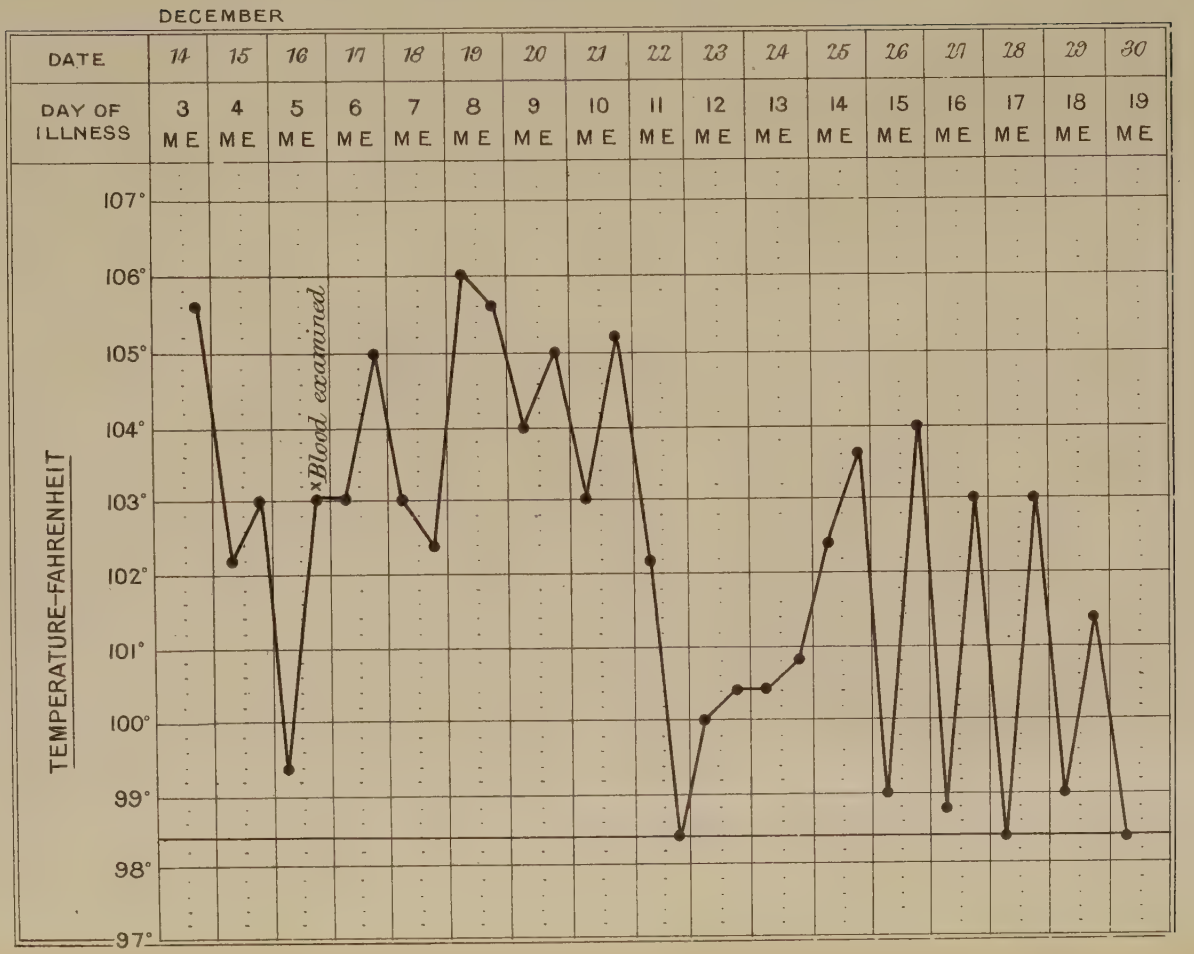
KHAJA ABDULLA.

(*See* Question, No. 18,913).

CASE OF PLAGUE SEPTICÆMIA.

THE ORGANISM RECOVERED FROM THE BLOOD ON 5TH DAY

CONVALESCENCE AND RECOVERY OF PATIENT



POONA PLAGUE HOSPITAL.

December, 1897.

Male, aged 40, small bubo left femoral region. Patient under a pronounced toxæmia on admission. Pulse feeble. Later (6th day) became semi-comatose and lung showed involvement on 5th day.

Small quantity of blood from vein at back of hand removed by pipette and inoculated on to agar. Four tubes inoculated, each tube yielded colonies of the plague micro-organism within 48 hours' incubation at 37°C. Characteristic broth cultures obtained. A quantity of 24 hours' old broth culture (·25c.c.) inoculated into a rat caused death of animal within 48 hours. Cultures of the plague micro-organism obtained from the blood of the rat.

Crisis on 11th day followed by fever of suppuration up to 19th day. After this slowly convalesced, and was dismissed well 45 days after admission.

E. L. MARSH.

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(*See* Question, No. 18,913).

CASE OF PLAGUE SEPTICÆMIA.

THE ORGANISM RECOVERED FROM THE BLOOD ON 6TH DAY

CONVALESCENCE AND RECOVERY OF PATIENT

POONA PLAGUE HOSPITAL.

January–February, 1898.

Male, aged 22, bubo in right axilla. Ill for two days before admission. Powerfully built man, lay in dorsal decubitus, semi-conscious, pupils contracted, pulse rapid, but of fairly good quality.

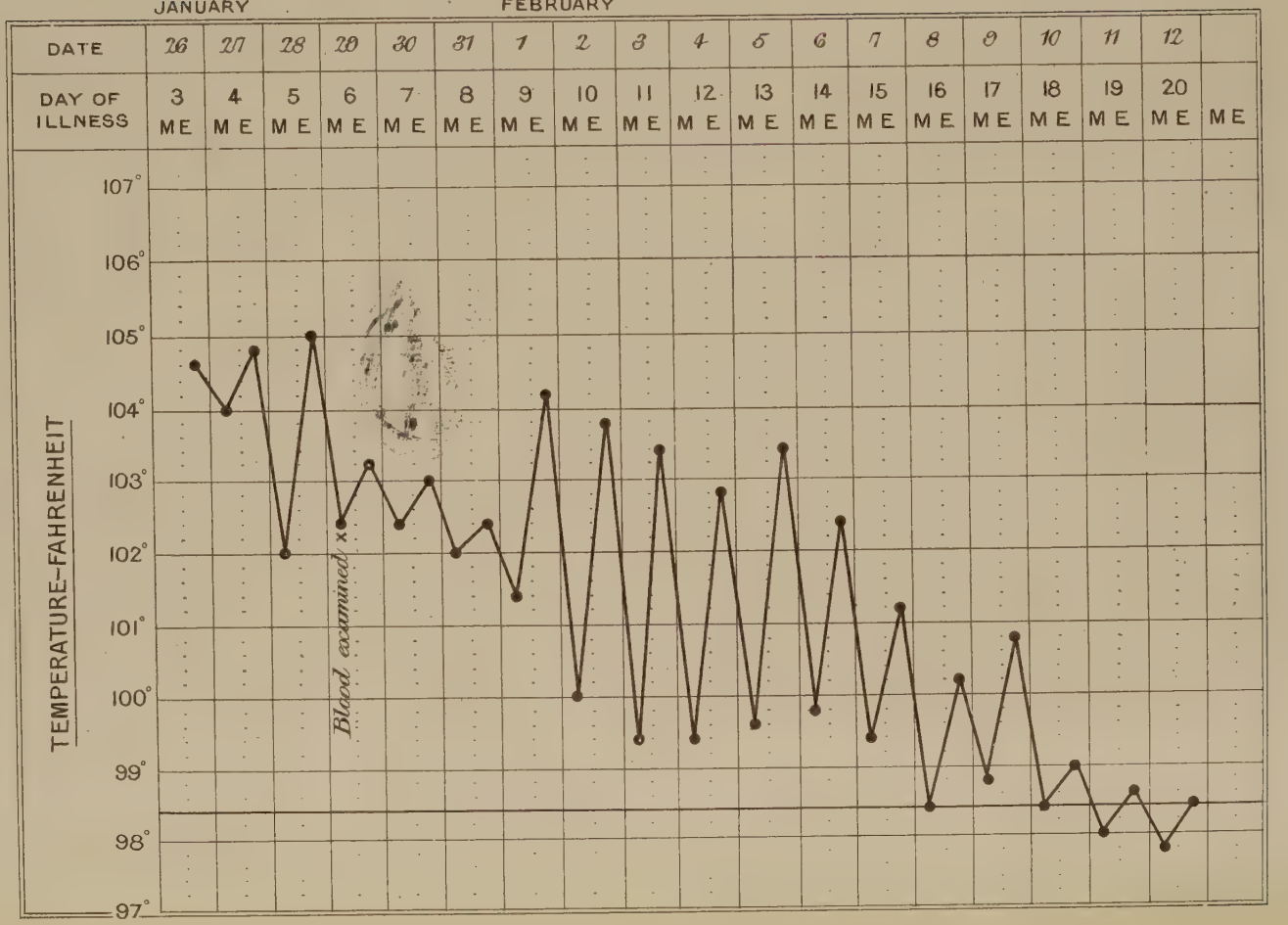
On morning of 6th day of illness small vein at back of left hand opened, and quantity of blood pipetted into sterile tube. Three agar tubes inoculated with the blood, and incubated at 37°C. At end of 24 hours two of the tubes showed several colonies, third tube only showed growth after 48 hours.

Microscopic appearances of growth characteristic of plague. Inoculated a rat with ·25c.c. of broth culture. Rat died of plague within 48 hours.

Four smear preparations of the fresh blood of patient showed the presence of bacteria, resembling B. Pestis in three.

Toxæmic symptoms present on admission lasted till 8th day of illness. Patient kept alive by nasal feeding. Made a good recovery.

E. L. MARSH.



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Collectorates.	1896.								1897.								1898.							
	Cholera.	Ratio per 1,000 of Population.	Fevers.	Ratio per 1,000 of Population.	*Plague.*	*Ratio per 1,000 of Population.*	All Causes.	Ratio per 1,000 of Population.	Cholera.	Ratio per 1,000 of Population.	Fevers.	Ratio per 1,000 of Population.	*Plague.*	*Ratio per 1,000 of Population.*	All Causes.	Ratio per 1,000 of Population.	Cholera.	Ratio per 1,000 of Population	Fevers.	Ratio per 1,000 of Population.	*Plague.*	*Ratio per 1,000 of Population.*	All Causes.	Ratio per 1,000 of Population.
Khandesh - -	1,740	1·19	43,271	29·63	*5*	*0·0*	56,948	39·00	3,351	2·29	43,137	29·54	*76*	*0·05*	62,510	42·81	—	—	17,234	11·80	*240*	*0·16*	41,491	28·41
Nasik - -	1,136	1·35	25,508	30·33	—	—	36,807	43·76	3,410	4·06	26,853	31·93	*1,075*	*1·28*	46,800	55·64	—	—	11,551	13·73	*2,016*	*2·39*	26,851	31·92
Thana - -	1,560	1·90	18,815	22·97	*95*	*0·11*	25,409	31·02	2,159	2·63	20,529	25·07	*4,156*	*5·27*	31,200	38·08	7	0·08	15,237	18·61	*3,310*	*4·04*	23,213	28·34
City of Bombay -	483	0·60	8,831	10·95	*1,803*	*2·23*	33,050	40·99	1,261	1·56	26,887	33·40	*10,237*	*12·70*	47,475	58·88	—	—	—	—	*18,130*	*22·49*	—	—
Kolaba - -	1,239	2·08	12,453	20·94	*13*	*0·02*	18,349	30·85	2,528	4·25	12,158	20·44	*1,309*	*2·20*	22,978	38·63	15	0·03	7,437	12·50	*660*	*1·11*	14,065	23·65
Ahmednagar - -	2,892	3·26	19,175	21·60	*4*	*0·01*	32,280	36·36	3,700	4·17	17,173	19·35	*386*	*0·13*	39,393	44·37	2	0·01	4,191	4·72	*263*	*0·29*	19,301	21·74
Poona - -	6,340	5·97	23,526	22·16	*39*	*0·02*	37,163	35·01	9,138	8·62	34,205	32·22	*8,871*	*8·36*	60,454	56·95	1	—	14,979	14·11	*1,332*	*1·25*	24,228	22·82
Sholapur - -	980	1·30	15,803	21·06	*1*	*0·0*	20,886	27·83	4,262	5·68	18,771	25·02	*2,353*	*3·14*	38,182	50·89	3	—	7,245	9·66	*1,735*	*2·31*	15,864	21·14
Satara - -	3,708	3·03	30,772	25·10	—	—	41,471	33·84	11,064	9·03	39,394	32·15	*10,031*	*8·18*	67,301	54·92	2	—	14,981	12·22	*6,907*	*5·63*	32,880	26·83
Ratnagiri - -	291	0·26	18,369	16·61	—	—	26,754	24·19	1,799	1·63	17,638	15·95	*26*	*0·02*	33,321	30·13	—	—	13,979	12·64	*373*	*0·34*	21,921	19·82
Belgaum - -	3,136	3·10	13,881	13·72	—	—	30,147	29·81	7,499	7·41	15,865	15·69	*444*	*0·44*	42,201	41·72	93	0·09	17,571	17·37	*16,739*	*16·55*	38,768	38·33*
Dharwar - -	2,008	1·91	16,541	15·75	—	—	29,764	23·33	1,760	1·68	20,464	19·48	*38*	*0·04*	38,809	36·94	3,986	3·79	29,686	28·26	*26,774*	*25·49*	55,502	52·83*
Bijapur - -	5,249	6·59	11,932	14·98	—	—	23,200	29·13	4,614	5·79	20,271	25·46	*3*	*0·00*	36,888	46·32	3	0·00	7,556	9·49	*1,076*	*1·35*	15,390	19·33
Kanara - -	120	0·27	6,765	15·17	—	—	11,781	26·41	413	0·93	8,176	18·33	—	—	15,656	35·09	79	0·18	6,340	14·65	*36*	*0·08*	11,985	26·86
Surat - -	1,142	1·76	18,433	28·37	*21*	*0·03*	22,695	34·92	123	0·19	15,650	24·08	*3,070*	*4·72*	25,270	38·88	—	—	10,403	16·00	*2,417*	*3·72*	20,420	31·42
Broach - -	677	1·98	11,346	33·23	*5*	*0·01*	13,652	39·98	10	0·03	9,159	26·82	*9*	*0·03*	11,824	34·63	—	—	9,288	27·20	*418*	*1·22*	13,016	38·12
Kaira - -	807	0·93	26,324	30·20	*10*	*0·01*	32,911	37·76	10	0·01	19,330	29·18	*3*	*0·00*	25,580	29·35	—	—	15,059	17·29	*402*	*0·46*	24,439	28·04
Panch Mahals -	342	1·09	8,687	27·74	—	—	9,661	30·82	7	0·02	6,179	19·72	—	—	6,616	21·11	—	—	7,684	24·52	*197*	*0·62*	8,670	27·67
Ahmedabad - -	1,549	1·68	26,344	28·61	*29*	*0·03*	32,960	35·78	—	—	20,077	21·80	*65*	*0·08*	26,668	28·95	1	0·00	18,662	20·26	*5*	*0·00*	25,332	27·51
Karachi -	—	—	11,807	21·04	*59*	*0·10*	13,965	24·89	1	--	13,590	24·22	*3,572*	*6·37*	17,115	30·51	—	—	9,616	17·14	*2,723*	*4·85*	12,951	23·09
Hyderabad - -	—	—	14,154	16·01	*1*	*0·0*	15,436	17·46	—	—	11,967	13·54	*499*	*0·57*	14,069	15·92	—	—	7,781	8·80	*1*	*0·00*	9,758	11·04
Thar and Parkar -	5	0·02	6,667	20·06	—	--	7,151	21·51	—	—	5,865	17·64	*2*	*0·00*	6,600	19·86	--	—	4,403	13·25	—	—	4,847	14·58
Shikarpur - -	—	—	19,010	20·77	*1*	*0·0*	20,050	21·91	—	—	25,465	27·83	*716*	*0·78*	28,974	31·66	—	—	15,801	17·2	—	—	18,279	19·98
Jacobabad - -	—	—	3,815	21·37	—	—	3,982	22·82	—	—	3,789	21·71	*3*	*0·02*	4,032	23·11	—	--	2,786	15·93	—	—	3,017	17·29

* The figures given in roman type against Belgaum and Dharwar for 1898 are for 11 months. The plague figures are for the entire year.

APPENDIX No. LVIII.

NOTES AND CHARTS

by

MR. N. A. F. MOOS,
DIRECTOR OF THE KOLABA OBSERVATORY, BOMBAY,

on

the RELATION of PLAGUE and METEOROLOGICAL CONDITIONS in BOMBAY,

With a Note by

DR. BUCHAN,
SECRETARY TO THE METEOROLOGICAL SOCIETY FOR SCOTLAND.

I.

SOURCE and EXPLANATION of the METHODS of REDUCTION of ENTRIES in the TABLES 1–5 and PLATES A, B, B (i.), B (ii.), and C.

In compiling the following tables, the entries given in the published volumes of observations at the Government Observatory, Kolaba, have been invariably adopted. It is only where a few entries were not so available, that the original manuscript abstracts were consulted and the missing entries filled in. In every case the modes of operation have been precisely the same as those adopted in reducing the entries given in the printed volumes, and in no case has a new departure from the usual methods been attempted.

TABLES 1 AND 2.

Barometer and Temperature of the Air.

1846.—Monthly means were taken from the manuscript abstracts from which annual means were calculated and adopted. All other entries of the year have been taken from the manuscript abstracts directly.

1847–1895.—From 1847 to 1872, all entries have been taken directly from the Meteorology of the Bombay Presidency, and from 1873 to 1895, from the volumes 23 to 34 of Bombay Magnetical and Meteorological Observations.

TABLE 3.

Pressure of Vapour.

1846.—Monthly means of dry and wet-bulb thermometers taken from manuscript abstracts were reduced to monthly means of pressure of vapour by the formula given in the Meteorology of the Bombay Presidency for the years 1847 to 1861. The maxima and minima were derived by a process of inspection and reduced as above.

1847–1895.—From 1847 to 1872, the entries have been taken directly from the Meteorology of the Bombay Presidency, and from 1873 to 1895, from Volumes 23 to 34 of Bombay Magnetical and Meteorological Observations.

TABLE 4.

Wind.

1846.—Monthly means of pressure in lbs. per square foot were taken from manuscript abstracts from which the annual mean was calculated and adopted. Hourly, daily, and monthly maximum pressures were directly obtained from the abstracts and adopted. For the average direction of the year, instead of getting the daily average from 24 hourly observations, a particular mean hourly direction was selected which corresponded with the average direction of the day, to which the usual average process was applied, and from the monthly means so derived the annual mean was finally obtained and adopted.

1847–1872.—Annual mean pressure in lbs. per square foot for the years 1847 to 1865, given in Volumes 3 to 22 were taken directly and adopted, as also the annual average directions for the years 1847 to 1863. The force of wind in eight principal directions during the years 1864 and 1865, and in 16 directions in the years 1866 and 1867, as given in the Volumes 21 and 22, were taken and resolved into north and east components, and resultant directions were found therefrom and adopted. Annual mean velocities for 1866 to 1872, as given in Volumes 22 and 23, were directly adopted. Mean resultant directions for 1868 to 1872 were derived from the annual components of wind given in Volumes 22 and 23, and adopted. All other entries for 1847 to 1872 were directly taken from the Meteorology of the Bombay Presidency.

1873–1895.—Annual mean velocities and maximum mean daily and monthly velocities have been taken from Bombay Magnetical and Meteorological Observations, Volumes 23 to 34. Mean resultant directions were reduced from the annual components of wind given in the above volumes and adopted. Maximum hourly mean velocities were derived from data prepared for the yearly Government administration reports.

TABLE 5.

Rainfall.

1846–1872.—All entries have been taken directly from the Meteorology of the Bombay Presidency, except that of maximum *daily* rainfall for the year 1846, which was taken from manuscript abstracts.

1873–1895.—All entries have been taken from Volumes 23 to 34 of the Bombay Magnetical and Meteorological Observations.

PLATES A, B, B (i), and B (ii).

Variations of the Temperature of the Air and Ground.

The accompanying plates have been put in to show that what is true for the yearly variation of ground evaporation is true also for the day. In the yearly fluctuations the resulting curve shows that in the colder and drier months the ground evaporation is a maximum, and in the hotter and moist months it is a minimum. The Tables A. and B. will show that during the day the worst hours are the early morning hours, when the temperature of and the tension of moisture in the air is a minimum; and the best hours are the afternoon hours, when both the temperature and tension of moisture are a maximum.

PLATE C.

Movement and Intensity of Wind.

In regard to Plate C., which represents the movement of wind and measures the intensity from hour to hour of land and sea breezes, it may be noted that this factor supplies a valuable health giving component, *ozone*, and should be adequately represented in any curve which is drawn to run coincident with the mortality curve. Unfortunately the treatment adopted in this inquiry gives the curve in terms of either pressure or temperature, and the velocity being a factor of a different order, it cannot be compounded with the above. The only possible way, therefore, to include this factor in the curve is by finding the ratio of the velocity in terms of temperature by the method of least squares, a ratio which gives the resultant curve coincident with the mortality curve with the least probable error. As such computations are heavy, no systematic effort has been made except in an arbitrary, rough manner, the result of which is shown by dotted curves in the series of trial curves which I have prepared.

Statement of Mean Values and Charts A. and B.

The tables 1 to 5 and the plates A. to C. are followed by a statement of the weekly mean values of the meteorological factors used in the calculations, and the result of the calculations is appended in Chart A., which gives a series of trial curves, and in Chart B. which gives the curves for plague mortality and ground evaporation in Bombay.

N. A. F., Moos,
Director,
Government Observatory, Bombay.

TABLE 1.

Barometric Pressure.

1846–1895.

Year.	Annual Mean.	Individual Hours.			Daily Means.			Monthly Means.		
		Maximum.	Minimum.	Extreme Range.	Maximum.	Minimum.	Range.	Maximum.	Minimum.	Range.
	In.	In.	In.	In.	In.	In.	In.	In.	In.	In.
1846	29·809	30·107	29·383	0·724	30·032	29·493	0·539	**29·993**	29·624	0·369
1847	·797	·075	·458	·617	·017	·498	·519	·917	·621	·296
1848	·805	·084	·445	·639	·012	·491	·521	·927	·649	·278
1849	·798	·094	·387	·707	·016	·428	·588	·945	·624	·321
1850	·808	·070	·482	·588	29·998	·521	·477	·949	·739	·310
1851	·796	·148	·414	·734	30·036	·462	·574	·958	**·599**	·359
1852	·805	·072	·433	·639	·001	·485	·516	·929	·630	·299
1853	·814	·113	·502	·611	·022	·536	·486	·955	·644	·311
1854	·804	·111	**·183**	·928	·031	·514	·517	·935	·618	·317
1855	·824	·119	·462	·657	·048	·513	·535	·944	·658	·286
1856	·806	·136	·439	·697	·068	·524	·544	·985	·610	·375
1857	·808	·105	·507	·598	·039	·541	·498	·981	·640	·341
1858	·812	·093	·464	·629	·018	·516	·502	·944	·670	·274
1859	·812	·061	·437	·624	29·994	·492	·502	·923	·628	·305
1860	·804	·095	·452	·643	30·012	·496	·516	·953	·631	·322
1861	·797	·053	·429	·624	29·975	·506	·469	·932	·639	·293
1862	·783	·101	·467	·634	30·017	·511	·506	·920	·625	·295
1863	·792	·108	·457	·651	·018	·500	·518	·947	·606	·341
1864	·832	·142	·486	·656	·054	·567	·487	·945	·660	·285
1865	·811	·109	·456	·653	·039	·496	·543	·972	·656	·316
1866	·822	**·164**	·342	·822	**·076**	·452	·624	·969	·661	·308
1867	·824	·143	·514	·629	·058	·564	·494	·983	·667	·316
1868	·836	·131	·344	·787	·053	·413	·640	·963	·673	·290
1869	·814	·132	·463	·669	·046	·503	·543	·966	·659	·307
1870	·797	·094	·418	·676	·015	·478	·537	·936	·637	·299
1871	·805	·068	·466	·602	29·996	·521	·475	·932	·641	·291
1872	·795	·067	·515	·552	·980	·569	·411	·932	·640	·292
1873	·811	·095	·440	·655	30·012	·513	·499	·943	·641	·302
1874	·810	·102	·378	·724	·024	·445	·579	·975	·628	·347
1875	·808	·080	·465	·615	·013	·500	·513	·938	·652	·286
1876	·816	·091	·465	·626	·022	·542	·480	·957	·631	·326
1877	·846	·108	·540	·568	·049	·600	·449	·976	·713	·263
1878	·800	·106	·467	·639	·022	·517	·505	·953	·654	·299
1879	·801	·080	·294	·786	·003	**·402**	·601	·924	·673	·251
1880	·818	·103	·477	·626	·033	·561	·472	·975	·668	·307
1881	·820	·120	·504	·616	·041	·540	·501	·963	·677	·287
1882	·807	·140	·366	·774	·058	·433	·625	·965	·633	332
1883	·812	·132	·469	·663	·053	·545	·508	·989	·639	·350
1884	·827	·136	·419	·717	·053	·472	·581	·993	·448	·345
1885	·826	·125	·404	·721	·051	·470	·581	·987	·652	·335
1886	·809	·132	·362	·770	·057	·461	·596	·947	·653	·294
1887	·818	·130	·406	·724	·060	·466	·594	·935	·662	·273
1888	·832	·130	·537	·593	·061	·585	·476	·975	·664	·311
1889	·818	·096	·394	·702	·028	·475	·553	·970	·647	·323
1890	·814	·094	·505	·589	·023	·552	·471	·947	·632	·315
1891	·834	·128	·471	·657	·045	·522	·523	·960	·653	·307
1892	·790	·084	·418	·666	·017	·492	·525	·968	·608	·360
1893	·815	·081	·432	·649	·014	·518	·496	·958	·648	·310
1894	·809	·094	·451	·643	·021	·498	·523	·945	·636	·309
1895	·819	·105	·390	·715	·026	·436	·590	·936	·654	·282
Mean of 50 years	29·811									

TABLE 2.

Temperature of the Air and Ground.

1846–95 extended to 1898.

Year.	Annual Mean.	Individual Hours.			Daily Means.			Monthly Means.			Ground Temperature: Annual Mean.	
		Maximum.	Minimum.	Extreme Range.	Maximum.	Minimum.	Range.	Maximum.	Minimum.	Range.	20-inch deep.	60-inch deep.
	°	°	°	°	°	°	°	°	°	°		
1846	89·5	98·8	63·8	35·0	88·4	72·6	15·8	86·7	76·4	10·3	—	—
1847	79·0	94·2	**53·3**	40·9	86·5	**62·2**	24·3	83·9	73·7	10·2	—	—
1848	79·8	98·0	57·0	41·0	86·7	67·1	19·6	84·6	75·1	9·5	—	—
1849	79·6	96·5	58·5	38·0	86·6	65·9	20·7	85·2	73·3	11·9	83·6	—
1850	79·9	97·0	60·0	37·6	87·8	68·1	19·7	84·4	72·4	12·0	84·1	—
1851	79·5	93·3	57·3	36·0	86·7	65·6	21·1	84·6	72·2	12·4	81·6	83·5
1852	80·2	95·4	57·4	38·0	88·3	66·7	21·6	86·3	71·9	14·4	81·7	83·6
1853	80·5	96·4	58·6	37·8	89·4	67·0	22·4	86·9	71·6	15·3	81·7	83·6
1854	80·6	98·4	63·0	35·4	89·4	71·3	18·1	86·9	75·0	11·9	82·2	83·9
1855	80·7	95·3	60·0	35·3	88·5	69·2	19·3	86·0	74·7	11·3	81·7	83·6
1856	80·5	94·6	63·0	31·6	88·1	72·6	15·5	86·1	75·4	10·7	82·0	84·0
1857	79·9	**100·2**	63·0	37·2	87·6	70·9	16·7	86·1	73·9	12·2	80·9	83·0
1858	80·1	94·9	60·8	34·1	88·0	69·0	19·0	86·2	73·3	12·9	80·8	82·5
1859	80·2	98·4	62·0	36·4	88·0	69·7	18·3	85·2	74·2	11·0	81·2	82·9
1860	80·0	93·7	59·6	34·1	88·1	68·3	19·8	85·0	72·3	12·7	82·2	83·6
1861	79·3	97·0	62·5	34·5	87·6	70·6	17·0	85·2	74·4	10·8	82·2	83·9
1862	80·6	94·0	61·4	32·6	87·8	69·6	18·2	85·8	72·9	12·9	84·0	84·2
1863	79·8	95·0	60·2	34·8	87·9	67·3	20·6	86·0	74·3	11·7	83·8	84·0
1864	79·9	94·0	60·7	33·3	87·6	68·9	18·7	84·4	71·3	13·1	83·2	83·6
1865	81·0	96·0	66·1	29·9	**89·5**	73·2	16·3	**87·6**	77·0	10·6	84·2	84·5
1866	78·3	91·8	56·6	35·2	86·3	65·3	21·0	82·7	71·8	10·9	82·5	83·3
1867	78·8	92·6	61·8	30·8	87·4	70·0	17·4	84·7	73·1	11·6	82·1	83·2
1868	79·1	92·5	62·2	30·3	85·9	69·4	16·5	84·5	72·9	11·6	82·2	83·4
1869	79·9	92·4	63·3	29·1	86·4	72·2	14·2	84·1	75·3	8·8	82·8	83·7
1870	79·0	92·7	62·9	29·8	86·3	68·9	17·4	83·7	73·6	10·1	82·5	83·7
1871	80·1	93·3	66·1	27·2	86·7	70·6	16·1	85·6	74·9	10·7	83·0	83·8
1872	78·9	93·3	60·4	32·9	86·7	68·1	18·6	84·1	71·7	12·4	82·2	83·1
1873	78·9	93·2	55·2	38·0	87·7	65·4	22·3	84·0	72·1	11·9	82·2	83·2
1874	78·3	93·0	60·9	32·1	85·8	66·7	19·1	83·7	**71·1**	12·6	81·6	82·6
1875	79·3	91·2	59·1	32·1	86·8	66·3	20·5	83·8	72·2	11·6	82·3	83·1
1876	79·3	91·1	65·8	25·3	86·6	71·3	15·3	84·4	74·9	9·5	82·6	83·5
1877	80·3	91·3	63·2	28·1	87·1	69·9	17·2	84·0	74·4	9·6	83·0	83·8
1878	80·1	94·8	62·8	32·0	87·4	69·6	17·8	85·1	73·3	11·8	83·3	84·0
1879	78·6	93·5	65·4	31·1	87·5	70·6	16·9	82·9	73·1	9·8	81·9	82·9
1880	79·5	91·9	60·4	31·5	86·1	67·6	18·5	84·5	73·0	11·5	82·5	82·9
1881	80·0	92·0	67·1	24·9	86·5	73·5	13·0	85·1	76·1	**9·0**	83·4	84·2
1882	79·1	92·1	65·1	27·0	85·6	71·7	13·9	83·6	74·9	8·7	82·9	83·8
1883	78·8	90·9	62·8	28·1	86·3	69·4	16·9	84·2	72·8	11·4	82·6	83·8
1884	78·8	91·9	63·1	28·8	86·4	68·9	17·5	83·5	71·9	11·6	82·7	83·4
1885	79·2	91·8	62·1	29·7	87·3	68·7	18·6	84·3	72·3	12·0	83·1	83·6
1886	79·5	95·4	59·5	35·9	87·4	67·4	20·0	85·0	73·5	11·5	83·5	84·1
1887	78·7	92·2	56·9	35·3	85·8	67·0	18·8	84·2	72·6	11·6	82·6	83·4
1888	80·1	93·8	65·7	28·1	86·6	71·2	15·4	84·1	74·1	10·0	83·7	84·1
1889	79·6	92·1	65·7	26·4	87·4	72·7	14·7	85·4	74·6	10·8	83·7	84·5
1890	79·3	91·1	66·9	24·2	86·1	72·1	14·0	83·9	75·1	8·8	83·3	84·0
1891	79.5	90·8	60·3	30·5	86·8	67·3	19·5	84·9	73·5	11·4	83·4	84·1
1892	79·9	93·9	64·4	29·5	89·0	70·4	18·6	85·7	75·5	10·2	83·7	84·4
1893	78·9	92·1	59·8	32·3	86·0	68·1	17·9	83·6	72·1	11·5	82·9	83·6
1894	79·6	90·4	65·1	25·3	86·7	71·9	14·8	84·6	75·1	9·5	83·7	84·4
1895	79·6	92·3	62·0	30·3	86·6	69·1	17·5	84·1	73·2	10·9	83·5	84·2
Mean of 50 years	79·6										Mean of 1849 to 1895, 82·7	Mean of 1851 to 1895, 83·6
1896	80·7										84·5	84·8
1897	79·5										83·9	84·9
1898	80·3										86·3	86·3

Table 3.

Pressure of Vapour.

1846–1895.

Year.	Annual Mean.	Individual Hours.			Daily Means.			Monthly Means.		
		Maximum.	Minimum.	Extreme Range.	Maximum.	Minimum.	Range.	Maximum.	Minimum.	Range.
	In.	In.	In.	In.	In.	In.	In.	In.	In.	In.
1846	0·809	1·116	0·318	0·798	1·022	0·478	0·544	0·961	0·612	0·349
1847	·795	·099	·189	·910	0·985	**·259**	·726	·934	·606	·328
1848	·803	·046	·303	·743	·987	·330	·657	·918	·629	·289
1849	·796	**·214**	·280	·934	1·060	·351	·709	**·969**	·588	·381
1850	·784	·076	·270	·806	0·983	·392	·591	·942	·566	·376
1851	·782	·030	·236	·794	·988	·348	·640	·929	·555	·374
1852	·769	·069	·265	·804	·961	·369	·592	·920	·508	·412
1853	·765	·028	·247	·781	·974	·353	·621	·934	·511	·423
1854	·795	·124	·198	·926	1·025	·337	·688	·946	·557	·389
1855	·794	·110	·297	·813	·002	·439	·563	·947	·596	·351
1856	·796	·076	·300	·776	0·977	·425	·552	·933	·625	·308
1857	·792	·125	·321	·804	**1·067**	·475	·592	·933	·566	·367
1858	·815	·139	·360	·779	·044	·479	·565	·968	·625	·343
1859	·809	·155	·369	·786	·040	·439	·601	·957	·608	·349
1860	·800	·088	·299	·789	·019	·465	·614	·956	·562	·394
1861	·772	·066	·260	·806	0·982	·402	·580	·921	·565	·356
1862	·778	·055	·304	·751	·983	·423	·560	·934	·553	·381
1863	·759	·000	·267	·733	·927	·390	·537	·902	·577	·325
1864	·748	·040	·219	·821	·948	·359	·589	·900	**·484**	·416
1865	·789	·107	·353	·754	·989	·488	·501	·930	·607	·323
1866	·762	·089	·235	·854	·978	·336	·642	·932	·545	·387
1867	·784	·046	·282	·764	·987	·428	·559	·947	·560	·307
1868	·785	·091	**·128**	·963	1·023	·335	·688	·953	·583	·370
1869	·787	·052	·272	·780	0·992	·433	·559	·930	·608	·322
1870	·772	·044	·244	·800	·976	·432	·544	·915	·566	·349
1871	·800	·051	·346	·705	·959	·474	·485	·915	·632	·283
1872	·758	·091	·217	·874	·996	·348	·648	·925	·515	·410
1873	·755	·055	·223	·832	1·005	·285	·720	·906	·532	·374
1874	·743	·033	·232	·801	0·964	·353	·611	·892	·513	·379
1875	·760	·031	·215	·816	·973	321	·652	·911	·515	·396
1876	·751	0·994	·340	·654	·956	·474	·482	·892	·584	·308
1877	·788	1·022	·257	·765	·964	·349	·615	·916	·583	·333
1878	·795	·100	·252	·848	·999	·383	·616	·921	·563	·358
1879	·748	·049	·281	·768	1·009	·400	·609	·909	·552	·357
1880	·767	·035	·262	·773	0·964	·379	·585	·891	·533	·358
1881	·774	·014	·286	·728	·959	·417	·542	·904	·579	·325
1882	·764	·047	·283	·764	1·007	·435	·572	·908	·597	·311
1883	·758	·027	·297	·730	0·963	·388	·575	·921	·525	·396
1884	·756	0·994	·330	·664	·938	·440	·498	·896	·544	·352
1885	·769	1·129	·273	·856	1·013	·355	·658	·926	·536	·390
1886	·780	·096	·194	·902	·048	·366	·682	·920	·570	·350
1887	·759	·014	·193	·821	0·958	·303	·655	·882	·574	·308
1888	·794	·067	·357	·710	·977	·412	·565	·931	·618	·313
1889	·787	·050	·329	·721	1·001	·461	·540	·939	·599	·340
1890	·771	·009	·337	·672	0·952	·478	·474	·907	·589	·318
1891	·757	·029	·206	·823	·970	·309	·661	·919	·568	·351
1892	·792	·050	·298	·752	·984	·452	·532	·923	·621	·302
1893	·759	·015	·253	·762	·970	·373	·597	·899	·548	·351
1894	·784	·087	·344	·743	·959	·485	·474	·929	·616	·313
1895	·776	·030	·333	·697	·965	·420	·545	·923	·580	·343
Mean of 50 years.	0·778									

TABLE 4.

Wind.

1846-1895.

Year.	Annual Mean Pressure.	Average Direction.	Absolute Maximum Pressure.	Maximum Mean Daily Pressure.	Maximum Mean Monthly Pressure.	Year.	Annual Mean Velocity.	Mean Resultant Direction.	Maximum Hourly Mean Velocity.	Maximum Mean Daily Velocity.	Maximum Mean Monthly Velocity.
	lb.		lb.	lb.	lb.		Miles.		Miles.	Miles.	Miles.
1846	·44	N.W.	4·16	1·94	0·65	1866	14·36	W.	58·7	46·2	22·5
1847	·39	N.W.	3·60	1·63	0·65	1867	12·4	W.N.W.	41	32·1	19·4
1848	·32	N.W.	5·26	3·21	0·68	1868	12·3	W.N.W.	50	39·5	19·9
1849	·25	N.W.	9·00	2·31	0·64	1869	12·5	W.N.W.	59	38·5	20·7
1850	·2	W.N.W.	15·00	1·60	0·37	1870	11·8	W.N.W.	41	31·5	19·5
1851	·18	N.W.	8·00	3·00	0·49	1871	12·6	W.N.W.	48	32·1	21·3
1852	·16	N.W.	10·70	1·00	0·39	1872	12·2	W.N.W.	46	32·3	19·8
1853	·2	N.W.	7·00	1·40	0·51	1873	12·5	W.N.W.	43	30·0	23·0
1854	·18	N.W.	35·00	8·06	0·33	1874	11·9	W.N.W.	47	36·5	19·6
1855	·24	N.W.	13·00	2·05	0·52	1875	12·1	W.N.W.	45	33·5	21·0
1856	·20	N.W.	16·00	3·08	0·37	1876	12·7	W.N.W.	43	30·7	22·7
1857	·21	N.W.	9·50	1·29	0·51	1877	11·7	W.N.W.	43	33·0	18·7
1858	·26	N.W.	12·00	2·38	0·55	1878	11·6	W.N.W.	47	30·7	15·7
1859	·33	N.W.	11·25	4·29	1·02	1879	12·4	W.N.W	56	39·5	17·2
1860	·29	W.N.W.	14·00	2·01	0·48	1880	12·0	W.N.W.	34	30·8	19·7
1861	·37	W.N.W.	8·75	2·26	0·71	1881	12·3	W.N.W.	40	31·6	20·5
1862	·41	N.W.	10·75	4·02	0·67	1882	11·8	W.N.W.	53	34·2	20·3
1863	·38	W.N.W.	7·75	3·05	0·92	1883	12·2	W.N.W.	45	31·8	20·4
1864	·32	*W.N.W	6·25	1·25	0·62	1884	12·1	W.N.W.	36	29·0	18·6
1865	·31	*W.N.W.	10·50	1·95	0·51	1885	12·4	W.N.W.	56	41·1	18·4
						1886	11·7	W.N.W.	48	35·2	16·9
						1887	12·0	W.N.W.	44	30·2	20·6
						1888	12·0	W.N.W.	50	27·3	19·9
						1889	11·5	W.N.W.	59	39·8	17·2
						1890	12·1	W.N.W.	40	26·4	20·4
						1891	12·0	W.N.W.	41	33·2	18·1
						1892	11·0	W.	48	31·6	19·0
						1893	11·1	W.N.W.	39	30·0	14·6
						1894	10·9	W.N.W.	42	34·1	17·5
						1895	11·1	W.N.W.	44	30·7	17·3

* From the year 1864 the average direction is replaced by resultant direction.

TABLE 5.

Rainfall.

1846–1895.

Year.	Jan.	Feb.	March.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.	Annual Total.	Number of Rainy Days.	Maximum Fall in One Day.	Time of Occurrence.
	In.	In.	In.	In.	In.	In.	In.	In.	In.	In.	In.	In.	In.		In.	
1846	—	—	—	—	—	26·41	33·19	6·65	6·65	1·03	—	—	73·93	104	5·16	16 July.
1847	0·90	0·02	—	0·31	1·40	37·56	16·45	8·68	5·68	0·28	**5·53**	—	76·00	120	12·54	11 June.
1848	—	—	—	**0·59**	5·67	39·98	13·90	7·37	**2·32**	5·84	0·19	—	75·86	96	8·14	14 June.
1849	0·34	—	—	—	—	23·41	**50·99**	12·63	26·13	0·75	0·61	—	**114·89**	100	6·35	25 July.
1850	—	—	—	—	—	14·80	20·15	5·39	4·77	4·89	0·25	—	50·25	103	4·61	17 July.
1851	—	—	—	—	0·52	24·50	47·02	20·03	3·89	0·04	0·07	—	96·07	89	6·51	25 August.
1852	—	—	0·01	—	0·30	21·76	22·17	11·16	12·67	0·19	—	1·01	69·27	100	7·09	22 June.
1853	—	0·05	0·02	—	—	33·70	13·05	5·95	9·83	—	—	—	62·60	84	9·89	18 June.
1854	—	—	—	—	—	16·34	38·95	3·90	13·61	7·49	1·85	—	82·14	114	5·55	14 June.
1855	—	—	—	—	—	20·18	11·83	3·82	5·29	0·06	—	—	41·18	91	4·79	20 June.
1856	—	—	—	—	2·09	24·66	24·48	6·73	7·78	0·06	—	0·12	65·92	109	10·28	9 June.
1857	—	—	—	—	0·57	9·26	**8·74**	15·71	14·21	2·78	—	—	51·27	108	5·27	1 August.
1858	—	—	—	0·15	1·57	14·44	20·12	7·48	15·47	3·22	—	—	62·45	105	6·15	11 September.
1859	—	—	—	—	—	26·87	28·70	14·59	5·94	1·06	—	—	77·16	91	8·20	12 June.
1860	—	—	—	—	0·03	21·97	22·39	7·11	8·15	2·50	—	—	62·15	93	7·31	13 June.
1861	—	—	—	—	0·69	15·43	25·34	30·35	3·24	1·86	—	—	76·91	99	6·08	18 August.
1862	—	—	—	—	0·01	22·31	15·10	12·65	21·69	1·42	0·45	—	73·63	107	6 16	21 June.
1863	0·41	—	—	0·11	0·07	23·41	30·78	10·60	9·98	2·32	—	—	77·68	109	8·14	24 July.
1864	—	—	—	—	0·01	15·42	13·36	10·72	5·59	—	0·47	—	45·57	95	3·56	26 June.
1865	0·38	—	—	—	—	10·61	18·28	36·42	4·86	6·16	1·14	—	77·85	104	7·86	2 July.
1866	—	0·11	—	—	—	13·47	40·34	20·34	3·44	0·74	—	—	78·44	98	7·63	11 July.
1867	—	—	—	—	—	8·70	29·39	22·21	6·80	5·07	0·13	—	62·30	110	5·41	16 July
1868	—	—	0·01	—	—	13·52	20·43	20·29	7·11	0·14	0·02	—	62·12	107	10·23	9 August.
1869	—	—	—	—	0·03	26·22	23·89	15·24	22·56	3·55	0·05	0·12	91·66	102	15·31	27 June.
1870	—	—	0·03	0·02	—	21·73	26·43	6·27	7·08	4·65	—	—	66·21	109	4·71	18 June.
1871	**2·22**	—	—	—	1·25	8·91	9·96	8·95	6·01	0·25	2·97	0·06	**40·58**	107	4·64	31 August.
1872	—	—	—	—	0·06	24·43	24·84	9·49	16·59	1·07	—	—	76·48	98	9·23	12 September.
1873	—	0·05	—	—	0·51	20·36	16·98	23·96	7·77	0·07	—	—	69·70	101	6·23	24 August.
1874	—	—	—	—	0·05	18·68	41·24	11·14	11·05	0·02	—	—	82·18	103	4·48	21 July.
1875	0·47	0·05	—	—	—	23·97	15·14	11·95	**31·51**	—	—	—	83·09	97	8·14	5 September.
1876	—	—	—	—	0·01	12·97	23·69	8·66	4·71	—	—	—	50·04	**83**	7·59	26 July.
1877	0·23	**0·51**	0·02	—	—	34·61	10·75	10·78	6·60	6·39	—	—	69·89	99	14·60	19 June.
1878	0·03	—	—	0·02	0·03	20·49	47·64	20·02	16·90	4·17	2·63	—	111·93	113	5·55	5 July.
1879	—	—	0·03	—	5·23	16·56	11·21	22·36	5·61	0·40	—	—	61·40	111	3·78	30 July.
1880	—	0·01	0·02	—	—	21·48	18·37	4·08	22·80	1·18	—	—	67·94	109	8·20	2 July.
1881	—	—	0·04	—	0·36	15·29	29·47	19·06	4·56	4·17	0·09	—	73·04	113	11·30	3 August.
1882	—	0·04	0·02	0·08	0·02	27·54	26·94	**3·36**	10·08	1·12	0·03	—	69·23	105	10·18	3 June.
1883	0·12	—	—	—	0·33	13·65	39·88	12·57	12·57	**10·40**	0·86	—	90·18	123	6·85	13 July.
1884	0·03	—	—	—	—	13·25	25·87	15·29	17·04	2·22	0·60	**1·14**	75·44	110	7·37	13 July.
1885	—	—	—	—	0·02	**5·11**	21·81	25·42	12·01	3·54	—	—	67·91	113	10·29	15 August.
1886	—	—	—	0·01	0·96	**43·45**	35·79	10·69	6·54	1·69	0·61	—	99·74	110	**16·10**	18 June.
1887	—	—	—	0·02	0·09	24·07	30·98	17·59	18·23	2·79	1·02	0·16	94·95	112	7·84	10 September.
1888	1·85	0·02	0·10	—	—	15·76	22·47	11·43	4·92	0·11	1·16	—	57·82	107	4·66	14 July.
1889	—	—	—	—	0·67	19·89	30·45	10·32	2·71	3·80	—	—	67·84	103	4·04	8 July.
1890	—	—	—	0·01	0·06	24·55	21·54	10·61	6·45	0·58	1·25	0·13	65·18	116	4·61	19 June.
1891	—	—	**0·20**	—	—	13·99	32·48	6·94	22·53	1·04	—	—	77·18	100	5·82	1 September.
1892	—	—	—	—	0·11	13·30	23·86	**36·56**	18·73	1·89	0·67	—	95·42	114	5·43	31 August.
1893	—	0·14	—	0·02	**6·30**	21·47	16·14	13·55	7·54	0·45	1·63	—	67·24	**124**	4·97	20 June.
1894	0·22	—	0·6	0·01	—	16·87	26·16	8·40	12·04	3·08	—	0·01	66·85	124	4·77	21 July.
1895	—	0·07	—	—	0·08	17·84	18·04	15·97	11·91	3·62	0·06	—	67·59	109	6·82	7 September.
Average of 50 years -	0·13	0·02	0·01	0·03	0·58	20·30	24·74	13·23	10·65	2·20	0·49	0·05	72·44	105	—	

PLATE A.

DIURNAL VARIATION IN THE TEMPERATURE OF THE AIR, (1866 TO 1872.)

From Observations recorded at the Kolaba Observatory, Bombay.

INEQUALITIES AT DIFFERENT HOURS IN THE SAME MONTH.

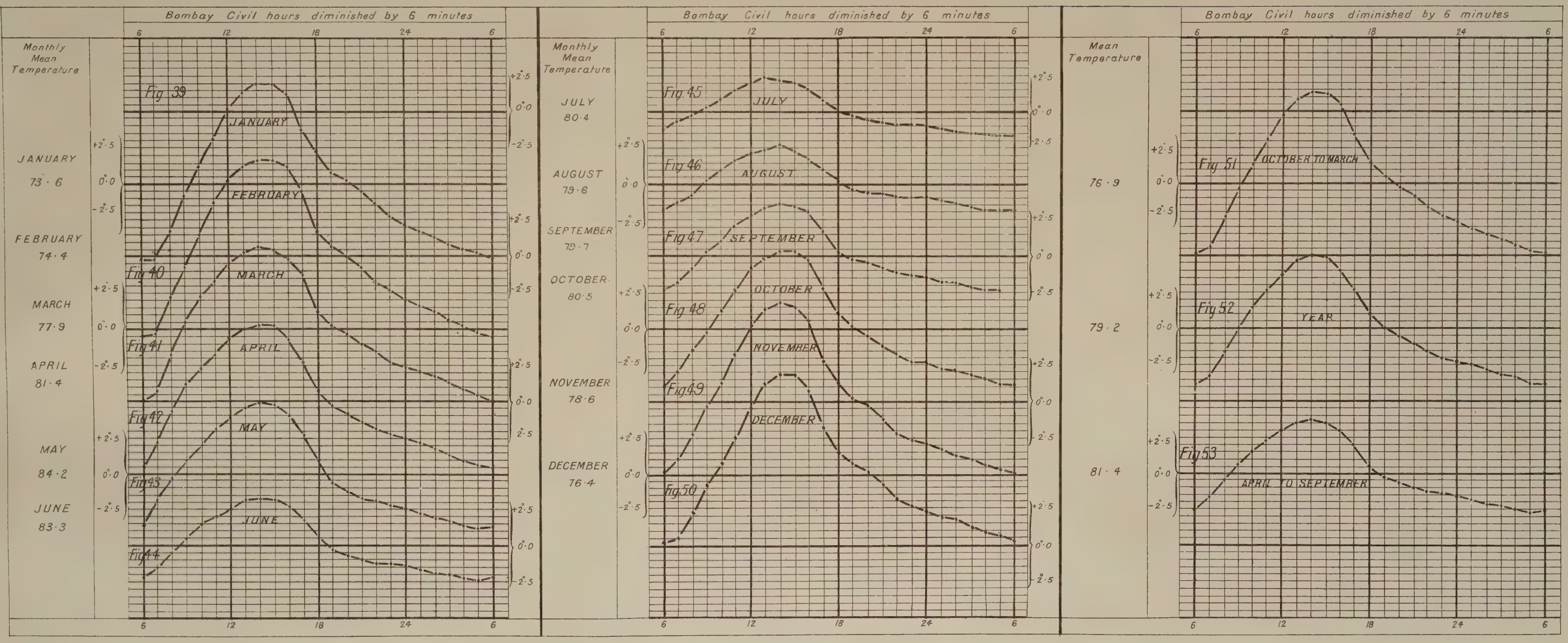
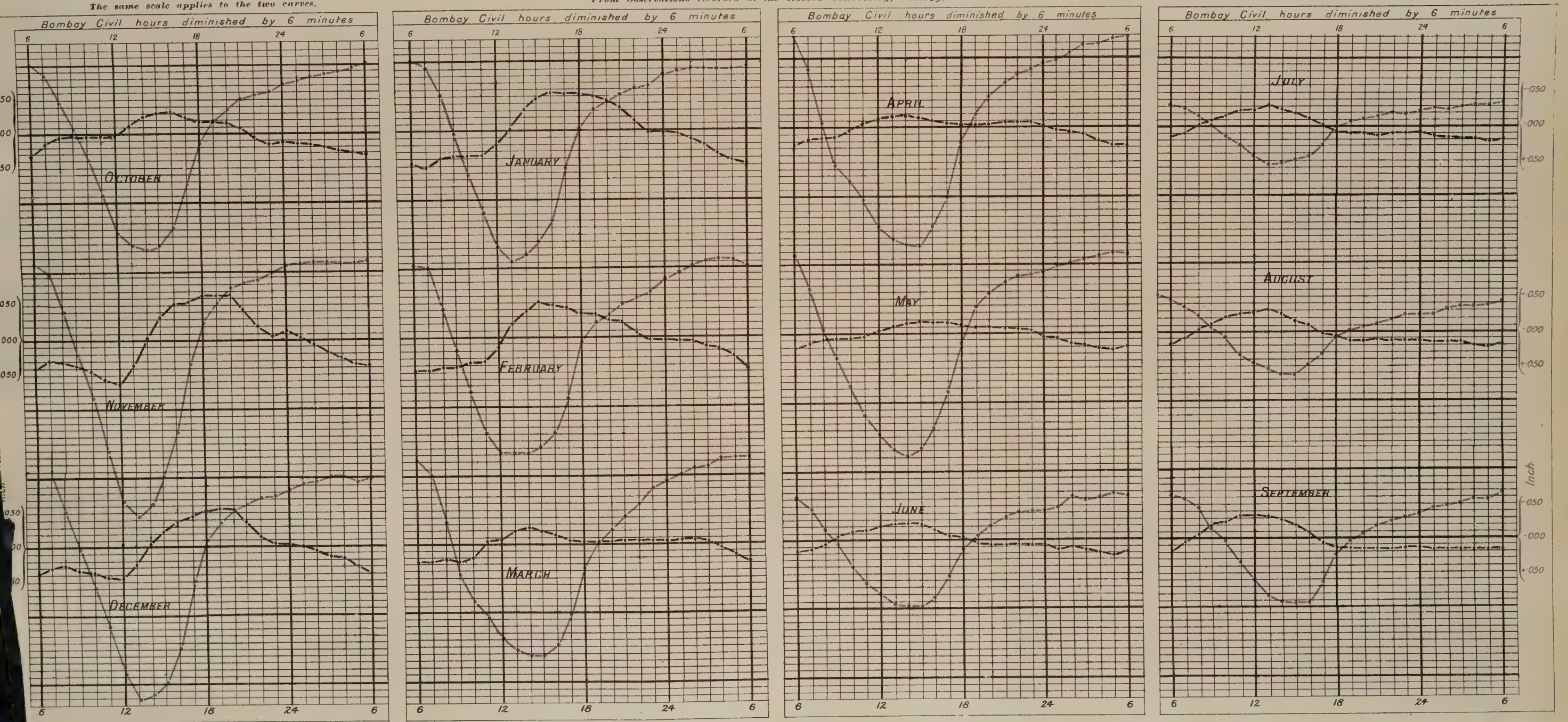


PLATE B

VARIATIONS IN THE TEMPERATURE OF THE GROUND.

From Observations recorded at the Kolaba Observatory, Bombay.

Pressure of vapour *Diurnal variation (1866 to 1872)* *Black curves.*
Additional capacity *Diurnal variation (1866 to 1872)* *Violet curves.*
Inequalities at different hours in the same month.
The same scale applies to the two curves.



Pressure of vapour *Diurnal variation (1866 to 1872)* *Black curves.*
Additional capacity *Diurnal variation (1866 to 1872)* *Violet curves.*
Inequalities at different hours in yearly and six-monthly periods.

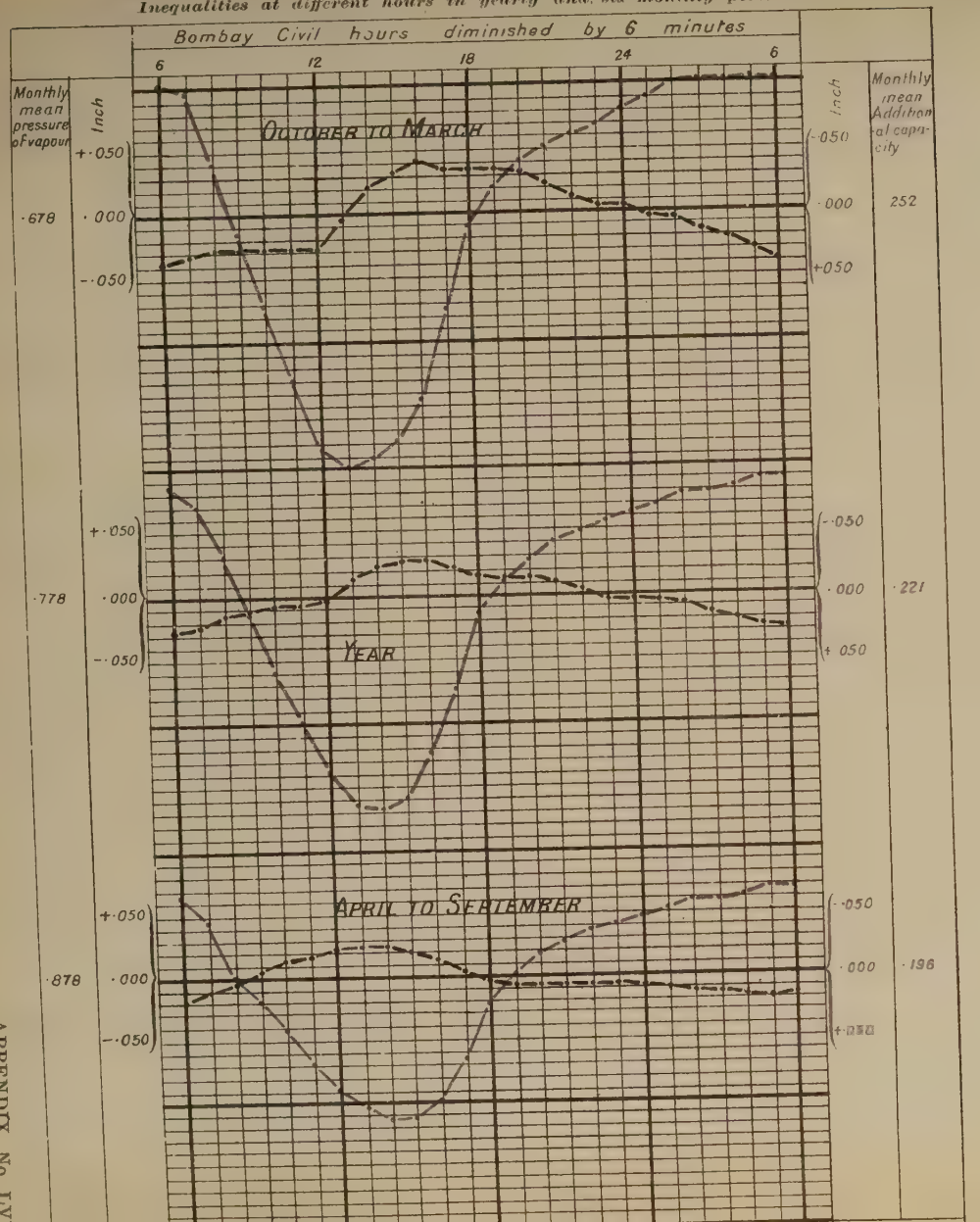
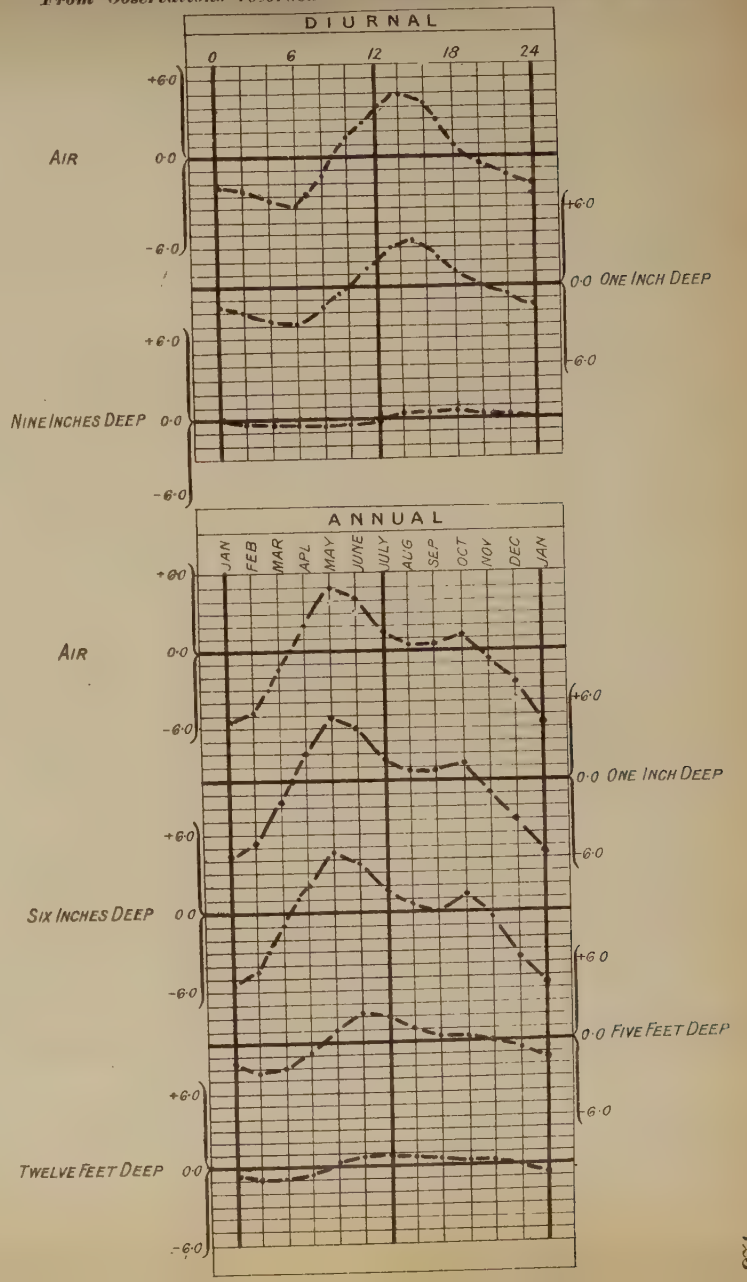


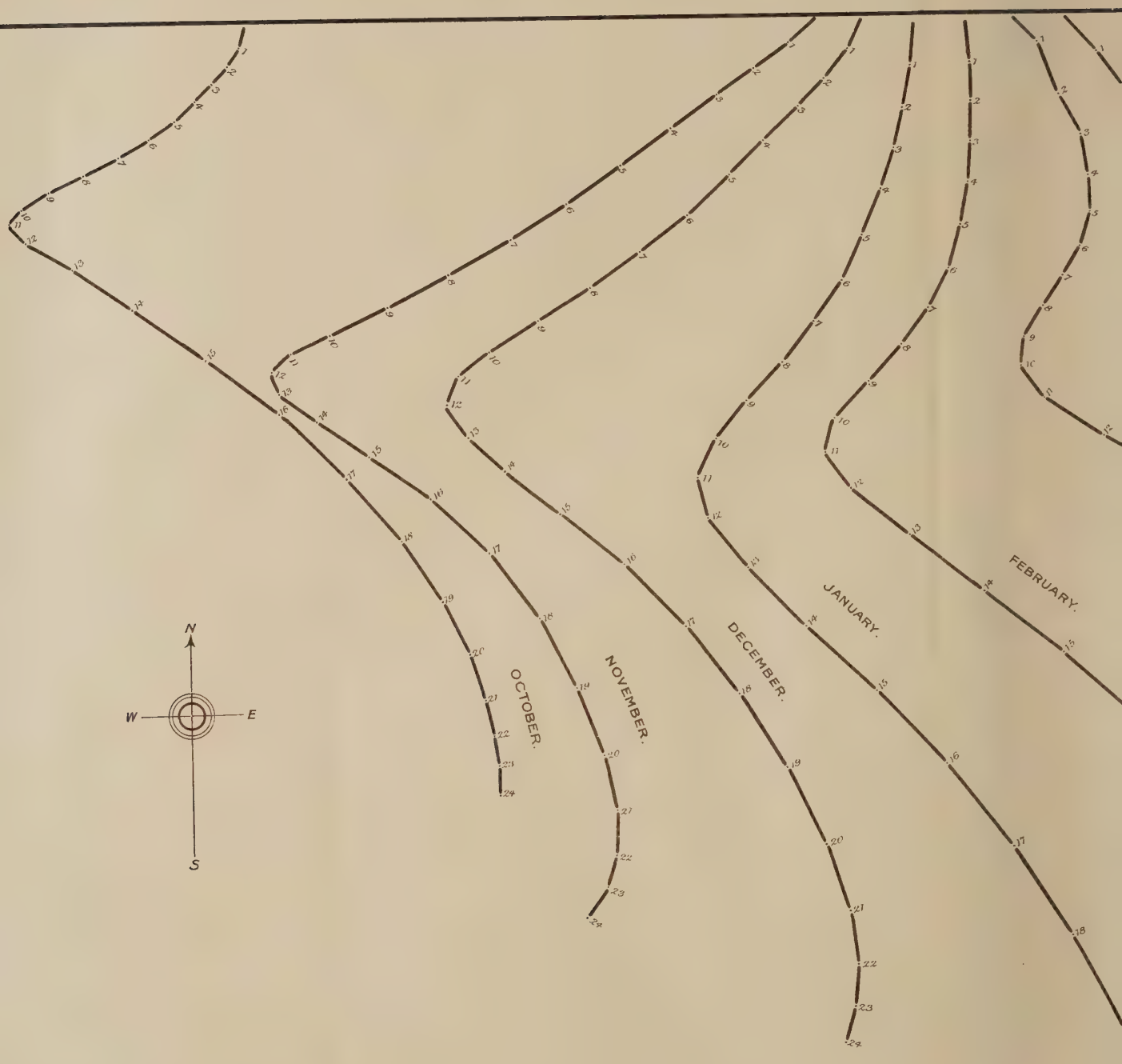
TABLE OF MONTHLY AVERAGES.

Month.	Monthly mean pressure of vapour.	Monthly mean additional capacity.
October	·847	·198
November	·692	·292
December	·644	·271
January	·587	·249
February	·595	·262
March	·707	·254
April	·811	·266
May	·874	·303
June	·931	·212
July	·904	·133
August	·882	·128
September	·869	·146

VARIATIONS IN THE TEMPERATURE OF THE GROUND.

From Observations recorded at the Kolaba Observatory, Bombay.





Scale: 15 miles to an inch.

PLATE C.

SHOWING THE MOVEMENT AND INTENSITY OF THE WIND.

From Observations recorded at the Kolaba Observatory, Bombay.

The Curves show the track, during the twenty-four hours of the typical days of each of the months October to April, of an imaginary balloon, moving always in the direction and with the velocity of the normal wind at Bombay.

The figures placed along the Course of the Curves mark the hours of the Bombay Civil day.

APPENDIX, No. LVIII.

Eyre & Spottiswoode. Lith. London

STATEMENT

SHOWING

THE WEEKLY MEAN VALUES

OF

DIFFERENT METEOROLOGICAL FACTORS

AND

OFFICIAL FIGURES OF MORTALITY

USED IN THE PREPARATION OF THE

FOLLOWING CHARTS.

STATEMENT showing the WEEKLY MEAN VALUES of different METEOROLOGICAL FACTORS used in the CALCULATIONS and FIGURES of MORTALITY published in the "GOVERNMENT GAZETTE."

Abbreviations:—t = Temperature of air; t′ = Temperature of evaporation; T = Temperature of dew point; t_{20} = Temperature of ground at 20″ depth; t_{60} = Temperature of ground at 60″ depth; F, f′ = Tension corresponding to saturation of T, t′; P = Weekly atmospheric pressure.

Date for the Week ending	t.	t′.	T.	t_{20}.	t_{60}.	$F = f' - a(t - t')\frac{P-f'}{30}$.	1 − F, or 1 − Col. 7.	$t_{20} - t$, or Col. 5 − Col. 2.	$t_{60} - t$, or Col. 6 − Col. 2.	$t_{60} - t'$, or Col. 6 − Col. 3.	$t_{60} - T$, or Col. 6 − Col. 4.	T − (t_{20} − t). In Terms of Temperature, or Col. 4 − Col. 9.	T − (t_{20} − t). In Terms of Tension corresponding to Temperatures.	T − (t_{60} − t). In Terms of Temperature, or Col. 4 − Col. 10.	T − (t_{60} − t). In Terms of Tension corresponding to Temperatures.	P.	Wind.	Pressure of Vapour derived from the formula, $F' = f' - a(t_{60} - t')\frac{P-f'}{30}$.	Dew Point corresponding to F′.	Mortality Figures as given in "Government Gazette."	Mortality Figures, Average of Five previous Years corresponding to Col. 19.	Mortality Figures from 7th August 1894, Average of Five previous Years, as given in "Government Gazette."	Mortality Figures from 2nd August 1897, Average of Five previous Years, as given in "Government Gazette."	Mortality Figures, Daily Average derived from Col. 22.	Col. 8 × Col. 10.
1.	2.	3.	4.	5.	6.	7.	8.	9.	11.	10.	12.	13.		14.		15.	16.	17.	18.	19.	20.	21.	22.	23.	24.
4 Aug. 1896	79·0	76·6	75·7	84·8	86·1	·890	·110	5·8	7·1	9·5	10·4	69·9	·731	68·6	·699	29·594	—	·811	73·0	564	564	492	470		·7810
11 „ „	80·3	77·3	76·3	84·4	85·6	·905	·095	4·1	5·3	8·3	9·3	72·2	·790	71·0	·758	·743	—	·845	74·2	598	534	482	466	66	·5035
18 „ „	78·6	76·2	75·3	84·0	85·4	·877	·123	5·4	6·8	9·2	10·1	69·9	·731	68·5	·697	·705	—	·802	72·7	650	538	474	472		·8364
25 „ „	80·7	76·9	75·5	83·8	85·0	·881	·119	3·1	4·3	8·1	9·5	72·4	·795	71·2	·764	·777	—	·835	73·9	669	581	545	449		·5117
1 Sept. „	80·9	76·1	74·3	83·9	84·7	·849	·151	3·0	3·8	8·6	10·4	71·3	·766	70·5	·746	·795	—	·805	72·8	667	540	495	469		·5738
8 „ „	81·8	76·1	74·0	84·1	84·6	·838	·162	2·3	2·8	8·5	10·6	71·7	·777	71·2	·764	·817	—	·806	72·8	593	531	505	461		·4536
15 „ „	81·9	76·0	73·7	84·2	84·5	·831	·169	2·3	2·6	8·5	10·8	71·4	·769	71·1	·761	·819	—	·803	72·7	618	492	497	450	65	·4394
22 „ „	82·2	76·1	73·8	84·2	84·4	·834	·166	2·0	2·2	8·3	10·6	71·8	·779	71·6	·774	·837	—	·808	72·9	647	506	489	457		·3652
29 „ „	83·1	78·4	76·8	84·5	84·4	·921	·079	1·4	1·3	6·0	7·6	75·4	·879	75·5	·882	·815	—	·905	76·3	720	527	492	435		·1027
6 Oct. „	85·4	79·7	77·8	85·1	84·6	·954	·046	−0·3	−0·8	4·9	6·8	78·1	·961	78·6	·977	·819	—	·959	78·0	791	491	462	432		−·0368
13 „ „	83 9	78·7	76·8	85·8	84·9	·923	·077	1·9	1·0	6·2	8·1	74·9	·865	75·8	·891	·855	—	·913	76·5	634	498	482	419		·0770
20 „ „	83·8	78·1	76·0	85·7	84·9	·900	·100	1·9	1·1	6·8	8·9	74·1	·842	74·9	·865	·892	—	·886	75·6	606	477	456	414	61	·1100
27 „ „	84·3	77·5	75·0	85·9	85·1	·870	·130	1·6	0·8	7·6	10·1	73·4	·822	74·2	·845	·920	—	·859	74·7	698	470	428	433		·1040
3 Nov. „	84·0	74·5	70·5	86·1	85·3	·751	·249	2·1	1·3	10·8	14·8	68·4	·694	69·2	·713	·902	—	·733	70·0	668	443	432	414		·3237
10 „ „	83·6	75·7	72·5	85·8	85·5	·802	·198	2·2	1·9	9·8	13·0	70·3	·741	70·6	·748	·873	—	·779	71·8	623	449	426	400	58	·3762
17 „ „	83·6	74·1	70·1	85·8	85·5	·737	·263	2·2	1·9	11·4	15·4	67·9	·682	68·2	·689	·830	—	·715	69·3	704	462	442	398		·4997
24 „ „	80·2	71·9	68·1	85·7	85·5	·696	·304	5·5	5·3	13·6	17·4	62·6	·568	62·8	·572	·824	—	·631	65·6	760	446	418	406		1·6112
1 Dec. „	80·2	74·7	72·5	84·1	85·3	·798	·202	3·9	5·1	10·6	12·8	68·6	·699	67·4	·671	·927	—	·742	70·4	772	457	433	403		1·0302
8 „ „	80·4	72·4	69·0	84·3	85·5	·711	·289	3·9	5·1	13·1	16·5	65·1	·619	63·9	·594	·955	—	·649	66·5	1,051	460	433	411		1·4739
15 „ „	79·8	68·6	62·7	84·0	85·0	·575	·425	4·2	5·2	16·4	22·3	58·5	·491	57·5	·474	·962	—	·515	59·9	1,310	470	453	410	60	2·2100
22 „ „	79·2	69·9	65·3	83·5	84·9	·633	·367	4·3	5·7	15·0	19·6	61·0	·537	59·6	·511	·915	—	·564	62·4	1,416	470	462	429		2·0919
29 „ „	77·1	69·8	66·1	83·0	84·6	·653	·347	5·9	7·5	14·8	18·5	60·2	·522	58·6	·493	·975	—	·562	62·3	1,853	—	462	431		2·6025
5 Jan. 1897	75·2	67·6	63·4	81·9	84·6	·599	·401	6·7	9·4	17·0	21·2	56·7	·461	54·0	·418	·952	—	·485	58·2	1,711	494	451	418		3·7694
12 „ „	78·1	71·5	68·5	81·8	84·3	·699	·301	3·7	6·2	12·8	15·8	64·8	·613	62·3	·562	·930	—	·628	65·5	1,638	484	457	440		1·8662
19 „ „	72·0	64·5	60·2	81·2	83·8	·526	·474	9·2	11·8	19·3	23·6	51·0	·374	48·4	·340	·972	—	·390	52·1	1,758	501	465	451	63	5·5932

19 „ „	72·0	64·5	60·2	81·2	83·8	·526	·474	9·2	11·8	19·3	23·6	51·0	·374	48·4	·340	·972	—	·390	52·1	1,758	501	465	451			5·5932
26 „ „	73·6	66·1	61·8	79·9	83·6	·564	·436	6·3	10·0	17·5	21·8	55·5	·441	51·8	·385	·965	—	·445	55·8	1,721	518	481	443			4·3600
2 Feb. „	73·7	66·0	61·8	79·7	83·3	·554	·464	6·0	9·6	17·3	21·5	55·8	·446	52·2	·391	·914	—	·445	55·8	1,645	526	488	434			4·2816
9 „ „	75·1	67·7	63·9	79·4	82·9	·597	·403	4·3	7·8	15·2	19·0	59·6	·511	56·1	·451	·918	—	·509	59·5	1,911	540	515	463			3·1434
16 „ „	74·4	66·5	62·1	79·6	82·6	·562	·438	5·2	8·2	16·1	20·5	56·9	·464	53·9	·416	·883	—	·470	57·3	1,728	569	510	450			3·5916
23 „ „	76·5	69·7	66·5	79·8	82·5	·653	·347	3·3	6·0	12·8	16·0	63·2	·580	60·5	·527	·894	—	·582	63·3	1,650	542	513	465			2·0820
2 Mar. „	75·5	68·0	64·2	80·0	82·6	·604	·396	4·5	7·1	14·6	18·4	59·7	·513	57·1	·467	29·916	—	·522	60·2	1,484	546	536	476			2·8116
9 „ „	77·3	70·6	67·6	80·2	82·5	·677	·323	2·9	5·2	11·9	14·9	64·7	·611	62·4	·564	·870	—	·615	64·9	1,326	544	571	475			1·6796
16 „ „	77·0	69·9	66·5	80·5	82·4	656	·344	3·5	5·4	12·5	15·9	63·0	·576	61·1	·539	·904	—	·591	63·8	1,258	534	553	469			1·8576
23 „ „	78·7	72·0	69·1	81·0	82·6	·711	·289	2·3	3·9	10·6	13·5	66·8	·657	65·2	·622	·835	—	·666	67·2	1,139	539	529	493			1·1271
30 „ „	79·2	72·7	70·0	81·6	82·9	·734	·266	2·4	3·7	10·2	12·9	67·6	·675	66·3	·646	·846	—	·690	68·2	1,141	544	505	441			·9842
6 April „	79·9	73·3	70·6	82·0	82·8	·749	·251	2·1	2·9	9·5	12·2	68·5	·697	67·7	·678	·849	—	·715	69·3	1,007	559	513	442			·7279
13 „ „	83·3	76·1	73·3	82·7	83·0	·824	·176	−0·6	−0·3	6·9	9·7	73·9	·836	73·6	·828	·809	—	·823	73·4	970	572	531	463			−·0528
20 „ „	84·0	77·7	75·4	83·9	83·4	·879	·121	−0·1	−0·6	5·7	8·0	75·5	·882	76·0	·897	·842	—	·886	75·6	836	614	564	455			−·0726
27 „ „	84·0	77·7	75·4	84·8	84·2	·880	·120	0·8	−0·2	6·5	8·8	74·6	·856	75·6	·885	·818	—	·877	75·3	671	625	565	466			− 0240
4 May „	84·9	77·9	75·4	85·5	84·6	·879	·121	0·6	−0·3	6·7	9·2	74·8	·862	75·7	·888	·758	—	·882	75·5	638	606	522	459			−·0363
11 „ „	85·2	77·9	75·3	86·1	85·0	·876	·124	0·9	−0·2	7·1	9·7	74·4	·850	75·5	·882	·788	—	·877	75·3	558	599	545	463			−·0248
18 „ „	85·5	78·5	76·1	86·3	85·3	·899	·101	0·8	−0·2	6·8	9·2	75·3	·876	76·3	·906	·799	—	·899	76·1	547	602	533	444			−·0202
25 „ „	85·6	79·0	76·7	87·0	86·0	·918	·082	1·4	+0·4	7·0	9·3	75·3	·876	76·3	·906	·765	—	·913	76·5	541	581	493	445			·0328
1 June „	87·0	80·2	78·0	87·4	86·1	·959	·041	0·4	−0·9	5·9	8·1	77·6	·946	78·9	·987	·759	—	·965	78·2	500	—	477	438			−·0369
8 „ „	87·6	80·8	78·6	87·9	86·6	·978	·022	0·3	−1·0	5·8	8·0	78·3	·968	79·6	1·010	·655	—	·986	78·9	585	545	489	452			−·0220
15 „ „	85·9	80·8	79·1	87·7	86·7	·993	·007	1·8	+0·8	5·9	7·6	77·3	·936	78·3	·968	·680	—	·985	78·8	531	514	457	431			·0056
22 „ „	84·2	79·9	78·5	87·4	87·3	·976	·024	3·2	3·1	7·4	8·8	75·3	·876	75·4	·879	·588	—	·939	77·4	501	465	433	403			·0744
29 „ „	84·7	79 0	77·1	87·1	87·1	·929	·071	2 4	2·4	8·1	10·0	74·7	·859	74·7	·859	·722	—	·901	76·1	549	468	431	400			·1704
6 July „	84·3	78·9	77·1	87·4	87·2	·929	·071	3·1	2·9	8·3	10·1	74·0	·839	74·2	·845	·697	—	·896	76·0	550	500	456	397			·2059
13 „ „	81·5	78·4	77·4	86·8	87·1	·940	·060	5·3	5·6	8·7	9·7	72·1	·787	71·8	·779	·550	—	·876	75·3	577	491	463	392			·3360
20 „ „	80·9	78·1	77·2	86·3	87·3	·933	·067	5·4	6·4	9·2	10·1	71·8	·779	70·8	·753	·662	—	·860	74·7	678	522	470	422			·4288
27 „ „	79·9	77·8	77·1	85·6	86·8	·931	·069	5·7	6·9	9·0	9·7	71·4	·769	70·2	·738	·645	—	·853	74·5	770	548	511	441			·4761
3 Aug. „	82·0	78·6	77·5	85·4	86·6	·944	·056	3·4	4·6	8·0	9·1	74·1	·842	72·9	·809	·645	12·7 W.	·889	75·7	1,071	585	536	468			·2576
10 „ „	80·8	78·1	77·1	85·2	86·1	·932	·068	4·4	5·3	8·0	9·0	72·7	·803	71·8	·779	·600	21·2 W.	·873	75·2	965	558	519	471			·3604
17 „ „	81·0	78·1	77·1	84·9	86·0	·930	·070	3·9	5·0	7·9	8·9	73·2	·817	72·1	·787	·651	14·1 W.	·874	75·2	978	552	517	486			·3500
24 „ „	81·7	78·1	76·7	84·7	85·8	·920	·080	3·0	4·1	7·7	9·1	73·7	·831	72·6	·800	·735	11·1 W.	·877	75·3	866	600	563	469			·3280
31 „ „	82·4	77·3	75·4	85·0	85·6	·881	·119	2·6	3·2	8·3	10·2	72·8	·806	72·2	·790	·705	8·4 W.	·845	74·2	814	—	520	491			·3808
7 Sept. „	82·1	77·9	76·4	85·2	85·6	·908	·092	3·1	3·5	7·7	9·2	73·3	·820	72·9	·809	·753	9·0 W.	·870	75·1	838	—	521	484	—		·3220
14 „ „	81·0	77·4	76·1	84·9	85·6	·899	·101	3·9	4·6	8·2	9·5	72·2	·790	71·5	·771	·740	6·6 W.	·850	74·4	876	—	490	467	—		·4646
21 „ „	83·0	78·1	76·6	84·7	85·3	·916	·084	1·7	2·3	7·2	8·7	74·9	·865	74·3	·848	·738	3·3 W.	·883	75·5	869	—	493	460	—		·1932
28 „ „	81·5	78·4	77·4	85·1	85·4	·940	·060	3·6	3·9	7·0	8·0	73·8	·833	73·5	·825	·759	0·6 E.	·894	75·9	823	—	—	—	—		·2340
5 Oct. „	82·1	78·7	77·5	85·1	85·5	·944	·056	3·0	3·4	6·8	8·0	74·5	·853	74·1	·842	·789	0·1 E.	·906	76·3	776	—	—	—	—		·1904
12 „ „	82·8	77·8	76·0	85·1	85·5	·898	·102	2·3	2·7	7·7	9·5	73·7	·831	73·3	820	·849	0·2 E.	·867	75·0	750	—	—	—	—		·2754
19 „ „	82·9	75·9	73·1	85·2	85·3	·821	·179	2·3	2·4	9·4	12·2	70·8	·753	70·7	·751	·809	1·3 W.	·790	72·2	776	—	—	—	—		·4296
26 „ „	81·9	76·4	74·3	85·1	85·5	·852	·148	3·2	3·6	9·1	11·2	71·1	·761	70·7	·751	29·853	2·6 W.	·808	72·9	754	—	—	—	—		·5328
2 Nov. „	81·7	74·2	71·1	85·3	85·6	·766	·234	3·6	3·9	11·4	14·5	67·5	·673	67·2	·666	·916	0·2 E.	·718	69·4	741	—	—	—	—		·9126
9 „ „	80·6	72·1	68·2	84·7	85·2	·694	·306	4·1	4·6	13·1	17·0	64·1	·598	63·6	·588	·898	2·4 E.	·641	66·1	701	—	—	—	—		1·4076
16 „ „	79·8	71·6	67·9	84·2	85·2	·688	·312	4·4	5·4	13·6	17·3	63·5	·586	62·5	·566	·871	0·2 W.	·622	65·2	700	—	—	—	—		1·6848

Table showing the Weekly Mean Values of different Meteorological Factors—*continued*.

Date for the Week ending.	t.	t'.	T.	t_{20}.	t_{60}.	$F' = f' - a\,(t-t')\frac{P-f'}{30}$.	1−F, or 1−Col. 7.	$t_{20}-t$, or Col. 5−Col. 2.	$t_{60}-t$, or Col. 6−Col. 2.	$t_{60}-t'$, or Col. 6−Col. 3.	$t_{60}-T$, or Col. 6−Col. 4.	T−(t_{20}−t). In Terms of Temperature, or Col. 4−Col. 9.	T−(t_{20}−t). In Terms of Tension corresponding to Temperatures.	T−(t_{60}−t). In Terms of Temperature, or Col. 4−Col. 10.	T−(t_{60}−t). In Terms of Tension corresponding to Temperatures.	P.	Wind.	Pressure of Vapour derived from the formulæ, $F' = f' - a\,(t_{60}-t')\frac{P-f'}{30}$.	Dew Point corresponding to F'.	Mortality Figures as given in "Government Gazette."	Mortality Figures, Average of Five previous Years corresponding to Col. 19.	Mortality Figures from 7th August 1894, Average of Five previous Years as given in "Government Gazette."	Mortality Figures from 2nd August 1887, Average of Five previous Years as given in "Government Gazette."	Mortality Figures, Daily Average derived from Col. 22.	Col. 8. × Col. 10.
1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	12.	13.		14.		15.	16.	17.	18.	19.	20.	21.	22.	23.	24.
23 Nov. 1897	78·3	69·1	64·5	83·9	85·2	·610	·390	5·6	6·9	16·1	20·7	58·9	·498	57·6	·476	29·914	1·7 E.	·531	60·7	682	—	—	—	—	2·6910
30 „ „	78·2	69·2	64·6	83·3	84·9	·613	·387	5·1	6·7	15·7	20·3	59·5	·509	57·9	·481	·902	2·7 E.	·538	61·1	704	—	—	—	—	2·5929
7 Dec. „	77·8	69·4	65·5	82·9	85·0	·627	·373	5·1	7·2	15·6	19·5	60·4	·525	58·3	·488	·941	3·2 E.	·544	61·4	706	—	—	—	—	2·6856
14 „ „	76·1	68·3	64·3	82·4	84·5	·606	·394	6·3	8·4	16·2	20·2	58·0	·482	55·9	·447	·937	0·7 E.	·511	59·6	785	—	—	—	—	3·3096
21 „ „	76·0	65·4	59·2	82·0	84·4	·511	·489	6·0	8·4	19·0	25·2	53·2	·406	50·8	·372	·943	3·7 E.	·413	53·7	835	—	—	—	—	4·1076
28 „ „	75·4	66·9	62·3	81·5	84·0	·566	·434	6·1	8·6	17·1	21·7	56·2	·452	53·7	·413	·963	2·7 E.	·468	57·2	975	—	—	—	—	3·7324
4 Jan. 1898	74·2	66·8	63·0	81·3	84·0	·578	·422	7·1	9·8	17·2	21·0	—	—	53·2	·406	·999	0·8 W.	·465	57·0	1,061	—	—	—	—	4·1356
11 „ „	74·2	65·8	61·1	80·7	83·6	·541	·459	6·5	9·4	17·8	22·5	—	—	51·7	·384	·982	0·7 E.	·435	55·1	1,307	—	—	—	—	4·3146
18 „ „	77·1	67·3	62 0	80·7	83·5	·563	·437	3·6	6·4	16·2	21·5	—	—	55·6	·443	·949	3·2 E.	·487	58·3	1,540	—	—	—	—	2·7968
25 „ „	76·9	68·3	64·0	81·5	83·4	·601	·399	4·6	6·5	15·1	19·4	—	—	57·5	·474	·957	1·3 E.	·523	60·3	1,726	—	—	—	—	2·5935
1 Feb. „	77·9	69·6	65·6	81·8	83·6	·634	·366	3·9	5·7	14·0	18·0	—	—	59·9	·516	·927	0·2 E.	·567	62·6	1,871	—	—	—	—	2·0862
8 „ „	77·3	68·8	65·1	82·0	83·5	·621	·379	4·7	6·2	14·7	18·4	—	—	58·9	·498	·864	1·2 W.	·540	61·2	2,067	—	—	—	—	2·3498
15 „ „	75·1	67·1	62·9	81·8	83·4	·577	·423	6·7	8·3	16·3	20·5	—	—	54·6	·427	·865	3·8 W.	·482	58·0	2,195	—	—	—	—	3·5109
22 „ „	75·0	66·5	61·5	81·4	83·4	·565	·435	6·4	8·4	16·8	21·9	—	—	53·1	·404	·857	3·3 W.	·465	57·0	1,974	—	—	—	—	3·6540
1 Mar. „	79·2	73·2	70·6	81·7	83·6	·754	·246	2·5	4·4	10·4	13·0	—	—	66·2	·644	·890	2·7 W.	·701	68·7	2,080	—	—	—	—	1·0824
8 „ „	77·2	67·8	62·3	82·2	83·5	·589	·411	5·0	6·3	15·7	21 2	—	—	56·0	·449	·879	2·6 W.	·505	59·3	2,184	—	—	—	—	2·5893
15 „ „	80·4	73·4	70·3	83·0	83·7	·746	·254	2·6	3·3	10·3	13·4	—	—	67·0	·662	·844	3·0 W.	·708	69·0	2,137	—	—	—	—	·8382
22 „ „	81·3	73·9	70·7	84·1	83·9	·758	·242	2·8	2·6	10·0	13·2	—	—	68·1	·687	·822	5·8 W.	·725	69·7	2,268	—	—	—	—	·6292
29 „ „	81·4	74·5	71·7	84·7	84·5	·778	·222	3·3	3·1	10·0	12·8	—	—	68·6	·699	·820	5·2 W.	·742	70·4	1,938	—	—	—	—	·6882
5 April „	82·9	73·7	69·7	85·4	84·9	·730	·270	2·5	2·0	11·2	15·2	—	—	67·7	·678	·808	6·9 W.	·707	68·9	1,519	—	—	—	—	·5400
12 „ „	83·0	75·9	73·1	—	85·1	·816	—	—	2·1	9·2	—	—	—	71·0	·758	·788	4·7 W.	·792	72·3	1,303	—	—	—	—	—
19 „ „	84·0	78·2	76·2	—	85·6	·904	—	—	1·6	7·4	—	—	—	74·6	·856	·796	5·8 W.	·884	75·6	1,202	—	—	—	—	—
26 „ „	84·7	78·7	76·6	—	86·1	·914	—	—	1·4	7·4	—	—	—	75·2	·873	·759	5·8 W.	·900	76·1	1,116	—	—	—	—	—
3 May „	86·3	79·7	77·5	—	86·3	·940	—	—	0·0	6·6	—	—	—	77·5	·943	·729	6·5 W.	·941	77·4	872	—	—	—	—	—
10 „ „	85·6	77·9	75·1	—	86·9	·872	—	—	1·3	9·0	—	—	—	73·8	·833	·743	7·8 W.	·856	74·6	725	—	—	—	—	—
17 „ „	85·8	77·4	74·4	—	87·3	·851	—	—	1·5	9·9	—	—	—	72·9	·809	·808	7·9 W.	·830	73·7	616	—	—	—	—	—
24 „ „	86·0	79·2	76·9	—	87·8	·925	—	—	1·8	8·6	—	—	—	75·1	·871	·786	9·1 W.	·902	76·2	633	—	—	—	—	—
31 „ „	86·9	80·1	77·7	—	88·1	·951	—	—	1·2	8·0	—	—	—	76·5	·912	·739	6·6 W.	·939	77·4	589	—	—	—	—	—

7 June	„	87·6	80·2	77·7	—	88·5	·950	—	—	0·9	8·3	—	—	—	76·8	·921	·712	9·9 W.	·939	77·4	525	—	—	—	—	—
14	„	82·9	79·1	77·8	—	88·8	·953	—	—	5·9	9·7	—	—	—	71·9	·782	·547	3·1 W.	·888	75·7	504	—	—	—	—	—
21	„	82·1	78·8	77·7	—	88·8	·949	—	—	6·7	10·0	—	—	—	71·0	·758	·601	14·9 W.	·875	75·3	448	—	—	—	—	—
28	„	82·9	78·1	76·5	—	88·8	·912	—	—	5·9	10·7	—	—	—	70·6	·748	·737	10·7 W.	·843	74·1	500	—	—	—	—	—
5 July	„	81·8	78·1	76·8	—	88·5	·921	—	—	6·7	10·4	—	—	—	70·1	·736	·655	12·5 W.	·847	74·3	410	—	—	—	—	—
12	„	82·7	78·5	77·0	—	88·5	·928	—	—	5·8	10·0	—	—	—	71·2	·764	·635	15·3 W.	·865	74·9	523	—	—	—	—	—
19	„	80·8	77·8	76·7	—	88·5	·919	—	—	7·7	10·7	—	—	—	69·0	·709	·659	16·1 W.	·834	73·8	568	—	—	—	—	—
26	„	79·7	77·4	76·5	—	88·2	·913	—	—	8·5	10·8	—	—	—	68·0	·685	·590	9·0 W.	·822	73·4	583	—	—	—	—	—
2 Aug.	„	80·9	77·7	76·6	—	87·9	—	—	—	7·0	10·2	—	—	—	69·6	·723	·631	16·3 W.	·837	73·9	615	—	—	—	—	—
9	„	81·3	77·4	76·0	—	87·6	—	—	—	6·3	10·2	—	—	—	69·7	·726	·704	18·7 W.	·827	73·6	655	—	—	—	—	—
16	„	80·9	76·9	75·5	—	87·5	—	—	—	6·6	10·6	—	—	—	68·9	·706	·750	16·1 W.	·807	72·8	743	—	—	—	—	—
23	„	81·1	76·8	75·2	—	87·3	—	—	—	6·2	10·5	—	—	—	69·0	·709	·763	11·9 W.	·805	72·8	765	—	—	—	—	—
30	„	81·4	77·3	75·8	—	87·1	—	—	—	5·7	9·8	—	—	—	70·1	·736	·797	11·5 W.	·827	73·6	811	—	—	—	—	—
6 Sept.	„	81·7	77·1	75·4	—	87·3	—	—	—	5·6	10·2	—	—	—	69·8	·728	·745	9·1 W.	·817	73·2	869	—	—	—	—	—
13	„	79·0	76·3	75·4	—	87·1	—	—	—	8·1	10·8	—	—	—	67·3	·668	·681	7·8 W.	·787	72·1	823	—	—	—	—	—
20	„	79·9	76·7	75·5	—	86·7	—	—	—	6·8	10·0	—	—	—	68·7	·701	·776	7·9 W.	·807	72·8	748	—	—	—	—	—
27	„	81·8	77·5	75·9	—	86·6	—	—	—	4·8	9·1	—	—	—	71·1	·761	·804	1·5 W.	·842	74·1	864	—	—	—	—	—
4 Oct.	„	82·8	78·5	77·1	—	86·4	—	—	—	3·6	7·9	—	—	—	73·5	·825	·790	—	·887	75·7	1,002	—	—	—	—	—
11	„	85·3	79·1	76·8	—	86·5	—	—	—	1·2	7·4	—	—	—	75·6	·885	·777	—	·913	76·5	990	—	—	—	—	—
18	„	83·7	78·2	76·3	—	86·6	—	—	—	2·9	8·4	—	—	—	73·4	·822	·826	—	·872	75·2	828	—	—	—	—	—
25	„	84·6	76·2	72·9	—	86·7	—	—	—	2·1	10·5	—	—	—	70·8	·753	·836	—	·787	72·1	711	—	—	—	—	—
1 Nov.	„	84·6	76·2	72·9	—	86·7	—	—	—	2·1	10·5	—	—	—	70·8	·753	·829	—	·787	72·1	654	—	—	—	—	—
8	„	83·7	74·1	70·0	—	86·8	—	—	—	3·1	12·7	—	—	—	66·9	·659	·774	—	·701	68·7	645	—	—	—	—	—
15	„	82·9	77·2	75·0	—	87·0	—	—	—	4·1	9·8	—	—	—	70·9	·756	·836	—	·824	73·5	604	—	—	—	—	—
22	„	81·4	72·1	68·0	—	87·0	—	—	—	5·6	14·9	—	—	—	62·4	·564	·877	—	·622	65·2	606	—	—	—	—	—
29	„	80·7	72·5	68·8	—	86·9	—	—	—	6·2	14·4	—	—	—	62·6	·568	·921	—	·638	66·0	616	—	—	—	—	—
6 Dec.	„	81·8	74·7	71·7	—	86·7	—	—	—	4·9	12·0	—	—	—	66·8	·657	·900	—	·726	69·7	679	—	—	—	—	—
13	„	80·4	73·6	70·7	—	86·6	—	—	—	6·2	13·0	—	—	—	64·5	·601	·927	—	·683	67·9	688	—	—	—	—	—
20	„	79·4	71·1	67·2	—	86·3	—	—	—	6·9	15·2	—	—	—	60·3	·524	·933	—	·593	63·9	798	—	—	—	—	—
27	„	77·5	68·8	64·3	—	86·3	—	—	—	8·8	17·5	—	—	—	55·5	·442	·905	—	·509	59·5	742	—	—	—	—	—
3 Jan.	1899	72·6	63·6	58·1	—	86·0	—	—	—	13·4	22·4	—	—	—	44·7	·296	·915	—	·337	48·2	894	—	—	—	—	—
10	„	72·4	62·0	55·2	—	85·5	—	—	—	13·1	23·5	—	—	—	42·1	·258	·971	—	·292	44·3	1,111	—	—	—	—	—
17	„	73·2	65·8	61·8	—	85·1	—	—	—	11·9	19·3	—	—	—	49·9	·373	·979	—	·418	54·0	1,239	—	—	—	—	—
24	„	75·3	66·5	61·8	—	84·6	—	—	—	9·3	18·1	—	—	—	52·5	·395	·945	—	·447	55·9	1,448	—	—	—	—	—
31	„	73·9	64·9	59·7	—	84·6	—	—	—	10·7	19·7	—	—	—	49·0	·348	·913	—	·395	52·5	1,601	—	—	—	—	—
7 Feb.	„	76·6	67·8	63·2	—	84·5	—	—	—	7·9	16·7	—	—	—	55·3	·438	·918	—	·494	58·7	1,612	—	—	—	—	—
14	„	76·1	69·6	66·5	—	84·2	—	—	—	8·1	14·6	—	—	—	58·4	·489	·925	—	·560	62·2	1,808	—	—	—	—	—
21	„	75·2	67·7	63·8	—	84·2	—	—	—	9·0	16·5	—	—	—	54·8	·430	·898	—	·480	58·5	1,907	—	—	—	—	—
28	„	77·4	69·8	66·1	—	84·4	—	—	—	7·0	14·6	—	—	—	59·1	·502	·833	—	·561	62·3	2,249	—	—	—	—	—
7 Mar.	„	78·6	71·3	68·0	—	84·3	—	—	—	5·7	13·0	—	—	—	62·3	·562	·864	—	·617	65·0	2,431	—	—	—	—	—
14	„	80·0	72·2	68·7	—	84·4	—	—	—	4·4	12·2	—	—	—	64·3	·603	·908	—	·651	66·6	2,312	—	—	—	—	—

N. A. F. Moos, Director,
Government Observatory, Bombay.

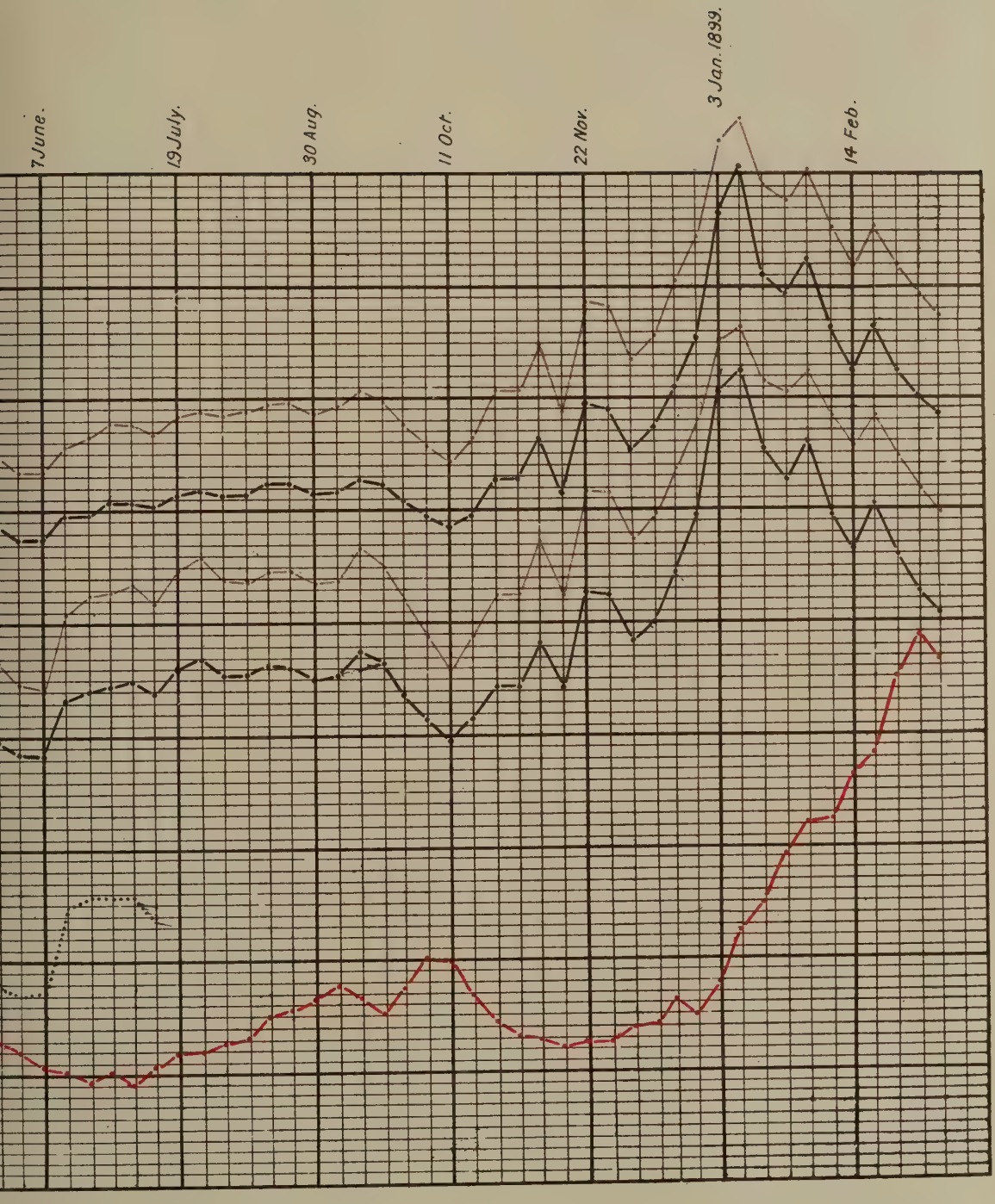
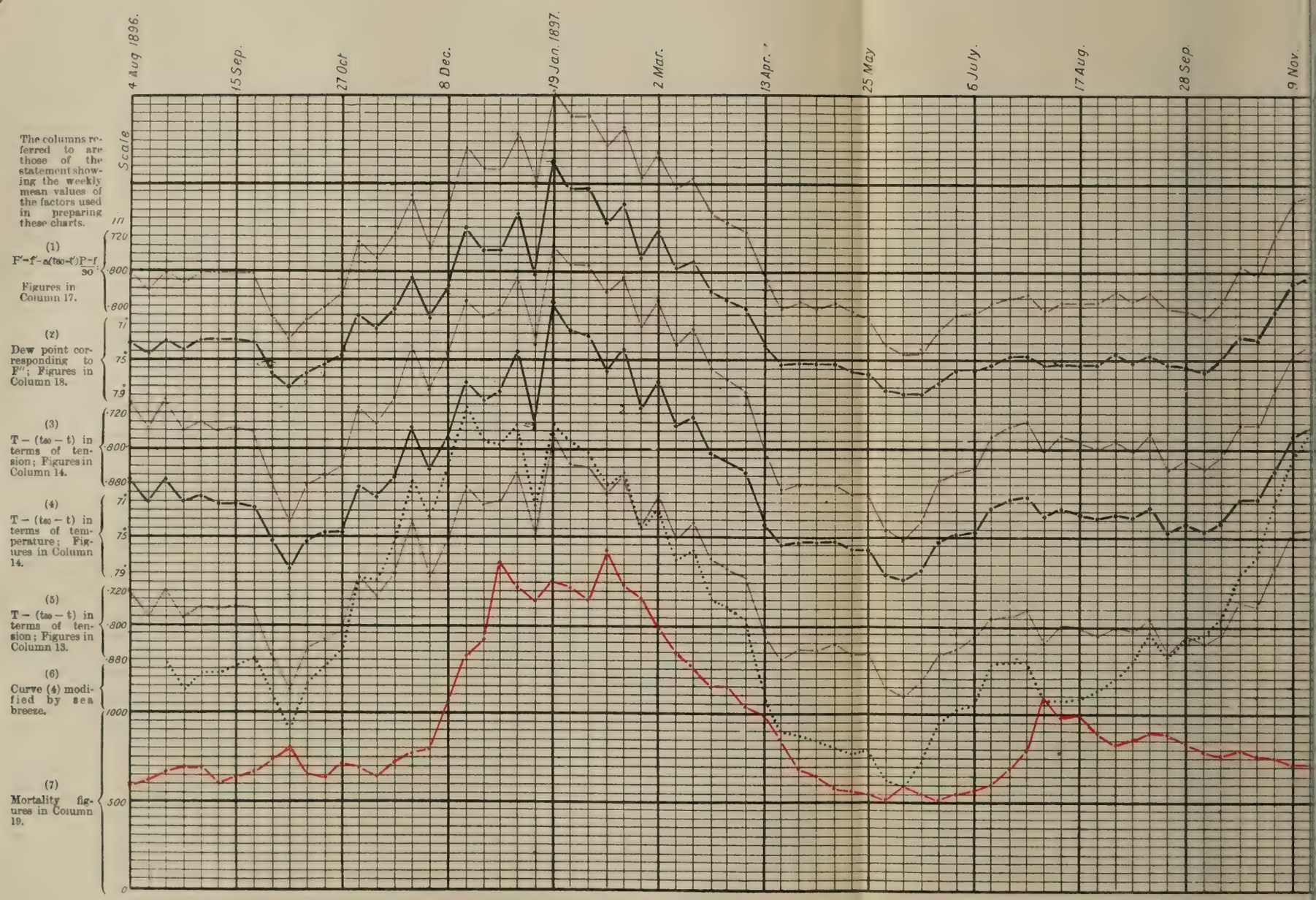

7 June.
19 July.
30 Aug.
11 Oct.
22 Nov.
3 Jan. 1899.
14 Feb.

CHART A.—TRIAL CURVES. SHOWING THE RELATION BETV



The columns referred to are those of the statement showing the weekly mean values of the factors used in preparing these charts.

(1) $F'=f-a(t_{80}-t')\frac{P-f}{30}$ Figures in Column 17.

(z) Dew point corresponding to F′; Figures in Column 18.

(3) $T-(t_{80}-t)$ in terms of tension; Figures in Column 14.

(4) $T-(t_{80}-t)$ in terms of temperature; Figures in Column 14.

(5) $T-(t_{80}-t)$ in terms of tension; Figures in Column 13.

(6) Curve (4) modified by sea breeze.

(7) Mortality figures in Column 19.

EN PLAGUE AND METEOROLOGICAL CONDITIONS IN BOMBAY.

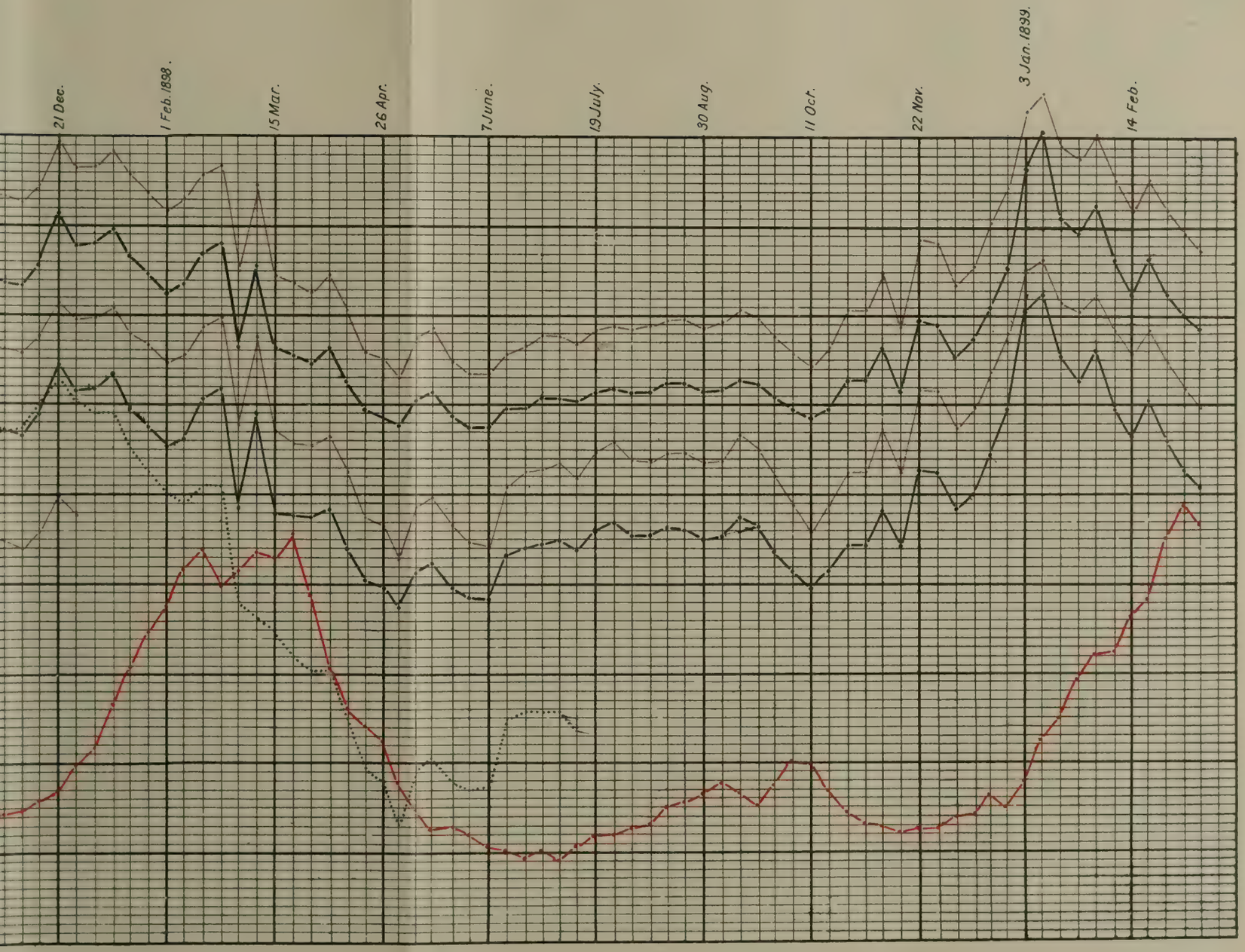
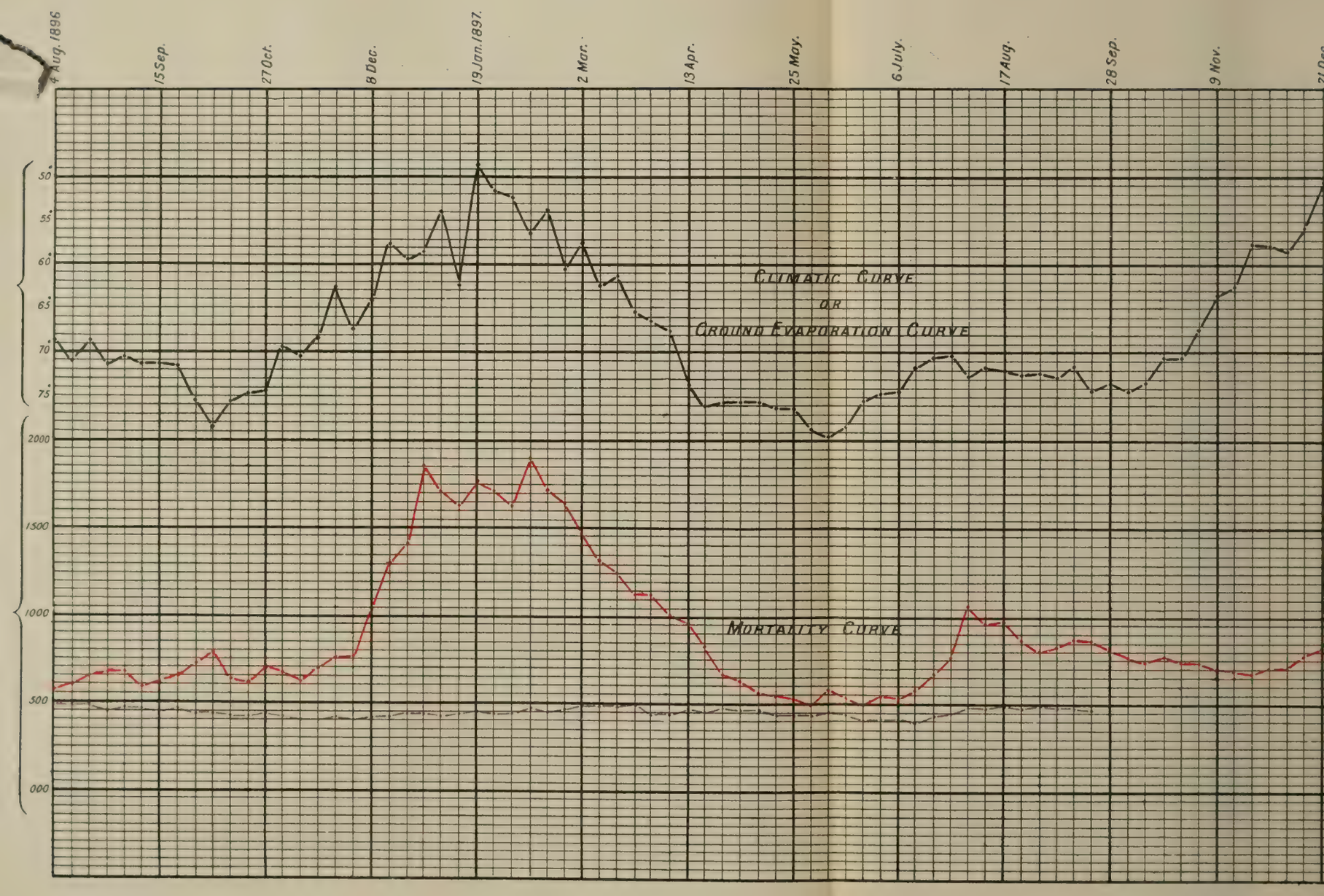


CHART B. SHOWING GROUND EVAPORATION A



The mortality curve (in violet) represents the mean mortality of five

PLAGUE MORTALITY IN BOMBAY.

hy years from 1883 - 1887. when the mortality was a minimum.

Remarks on the Weather and Plague at Bombay, 1896–99,

by Alexander Buchan, LL.D., F.R.S.,

Secretary to the Meteorological Society for Scotland.

In the table appended to these remarks are set forth the mean weekly averages calculated from Professor Moos's figures for the 2¾ years, from August 1896 to March 1899, as follows:—

1. The average weekly number of deaths from all causes during the period dealt with. To this is added the average weekly number of deaths for the five years, 1893–97, when there was no plague; and the differences between these two sets of average weekly mortality which show, presumably, the average weekly mortality from the plague from its commencement to March 1899, embraced in Professor Moos's Report.
2. The mean weekly dry bulb and wet bulb at Bombay for the 2¾ years, these columns showing respectively the temperature of the air, and the temperature of the evaporation at Bombay.
3. The mean weekly relative humidity calculated from the dry and wet bulb means, this column showing the degree of humidity of the air at Bombay.
4. The mean weekly temperature of the soil at a depth of five feet, to which is added a column showing the weekly differences between this soil temperature and that of the air over the soil.
5. The mean weekly rainfall, in inches, at Bombay during the time.

These results are graphically represented on the diagram accompanying the tables.

I do not think it necessary to give any of the other meteorological data in illustration of the remarks, such as dew-point, barometric pressure, &c., as these do not appear to show any connection with the curve of mortality.

Weekly Mortality.—It will be seen that during the five years when there was no plague at Bombay, the weekly mortality varied only slightly during the year, the extremes being 475 deaths in the tenth week, and 392 deaths in the twenty-eighth week, the weekly average for the year being 439 deaths.

In striking contrast with this remarkable steadiness of the weekly mortality is the weekly mortality during the 2¾ years of the plague, the extremes now being 1,980 deaths on the tenth week of the year, and 474 deaths on the twenty-fifth week.

The annual march of the mortality through the weeks is shown by Diagram A. The result shows two maxima and two minima, the former, and by far the larger, maximum reaching its culmination from January to March, and the latter from August to October.

The larger maximum is practically coincident with the coldest weather of the year, and such weather particularly during the night, and, considered in connection with the habits, housing, and clothing of the population, may perhaps account for a part of this great mortality. The mortality falls steadily from the absolute maximum to the end of June, when the absolute minimum of the mortality occurs, all this taking place with a rising temperature which attains its maximum in June. From this time the humidity increases to the maximum which stretches over the next four months. This period covers the rainy season, and it is during this time that the secondary maximum mortality from plague occurs.

Thus, the two maxima occur respectively when the weather at Bombay is coldest and driest, and when the weather is wettest and still very warm.

A prominent feature of the mortality curve is that the curve of no single meteorological element taken by itself—such as temperature of air, B; humidity, C; temperature of soil, D; or rainfall, E—agrees with the mortality curve.

Such a curve of agreement, however, is obtained from the differences between the temperature of the soil at a depth of 5 feet, D, to which the daily changes of temperature do not penetrate, and the temperature of the air over the soil, F. To show this more impressively, the figures in the diagram A and F are both coloured black. A very important point in the relations of the two curves is that all the phases of F precede in time by a week or two the corresponding phases of A, just as in the case of London, where the commencement of hot or cold weather precedes by a week or two the increased mortality which results from these types of weather.

Since the temperature of the soil, at a depth to which the daily changes do not reach, is higher than the air over it, amounting to from 4° to 11° during the high mortality from plague, it follows that at these times the "*Grundluft*," or air in the soil, will rise out of the soil most readily and pass into the air, carrying with it the noxious impurities in the soil.

This connection between the curve of mortality and these temperatures' curve opens up a field of research to the physician and the bacteriologist, and the research is all the more likely to lead to good results when it is kept in mind that the principal maximum mortality of the year occurs when the weather is coldest and driest, and the secondary maximum when the weather is moistest and wettest, and the temperature very high.

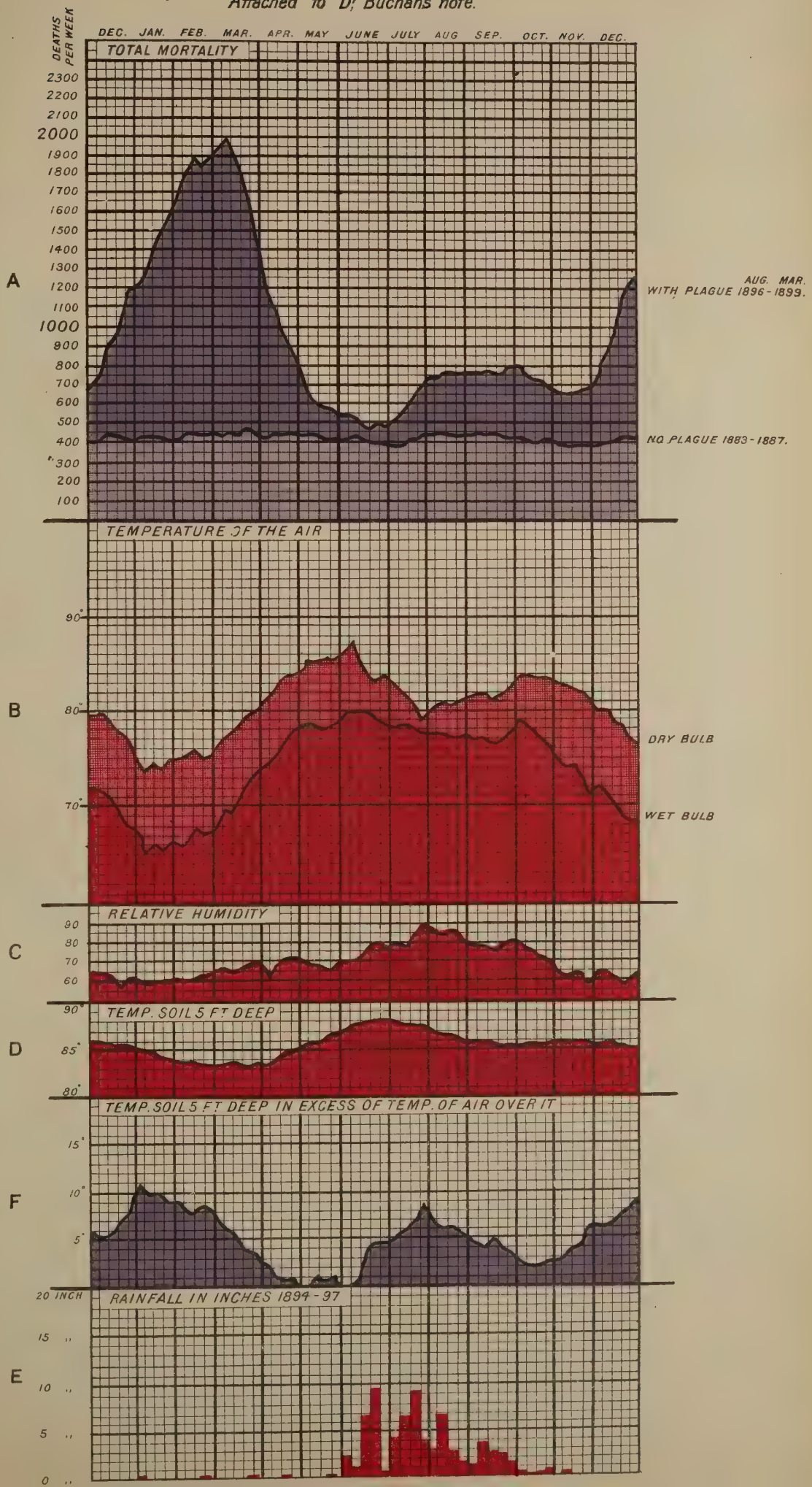
Edinburgh, 2nd September 1899. (Signed) A. Buchan.

TABLE showing the MEAN WEEKLY MORTALITY, TEMPERATURE, HUMIDITY, and RAINFALL at BOMBAY, in Connection with the PLAGUE there.

Attached to Dr. Buchan's Note.

—	—	Deaths.		Excess with Plague.	Temperature.		Relative Humidity.	Temperature of Soil. 5 feet.	Excess of Soil. Temperature.	Rain.
		1883-7. No Plague.	1896-9. With Plague.		Dry.	Wet.				
					°	°		°	°	Ins.
January - -	1	418	1,222	804	74·0	66·0	61	84·9	10·9	·04
	2	440	1,352	912	74·9	66·4	60	84·5	9·6	—
	3	451	1,512	1,061	74·1	65·9	61	84·1	10·0	—
	4	443	1,632	1,189	75·3	67·0	61	83·9	8·6	—
February - -	5	434	1,706	1,272	75·2	66·8	61	83·8	8·6	—
	6	463	1,863	1,400	76·3	68·1	62	83·6	7·3	—
	7	450	1,910	1,460	75·2	67·7	64	83·4	8·2	·02
	8	465	1,844	1,378	75·4	68·0	64	83·4	8·0	·02
March - -	9	467	1,938	1,471	77·4	70·3	66	83·5	6·1	—
	10	475	1,980	1,505	77·7	69·9	64	83·4	5·7	—
	11	469	1,902	1,433	79·1	71·8	66	83·5	4·4	—
	12	493	1,704	1,211	80·0	73·0	68	83·3	3·3	·01
	13	441	1,540	1,099	80·3	73·6	69	83·7	3·4	·01
April - -	14	442	1,263	821	81·4	73·5	64	83·5	2·1	—
	15	463	1,137	674	83·2	76·0	68	84·1	0·9	—
	16	455	1,019	564	84·1	78·0	72	84·5	0·4	·01
	17	466	894	428	84·4	78·2	72	85·2	0·8	—
May - - -	18	459	755	296	85·6	78·8	69	85·5	0·1	—
	19	463	642	179	85·4	77.9	67	86·0	0·6	—
	20	444	582	138	85·7	78·0	66	86·3	0·6	·01
	21	445	587	142	85·8	79·1	70	86·9	1·1	·06
June - -	22	438	545	107	87·0	80·2	70	87·1	0·1	2·23
	23	452	555	103	87·6	80·1	69	87·6	0·0	1·29
	24	431	508	77	84·4	80·0	78	87·8	3·4	6·66
	25	403	474	71	83·2	79·4	82	88·1	4·9	9·56
	26	400	525	125	83·8	78·6	75	80·0	4·2	·65
July - -	27	397	495	98	83·1	78·5	79	87·9	4·8	4·27
	28	392	550	158	82·1	78·5	76	87·8	5·7	6·80
	29	422	623	201	80·9	78·0	86	87·4	6·5	9·26
	30	441	677	236	78·9	77·6	88	87·5	8·6	3·95
August - -	31	470	750	280	80·6	77·6	85	86·9	6·3	3·00
	32	466	739	273	80·8	77·6	84	86·4	5·6	6·93
	33	472	794	322	80·2	77·0	85	86·3	6·1	2·76
	34	449	767	318	81·2	77·3	81	86·0	4·8	1·59
September -	35	469	764	295	81·6	76·9	77	85·8	4·2	1·17
	37	461	767	306	81·9	77·0	77	85·8	3·9	3·85
	37	450	772	322	80·6	76·6	74	85·7	5·1	2·53
	38	457	755	298	81·7	77·0	77	85·5	3·2	2·25
	39	435	802	367	82·1	78·1	81	85·5	3·4	1·47
October - -	40	432	856	424	83·4	79·0	79	85·5	2·1	·33
	41	419	791	372	84·0	78·5	74	85·6	1·6	·16
	42	414	737	323	83·5	77·4	72	85·6	2·1	·25
	43	433	721	288	83·6	76·7	69	85·8	2·2	·82
November -	44	414	688	274	83·4	75·0	63	85·9	2·5	·02
	45	400	656	256	82·6	74·0	61	85·8	3·2	·30
	46	398	669	271	82·1	74·3	64	85·9	3·8	·07
	47	406	683	277	80·0	71·0	60	85·9	5·9	·03
December -	48	403	697	294	79·7	72·1	65	85·7	6·0	—
	49	411	812	401	80·0	71·8	65	85·7	5·7	—
	50	410	928	518	78·8	70·2	62	85·4	6·6	—
	51	429	1,916	587	78·0	68·8	57	85·2	7·2	—
	52	431	1,223	792	76·7	68·5	61	85·0	8·3	—
		159	1006	567	80·1	74·6	72	85·5	5·4	72·38

Attached to Dr Buchan's note.



PARTICULARS REGARDING INOCULATIONS AND PLAGUE ATTACKS IN THE UMARKHADI JAIL, BOMBAY.

A.

NOMINAL ROLL and PARTICULARS of PRISONERS in the COMMON PRISON, BOMBAY, who were inoculated, and also those attacked with PLAGUE during the outbreak in that prison in 1897 and 1898.

Register No.	Names.	Age.	Date received into Jail.	Date of Inoculation.	Date of Discharge from Jail.	Date of Attack.	Result.
				1898.			
3	Heera Jiwa - - - 6022	45	4.5.97	1st Jan.	9.4.98		
5	Ratansey Dewroo - - - 6224	23	5.6.97	,,	11.2.98		
6	Abdulla Ghorey - - 6250	28	9.6.97	,,	8.1.98		
16	Sadu Sauwar - - - - 6610	45	20.7.97	,,			
17	Yedu Appa - - - 6611	33	20.7.97	,,			
18	Guljar Ajmeri - - - 6612	43	20.7.97	,,			
19	Govind Bawa Gosain - - 6613	40	20.7.97	,,	8.11.98		
21	Genu Gopala - - - 6615	32	20.7.97	,,	13.6.98		
23	Tulsia Khandu - - 6618	32	20.7.97	,,	16.2.99		
26	Bhan Sidu - - - - 6695	30	29.7.97	,,	28.4.98		
27	Domingo Fernandez - - 6708	40	31.7.97	,,	29.1.98		
29	Mangal Khan Karimulla - 6758	40	6.8.97	,,	5.1.98		
31	Waman Sadasir - - 6784	35	9.8.97	,,	8.1.98		
34	Sk. Md. Ameen - - 6873	28	18.8.97	,,	17.2.98		
37	Purushotam Khushal - - 6991	30	31.8.97	,,	28.2.98		
38	Bhika Rama - - - - 7010	25	1.9.97	,,	28.2.98		
39	Ruben Benjamin - - 7020	30	2.9.97	,,	1.3.98		
40	Md. Buden - - - - 7021	28	2.9.97	,,	1.3.98		
41	Sherdal Sabgul - - 7034	28	3.9.97	,,	2.1.98		
45	Laxuman Balkishan - - 7112	25	15.9.97	,,	14.3.98		
46	Rangia Gopal - - 7142	23	20.9.97	,,	19.3.98		
48	Khatar Laxuman - - - 7146	40	21.9.97	,,	19.3.98		
49	Sk. Md. Usman - - 7147	35	21.9.97	,,	19.3.98		
51	Francis Marian - - - 7155	40	21.9.97	,,	19.3.98		
53	Tukaram Sakharam - - 7180	23	23.9.97	,,	22.3.98		
54	Abdul Karim - - - - 7206	26	24.9.97	,,	22.1.98		
56	Bachee Sk. Khajoo - - 7233	32	27.9.97	,,	26.1.98		
58	Husein Kondaji - - - 7252	25	29.9.27	,,	28.1.98		
60	Sheoram Punaji - - 7258	26	30.9.97	,,	29.3.98		
61	Laxuman Raghu - - - 7259	23	30.9.97	,,	29.3.98		
62	Mohanlal Mulchaud - - 7275	18	2.10.97	,,	1.4.98	18.3.98	Had been 168 days in jail when attacked. Recovered, and was released cured on 1.4.98.
68	Hanmanta Chandrin - - 7298	22	4.10.97	,,	3.2.98		
69	Laxuman Mahadu - - 7299	33	4.10.97	,,	3.6.98		
72	Fakir Merwangi - - - 7316	30	6.10.97	,,	5.3.98		
79	Deen Md. - - - - 7857	32	11.10.97	,,	10.1.98		
81	Konda Manaji - - - 7359	26	11.10.97	,,	10.2.98		
85	Heera Laxuman - - 7373	25	13.10.97	,,	12.1.98		
87	Bheewa Hamanta - - 7375	22	13.10.97	,,	12.2.98		
88	Laxuman Sewji - - 7376	35	13.10.97	,,	12.2.98		
89	Nurkhan Husein Khan - 7377	27	13.10.97	,,	11.7.98		
92	Suleman Sirdar - - 7392	45	15.10.97	,,	14.4.98		
93	Joseph Antone - - 7393	25	15.10.97	,,	14.7.98		
94	Balu Raoji - - - - 7416	22	18.10.97	,,	15.2.98		
95	Raoji Yenku - - - 7417	40	18.10.97	,,	17.3.98		
102	Fatte Eshwar - - 7438	22	21.10.97	,,	20.4.98		
103	Seena Govind - - - 7439	30	21.10.97	,,	20.4.98		
105	Khemram Vallabram - - 7441	35	21.10.97	,,	20.4.98		
107	Abdul Rahman - - 7455	30	22.10.97	,,	21.1.98		
108	Sheobodin Gafur - - 7456	22	22.10.97	,,	21.1.98		
109	Lawrence Lobo - - 7457	45	22.10.97	,,	21.1.98		
110	Mansing Shikaram - - 7458	22	22.10.97	,,	21.1.98		
111	Yacoob Ahmed - - 7481	30	25.10.97	,,	24.2.98		
113	Bhiwa Vithu - - 7486	30	26.10.97	,,	25.1.98		
114	Aharkam Aharlang - - 7487	40	26.10.97	,,	25.4.98		
116	Laxumandas Kalyanji - 7498	32	27.10.97	,,	19.4.98	10.2.98	Had been 107 days in jail when attacked. Recovered, and was released cured.
117	Samboo Yesu - - 7504	26	28.10.97	,,	27.1.98		
121	Maney Hinga - - - 7516	30	30.10 97	,,	29.1.98		
122	Kasam Ebram - - 7517	35	30.10.97	,,	29.4.98		
125	Nagarchand Jeibhovan - - 7527	22	1.11.97	,,	30.6.98		
126	Ganu Rama - - - 7530	35	20.11.97	,,	1.3.98		
129	Waman Sitaram - - - 7541	25	8.11.97	,,	1.3 98		
130	Seetal Vory - - - 7542	22	2.11.97	,,	30.4.98	28.2.98	Had been 119 days in jail when attacked. Recovered, and was released cured.
131	Haji Abdul Husein Zand - 7543	35	21.1.97	,,	30.4.98		
133	Hari Tulsiram - - - 7549	25	3.11.97	,,	2.2.98		
137	Laxuman Manohar - - 7559	35	5.11.97	,,	4.1.98		
139	Rama Pandu - - - 7561	25	5.11.97	,,	4.3.98		
141	Gopal Sunderji - - 7563	30	5.11.97	,,	4.7.98		
142	Bhima Kesha - - - 7569	30	6.11.97	,,	28.1.98		
143	Sakharam Vithu - - 7570	28	6.11.97	,,	4.2.98		
144	Malji Janu - - - 7571	40	6.11.97	,,	5.5.98		
145	Booken Narayan - - - 7578	22	8.11.97	,,	7.1.98		
146	Bala Manaji - - 7580	25	8.11.97	,,	7.2.98		
155	Ramjoo Khanusa - - - 7597	45	9.11.97	,,	7.5.98		
157	Gaji Khan Noorkhan - - 7604	45	10.11.97	,,	9.2.98		
159	Frank Marine - - - 7606	25	10.11.97	,,	9.5.98		
160	Ganu Nabaji - - - - 7607	30	10.11.97	,,	9.5.98		
161	Achana Narsoo - - 7614	20	11.11.97	,,	10.2.98		

Register No.	Names.		Age.	Date received into Jail.	Date of Inoculation.	Date of Discharge from Jail.	Date of Attack.	Result.
					1898.			
164	Sk. Husein Yacoob	7618	25	12.11.97	1st Jan.	11.2.98		
165	Md. Khan Husein Khan	7619	50	12.11.97	,,	11.2.98		
167	Daood Husein	7621	25	12.11.97	,,	11.2.98		
168	Deen Md. Lati	7622	25	12.11.97	,,	11.5.98		
170	Liana Dias	7633	35	13.11.97	,,	12.1.98		
171	Sheoram Govind	7634	22	13.11.97	,,	12.2.98		
172	Deoji Sadu	7635	25	13.11.97	,,	12.2.98		
175	Ebram Enoo	7646	20	15.11.97	,,	14.3.98		
178	Madarbux Juman	7656	26	16.11.97	,,	15.2.98		
179	Sk. Ahmed Haji Abdulla	7657	28	16.11.97	,,	15.2.98		
180	Joseph Antone	7658	18	16.11.97	,,	14.5.98		
182	Fatma Sakina	7661	35	17.11.97	,,	15.1.98		
184	Mego Ladanji	7669	39	17.11.97	,,	16.5.98		
185	Hansa Hama	7676	20	18.11.97	,,	17.3.98		
191	Nathia Kundlik	7713	24	23.11.97	,,	22.1.98		
192	Azib Md.	7714	35	23.11.97	,,	31.3.98		
195	Gunaji Sanjahu	7721	45	24.11.97	,,	23.3.98		
196	Bhiwa Vethu	7727	28	25.11.97	,,	29.10.98		
197	Laxuman Yesu	7728	20	25.11.97	,,	24.2.98		
198	Jangoo Yellu	7732	22	26.11.97	,,	25.1.98		
201	Yacoob Ahmed	7735	22	26.11.97	,,	25.3.98		
203	Gulam Dost Md.	7737	30	26.11.97	,,	31.3.98		
204	Reishuarao Hanmantrao	7738	23	26.11.97	,,	16.5.98		
210	Daya Rama	7752	30	29.11.97	,,	19.4.98		
211	Vithu Sidu	7753	30	29.11.97	,,	28.7.98		
212	Pandu Ramji	7803	22	30.11.97	,,	28.2.98		
217	Khudabux Alibux	7826	18	3.12.97	,,	2.3.98		
218	Ahmed Md.	7827	25	3.12.97	,,	2.3.98		
221	Sk. Baban Balla	7832	18	3.12.97	,,	2.3.98		
222	Sk. Mohidin Sk. Adam	7833	18	3.12.97	,,	2.3.98		
223	Sk. Husein Sd. Alli	7834	30	3.12.97	,,	2.3.98		
224	Ganpat Nagu	7835	25	3.12.97	,,	2.6.98		
227	Daji Laxuman	7851	27	6.12.97	,,	5.1.98		
231	Withu Govind	7855	25	6.12.97	,,	5.4.98		
234	Rama Manaji	7864	23	7.12.97	,,	5.3.98		
235	Bhoomaya Malu	7865	28	7.12.97	,,	6.6.98		
239	Naveoji Shapurji	7877	30	8.12.97	,,	7.4.98		
240	Dhanji bhoy Naveoji	7878	28	8.12.97	,,	7.6.98		
243	Haji Abdul Rahman	7881	25	8.12.97	,,	7.9.98		
244	Elahi Amiron	7894	40	9.12.97	,,	19.1.98		
246	Kamla Pandu	7897	22	10.12.97	,,	9.6.98		
247	Laxuman Baboo	7898	20	10.12.97	,,	9.6.98		
248	Bheewa Kishna	7899	35	10.12.97	,,	9.6.98		
249	Bechar Lakha	7904	25	11.12.97	,,	9.4.98		
252	Antone Fernandez	7911	25	13.12.97	,,	24.1.98		
253	Abdul Razak Karimbux	7912	22	13.12.97	,,	12.2.98		
254	Deoji Kerson	7913	26	13.12.97	,,	12.4.98		
255	Clumilal Atmaram	7914	25	13.12.97	,,	12.4.98		
256	Nanabhai Thakurdas	7915	25	13.12.97	,,	12.4.98		
258	Madan Mohan	7919	35	13.12.97	,,	12.5.98		
260	Pandurang Keshow	7921	25	13.12.97	,,	12.6.98		
262	Sk. Md. Imambux	7924	40	14.12.97	,,	13.1.98		
263	Sakharam Bhikaji	7925	25	14.12.97	,,	13.1.98		
271	Sadu Babaji	7937	22	16.12.97	,,	15.2.98		
272	Arjan Ganu	7939	30	16.12.97	,,	26.1.98		
274	Bala Keru	7940	30	16.12.97	,,	5.1.98		
276	Fakir Pandu	7943	30	17.12.97	,,	16.7.98		
277	Heera Ramji	7948	50	18.12.97	,,	16.4.98		
284	Vishnu Nagesh	7960	20	20.12.97	,,	19.2.98		
286	Vishnu Parashram	7962	35	20.12.97	,,	19.3.98		
290	Comer Jan Md.	7970	36	21.12.97	,,	20.1.98		
291	Suleman Khanusa	7971	30	21.12.97	,,	20.1.98		
292	Ismail Md.	7972	30	21.12.97	,,	20.1.98		
293	Bhagia Fakir	7973	25	21.12.97	,,	19.2.98		
295	Rama Babaji	7975	24	21.12.97	,,	20.4.98		
296	Ramjan Jaman	7976	25	21.12.97	,,	20.6.98		
300	Raghu Rama	7983	30	23.12.97	,,	22.6.98		
302	Sita, daughter of Heera	7985	30	23.12.97	,,	22.3.98		
301	Kundlik Balu	7987	40	23.12.97	,,	16.5.98		
304	Noor Md. Vazir Md.	7988	37	24.12.97	,,	23.2 98		
305	Vitnak Sermack	7989	20	24.12.97	,,	23.3.98		
307	Babaji Harichand	7991	35	27.12.97	,,	3.1.98		
309	Husein Khan Jaferkhan	7993	30	27.12.97	,,	5.1.98		
310	Gulam Husein Abdul	7994	22	27.12.97	,,	10.1.98		
313	Sk. Md. Akal Md.	7997	22	27.12.97	,,	26.1.98		
314	Pandu Vithu	7998	35	27.12.97	,,	2.4.98		
315	Govind Laxuman	7999	35	27.12.97	,,	26.2.98		
317	Keshow Babaji	8001	30	27.12.97	,,	26.6.98		
318	Jiwaji Musaji	8002	26	27.12.97	,,	25.6.98		
319	Bhagia Ganpat	8003	30	27.12.97	,,	16.5.98		
320	Mashala	8004	30	27.12.97	,,	4.2.98		
321	Dharma Chodoo	8005	45	27.12.97	,,	26.1.98		
322	Dhondi Ganu	8007	25	28.12.97	,,	26.2.98		
323	Bhimaji Dhondiba	8008	28	28.12.97	,,	26.3.98		
324	Naboo Gulam H.	8009	25	28.12.97	,,	26.3.98		
325	Bhimi Woman	8010	60	28 12.97	,,	12.2.98		
330	Maruti Baboo	8015	40	29.12.97	,,	4.1.98		
332	Baldeo Dearlal	8017	20	29.12.97	,,	4.1.98		
335	Rama Tukaram	8020	30	29.12.97	,,	18.1.98		

Register No.	Names.	Age.	Date received into Jail.	Date of Inoculation.	Date of Discharge from Jail.	Date of Attack.	Result.
				1898.			
336	Howdi Ramchandra - - 8021	50	29.12.97	1st Jan.	4.1.98		
340	Louis Dias - - - 8025	25	30.12.97	„	29.1.98		
341	Sk. Abdul Rahman Sk. R. - 8026	32	30.12.97	„	12.2.98		
342	Md. Gulamali - - - 8027	27	30.12.97	„	12.2.98		
343	Nathu Rama - - - 8028	22	30.12.97	„	28.2.98		
344	Dhondu Laxuman - - 8029	30	30.12.97	„	28.2.98		
345	Soorga Mogha - - - 8030	25	30.12.97	„	29.4.98		
346	Noor Md. Gafoor - - 8031	27	30.12.97	„	13.6.98		
350	Pitai Badal - - 8036	30	31.12.97	„	6.1.98		
351	Mata Badal Sheoram - - 8037	30	31.12.97	„	6.1.98		
353	Gaya Parshad - - 8039	35	31.12.97	„	6.1.98		
354	Premdas Soorju - - 8040	30	31.12.97	„	6.1.98		
358	Bombaysha Gujarsha - - 8044	35	31.12.97	„	6.1.98		
359	Madu Tenubuk - - 8045	30	31.12.97	„	6.1.98		
360	Kishna Anaji - - - 8046	50	31.12.97	„	6.1.98		
363	Ramparshad Ramotai - - 8049	40	31.12.97	„	6.1.98		
364	Narayandas Mukundas - - 8050	30	31.12.97	„	6.1.98		
366	Md. Sukar - - - 8052	22	31.12.97	„	6.1.98		
368	Miya Baba - - 8054	20	31.12.97	„	14.1.98		
370	Sk. Ebrahim Sk. Husein - - 8056	29	31.12.97	„	28.2.98		
371	Mustafa Jafer - - 8057	30	31.12.97	„	30.6.98		
373	Gangabai, wife of Bapu -	50	31.12.97	„	6.1.98		
375	Ladi, wife of Laxuman - 8061	30	31.12.97	„	6.1.98		
378	Abar, wife of Dhondu - 8064	40	31.12.97	„	6.1.98		
1614	Kalia Mahadu - - -	25	10.6.98	4th July	9.8.98	29.7.98	Had been 49 days in jail when attacked. Recovered.
	UNDER TRIAL PRISONERS.						
				1898.			
2	Suleman Jamal - - -	30	30.11.97	1st Jan.	20.4.98		
3	Ismail Baker Khan - - - -	35	2.12.97	„	20.4.98		
8	Jessa Hukmaji - - -	30	23.12.97	„	4.1.98		
10	Md. Kaiser Khan - -	35	29.12.97	„	21.1.98		
11	Chedi Khan Sujat Khan - -	30	31.12.97	„	25.1.98		
13	Balvant Dhondu - - -	30	31.12.97	„	13.1.98		
15	Rama Govind - - -	28	31.12.97	„	6.1.98		
16	Yashwant Darku - - -	28	31.12.97	„	8.1.98		
17	Bhajra Vithu - - -	26	31.12.97	„	8.1.98		
	Total 199 + 1 inoculated in July 1898.						
	NON-INOCULATED PRISONERS WHO SUFFERED FROM PLAGUE.			Number of Days in Prison when attacked.			
151	Maulabux Rahimbux - -	20	8.11.97	23	1.1.98	31.12.97	Died 1.1.98.
215	Arjan Raghu - - -	26	2.12.97	29	6.1.98	31.12.97	Died 6.1.98.
13	Budha Haunsa - - -	48	20.7.97	164	4.1.98	1.1.98	Died 4.1.98.
401	Jetha Narsey - - -	40	4.1.98	3	2.5.98	7.1.98	Recovered.
522	Bhagubai - - - -	65	14.1.98	1	3.2.98	15.1.98	Recovered.
186	Ramjan Elahibux - -	25	19.11.97	59	22.1.98	17.1.98	Died 22.1.98.
147	Sadu Rama - - - -	22	8.11.97	70	7.3.98	18.1.98	Recovered.
33	Sk. Husein Sk. Baban - -	20	18.8.97	120	20.1.98	19.1.98	Died 20.1.98.
65	Govind Rama - - -	20	4.10.97	107	3.2.98	20.1.98	Recovered.
163	Bhow Sakharam - - -	28	11.11.97	70	10.5.98	20.1.98	Recovered.
39	Limba Babaji - - -	25	4.1.98	18	24.1.98	22.1.98	Died 24.1.98.
35	Maruti Babaji - - -	25	23.8.97	153	26.1.98	24.1.98	Died 26.1.98.
283	Karim Khan Ajamkhan - -	32	20.12.97	36	27.1.98	24.1.98	Died 27.1.98.
104	Girdhar Motiram - - -	30	21.10.97	101	3.2.98	30.1.98	Died 3.2.98.
278	Andrew Meuse - - -	20	18.12.97	47	17.6.98	2.2.98	Recovered.
1705	Md. Ahmedkhan - - -	35	23.6.98	5	21.7.98	29.6.98	Recovered.

The number of dead rats or sick rats found during the attack was quite inappreciable. I should say that not more than half a dozen in all; one thing, however, is, I think, noteworthy, and that is, that as the plague increased in virulence in the Prison surroundings, the number of rats decreased, and *vice versâ*, and at the recrudescence of the plague they disappeared altogether, not a rat was to be seen for weeks, but about three months ago they commenced to return, and now the Prison is simply surrounded with them.

B.

DATES of DISCHARGE from the COMMON PRISON, BOMBAY, of PRISONERS INOCULATED in JANUARY 1898.

Dates.	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	12.	13.	14.	15.	16.
1898.																
January -	–	–	1	5	4	13	1	4	–	2	–	2	3	2	1	–
February	–	1	1	2	–	–	1	–	1	1	4	8	–	–	4	–
March -	4	6	–	1	2	–	–	–	–	–	–	–	–	2	–	–
April -	1	1	–	–	1	–	1	–	2	–	–	3	–	1	–	–
May -	1	–	–	–	1	–	1	–	2	–	1	1	–	1	–	2
June -	1	1	1	–	–	2	1	–	3	–	1	–	2	–	–	–
July -	–	–	–	1	–	–	–	–	–	–	1	–	–	1	–	1
August -	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–
September	–	–	–	–	–	–	1	–	–	–	–	–	–	–	–	–
October -	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–
November	–	–	–	–	–	–	–	1	–	–	–	–	–	–	–	–
December	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–

continued.

Dates.	17.	18.	19.	20.	21.	22.	23.	24.	25.	26.	27.	28.	29.	30.	31.	Total.
1898.																
January -	–	1	1	3	5	3	–	1	3	4	1	3	3	–	–	66
February	1	–	2	–	–	–	1	2	–	1	1	5	–	–	–	36
March -	2	–	5	–	–	2	2	–	1	2	–	–	2	–	–	31
April -	–	–	1	6	–	–	–	–	1	–	–	–	2	2	–	23
May -	–	–	–	–	–	–	–	–	1	1	–	–	–	–	–	12
June -	–	–	–	1	–	1	–	–	1	1	–	–	–	1	–	17
July -	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	4
August -	–	–	1	–	–	–	–	–	–	–	–	–	–	–	–	1
September	–	–	–	–	–	1	–	–	–	1	–	–	–	–	–	3
October -	–	–	–	–	–	–	–	–	–	–	–	–	1	–	–	1
November	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	1
December	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–

Total 195 (+ 4 prisoners not discharged).

DATES of DISCHARGE from the COMMON PRISON of NON-INOCULATED PRISONERS in JAIL during the INOCULATION of JANUARY 1898.

Dates.	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	12.	13.	14.	15.	16.
1898.																
January -	7	–	6	11	4	20	1	4	–	2	1	1	1	4	2	–
February	–	1	5	1	3	–	3	1	2	1	1	2	–	1	–	–
March -	–	2	1	–	–	1	1	–	–	2	–	2	–	–	–	1
April -	–	–	–	2	3	–	–	–	–	–	–	–	1	1	–	1
May -	–	–	–	–	1	–	1	1	–	1	–	2	–	3	–	8
June -	–	–	–	–	–	2	2	–	1	2	–	–	3	–	–	–
July -	–	–	1	–	4	–	–	–	–	–	–	–	–	–	–	–
August -	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–
September	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–
October -	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–
November	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–
December	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–

continued.

Dates.	17.	18.	19	20.	21.	22.	23.	24.	25.	26.	27.	28.	29.	30.	31.	Total.
1898.																
January	–	–	2	1	3	3	–	–	2	4	1	2	2	–	1	85
February	–	–	5	–	–	–	1	–	–	1	–	–	–	–	–	28
March -	–	1	5	–	1	–	2	1	–	–	–	2	2	–	2	26
April -	–	1	2	1	–	–	–	–	–	2	1	–	–	–	–	15
May -	1	–	1	–	–	–	–	–	–	–	–	–	–	–	–	19
June -	1	–	–	1	–	–	–	–	–	1	–	–	–	1	–	14
July -	–	–	–	1	–	–	–	–	–	–	–	–	–	–	–	6
August -	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–
September	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–
October -	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–
November	–	–	1	–	–	1	–	–	–	–	–	–	–	–	–	2
December	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–

Total 195 (+ 5 not discharged).

A. G. MACKENZIE,
Superintendent H.M.'s Common Prison,
Bombay.

APPENDIX No. LX.

STATISTICS

CONCERNING

PRISONERS

IN THE

HOUSE OF CORRECTION, BYCULLA,

DURING THE

PLAGUE OUTBREAKS

OF 1897 AND 1898.

APPENDIX No. LX.

STATISTICS concerning PRISONERS in the HOUSE OF CORRECTION, BYCULLA, during the PLAGUE OUTBREAKS of 1897 and 1898.

A.

LIST of NATIVE PRISONERS in the HOUSE of CORRECTION on the 30th January 1897, showing the Date of INOCULATION and PARTICULARS regarding PLAGUE

Serial No	Registration No.	Name.	Caste.	Age.	Date of Admission into the Prison.	Date of *Discharge* from the Prison.	Date of Inoculation.	Date Attacked by Plague.	Date of Recovery.	Date of Death.	Remarks.
1	1	Moru Bin Balkudas	Hindu	35	12 May 1886	22 June 1897	—	—	—	—	
2	98	Mahadu Bin Nargapa	„	30	30 April 1894	12 Nov. „	—	—	—	—	
3	210	Abdulla Binder	Muhammadan	40	19 Sept. „	26 Mar. „	—	—	—	—	Sent to Ratnagiri.
4	265	Dewju Mahadu *alias* Rama Chandra	Hindu	28	22 Nov. „	20 „ „	30 Jan. 1897	—	—	—	
5	352	Shiva Anarji	„	40	16 Jan. 1895	26 „ „	30 „ „	—	—	—	
6	432	Hunmanta Narsoo	„	39	18 Mar. „	17 July „	—	—	—	—	
7	494	Dhurma Tannoo	„	36	1 May „	15 May „	30 Jan. 1897	—	—	—	
8	496	Esmail Hossein	Muhammadan	31	1 „ „	20 July 1896	—	—	—	—	
					23 July 1896						
9	513	Luxuman Harry	Hindu	26	16 May 1895	21 Feb. 1898	—	—	—	—	
10	514	Kassim Hossein	Muhammadan	28	16 „ „	24 Mar. 1897	30 Jan. 1897	—	—	—	
							2 Feb. „				
11	515	Mahomed Muda Khan	„	24	16 „ „	22 „ „	—	—	—	—	
12	534	Jani Abraham, &c.	Jew	28	31 „ „	8 April „	30 Jan. 1897	—	—	—	
							2 Feb. „				
13	570	Mool Chand Oosuagu	Hindu	28	27 June ..	19 Mar. „	—	—	—	—	Transferred to Ratnagiri Prison.
14	582	Govind Waman *alias* Gobin, &c.	Muhammadan	32	8 July „	28 May „	—	—	—	—	
15	591	Mansark Bachoo	„	34	13 „ „	22 June „	30 Jan. 1897	—	—	—	
16	594	Gessu Hariju	Hindu	50	13 „ „	26 Mar. „	—	—	—	—	
17	598	Rama Govind, &c.	„	24	19 „ „	29 May „	30 Jan. 1897	—	—	—	„ „
18	599	Ram Chunder Rugbir	„	25	19 „ „	27 April 1898	—	—	—	—	
19	608	Vithul Narayen	„	30	29 „ „	8 June 1897	30 Jan. 1897	—	—	—	
20	622	Mariam Francis, &c.	R. Catholic, Christian.	21	6 Aug. „	13 Sept. „	—	—	—	—	
21	624	Hurry Gajaba	Hindu	40	8 „ „	26 Mar. „	—	—	—	—	Transferred to Ratnagiri.
22	657	Ramdyal Khandhai, &c.	„	35	9 „ „	24 June „	30 Jan. 1897	—	—	—	
23	669	Janki Gujadhur	„	23	15 „ „	30 „ „	—	—	—	—	
24	672	Keshav Umishankar	„	24	16 „ „	—	30 Jan. 1897	1 Feb. 1897	—	5 Feb. 1897	Bubonic plague.
25	677	Bechar Asha, &c.	„	38	19 „ „	28 June 1897	—	—	—	—	
26	694	Soonoo Luxman	„	35	17 Sept. „	27 July „	30 Jan. 1897	—	—	—	
27	700	Dhondi Kendiba, &c.	„	30	23 „ „	9 Aug. „	—	—	—	—	
28	705	Kerwan Sidaju Wn. Mama	„	42	26 „ „	22 June „	30 Jan. „	—	—	—	
29	706	Bawaji Rugiju	„	22	26 „ „	22 „ „	—	—	—	—	
30	707	Lukshman Pandu	„	30	26 „ „	18 Aug. „	—	—	—	—	
31	708	Abdul Karim Buksh	Muhammadan	22	26 „ „	12 Mar. „	—	—	—	—	
32	709	Budhim Shunwas	Hindu	25	26 „ „	5 June „	—	—	—	—	
33	746	Gangu Dongersey	„	50	2 Nov. „	—	—	6 Feb. 1897	—	7 Feb. 1897	Bubonic plague.
34	769	Rama Yenkuti	„	24	21 „ „	14 May 1897	30 Jan. 1897	—	—	—	Transferred to Ratnagiri Prison.
35	778	Santon Fernandez	R. Catholic, Christian.	25	29 „ „	19 Mar. „	—	—	—	—	„ „
36	786	Ganesh Luxuman	Hindu	35	30 „ „	19 „ „	—	—	—	—	„ „
37	828	Mahomed Neosery	Muhammadan	32	12 Dec. „	—	30 Jan. 1897	—	—	—	
							2 Feb. „				} Still in the prison.
38	832	Rama Lakshman	Hindu	25	12 „ „	—	30 Jan. „	1 Feb. 1897	17 Feb. 1897	—	
39	838	Shaik Hossein Ahmed	Muhammadan	24	12 „ „	27 April 1898	30 „ „	—	—	—	

												Transferred to Ratnagiri Prison.
40	849	Ram Chandra Mahadeo	Hindu	24	13 Dec. 1895	26 Mar. 1897	30 Jan. 1897	—	—	—	}	Still in the prison.
41	890	Bessesser Chirkut	„	28	14 Jan. 1896	—	—	—	—	—		
42	891	Ramsurrup Gopall	„	30	14 „ „	—	—	—	—	—		
43	892	Jugdeo Gunesh	„	30	14 „ „	31 Jan. 1899	30 Jan. 1897	—	—	—		
44	899	Ahmed Ali Surrup Ali	Muhammadan	40	18 „ „	16 June 1897	30 Mar. „	—	—	—		
							2 Feb. „					
45	905	Tatya Hurri, &c.	Hindu	30	30 „ „	22 „ „	—	—	—	—		Transferred to Ratnagiri Prison.
46	945	Sitaram Yesju	„	30	4 Feb. „	26 Mar. „	—	—	—	—		
47	946	Luchman Narayen Jodraj	„	32	4 „ „	22 June „	30 Jan. 1897	—	—	—		
48	974	Abdool Rahman	Muhammadan	55	26 „ „	22 „ „	30 „ „	—	—	—		
49	978	Bhima *alias* Kutappa Gungappa	Hindu	25	26 „ „	19 Mar. „	—	—	—	—		„ „
50	979	Kasna Kamlu	„	20	26 „ „	19 „ „	—	—	—	—		„ „
51	980	Baladin Subhai	„	25	26 „ „	22 June „	—	—	—	—		
52	981	Rama Lukappa	„	30	26 „ „	22 „ „	—	—	—	—		
53	982	Doolaji Dhensoo	„	29	26 „ „	—	—	—	—	—		Still in prison.
54	983	Sheik Ismail Khudabux	Muhammadan	46	26 „ „	22 June 1897	—	—	—	—		
55	984	Genoo Vithu	Hindu	21	26 „ „	25 Mar. „	30 Jan. „	—	—	—		
*56	*988	Mahomed Hossein	Muhammadan	30	27 „ „	—	30 „ „	4 Feb. 1897	—	8 Feb. 1897		Bubonic plague.
57	992	Tookaram Gopul	Hindu	28	2 Mar. „	26 Mar. 1397	30 „ „	—	—	—		Sent to Ratnagiri.
58	993	Mamu Ibrahim	Muhammadan	32	2 „ „	27 Aug. „	—	7 Feb. 1897	15 Mar. 1897	—		
59	1,002	Pestonjee Kowasjee	Parsee	50	9 „ „	19 Oct. „	30 Jan. 1897	—	—	—		
							2 Feb. „					
60	1,004	Vishnu Tatia	Hindu	25	9 „ „	26 Mar. „	30 Jan. „	—	—	—		„ „
							2 Feb. „					
61	1,006	Gunesh Shunker Dravid	„	27	9 „ „	6 Sept. „	30 Jan. „	—	—	—		Transferred to Yerowda Jail.
							2 Feb. „					
62	1,020	Ram Chunder Narayen, &c.	„	23	17 „ „	26 Mar. „	30 Jan. „	—	—	—		„ Ratnagiri Prison.
							2 Feb. „					
63	1,022	Ibrahim Mahomed Ali, &c.	Muhammadan	28	19 „ „	26 Feb. „	30 „ „	—	—	—		
64	1,023	Abdool Karim	„	22	20 „ „	14 May „	30 „ „	—	—	—		„ „
65	1,026	Mohideen Mahomed	„	25	25 „ „	27 Feb. „	30 „ „	—	—	—		
							2 Feb. „					
66	1,030	Ali Oomer	„	55	30 „ „	6 May 1898	—	—	—	—		
67	1,033	Abdoola Aladin	„	30	31 „ „	12 Feb. „	30 Jan. 1897	—	—	—		
68	1,034	Bhugoo Nabibux	„	38	31 „ „	19 Mar. 1897	30 „ „	—	—	—		„ „
							2 Feb. „					
69	1,040	Dhuna Radha	Hindu	40	5 April „	15 July „	—	—	—	—		
70	1,053	Jooman Kulloo	Muhammadan	35	9 „ „	19 Mar. „	—	—	—	—		„ „
71	1,054	Ahmed Ali Tyeb Ali	„	28	10 „ „	11 June „	30 Jan. 1897	—	—	—		
							2 Feb. „					
72	1,055	Aboo Baloo	„	40	10 „ „	6 Sept. „	30 Jan. „	—	—	—		
73	1,061	Aboo Mahomed	„	25	14 „ „	11 June „	—	—	—	—		
74	1,062	Hafiz Wazir Mirza Beg	„	32	14 „ „	25 Dec. „	30 Jan. 1897	—	—	—		
75	1,071	Esak Khuda Bux	„	50	21 „ „	25 Mar. „	—	—	—	—		
76	1,072	Raghunath Geswunt	Hindu	25	25 „ „	13 Aug. „	—	—	—	—		
77	1,074	Paulos Augustin Fernandez	R. Catholic, Christian.	45	25 „ „	19 Mar. „	30 Jan. 1897	—	—	—		„ „
78	1,075	Sondia Hamr	Hindu	40	25 „ „	22 June „	30 „ „	—	—	—		
							2 Feb. „					
79	1,076	Keshow Lall Chota Lal, &c.	„	35	25 „ „	26 Mar. „	30 Jan. „	—	—	—		„ „
							2 Feb. „					
80	1,077	Mahadu Bhicksett	„	45	28 „ „	7 Aug. „	—	—	—	—		

* Regarding this prisoner, *see* the correspondence published in Appendix No. LXXXVIII. below in this Volume.

LIST of NATIVE PRISONERS in the HOUSE OF CORRECTION, &c.—*continued.*

Serial No.	Registration No.	Name.	Caste.	Age.	Date of Admission into the Prison.	Date of *Discharge* from the Prison.	Date of Inoculation.	Date Attacked by Plague.	Date of Recovery.	Date of Death.	Remarks.
81	1,084	Abdul Rahim Khan	Muhammadan	18	29 April 1896	22 June 1897	30 Jan. 1897	—	—	—	
82	1,087	Cassim Ebrahim	„	32	30 „ „	3 April „	—	—	—	—	
83	1,091	Arjuna Balla	Hindu	25	4 May „	3 Feb. „	—	—	—	—	
84	1,092	Golam Hossein Taj Khan	Muhammadan	20	4 „ „	10 April „	—	—	—	—	
85	1,093	Rama Govind, &c.	Hindu	22	5 „ „	—	—	26 Jan. 1897	—	30 Jan. 1897	Bubonic plague.
86	1,094	Ramvalab Rampartab	„	20	5 „ „	10 April 1897	—	27 „ „	17 Feb. 1897	—	
87	1,095	Ali Mahomed Tayab Ali	Muhammadan	18	5 „ „	19 Mar. „	30 Jan. 1897	—	—	—	Transferred to Ratnagiri Prison.
							2 Feb. „				
88	1,097	Bablia Luxaman	Hindu	28	5 „ „	19 „ „	—	—	—	—	
89	1,100	Murad Gull Mahomed, &c.	Muhammadan	22	7 „ „	6 Feb. „	30 Jan. 1897	—	—	—	„ „
90	1,104	Kessoo Purshotum, &c.	Hindu	25	11 „ „	27 April „	30 „ „	—	—	—	
91	1,105	Kumoo Imam, &c.	Muhammadan	18	14 „ „	23 „ „	—	—	—	—	
92	1,106	Mahomed Abdullah, &c.	„	16	14 „ „	13 Mar. „	—	—	—	—	
93	1,107	Currim Hasan	„	17	14 „ „	13 „ „	—	—	—	—	
94	1,108	Ramju Ahmed	„	18	19 „ „	24 April „	30 Jan. 1897	—	—	—	
							2 Feb. „				
95	1,110	Sadoo Mahadeo	Hindu	32	22 „ „	27 „ „	30 Jan. „	—	—	—	
							2 Feb. „				
96	1,114	Rama Khandu	„	25	26 „ „	1 May „	30 Jan. „	—	—	—	
							2 Feb. „				
97	1,116	Khrishna Bhugnak	„	27	30 „ „	27 Feb. „	30 Jan. „	—	—	—	
98	1,117	Burapa Tolapa, &c.	„	30	30 „ „	5 May „	30 „ „	—	—	—	
99	1,118	Fazal Kasam, &c.	Muhammadan	21	1 June „	13 „ „	—	—	—	—	
100	1,119	Vithu Rama	Hindu	28	1 „ „	7 „ „	—	—	—	—	
101	1,123	Kallan Himat Khan	Muhammadan	30	5 „ „	29 „ „	—	—	—	—	
102	1,124	Abba Tookaram	Hindu	22	5 „ „	14 „ „	—	—	—	—	Transferred to Ratnagiri Prison.
103	1,128	Abdul Illahi Bux, &c.	Muhammadan	23	8 „ „	14 „ „	30 Jan. 1897	—	—	—	
104	1,129	Raoji Jothu Powar	Hindu	25	8 „ „	—	—	30 Jan. 1897	—	1 Feb. 1897	Bubonic plague.
105	1,130	Mahomed Kasim Nur Mahomed	Muhammadan	28	9 „ „	26 Mar. 1897	30 Jan. 1897	—	—	—	Transferred to Ratnagiri Prison.
							2 Feb. „				
106	1,131	Deo Vithoo	Hindu	42	9 „ „	18 May „	—	—	—	—	
107	1,132	Maruti Babaji	„	30	9 „ „	19 Aug. „	—	—	—	—	
108	1,133	Sahibdin Fakir	Muhammadan	28	9 „ „	8 Mar. „	30 Jan. 1897	—	—	—	
109	1,136	Mawji Bhowan	Hindu	27	10 „ „	9 „ „	—	5 Feb. 1897	12 Feb. 1897	—	
110	1,137	Esoof Ali Jiwaji, &c.	Muhammadan	28	10 „ „	19 „ „	30 Jan. 1897	—	—	—	Transferred to Ratnagiri Prison.
111	1,138	Bhagoobhai Nihal Chand	Hindu	21	13 „ „	19 „ „	—	—	—	—	„ „
112	1,140	Mahomed Isak Mirza Sulleman	Muhammadan	50	16 „ „	28 May „	—	—	—	—	
113	1,141	Kondi Balla	Hindu	48	18 „ „	14 „ „	30 Jan. 1897	—	—	—	
114	1,142	Ramdyal Jardeo, &c.	„	22	18 „ „	17 Feb. „	30 „ „	—	—	—	„ „
115	1,143	Gungaram Mahadeo	„	21	19 „ „	14 May „	—	—	—	—	
116	1,144	Jiva Virchand	„	24	19 „ „	19 Mar. „	—	30 Jan. 1897	17 Feb. 1897	—	„ „
117	1,147	Hari Shivchurn	„	30	27 „ „	26 „ „	—	—	—	—	„ „
118	1,148	Girdhar Kullianji, &c.	„	27	30 „ „	27 Feb. „	—	—	—	—	„ „
119	1,150	Fakira Pandoo	„	25	1 July „	9 June „	30 Jan. 1897	—	—	—	
							2 Feb. „				
120	1,151	Juva Laxuman	„	21	1 „ „	26 Mar. „	—	—	—	—	„ „

121	1,153	Ram Chandra Govind	„	28	2 July 1896	14 May 1897	30 Jan. 1897	—	—	—	„	„
							2 Feb. „					
122	1,154	Narayen Nagoo	„	20	4 „ „	11 June „	—	—	—	—		
123	1,155	Govind Atmaram	„	29	4 „ „	26 Mar. „	30 Jan. 1897	—	—	—	„	„
124	1,156	Govind Birbul	„	25	4 „ „	14 May „	—	—	—	—	„	„
125	1,159	Abdul Habib Abdulla Nabi Bux	Muhammadan	24	7 „ „	6 April „	30 Jan. 1897	—	—	—		
126	1,160	Ebrahim Allaodin	„	28	7 „ „	14 May „	30 „ „	—	—	—		
127	1,161	Merwanji Jiwaji	Parsee	28	7 „ „	22 June „	—	—	—	—		
128	1,162	Luxaman Narayen	Hindu	40	7 „ „	12 „ „	—	—	—	—		
129	1,164	Luxaman Bulloo	„	32	8 „ „	25 Oct. „	30 Jan. 1897	—	—	—		
130	1,165	Ramdas Lilladhar	„	28	8 „ „	19 Mar. „	30 „ „	—	—	—	„	„
131	1,166	Raoji Sutwa, &c.	„	50	9 „ „	15 June „	30 Jan. „	—	—	—		
							2 Feb. „					
132	1,171	Babia Manaji	„	33	13 „ „	19 Mar. „	—	—	—	—	„	„
133	1,172	Raghu Ganu	„	37	13 „ „	19 „ „	30 Jan. 1897	—	—	—	„	„
							2 Feb. „					
134	1,173	Esmail Joosuf	Muhammadan	32	13 „ „	19 „ „	—	—	—	—	„	„
135	1,174	Antoo Vithoba	Hindu	27	13 „ „	12 April „	—	—	—	—		
136	1,176	Fakir Mahadeo	„	32	14 „ „	18 June „	30 Jan. 1897	—	—	—		
137	1,177	Kanji Nagji	„	36	15 „ „	28 May 1898	—	—	—	—		
138	1,178	Anandrao Balchrishna	„	21	15 „ „	10 Feb. 1897	30 Jan. 1897	—	—	—		
139	1,179	Dhanalal Ramcharan	„	27	16 „ „	15 Mar. „	—	—	—	—		
140	1,180	Dawood Golam Kanzia, &c.	Muhammadan	25	17 „ „	18 June „	—	—	—	—		
141	1,181	Abdulla Sheik Ebram	„	25	17 „ „	15 May „	—	—	—	—		
142	1,182	Abdulla Hasham	„	23	17 „ „	16 Mar. „	30 Jan. 1897	—	—	—		
143	1,183	Abdull Salla, &c.	„	27	18 „ „	17 April „	30 „ „	—	—	—		
144	1,184	Hasan Bachoo	„	25	18 „ „	22 June „	—	—	—	—		
145	1,185	Tukaram Vithoo, &c.	Hindu	28	20 „ „	29 „ „	30 Jan. 1897	—	—	—		
146	1,186	Jadowji Nund Lall	„	46	21 „ „	1 Nov. „	30 „ „	—	—	—		
147	1,187	Narayen Nana, &c.	„	22	22 „ „	20 Sept. „	—	—	—	—		
148	1,188	Fateh Khan Khudabux, &c.	Muhammadan	30	23 „ „	10 Dec. „	30 Jan. 1897	—	—	—		
149	1,189	Enayet Hossein Fazal Hossein	„	28	23 „ „	19 Mar. „	30 „ „	—	—	—	„	„
							2 Feb. „					
150	1,190	Barkia Dhondoo	Hindu	25	25 „ „	26 „ „	—	—	—	—	„	„
151	1,191	Sher Mahomed Yar Mahomed, &c.	Muhammadan	26	27 „ „	26 April „	—	—	—	—		
152	1,192	Chandoo Pirsab	„	26	27 „ „	19 Mar. „	—	—	—	—	„	„
153	1,193	Noor Mahomed Abdoola	„	53	27 „ „	19 Mar. „	—	—	—	—	„	„
154	1,194	Yacoob Hussan	„	34	29 „ „	22 June „	30 Jan. 1897	—	—	—		
							2 Feb. „					
155	1,195	Atmaram Arjoon	Hindu	26	29 „ „	27 April „	—	—	—	—		
156	1,196	Raghunath Lilladhar	„	48	29 „ „	27 „ „	—	—	—	—		
157	1,197	Kalhanji Cooverji	„	45	1 Aug. „	30 „ „	—	—	—	—		
158	1,200	Keshew Gopall	„	36	3 „ „	26 Mar. „	—	—	—	—	„	„
159	1,201	Narayen Govind	„	51	3 „ „	2 „ „	—	—	—	—		
160	1,202	Janoo Krishna	„	33	3 „ „	2 Feb. „	—	—	—	—		
161	1,203	Jankidas Murlidhar	Hindu	22	3 „ „	22 June „	—	—	—	—		
162	1,204	Wali Mahomed Razakdin	Muhammadan	32	3 „ „	28 „ 1898	30 Jan. 1897	—	—	—		
163	1,205	Gulam Hossein Sheikh Ismail	„	27	4 „ „	15 July 1897	—	—	—	—		
164	1,207	Maroti Babaji	Hindu	25	4 „ „	19 Mar. „	—	—	—	—	„	„
165	1,208	Yasin Abdulla	Muhammadan	18	4 „ „	3 May „	30 Jan. 1897	—	—	—		
							2 Feb. „					
166	1,209	Antone Lazarus, &c.	R. Catholic, Christian.	25	5 „ „	13 July „	30 Jan. „	—	—	—		
							2 Feb. „					

List of Native Prisoners in the House of Correction, &c.—*continued.*

Serial No.	Registration No.	Name.	Caste.	Age.	Date of Admission into the Prison.	Date of *Discharge* from the Prison.	Date of Inoculation.	Date Attacked by Plague.	Date of Recovery.	Date of Death.	Remarks.
167	1,210	Dhondia Hiroo	Hindu	32	10 Aug. 1896	17 July 1897	30 Jan. 1897 2 Feb. „	—	—	—	Transferred to Ratnagiri Prison.
168	1,211	Rameshwar Bhayalal	„	30	11 „ „	19 Mar. „	—	—	—	—	
169	1,212	Domla Kika	„	32	12 „ „	22 June „	30 Jan. 1897	—	—	—	
170	1,213	Vasanji *alias* Vishram Kanji	„	28	14 „ „	14 May „	30 „ „ 2 Feb. „	—	—	—	„ „
171	1,214	Sowlia Luxaman	„	35	15 „ „	14 „ „	30 Jan. „ 2 Feb. „	—	—	—	
172	1,215	Sowlia Atmaram	„	25	20 „ „	19 „ „	—	—	—	—	
173	1,216	Vishram Lakhansey	„	23	20 „ „	19 „ „	—	—	—	—	
174	1,217	Ahmed Warin	Muhammadan	23	21 „ „	20 „ „	30 Jan. 1897	—	—	—	
175	1,218	Bhawoo Sadoba	Christian	22	22 „ „	26 „ „	—	—	—	—	
176	1,219	Nurmahomed Abdul Rahiman	Muhammadan	20	25 „ „	23 Mar. „	—	—	—	—	
177	1,220	Bhimji Gokul	Hindu	22	25 „ „	22 June „	—	—	—	—	
178	1,221	Dharsey Ramji	„	28	25 „ „	9 July 1898	—	—	—	—	
179	1,222	Govind Pandu	„	26	25 „ „	—	30 Jan. 1897	1 Feb. 1897	—	3 Feb. 1897	Bubonic plague.
180	1,223	Hassan Bachoo	Muhammadan	22	25 „ „	29 July 1897	30 „ „ 2 Feb. „	—	—	—	
181	1,224	Anna Vishnu	Hindu	23	25 „ „	22 June „	—	31 Jan. 1897	8 April 1897	—	
182	1,225	Maneck Chand Bhudan	„	50	26 „ „	2 July „	30 Jan. 1897 2 Feb. „	—	—	—	
183	1,227	Abdoola Alli	Muhammadan	30	28 „ „	11 „ 1898	—	—	—	—	
184	1,228	Mahomed Lal Khan	„	23	31 „ „	7 Aug. 1897	—	—	—	—	
185	1,229	Ganno Esso	Hindu	32	31 „ „	—	—	31 Jan. 1897	—	4 Feb. 1897	Bubonic fever.
186	4,230	Shaik Dugdoo Jan Mahomed	Muhammadan	30	31 „ „	14 July 1898	30 Jan. 1897 2 Feb. „	—	—	—	
187	1,231	Gangaram Lachram, &c.	Hindu	30	2 Sept. 1896	26 „ „	—	—	—	—	
188	1,232	Ismailji Tyebji	Muhammadan	26	3 „ „	14 „ „	30 Jan. 1897	—	—	—	
189	1,233	Haji Ismail Kazam	„	33	3 „ „	2 June 1897	—	—	—	—	
190	1,234	Abdulla Ebrahim	„	20	4 „ „	26 Mar. „	—	—	—	—	
191	1,235	Khatow Lakhmidas	Hindu	26	4 „ „	1 May „	—	—	—	—	Transferred to Ratnagiri Prison.
192	1,237	Mahomed Abdulla	Muhammadan	23	7 „ „	6 April „	30 Jan. 1897	27 Jan. 1897	10 Feb. 1897	—	
193	1,238	Babaji Sambhaji	Hindu	28	7 „ „	—	—	—	—	—	
194	1,239	Rania Gunpat	„	35	8 „ „	—	—	5 Feb. 1897	—	7 Feb. 1897	Bubonic plague.
195	1,240	Sakaram Vithal	„	25	8 „ „	16 Mar. 1897	—	—	—	12 Mar. 1898	Died in Jail hospital.
196	1,242	Gannoo Baloo	„	24	10 „ „	—	—	27 Jan. 1897	—	31 Jan. 1897	Transferred to Ratnagiri Prison.
197	1,243	Nanya Ruthosa	„	25	12 „ „	24 Aug. 1897	38 Jan. 1897	—	—	—	Bubonic plague.
198	1,244	Luxman Mahadeo	„	29	12 „ „	10 June „	—	—	—	—	
199	1,245	Cassim Mahomed	Muhammadan	29	12 „ „	30 April 1898	—	—	—	—	
200	1,246	Oomerodeen Gaus Khan	„	22	13 „ „	11 June 1897	—	—	—	—	
201	1,247	Antone Salvadore	R. Catholic, Christian.	23	14 „ „	13 May „	—	27 Jan. 1897	17 Feb. 1897	—	
202	1,250	Sai Kallian, &c.	Hindu	48	17 „ „	13 Dec. „	—	—	—	—	
203	1,251	Mahadu Sadhu, &c.	„	40	18 „ „	19 Mar. „	30 Jan. 1897	—	—	—	Transferred to Ratnagiri Prison.
204	1,252	Jaymohan Ganesh	„	25	19 „ „	—	—	2 Feb. 1897	—	11 Feb. 1897	Bubonic fever.
205	1,253	Juan Baptist D'Souza	R. Catholic, Christian.	32	19 „ „	26 July 1897	—	—	—	—	

206	1,254	Ram Prasad Terva, &c.	Hindu	35	19 „ „	1 Oct. „	—	—	—	—		
207	1,255	Tanoo Ganoo	„	27	21 „ „	19 June „	30 Jan. 1897	—	—	—		
208	1,259	Dhakul Ramchunder	„	27	22 „ „	21 „ „	—	—	—	—		
209	1,260	Raghunath Narayen, &c.	„	18	23 „ „	19 Mar. „	—	—	—	—		Transferred to Ratnagiri Prison.
210	1,261	Kallu Bhima	„	35	24 „ „	12 Feb. „	—	1 Feb. 1897	—	12 Feb. 1897		Plague.
211	1,262	Sakharam Raghu	„	32	26 „ „	5 Aug. „	30 Jan. 1897 2 Feb. „	—	—	—		
212	1,267	Baloo Krishna	„	30	30 „ „	22 June „	—	6 Feb. 1897	15 Mar. 1897	—		
213	1,268	Keshowji Kavji	„	32	2 Oct. „	19 Mar. „	—	—	—	—		Transferred to Ratnagiri Prison.
214	1,269	Ghopal Bhicoo	„	25	2 „ „	31 May „	—	—	—	—		
215	1,270	Mahadeo Hari	„	27	2 „ „	31 „ „	—	—	—	—		
216	1,271	Soonoo Ganoo	„	25	2 „ „	12 Aug. 1898	30 Jan. 1897	—	—	—		
217	1,272	Balkrishna Mahadu	„	22	2 „ „	—	—	27 Jan. 1897	—	30 Jan. 1897		Bubonic plague.
218	1,273	Megju Kurmsey	„	52	2 „ „	—	—	—	—	16 June „		Dysentery.
219	1,274	Luxman Dhundu	„	28	3 „ „	15 Sept. 1897	30 Jan. 1897	—	—	—		
220	1,275	Bala Keshow	„	26	3 „ „	8 „ „	30 „ „	—	—	—		
221	1,276	Hossein Mahomed	Muhammadan	25	5 „ „	26 Mar. „	—	30 Jan. 1897	25 Feb. 1897	—		Transferred to Ratnagiri Prison.
222	1,277	Mahomed Bala	„	28	5 „ „	21 Aug. „	—	—	—	—		
223	1,278	Manuel Fernandez	R. Catholic, Christian.	49	5 „ „	—	30 Jan. 1897	—	—	26 July		Cholera.
224	1,278	Haji Moosa Dada	Muhammadan	36	6 „ „	5 June 1897	—	—	—	—		
225	1,280	Mahomed Jansen, &c.	„	28	6 „ „	4 Sept. „	30 Jan. 1897	—	—	—		
226	1,281	Mahomed Ebrahim	„	28	6 „ „	22 June „	—	—	—	—		
227	1,282	Dalsook Bhagoo	Hindu	29	6 „ „	12 Aug. „	—	—	—	—		
228	1,283	Nanabhoy Ananta	„	30	8 „ „	7 June „	—	—	—	—		
229	1,284	Mahomed Hossien Khan Duktukhan	Muhammadan	25	8 „ „	26 Mar. „	—	—	—	—		Transferred to Ratnagiri Prison.
230	1,285	Rowju Bhiwa, &c.	Hindu	30	14 „ „	19 „ „	—	—	—	—		„ „
231	1,286	Dowlatrao Khamo	„	22	15 „ „	20 Aug. „	—	—	—	—		
232	1,287	Sakharam Atmaram	„	37	16 „ „	15 June „	—	—	—	—		
233	1,289	Bhimsing Sileysing	„	36	17 „ „	23 Aug. „	30 Jan, 1897 2 Feb. „	—	—	—		
234	1,290	Haji Isoof	Muhammadan	34	19 „ „	26 Mar. „	—	—	—	—		Transferred to Ratnagiri Prison.
235	1,291	Fazel Dharsey	„	28	19 „ „	16 Oct. „	—	—	—	—		
236	1,294	Luxman Sakharam	Hindu	22	20 „ „	24 Aug. „	—	—	—	—		
237	1,295	Luxman Malloo	„	32	20 „ „	28 „ „	30 Jan 1897 2 Feb. „	—	—	—		
238	1,296	Kammoo Umer	Muhammadan	37	20 „ „	7 Sept. 1898	—	—	—	—		
239	1,297	Dhondu Sulkia	Hindu	32	20 „ „	17 Aug. 1897	—	6 Feb. 1897	25 Mar. 1897	—		Transferred to Yerowda Prison.
240	1,298	Syed Mahomed Kabais	Muhammadan	38	20 „ „	19 Mar. „	30 Jan. 1897 2 Feb. „	—	—	—		Transferred to Ratnagiri Prison.
241	1,999	Magan Tribhowan	Hindu	29	21 „ „	—	—	—	—	13 July 1897		Died in Jail hospital.
242	1,300	Golam Hossein Makon	Muhammadan	38	22 „ „	21 July 1897	—	—	—	—		
243	1,302	Golam Hussein Sharofati	„	50	24 „ „	23 June „	30 Jan. 1897	—	—	—		
244	1,303	Makhan Lala	Hindu	30	26 „ „	25 May „	30 „ „	—	—	—		
245	1,304	Gunpatti Babajee	„	28	27 „ „	2 Sept. „	30 „ „ 2 Feb „	—	—	—		
246	1,305	Fakir Isoof	Muhammadan	54	27 „ „	26 July „	—	—	—	—		
247	1,306	Mahadeo Ramjee	Hindu	32	28 „ „	4 Sept. „	30 Jan. 1897 2 Feb. „	—	—	—		
248	1,308	Jycbali Musabhai	Muhammadan	32	31 „ „	2 Sept. „	30 Jan. „ 2 Feb. „	—	—	—		
249	1,309	Jageshwar Saju	Hindu	45	2 Nov. „	—	—	—	—	—		Still in the prison.
250	1,310	Vishram Jairam	„	50	2 „ „	—	—	30 Jan. 1897	—	5 Feb. 1897		Bubonic plague.
251	1,311	Joseph Antone D'Souza	R. Catholic, Christian.	28	3 „ „	26 Mar. 1897	30 Jan. 1897	—	—	—		Transferred to Ratnagiri Prison.

App. LX.

LIST of NATIVE PRISONERS in the HOUSE OF CORRECTION, &c.—*continued.*

Serial No.	Registration No.	Name.	Caste.	Age.	Date of Admission into the Prison.	Date of *Discharge* from the Prison.	Date of Inoculation.	Date Attacked by Plague.	Date of Recovery.	Date of Death.	Remarks.
252	1,312	Walli Chand	Muhammadan	30	6 Nov. 1896	—	—	30 Jan. 1897	—	3 Feb. 1897	Bubonic plague.
253	1,313	Babajee Luximon	Hindu	32	6 „ „	23 Sept. 1898	—	—	—	—	
254	1,315	Kalloo Abdulla	Muhammadan	47	7 „ „	16 „ 1897	—	—	—	—	
255	1,316	Rama Deojee	Hindu	25	9 „ „	—	30 Jan. 1897	—	—	—	Still in the prison.
256	1,317	Dajei Tookaram	„	25	9 „ „	26 Mar. 1897	—	—	—	—	Transferred to Ratnagiri Prison.
257	1,318	Kasam Sileman	Muhammadan	32	9 „ „	19 „ „	30 Jan. 1897	—	—	—	
258	1,319	Shaik Mahomed Nur Mahomed	„	40	9 „ „	26 „ „	2 Feb. „	—	—	—	„ „
259	1,320	Ebraim Hassan	„	35	10 „ „	9 July „	30 Jan. „	—	—	—	„ „
260	1,321	Antone Joseph Fernandez	R. Catholic, Christian.	23	10 „ „	19 Mar. „	30 „ „	—	—	—	„ „
261	1,323	Dhondu Dhurma, &c.	Hindu	40	12 „ „	29 Sept. 1898	—	—	—	—	
262	1,324	Abdul Kussum	Muhammadan	31	13 „ „	22 June 1897	30 Jan. 1897	—	—	—	
263	1,325	Abdul Karim	„	36	17 „ „	29 Nov. „	—	—	—	—	
264	1,326	Shakri Khan Ismail Khan	„	21	17 „ „	26 Mar. „	30 Jan. 1897	—	—	—	
265	1,327	Marooti Sadu	Hindu	25	17 „ „	22 June „	—	—	—	—	
266	1,328	Shaik Abdulla Shaik Abas	Muhammadan	57	17 „ „	26 Mar. „	—	—	—	—	„ „
267	1,329	Mahadoo Dhondoo	Hindu	34	17 „ „	22 June „	—	—	—	—	
268	1,330	Pestonjee Sorabjee	Parsee	35	18 „ „	22 „ „	—	—	—	—	„ „
269	1,331	Bhooria Sunder	Hindu	24	18 „ „	22 „ „	—	—	—	—	
270	1,332	Kalloo Luxman	„	26	18 „ „	5 Oct. 1898	—	6 Feb. 1897	17 Feb. 1897	—	
271	1,336	Ganput Babajee	„	26	19 „ „	26 Mar. 1897	30 Jan. 1897	2 „ „	10 „ „	—	
272	1,338	Shaik Mahomed Shaik Hussan, &c.	Muhammadan	33	23 „ „	22 Sept. 1898	—	—	—	—	
273	1,340	Nandoo Bandoo	Hindu	23	24 „ „	22 June 1897	—	—	—	—	
274	1,341	Narayen Ganoo	„	27	24 „ „	19 Mar. „	30 Jan. 1897	—	—	—	
275	1,342	Narayen Magojee	„	30	24 „ „	22 June „	2 Feb. „	—	—	—	„ „
276	1,343	Vachraj Mansing	„	25	24 „ „	19 Mar. „	—	—	—	—	
277	1,344	Rama Noonjiajee	„	30	24 „ „	26 „ „	30 Jan. 1897	—	—	—	„ „
278	1,345	Sakaram Arjoon	„	22	24 „ „	19 „ „	30 „ „	—	—	—	„ „
279	1,346	Bhickin Pandoo	„	26	24 „ „	26 „ „	30 „ „	—	—	—	„ „
280	1,348	Abdul Rahman Karim	Muhammadan	35	26 „ „	14 May „	30 „ „	—	—	—	„ „
281	1,349	Adamjee Alibhai	„	28	30 „ „	26 Mar. „	2 Feb. „	—	—	—	„ „
282	1,350	Vithu Mankia	Hindu	28	1 Dec. „	11 Oct. 1898	—	27 Jan. 1897	10 Feb. 1897	—	
283	1,351	Jhawar Jeja	Muhammadan	48	3 „ „	6 „ 1897	30 Jan. 1897	—	—	—	„ „
284	1,352	Abbas Ally Ramzoo	„	28	4 „ „	19 Mar. „	—	—	—	—	
285	1,353	Ali Shaik Mahomed	„	53	4 „ „	19 „ „	—	—	—	—	
286	1,355	Kishan Manajee	Hindu	25	7 „ „	6 Aug. „	30 Jan. 1897	—	—	—	„ „
287	1,356	Abdul Karim Golam Hussain	Muhammadan	38	8 „ „	—	30 „ „	1 Feb. 1897	—	8 Feb. 1897	Bubonic plague.
288	1,357	Mahadeojee Mamekchand	Hindu	23	8 „ „	26 Mar. 1897	—	—	—	—	Transferred to Ratnagiri Prison.
289	1,358	Shaik Ebraim Shaik Ally	Muhammadan	52	9 „ „	8 June „	30 Jan. 1897	—	—	—	
290	1,359	Narayen Dhondu	Hindu	25	9 „ „	—	2 Feb. „	—	—	—	
291	1,361	Karim Khan Rahiman Khan	Muhammadan	36	10 „ „	22 June „	—	27 Jan. 1897	—	30 Jan. 1897	Bubonic plague.
292	1,362	Syed Jonas Ali	„	27	10 „ „	22 „ „	—	—	—	—	
293	1,363	Jagunath Mahanand	Hindu	39	10 „ „	8 Oct. 1898	30 Jan. 1897	30 Jan. 1897	15 Mar. 1897	—	

294	1,364	Tara Chand Dhossa	,,	30	11 Dec. ,,	26 Mar. 1897	—	—	—	—	Transferred to Ratnagiri Prison.
295	1,365	Mahomed Ismail	Muhammadan	35	11 ,, ,,	26 ,, ,,	30 Jan. 1897	—	—	—	,, ,,
296	1,366	Shamrao Sittaram	Hindu	27	12 ,, ,,	14 May ,,	30 ,, ,,	7 Feb. 1897	16 Mar. 1897	—	,, ,,
							2 Feb. ,,				
297	1,367	Abdoola Imam	Muhammadan	56	12 ,, ,,	19 Mar. ,,	—	—	—	—	,, ,,
298	1,368	Manji Khatao	Hindu	24	14 ,, ,,	19 ,, ,,	30 Jan. 1897	—	—	—	,, ,,
299	1,372	Deysey Nursey	,,	24	15 ,, ,,	26 ,, ,,	30 ,, ,,	—	—	—	,, ,,
300	1,373	Syed Nur Syed Takrudin, &c.	Muhammadan	56	16 ,, ,,	14 Aug. ,,	—	—	—	—	,, ,,
301	1,376	Pedro Pedar	R. Catholic, Christian.	25	17 ,, ,,	19 Mar. ,,	30 Jan. 1897	—	—	—	Transferred to Kolaba Lunatic Asylum.
302	1,377	Poodisingaya John Pereira	Budhist	22	18 ,, ,,	17 ,, ,,	—	—	—	—	} Still in the prison.
303	1,378	Jeysing Sewa	Hindu	32	18 ,, ,,	—	30 Jan. 1897	—	—	—	
304	1,379	Shivram Antajee	,,	19	18 ,, ,,	—	30 ,, ,,	—	—	—	
305	1,380	Abdul Rahiman	Muhammadan	29	18 ,, ,,	—	30 ,, ,,	—	—	—	Sent to Hyderabad, Sind.
306	1,381	Hakim Abdulla	,,	43	18 ,, ,,	10 May 1898	30 ,, ,,	—	—	—	Still in the prison.
307	1,382	Mowlabux Bahadur	,,	30	18 ,, ,,	—	30 ,, ,,	—	—	—	Sent to Hyderabad, Sind.
308	1,383	Sitaram Bholuram	Hindu	35	18 ,, ,,	10 May 1898	—	—	—	—	,, ,,
309	1,384	Killoo Kaloona	,,	22	18 ,, ,,	10 ,, ,,	30 Jan. 1897	—	—	—	
310	1,385	Yacoob Samsher	Jew	35	18 ,, ,,	26 Mar. 1897	30 ,, ,,	—	—	—	
311	1,386	Luxman Narayen	Hindu	27	21 ,, ,,	14 May ,,	30 ,, ,,	—	—	—	
312	1,387	Sadasew Khanderao	,,	24	23 ,, ,,	3 Sept. 1898	30 ,, ,,	—	—	—	
313	1,388	Vithal Meghajee	,,	32	24 ,, ,,	26 Mar. 1897	—	—	—	—	Transferred to Ratnagiri Prison.
314	1,389	Dhanjee Mitha	Muhammadan	32	24 ,, ,,	5 Sept. 1898	30 Jan. 1897	—	—	—	
							2 Feb. ,,				,, ,,
315	1,390	Alli Manji	,,	35	24 ,, ,,	19 Mar. 1897	30 Jan. ,,	—	—	—	
							2 Feb. ,,				
316	1,398	Mahomed Badan Khan	,,	23	2 Jan. 1897	1 Sept. ,,	30 Jan. ,,	—	—	—	
							2 Feb. ,,				
317	1,399	Luxman Babu, &c.	Hindu	25	2 ,, ,,	1 ,, ,,	30 Jan. ,,	—	—	—	
318	1,400	Sukdeo Jaigovind	,,	30	2 ,, ,,	26 Mar. ,,	30 Jan. ,,	—	—	—	,, ,,
							2 Feb. ,,				
319	1,401	Ranchod Bhajee	,,	33	2 ,, ,,	1 July ,,	30 Jan. ,,	—	—	—	
							2 Feb. ,,				
320	1,402	Narayen Marroo, &c.	,,	27	4 ,, ,,	3 Sept. ,,	30 Jan. ,,	—	—	—	
321	1,403	Mirkhan Yarkhan	Muhammadan	20	5 ,, ,,	22 June ,,	,, ,,	—	—	—	
322	1,404	Bhoka *alias* Hussain Alli	,,	25	5 ,, ,,	22 ,, ,,	30 ,, ,,	—	—	—	
323	1,405	Rama Govinda	Hindu	26	5 ,, ,,	22 ,, ,,	—	—	—	—	
324	1,406	Moossa Rakabhai	Muhammadan	25	5 ,, ,,	22 ,, ,,	—	—	—	—	
325	1,407	Muria Nagapa	Hindu	52	5 ,, ,,	22 ,, ,,	—	—	—	—	
326	1,408	Kessu Chima	,,	32	5 ,, ,,	22 ,, ,,	—	—	—	—	Still in the prison.
327	1,410	Motiram Makanjee	,,	25	7 ,, ,,	—	—	—	—	—	Transferred to Ratnagiri Prison.
328	1,411	Isuf Dhakatia	Muhammadan	27	8 ,, ,,	26 Mar. 1897	30 Jan. 1897	—	—	—	
329	1,412	Kalloo Padamsey	Hindu	33	9 ,, ,,	26 Sept. 1898	30 ,, ,,	—	—	—	
							2 Feb. ,,				
330	1,413	Deochand Rowjee	,,	35	9 ,, ,,	26 Mar. 1897	30 Jan. ,,	—	—	—	,, ,,
331	1,414	Harpal Singh Ratan Sing	,,	36	9 ,, ,,	19 ,, ,,	30 ,, ,,	—	—	—	,, ,,
							2 Feb. ,,				
332	1,415	Ram Chandra Gopal	,,	35	9 ,, ,,	26 ,, ,,	—	—	—	—	
333	1,416	Golam Mohidin Kamruddin	Muhammadan	29	18 ,, ,,	1 Feb. ,,	—	—	—	—	
334	1,417	Pomersey Coverjee	Hindu	25	26 ,, ,,	8 Nov. 1898	—	—	—	—	
335	1,418	Dharshan Sing Raghunath Sing	,,	35	27 ,, ,,	26 July 1897	—	—	—	—	
336	1,420	Ram Surroop Hanumandeo	,,	36	28 ,, ,,	—	—	—	—	12 Feb. 1898	Died in Jail hospital.
337	1,421	Abas Abdoola	Muhammadan	45	29 ,, ,,	14 May 1897	—	—	—	—	Transferred to Kolaba Lunatic Asylum.
338	976	Ashama Erasawmy	Hindu	25	26 Feb. 1896	3 Aug. ,,	—	—	—	—	

The following 51 prisoners were inoculated a second time on the 2nd February, 1897:—Prisoners Nos. 534, 828, 1348, 1076, 1108, 1130, 1114, 1230, 1412, 1400, 1318, 1414, 1006, 1054, 1075, 1002, 1400, 1304, 1262, 1020, 1194, 1004, 1213, 1026, 1398, 1366, 1390, 1295, 1389, 1306, 1110, 1150, 1289, 1208, 1223, 1153, 1210, 1172, 1189, 514, 1341, 1358, 1214, 1166, 1225, 899, 1034, 1298, 1095, 1209, 1308.

B.

INOCULATIONS with M. HAFFKINE'S PROPHYLACTIC SERUM in 1898: showing the reaction in each case inoculated in Her Majesty's House of Correction, Byculla, Bombay.

Serial No.	Prison Register No.	Names of Prisoners.	Date of Inoculation, 1st time.	Temperature, 3rd.		Temperature 4th.		Temperature, 5th.		Date of Inoculation, 2nd time.	Temperature 9th.		Temperature, 10th.		Temperature, 11th.		Remarks.	Date of leaving Jail.
			1898.	A.M	P.M.	A.M.	P.M.	A.M.	P.M.	1898.	A.M.	P.M.	A.M.	P.M.	A.M.	P.M.		
1	213	Ganesh Shunkar Dravid	3rd Jan.	—	97·0	100·0	99·0	97·4	N.	9th Jan.	—	100·4	100·8	99·0	98·2	N.	Bottle No. 2579. 5 c.c.	21. 6.98
2	31	Jagnath Mahanand	,,	—	99·4	100·2	102·0	98·8	N.	,,	—	102·8	99·2	100·0	99·0	N.	,, ,,	8.10.98
3	73	Vicharam Pudamsey	,,	—	98·4	99·8	98·4	98·2	N.	,,	—	Not done.					,, ,, (No. 3)	14. 5.98
4	32	Jansing Shiva	,,	—	98·4	99·6	102·2	99·2	99·2	,,	—	100·8	99·0	99·0	98·2	N.	,, ,, [Syringe	—
5	36	Mowlabux Bahadur	,,	—	100·2	100·0	100·2	99·4	98·4	,,	—	102·0	100·6	99·4	98·4	N.	,, ,, [leaked.	—
6	4	Mahomed Nansey	,,	—	99·6	100·2	101·8	98·4	N·	,,		102·1	100·8	99·0	98·0	N.	,, ,,	—
7	26	Rama Dewji	,,	—	98·4	100·0	99·0	97·0	N.	,,	-	Not done.					,, ,,	—
8	59	Mahomed Ismail	,,	—	100·0	99·0	99·0	98·4	N.	,,	—	101·8	101·2	100·0	100·0	N.	,, ,,	24. 9.98
9	5	Rama Luximon	,,	—	99·0	100·0	100·8	98·0	N.	,,	—	101·8	99·6	100·0	98·2	N.	,, ,,	—
10	94	Dharma Tannoo	,,	—	100·4	99·8	100·0	99·0	99·4	,,	—	99·8	102·0	100·0	98·0	N.	,, ,,	30. 9.98
11	12	Abdoola Ailadin	,,	—	101·0	99·0	100·4	97·8	N.	,,	—	102·2	99·6	99·8	98·0	N.	,, ,,	12. 9.98
12	214	Bala Narayan	,,	—	98·0	98·4	N.	N.	N.	,,	—	100·6	99·6	98·6	98·0	N.	,, ,,	18.10.98
13	210	Luximon Balu	,,	—	100·0	98·4	102·0	98·4	N.	,,	—	101·6	101·2	100·0	98·0	N.	,, ,,	15.10.98
14	136	Tookaram Kundoo	,,	—	98·4	100·4	101.2	98·2	99·6	,,	—	101·0	99·4	99·0	98·0	N.	,, ,,	—
15	89	Karim Rabimin	,,	—	100·6	101·2	100·4	98·6	N.	,,	—	102·0	101·6	100·0	98·2	N.	,, ,,	6. 1.98
16	35	Hakim Abdoola	,,	—	99·2	100·0	101·0	99·0	N.	,,	-	101·2	98·8	99·4	98·4	N.	,, ,, (No. 16)	10. 5.98
17	24	Jageswar Sayee	,,	—	97·0	100·0	101·0	97·6	N.	,,	-	Not done.					,, ,, [Syringe	—
18	97	Maheshwar Kannoo	,,	—	98·0	99·2	100·8	98·4	N.	,,	—	102·2	100·2	99·6	100·0	99.0	,, ,, [leaked.	—
19	107	Jakha Lakha	,,	—	99·8	99·6	99·0	98·4	N.	,,	—	100·0	100·2	99·4	98·0	N.	,, ,,	—
20	52	Goolamalli Aboodalli	,,	—	99·2	100·2	102·0	99·6	101·4	,,	—	103·2	100·8	101·0	99·0	N.	,, ,,	21. 7·98
21	182	Adamji Tayabji	,,	—	99·0	100·4	99·0	99·0	N.	,,	—	101·4	100·0	99·0	99·0	N.	,, ,,	—
22	209	Jaffer Ismail	,,	—	100·4	99·0	99·0	98·2	N.	,,	—	103·2	101·0	101·2	N.	N.	,, ,,	30. 9.98
23	143	Hosein Karim	,,	—	101·4	99·4	99·4	98·4	N.	,,	—	101·2	100·2	100·4	N.	N.	,, ,,	28.10.98
24	216	Habeebkhan Garibulla	,,	—	100·2	98·0	99·8	97·0	N.	,,	—	99·4	98·0	N.	N.	N.	,, ,,	15.11.98
25	160	Pandu Ganu	,,	—	101·4	100·4	101·0	98·0	N.	,,	—	99·2	101·8	100·4	98·6	N.	Bottle No. 2068. 5 c.c.	10. 5.98
26	6	Sheik Hussein Ahmed	,,	—	101·2	99 0	101·0	99·4	100·0	,,	—	101·2	101·4	101·4	99·0	99·6	,, ,,	27. 4.98
27	96	Jankidas Chunilal	,,	—	99·6	99·6	N.	98·0	N.	,,	—	100·4	99·0	99·0	N.	N.	,, ,,	16. 6.98
28	135	Khanusar Salene	,,	—	99·2	98·6	100·0	98·0	N.	,,	—	101·4	99·6	100·0	N.	N.	,, ,,	6. 8.98
29	194	Suleman Walji	,,	—	99·0	99 0	98·0	98·0	N.	,,	—	99·8	100·6	100·2	98·8	N.	,, ,,	—
30	157	Shaik Amir Shaik Chand	,,	—	101·0	99·8	98·8	98 0	N.	,,	—	101·0	101·4	99·4	N.	N.	,, ,,	3. 9.98
31	255	Bachu Hassan	,,	—	98·0	99·4	98·8	98·0	N.	,,	—	100·6	100·0	100·8	99·2	N.	,, ,,	—
32	21	Kassim Mahomed	,,	—	99·6	101·2	99·2	99·2	99·4	,,	—	99·4	100·0	100·2	98·8	N.	,, ,,	30. 4.98
33	88	Murli Rama	,,	—	98·4	100·0	98 0	98·0	N.	,,	—	102·8	102·4	101·0	98·4	N.	,, ,,	11. 6.98
34	109	Framjee Nanabhoy	,,	—	98·0	99·0	100·4	98·0	N.	,,	—	102·0	99·8	100·2	N.	N.	,, ,,	—
35	206	Molu Baboo Devi Baboo	,,	—	100·0	100·4	101·4	98·0	N.	,,	—	103·6	102·0	101·4	99·4	99·2	,, ,,	30. 9.98
36	176	Muhamad I. A. Karim	,,	—	98·2	100·0	100·2	99·0	100·4	,,	—	103·2	101·0	101·4	N.	N.	,, ,,	19. 9.98
37	120	Goolam Alli	,,	—	99·0	99·2	99·0	99·0	N.	,,	—	100·0	100·2	101·4	N.	N.	,, ,,	—
38	50	Shaik M. Shaik Hussein	,,	—	99·4	100·0	101·2	98·2	N.	,,	—	102·0	99·2	101·0	98·0	N.	,, ,,	31. 1.98
39	183	Alli Mahamad Essufobhoy	,,	—	100·6	99·2	101·0	99·4	10 ·0	,,	—	101·0	101·8	101·0	99·2	N.	,, ,,	10. 5.98
40	71	Moti Arjan	,,	—	98·4	99·2	99·6	97·2	N.	,,	—	100·4	100·8	100·6	N.	N.	,, ,,	12. 2.98
41	234	Toolasing Amirsing	,,	—	99·0	98·8	98·0	98·0	N.	,,	—	99·2	98·6	100·0	98·0	N.	,, ,,	13. 5·98
42	249	Shaik Mohamad Buni Sahib	,,	—	99·2	98·6	99·0	98·4	N.	,,	—	99·8	98·2	99·0	N.	N.	,, ,,	—
43	158	Kassim Muhamad	,,	—	100·0	99·0	99·0	98·0	N.	,,	—	99·4	99·2	101·0	N.	N.	,, ,,	—

44	211	Abdoolalli Muhamad Alli	„	—	98·6	101·0	99·0	99·0	N.	„	—	100·6	99·6	100·0	98·8	N.	Bottle No. 2040.	5 c.c.		13.11.98
45	165	Babu Gungadhar Tilak	„	—	99·0	100·8	101·0	100·4	100·0	„	—	Not done.					Bottle No. 2579.	5 c.c.		28. 1.98
46	23	Kumoon Amir	„	—	101·0	100·6	100·4	101·4	N.	„	—	100·6	99·6	100·8	99·0	N.	„	„		7. 9.98
47	161	Abdool Hassein	„	—	102·8	102·2	99·0	98·8	100·8	„	—	102·0	103·0	103·0	100·2	N.	„	„		6. 9.98
48	147	Ganu Tukaram	„	—	100·8	101·0	99·6	97·8	N.	„	—	100·2	99·6	99·4	N.	N.	„	„		—
49	156	Nasarwauji Hormusji	„	—	103·6	103·0	100·6	102·0	100·4	„	—	103·8	102·2	101·0	98·8	N.	„	„		3. 9.98
50	87	Durga Gunesh	„	—	99·4	100·8	100·4	98·8	N.	„	—	99·0	99·8	101·6	98·0	N.	„	„		—
51	221	Bala Keshav	„	—	99·0	99·4	99·4	97·6	N.	„	—	99·4	101·6	100·4	98·8	N.	Bottle No. 2068.	5 c.c.		—
52	83	Illahibux Madarbux	„	—	100·0	101·0	98·8	N.	N.	„	—	Not done.					„	„		—
53	197	Gopalia Girdhalall	„	—	99·0	100·2	99·6	97·8	N.	„	—	99·6	99·6	99·6	99·0	99·2	„	„		28. 9.28
54	146	Ganasham Lalkalia	„	—	N.	99·2	99·0	97·8	N.	„	—	101·8	101·0	101·6	N.	N.	„	„		10. 5.98
55	62	Girghar Magan	„	—	102·0	102·0	100·6	99·6	N.	„	—	102·0	102·4	102·8	99·6	98·6	„	„		9. 9.98
56	226	Mahomed Balla	„	—	103·0	100·0	98·0	98·8	N.	„	—	99·6	101·0	101·2	N.	N.	„	„		10. 5.98
57	65	Mahomed Moosa	„	—	99·6	100·0	99·8	98·8	N.	„	—	100·4	100·4	101·2	99.0	99·0	„	„		22.10.98
58	126	Mahomed Lalkhan	„	—	100·2	99·8	100·0	97·8	N.	„	—	102·2	101·4	101·0	98·0	N.	„	„		16.11.98
59	132	Mahomed Sallan	„	—	100·0	99·0	99·6	99·0	N.	„	—	Not done.					„	„		—
60	92	Anala Narayan	„	—	99·0	99·0	99·8	97·8	N.	„	—	98·8	101·4	102·0	98·8	N.	„	„		13. 6.98
61	200	Shumshereali Allabux	„	—	100·0	100·0	100·8	99·6	N.	„	—	Not done.					„	„		10. 5.98
62	57	Abdoola Kurimbux	„	—	99·2	99·4	99·4	N.	N.	„	—	99·0	99·2	101·0	99·0	N.	„	„		4. 2.99
63	219	M. E. Mirza Sulleiman	„	—	100·4	101·0	99·6	N.	N.	„	—	101·2	101·6	101·6	N.	N.	„	„		—
64	248	Ramchandra Jamrawalla	„	—	100·0	99·6	99·2	N.	N.	„	—	100·8	99·8	99·6	99·0	N.	„	„		—
65	131	Dashrah Mahadoo	„	—	99·6	100·2	100·2	99·0	N.	„	—	100·0	98·0	99·0	N.	N.	„	„		—
66	85	Ramchandra Bulakichand	„	—	100·4	99·6	100·0	98·0	N.	„	—	Not done.					„	„		—
67	90	Babaji alias Babha Janoo	„	—	99·2	100·0	100·6	99·6	N.	„	—	99·4	100·4	101·2	100·0	N.	„	„		—
68	141	Panda Narayan	„	—	100·0	100 0	99·2	97·0	N.	„	—	100·0	102·4	101·0	N.	N.	„	„	(No. 67)	13. 8.98
69	101	Baldeo Durga	„	—	100·0	99·6	100·2	97·6	N.	„	—	101·4	101·8	101·8	99·4	100·0	„	„	[Syringe	—
70	99	Pedro Chimaya	„	—	98·0	98·4	99·4	N.	N.	„	—	99·0	98·6	100·0	N.	N.	„	„	[leaked.	—
71	81	Vishram Dayal	„	—	99·6	101·0	100·2	100·8	N.	„	—	101·4	101·4	102·0	99·2	N.	„	„		10. 5.98
72	125	Babaji Luximon	„	—	99·2	92·4	99·0	99·4	N.	„	—	100·0	100·4	100·0	98·0	N.	„	„		23. 9.98
73	15	Dharsey Ramji	„	—	99·6	99·6	99·0	99·8	N.	„	—	100·6	100·4	100·2	98·0	N.	„	„		9. 7.98
74	90	Hurri Luximon	„	—	103·0	100·6	100·4	98·4	N	„	—	98·8	99·0	99·2	N.	N.	Bottle No. 2040.	5 c.c.		29. 9.98
75	137	Sillaram Bollaram	„	—	98·4	100·0	99·4	100·0	N.	„	—	101·2	101·2	99·0	N.	N.	„	„		10. 5.98
76	231	Khana Pokaldas	„	—	100·2	101·0	101·6	101·8	N.	„	—	102·4	102·0	103·4	101·0	99·4	„	„	(No. 78)	30. 9.98
77	152	Aziz Wahab	„	—	99·2	99·6	99·8	98·8	N.	„	—	101·0	100·4	101·4	N.	N.	„	„	[Had plague	26. 8.98
78	69	Antone Salvadore	„	—	98·2	102·4	99·2	99·0	N.	„	—	100·8	101·4	101·4	N.	N.	„	„	[in 1897.	25.10.98
79	254	Waikia Luximon	„	—	98·4	98·8	99·0	N.	N.	„	—	98·6	98·6	99·0	N.	N.	„	„	(No. 82)	24.11.98
80	247	Jamal Murad	„	—	99·4	99·0	99·6	N.	N.	„	—	99·4	99·4	100·4	98·0	N.	„	„	Severe epi-	30. 9.98
81	64	S. S. Shaik Bahadur	„	—	99·0	99·0	99·0	98·0	N.	„	—	98·8	101·4	100·0	N.	N.	„	„	lepsy after	19. 2.98
82	191	Tasduk Hossein	„	—	99·6	101·4	101·0	99·4	N.	„	—	100·8	100·6	100·6	99·0	98·6	„	„	inoculation;	10. 5.98
83	113	Luximon Sadu	„	—	99·4	99·0	99·2	97·0	N.	„	—	Not done.					„	„	bad attack	—
84	20	Rama Gunpat*	„	—	104·0	102·0	102·2	100·4	98·6	„	—	Not done.					„	„	about 12	12. 3.98
85	118	Abdool E. Dekri	„	—	100·0	100·0	99·0	98·0	N.	„	—	102·6	101·0	101·6	99·2	N.	„	„	years ago;	11. 7.98
86	80	Balla Gunpat	„	—	99·0	99·8	100·0	98·0	N.	„	—	100·8	101·0	100·8	99·0	N.	„	„	none since.	9. 5.98
87	193	Mahudo Balloo	„	—	100·0	101·0	100·4	99·0	N.	„	—	101·2	100·4	100·8	98·8	N.	Bottle No. 2579.	5 c.c.		—
88	192	Kairoo Govind	„	—	99·0	100·4	101·2	99·0	N.	„	—	101·0	100·0	99·8	98·6	N.	„	„		—
89	263	Dhondu Fakira	„	—	99·4	100·0	100·0	97·8	N.	„	—	101·6	101·2	99·8	N.	N.	„	„		2. 7.98
90	102	Diashaw Rustomji	„	—	99·4	100·6	100·0	98·0	N.	„	—	103·2	99·6	99·6	N.	N.	„	„		21. 6.98
91	106	Mohadu Govind	„	—	102·4	100·6	100·8	98·0	N.	„	—	103·2	100·6	100·4	N.	N.	„	„		—
92	13	Kanji Magji	„	—	98·8	100·6	N.	97·8	N.	„	—	101·8	101·2	100·4	99·0	N.	„	„		28. 5.98
93	215	Idu Bukurdeen	„	—	100·2	99·0	100·0	99·0	N.	„	—	100·6	99·0	101·0	N.	N.	Bottle No. 2040.	5 c.c.		—
94	49	Dhunaji Khushuaji	„	—	100·2	99·6	99·6	98·4	N.	„	—	101·0	101·2	100·8	98·6	N.	„	„	(No. 94)	2. 7.98
95	86	Noorbhoy Budrudeen	„	—	99·4	99·0	99·2	N.	N.	„	—	101·2	101·0	100·2	98·6	N.	„	„	[Hysteria after	—
96	72	Tricam Jago	„	—	99·2	100·4	100·2	N.	N.	„	—	101·0	98·4	101·4	98·4	N.	„	„	[inoculation.	—
97	100	Abdool Nasir	„	—	99·6	99·4	99·8	N.	N.	„	—	100·4	100·6	102·0	99·2	100·0	„	„		23. 6.98
98	51	Abdool Alaf Khan	„	—	100·0	100·6	99·4	N.	N.	„	—	100·0	101.6	101·0	99·4	N.	„	„		10. 5.98

INOCULATIONS with M. HAFFKINE'S PROPHYLACTIC SERUM: showing the reaction in each case inoculated in Her Majesty's House of Correction, Byculla, Bombay—*continued*.

Serial No.	Prison Register No	Names of Prisoners.	Date of Inoculation, 2nd time.	Temperature, 3rd.		Temperature, 4th.		Temperature, 5th.		Date of Inoculation, 2nd time.	Temperature, 9th.		Temperature, 10th.		Temperature, 11th.		Remarks.			Date of leaving Jail.
			1898.	A.M.	P.M.	A.M.	P.M.	A.M.	P.M.	1898.	A.M.	P.M.	A.M.	P.M.	A.M.	P.M.				
99	36	Nanji Khalow	3rd Jan.	—	99·0	100·0	101·0	97·8	N.	9th Jan.	—	101·2	100·4	101·0	99·4	98·8	Bottle No. 2040.	5 c.c.		23. 8.98
100	196	Adam Abdoola	,,	—	99·0	99·6	99·0	98·0	N.	,,	—	100·0	99·4	101·0	98·4	N.	,,	,,		—
101	264	Ramchandra Heero	,,	—	99·6	98·0	99·6	96·0	N.	,,	—	100·2	100·6	101·0	98·2	N.	,,	,,		20. 1·99
102	129	Shiva Bapu	,,	—	99·4	99·8	102·0	99·2	N.	,,	—	100·4	100·6	100·6	100·2	N.	,,	,,		18. 1.99
103	47	Bhowani Shankar Keshaw	,,	—	100·4	100·4	101·0	99·6	N.	,,	—	101·4	102·0	100·2	98·0	N.	,,	,,		—
104	16	Abdool Alli	,,	—	100·2	99·6	101·0	96·0	N.	,,	—	101·0	101·0	99·2	98·0	N.	,,	,,		11. 7.98
105	84	Dhama Rama	,,	—	100·0	99·4	100·0	N.	N.	,,	—	102·4	99·0	101·0	98·0	N.	,,	,,		—
106	142	N. M. Haji Ahmed	,,	—	98·4	99·0	100·0	N.	N.	,,	—	102·0	101·4	101·0	99·4	N.	,,	,,		—
107	150	Dewji Kanji	,,	—	98·4	99·2	99·4	98·8	N.	,,	—	101·2	100·8	100·6	98·6	N.	,,	,,		—
108	117	Pandoo Bhon	,,	—	100·4	99·4	100·6	98·8	N.	,,	—	102·0	99·0	100·0	99·6	N.	,,	,,		11. 7.98
109	10	Doolaji Dhondu	,,	—	98·4	99·8	100·0	100·6	99·0	,,	—	103·0	101·0	101·0	99·6	99·2	,,	,,		—
110	265	Gana Krishna	,,	—	98·4	99·4	100·0	N.	N.	,,	—	101·4	101·0	102·2	98·4	N.	,,	,,		16.11.98
111	228	Hosain Abdool	,,	—	98·6	99·0	100·0	N.	N.	,,	—	100·0	99·0	100·0	99·4	99·0	,,	,,		30. 9.98
112	170	Kandas Narayandas	,,	—	100·4	100·2	100·4	99·0	99·0	,,	—	101·6	99·0	100·6	98·4	N.	,,	,,		10. 5.98
113	180	Bhailal Kishore	,,	—	99·6	99·6	100·0	99·0	N.	,,	—	99·6	101·6	101·0	99·0	N.	,,	,,		30. 9.98
114	95	Joseph A. De Souza	,,	—	98·6	100·0	99·2	98·8	N.	,,	—	103·0	100·2	101·6	N.	N.	,,	,,		5.12.98
115	70	Antone Minguel	,,	—	98·4	102·0	101·0	N.	N.	,,	—	103·2	100·0	100·0	98·2	N	Bottle No. 2579.			—
116	93	Bhow Luximon	,,	—	101·2	99·4	100·2	98·2	N.	,,	—	102·0	102·0	101·2	98·6	N.	,,			—
117	223	Narayandas Nathasing	,,	—	101·0	102·0	100·0	98·4	N.	,,	—	100·6	99·0	100·2	98·4	N.	,,			30. 9.98
118	134	Goolam H. Mohideen	,,	—	99·4	98·8	100·6	98·4	N.	,,	—	102·4	101·2	100·6	99·0	N.	,,			—
119	112	Ganoo Rama	,,	—	100·0	99·6	100·0	98·4	N.	,,	—	101·0	99·0	100·4	98·4	N.	,,			—
120	179	Luximon Dooka	,,	—	102·6	99·6	98·4	96·8	N.	,,	—	102·0	98·4	98·4	98·0	N.	,,			12.12.98
121	127	Vishram Soma	,,	—	99·0	98·6	100·0	98·2	N.	,,	—	101·4	100·0	101·0	99·0	N.	,,			19. 7.98
122	241	Marian Francis	,,	—	98·0	100·4	100·0	98·6	N.	,,	—	101·0	99·0	101·6	98·0	N.	,,			—
123	240	Adam L. Yacoob	,,	—	99·2	98·8	98·4	98·4	N.	,,	—	100·0	99·0	99·8	98·2	N.	,,			30. 9.98
124	344	Luximon Devidatt	,,	—	100·0	99·8	100·8	99·0	N.	,,	—	104·0	99·0	103·0	99·0	N.	,,			—
125	202	Appa Babaji	,,	—	100·2	98·8	102·6	101·0	N.	,,	—	102·0	101·2	101·0	101·0	98·2	Bottle No. 2104.	5 c.c.		10.10.98
126	153	Kallia Vitho	,,	—	99·0	100·2	101·0	98·0	N.	,,	—	101·0	102·0	102·6	99·0	N.	,,	,,		—
127	68	Rowji Balkia	,,	—	98·4	100·6	98·8	98·6	N.	,,	—	100·0	99·4	98·6	98·4	N.	,,	,,		—
128	164	Chand Kasim	,,	—	99·0	99·2	99·8	97·2	N.	,,	—	100·6	98·4	98·8	98·4	N.	,,	,,		22. 1.98
129	11	Ali Omer	,,	—	98·6	98·0	98·0	97·0	N.	,,	—	101·0	N.	99·0	98·0	N.	,,	,,	(No. 129)	6. 5.98
130	251	John R. Jones	,,	—	99·0	100·0	100·4	N.	N.	,,	—	101·0	99·0	98·0	N.	N.	,,	,,	[Syringe	16. 6.98
131	257	Charles Moore	,,	—	N.	101·0	100·4	N.	N.	,,	—	100·0	99·0	98·0	N.	N.	,,	,,	[leaked.	5. 2.98
132	243	Kristo Chundiadas	,,	—	99·0	100·0	101·6	98·4	N.	,,	—	99·4	101·6	101·0	100·4	99·0	,,	,,		24. 6.98
133	14	Mahomedalli Ragadin	,,	—	98·6	100·6	100·4	97·2	N.	,,	—	100·0	101·0	101·0	98·0	N.	,,	,,		27. 6.98
134	98	Mahudu Jaishankar	,,	—	99·2	99·4	101·0	99·8	N.	,,	—	102·0	100·0	101·4	98·4	N.	,,	,,		21. 6.98
135	198	Ratna Gosoo	,,	—	98·0	99·0	100·6	98·2	N.	,,	—	Not done.					,,	,,		30. 9.98
136	256	Rustonji Pestonji	,,	—	98·0	100·0	99·4	N.	N.	,,	—	100·4	N.	99·2	98·4	N.	,,	,,		10. 5.98
137	258	Dagdu Rama	,,	—	99·2	99·6	100·2	98·4	N.	,,	—	100·0	100·0	100·0	98·4	N.	,,	,,		—
138	166	Bhagwandas Devidas	,,	—	100·2	102·4	101·4	97·8	N.	,,	—	101·2	100·0	100·2	98·2	N.	,,	,,		10. 5.98

* (Serial No. 84; reg. No. 20) This case died in Jail from tubercle of lung, the symptoms of the disease set in rapidly after inoculation.

The following prisoners were not inoculated on the 9th of January, 1898:—Nos. 73, 26, 24, 165, 82, 132, 200, 85, 113, 20, and 198.

The 50 prisoners against whose names no dates are recorded in the last column were still in Jail on 25th February 1899, when this list was prepared.

A. M. KING,
Assistant Surgeon.

GEORGE WATERS, Lieut.-Colonel, I.M.S.
In Medical Charge, Her Majesty's House of Correction, Byculla.

APPENDIX No. LXI.

STATISTICS

OF

PLAGUE CASES TREATED

IN THE

ARTHUR ROAD HOSPITAL, BOMBAY.

A.

TABLE showing the ADMISSIONS, DEATHS, &c., from PLAGUE at the ARTHUR ROAD HOSPITAL, BOMBAY, from September 1893 to February 1899.

	Number.	Died within 24 hours.	Died within 48 hours.	Total Deaths.	Recovered.	Percentage of Mortality.
1896.						
Sept. and Oct.	88	17	10	46	42	52·23
November	7	9	3	18	9	66·67
December	182	60	63	135	47	74·12
Total	297	86	76	199	98	67·00
1897.						
January	277	90	43	191	86	68·95
February	365	130	46	298	67	81·64
March	193	54	34	130	63	67·35
April	60	17	10	34	26	56·66
May	26	7	1	10	16	38·46
June	42	2	1	6	36	14·28
July	13	1	1	7	6	53·84
August	11	3	—	7	4	63·63
September	33	11	4	22	11	66·66
October	31	6	7	16	15	51·61
November	52	11	6	23	26	50·00
December	167	32	44	115	52	68·68
Total	1,270	364	197	862	408	67·87
1898.						
January	496	139	103	362	134	72·98
February	660	185	140	514	146	77·87
March	454	143	86	341	113	75·11
April	117	29	9	58	59	49·57
May	98	19	6	44	54	44·89
June	12	2	1	4	8	33·33
July	28	5	5	22	6	78·57
August	73	27	15	56	17	76·71
September	91	27	17	75	16	82·41
October	63	25	9	53	10	84·12
November	22	6	2	18	4	81·81
December	72	25	8	55	17	76·38
Total	2,186	632	407	1,602	584	73·28
1899.						
January	166	67	36	137	29	82·53
Grand Total	3,919	1,149	706	2,800	1,119	71·44

B.

TABLE comparing by Months the MORTALITY from PLAGUE at the ARTHUR ROAD HOSPITAL, BOMBAY, from September 1896 to February 1899.

	1896.		1897.		1898.		1899.	
	Admissions.	Mortality.	Admissions.	Mortality.	Admissions.	Mortality.	Admissions.	Mortality.
January	—	—	277	68·95	496	72·98	166	82·53
February	—	—	365	81·64	660	77·87	—	—
March	—	—	193	67·35	454	75·11	—	—
April*	—	—	60	56·66	117	49·57	—	—
May*	—	—	26	38·46	98	44·89	—	—
June*	—	—	42	14·28	12	33·33	—	—
July	—	—	13	53·84	28	78·57	—	—
August	—	—	11	63·63	73	76·71	—	—
September	—	—	33	66·66	91	82·41	—	—
October	88	52·23	31	51·61	63	84·12	—	—
November	27	66·67	52	50·00	22	81·81	—	—
December	182	74·12	167	68·68	72	76·38	—	—
Total	297	67·00	1,270	67·87	2,186	73·28	—	—

* These months show the lowest mortality rates, because convalescent patients from other hospitals were transferred to Arthur Road. these be excluded, the mortality would be as follows :—

1897, May	-	40·00 per cent.
„ June	-	22·22 „
1898, April	-	58·41 „
„ May	-	54·83 „
„ June	-	57·14 „

O.

STATEMENT showing the POSITIONS of BUBOES, MORTALITY, &c. in 2,723 PLAGUE CASES TREATED in the ARTHUR ROAD HOSPITAL, BOMBAY.

——	Number.	Died.	Recovered.	Per-centage of Mortality.	Right.		Left.	
					Number.	Per-centage of Mortality.	Number.	Per-centage of Mortality.
Cervical	96	69	27	71·87	67	68·65	29	79·31
Parotid	32	20	12	62·50	12	58·33	20	65·00
Sub-Maxillary	28	21	7	75·00	19	68·42	9	88·88
Axillary	402	298	104	74·12	240	72·50	162	76·54
Pectoral	9	6	3	66·66	5	60·00	4	75·00
Supra-Trochlear	7	4	3	57·14	4	50·00	3	66·66
Femoral	675	429	246	63·55	365	64·38	310	62·58
Inguinal	360	246	114	68·33	180	69·44	180	67·22
Iliac	11	9	2	81·81	4	75·00	7	85·71
Femoral + Inguinal	328	250	78	76·21	137	74·45	191	77·48
Femoral + Inguinal + Iliac	121	91	30	75·20	77	72·85	44	79·54
Femoral + Iliac	62	47	15	75·80	33	75·75	29	75·86
Inguinal + Iliac	73	57	16	78·08	40	77·50	33	78·78
Popliteal	8	4	4	50·00	4	50·00	4	50·00
Occipital	2	1	1	50·00	—	—	—	—
Double Cervical	20	19	1	95·00	—	—	—	—
Double Parotid	11	7	4	63·63	—	—	—	—
Double Axillary	5	4	1	80·00	—	—	—	—
Double Femoral	9	7	2	77·77	—	—	—	—
Double Inguinal	7	7	—	100·00	—	—	—	—
Double Iliac	2	1	1	50·50	—	—	—	—
Double Femoral + Double Iliac	4	1	3	25·00	—	—	—	—
Cervical + Parotid	28	22	6	78·57	10	80·00	18	77·77
Cervical + Parotid + Sub-Maxillary	6	4	2	66·66	—	—	—	—
Double Cervical + Parotid	5	5	—	100·00	—	—	—	—
Cervical + Sub-Maxillary	12	10	2	83·33	—	—	—	—
Cervical + Sub-Inguinal	4	4	—	100·00	—	—	—	—
Cervical + Parotid + Femoral	15	12	3	80·00	—	—	—	—
Double Parotid + Supra-Trochlear	2	1	1	50·00	—	—	—	—
Axillary + Supra-Trochlear + Brachial	16	10	6	62·50	8	50·00	8	75·00
Axillary + Femoral	11	6	5	54·54	—	—	—	—
Axillary + Femoral + Inguinal	24	13	11	54·16	—	—	—	—
Axillary + Parotid	8	7	1	87·50	—	—	—	—
Axillary + Parotid + Double Supra-Trochlear	4	3	1	75·00	—	—	—	—
Axillary + Inguinal	4	2	2	50·00	—	—	—	—
Double Femoral + Inguinal	12	7	5	58·33	—	—	—	—
Femoral + Parotid	7	5	2	71·42	—	—	—	—
Femoral + Iliac + Parotid	6	4	2	66·66	—	—	—	—
Femoral + Iliac + Cervical	13	8	5	61·53	—	—	—	—
Femoral + Inguinal + Cervical	4	2	2	50·00	—	—	—	—
Left Inguinal + Right Femoral	6	5	1	83·33	—	—	—	—
Femoral + Inguinal + Axillary + Supra-Trochlear	2	1	1	50·00	—	—	—	—
Femoral + Inguinal + Cervical + Axillary + Brachial	3	2	1	66·00	—	—	—	—
Inguinal + Cervical	1	—	1	00·00	—	—	—	—
Pneumonic Plague	110	105	5	95·45	—	—	—	—
Without Apparent Buboes	150	102	48	68·00	—	—	—	—
Total	2,723	1,937	786	71·13	—	—	—	—

D.

TABLE showing the SEXES and MORTALITY RATE of each SEX in the 4,000 PLAGUE PATIENTS treated at the ARTHUR ROAD HOSPITAL, BOMBAY, from 24th September, 1896 to 10th February, 1899.

Sex.	Admissions.	Deaths.	Recoveries.	Percentage of Mortality.
Males - -	2,924	2,106	818	72·02
Females - -	806	532	224	72·20
Children -	270	173	97	64·07
Total -	4,000	2,861	1,139	71·52

E.

TABLE showing the RACES of 4,000 PLAGUE PATIENTS treated at the ARTHUR ROAD HOSPITAL, BOMBAY, from 24th September, 1896 to 10th February, 1899.

Races.	Admissions.	Deaths.	Recoveries.	Percentage of Mortality.
Hindus -	3,372	2,468	904	73·19
Musalmans -	272	178	94	65·44
Native Christians.	315	194	121	61·58
Parsees - -	20	7	13	35·00
Beni-Israel -	21	14	7	66·66
Total -	4,000	2,861	1,139	71·52

F.

TABLE showing by SEXES the PERCENTAGE of each SEX and MORTALITY in 3,372 HINDU PATIENTS, treated at the ARTHUR ROAD HOSPITAL, BOMBAY, from 24th September, 1896 to 10th February, 1899.

Sex.	Admissions.	Deaths.	Recoveries.	Percentage of Mortality.
Males - -	2,446	1,814	632	74·16
Females - -	695	505	190	72·37
Children -	231	149	82	64·50
Total -	3,372	2,468	904	73·19

G.

TABLE showing by SEXES, the PERCENTAGE of each SEX and MORTALITY in 272 MUSALMAN PATIENTS treated at the ARTHUR ROAD HOSPITAL, BOMBAY, from 24th September, 1896 to 10th February, 1899.

Sex.	Admissions.	Deaths.	Recoveries.	Percentage of Mortality.
Males - -	229	145	84	63·31
Females - -	34	27	7	79·70
Children -	9	6	3	66·66
Total -	272	178	94	65·44

H.

TABLE showing the PERCENTAGE of each SEX and MORTALITY in 315 NATIVE CHRISTIAN PATIENTS treated at the ARTHUR ROAD HOSPITAL, BOMBAY, from 26th September 1896 to 10th February 1899.

Sex.	Admissions.	Deaths.	Recoveries.	Percentage of Mortality.
Males - -	224	135	89	60·26
Females -	69	45	24	65·21
Children -	22	14	8	63·63
Total -	315	194	121	61·58

I.

TABLE showing the PERCENTAGE of each SEX and MORTALITY in 20 PARSEE PATIENTS treated at the ARTHUR ROAD HOSPITAL, BOMBAY, from 24th September 1896 to 10th February 1899.

Sex.	Admissions.	Deaths.	Recoveries.	Percentage of Mortality.
Males - -	15	5	10	33·33
Females -	4	2	2	50·00
Children -	1	—	1	00·00
Total -	20	7	13	35·00

J.

TABLE showing the PERCENTAGE of each SEX and MORTALITY in 21 BENI-ISRAEL PATIENTS treated at the ARTHUR ROAD HOSPITAL, BOMBAY, from 24th September 1896 to 10th February 1899.

Sex.	Admissions.	Deaths.	Recoveries.	Percentage of Mortality.
Males - -	10	7	3	70·00
Females -	4	3	1	75·00
Children -	7	4	3	57·14
Total -	21	14	7	66·66

K.

TABLE showing the MORTALITY RATE according to AGE PERIODS in 4,000 PLAGUE PATIENTS treated at the ARTHUR ROAD HOSPITAL, BOMBAY, from 24th September 1896 to 10th February 1899.

Age Periods.	Admissions.	Deaths.	Recoveries.	Percentage of Mortality.
Under 1 year -	5	—	5	00·00
From 1 to 5 years	52	33	19	63·46
„ 5 „ 10 „	188	125	63	66·48
„ 10 „ 20 „	714	476	238	66·66
„ 20 „ 30 „	1,333	941	392	70·59
„ 30 „ 40 „	1,028	712	316	69·26
„ 40 „ 50 „	414	344	70	83·09
„ 50 „ 60 „	168	146	22	86·90
„ 60 „ 70 „	82	71	11	86·58
„ 70 „ 80 „	13	11	2	84·61
„ 80 „ 90 „	3	2	1	66·66
Total - -	4,000	2,861	1,139	71·52

L.

TABLE showing the MORTALITY RATE by OCCUPATION in 4,000 PLAGUE PATIENTS treated at the ARTHUR ROAD HOSPITAL, BOMBAY, from the 24th of September, 1886 to the 10th of February, 1899.

No.	Occupation.	Admissions.	Deaths.	Recoveries.	Percentage of Mortality.
1	Artist - -	1	—	1	00·00
2	Ayah (Nurse) -	28	21	7	75·00
3	Baker - -	7	3	4	42·85
4	Barber - -	25	20	5	80·00
5	Basket-maker -	3	2	1	66·66
6	Beedee (country cigarettes seller).	1	—	1	00 00
7	Beggar - -	220	167	53	75·90
8	Blacksmith -	13	10	3	76·92
9	Bookbinder - -	3	2	1	66·66
10	Butcher - -	6	5	1	83·33
11	Butler - -	45	33	12	73·33
12	Carpenter - -	32	24	8	75·00
13	Cart-driver - -	33	28	5	84·84
14	Clerk - -	16	10	6	62·50
15	Coachman - -	41	31	10	75·60
16	Compositor -	9	4	5	44·44
17	Compounder - -	1	—	1	00·00
18	Cook - -	111	76	35	68·46
19	Coolie - -	1,100	795	305	72·27
20	Coppersmith -	3	2	1	66·66
21	Cotton cleaner -	1	1	—	100·00
22	Cultivator - -	3	3	—	100·00
23	Dhobi (Washerman).	31	25	6	80·64
24	Dyer - -	3	3	—	100·00
25	Engineer - -	1	1	—	100·00
26	Fisherman - -	14	12	2	85·71
27	Fitter - -	10	4	6	40·00
28	Goldsmith - -	18	12	6	66·66
29	Goondlee (Musician, religious).	3	3	—	100·00
30	Gawlee (Milkman)	19	17	2	89·47
31	Hawker - -	22	14	8	63·63
32	Hospital Assistant -	1	—	1	00·00
33	Lascar (Arsenal) -	1	—	1	00·00
34	Lascar (Fire Brigade)	1	—	1	00·00
35	Malee (Gardener) -	19	15	4	78·94
36	Mason - -	5	2	3	40·00
37	Medical Practitioner	1	1	—	100·00
38	Mill-hand - -	288	211	77	77·08
39	Painter - -	9	5	4	55·55
40	Peon - -	57	42	15	73·68
41	Pleader - -	1	1	—	100·00
42	Policeman - -	30	21	9	70·00
43	Polisher - -	1	1	—	100·00
44	Potter - -	1	—	1	100·00
45	Printer - -	11	8	3	72·72
46	Prisoner - -	17	9	8	52·94
47	Private (Native Infantry).	3	2	1	66·66
48	Railway servant -	7	4	3	57·14
49	Sailor - -	12	5	7	41·66
50	Servant (domestic)	380	274	106	72·10
51	Shoemaker -	40	32	8	80·00
52	Shopkeeper - -	40	26	14	65·00
53	Silk-cleaner -	1	1	—	100·00
54	Student - -	21	10	11	47·61
55	Sweeper - -	18	10	8	55·55
56	Syce (Groom) -	51	43	8	84·31
57	Tailor - -	48	36	12	75·00
58	Teacher - -	3	3	—	100·00
59	Teli (Oilman) -	5	4	1	80·00
60	Tin-maker -	1	1	—	100·00
61	Warder - -	1	1	—	100·00
62	Ward boy (Hospital).	6	2	4	33·33
63	Weaver - -	10	5	5	50·00
64	Wool-cleaner -	3	—	3	00·00
65	No occupation -	1,081	756	325	69·93

APPENDIX No. LXII.

REPORT
BY
KHAN BAHADUR N. H. CHOKSY
ON
CASES OF PLAGUE TREATED WITH PROFESSOR LUSTIG'S SERUM
AT THE
ARTHUR ROAD HOSPITAL, BOMBAY.

Arthur Road Hospital, Bombay,
3rd May 1899.

To the Municipal Commissioners, through the Special Medical Officer and the Executive Health Officer.

SIR,

I HAVE the honour to submit herewith the following report and return of cases treated with Professor Lustig's serum during April 1899:—

1. The number of cases treated was 39, of whom 21 died and 18 recovered, giving a mortality rate of 53·84 per cent. The mortality rate of cases not similarly treated was 77·51 per cent., thus showing a difference of 23·67 per cent. in favour of the former.

2. The cases were divided into the following series:—

Series.	Horse No.	Bleeding.	No. Treated.	No. Died.	No. Recovered.
VIII. (completed).	5	Second	16	10	6
IX.	Donkey	First	2	1	1
X.	3	Second	14	7	7
XI. (to be continued).	1	„	7	3	4
Total -	- -	- -	39	21	18

3. From a comparison of the returns for February, March, and April, it appears that each succeeding month the serum shows better results as it becomes stronger and more active. And considering that the present epidemic did not show any marked decline till about the middle of April, and that the individual cases do not even yet show any marked diminution in their virulence, in spite of the fact that the epidemic is steadily declining, the results so far achieved should be considered satisfactory. This is better illustrated by the following table, which compares the results during the period:—

Months, 1899.	Mortality Rate of Cases not treated with the Serum.	Mortality Rate of Cases treated with the Serum.	Difference in favour of Serum Cases.
February - -	79·16	70·58	8·58
March - -	79·48	66·21	13·27
April - -	77·51	53·84	23·67

4. The above table shows that whereas the mortality rate has varied very little amongst the cases not treated with the serum, and is not higher than the average rate of all the other hospitals, it shows a very appreciable improvement in those treated with it. And the improvement has steadily bettered from 8·58 per cent. to 23·67 per cent., a difference of over 15 per cent.

5. When all the series of cases are compared, two points are clearly brought out, viz.:—(1.) That the serum obtained from the second bleedings is more active than from the first, and (2) that the serum from the different horses shows different degrees of activity:—

Series.	Horse No.	Bleeding.	No. Treated.	No. Died.	No. Recovered.
I.	5	First	18	14	4
II.	2	„	11	8	3
III.	3	„	17	10	7
IV.	2	„	13	11	2
V.	1	„	17	12	5
VI.	4	„	47	30	17
VII.	6	„	13	8	5
VIII.	5	Second	22	14	8
IX.	Donkey	First	2	1	1
X.	3	Second	14	7	7
XI.	1	„	7	3	4
Total -	- -	- -	181	118	63

The above statement indicates that the results from the first and second bleedings were 68·38 per cent. and 55·81 per cent. respectively, thus clearly indicating a difference in greater activity of 12·57 per cent. from the latter. And as the serum gets stronger at each subsequent bleeding, it is to be expected that better results would be shown in the near future. The serum supplied from Florence last year also showed similar variations between the first and second bleedings.

The table demonstrates that different horses give serums of different activities, and this also materially corroborates the experience of last year, when it was observed that the different horses under experiment at Florence gave serum of different curative values.

6. Including the observations made last year, the total number of cases treated with Professor Lustig's serum now amounts to 349, and the following deductions have been drawn from the experience thus acquired:—

1. That the only known method of treatment that holds out any hope of reducing the high mortality from plague is that by Professor Lustig's curative serum.
2. That where the serum does not avert death, it considerably ameliorates the symptoms, and prolongs the life of the patient.
3. That the serum has, and cannot but have, a limited value in the treatment of the class of patients admitted into the Arthur Road Hospital; patients drawn from the lowest strata of society, badly housed, badly nourished, or half starved, the homeless, the friendless, and the vagrant, picked up from the streets and bye-lanes of the city, and brought to the hospital in a moribund or semi-moribund condition, and after the vital organs have become thoroughly disintegrated by saturation with the plague poison.
4. That if the mortality rate in such patients could be reduced by even 15 per cent., the results should be considered extremely satisfactory.
5. That the serum treatment has long since passed the experimental stage, and that its use should be freely encouraged, so as to collect data on the largest scale possible.
6. That with a view to test the full efficacy and value of the serum, it should be used extensively in early cases, and also in those that are allowed to be treated in their houses by private practitioners.
7. That the best method of using the serum in hospital practice is to exclude all moribund and all convalescent and semi-convalescent patients, and to treat the rest.
8. That if the method above indicated were not followed, it would lead to much unnecessary waste of serum, which could be better applied in more suitable cases.

9. That the statistical method is not the only method of judging of the efficacy of the serum, as it takes no cognisance of the effects in individual cases, and that clinical observation should not be totally ignored as it is of as much, if not more, value in determining its effects.
10. That if any statistical method has to be relied upon for purposes of comparison, it should be between the cases treated with and without serum after the moribund, the convalescent, and semi-convalescent have been excluded from each class.
11. That this method, even, is not free from objections, inasmuch as it takes no account of the individual differences in castes and races, sex, age, location of buboes, duration of illness at the time of treatment, the general condition of the patients, &c., &c., factors which considerably influence the mortality rate in plague, and which must, to a great extent, be similar, if not actually identical, in the two classes of cases taken for comparison.
12. That it is not generally possible to obtain such exact control cases, even if the admissions into the hospitals be very large, and that all deductions drawn from comparison of control cases that do not fulfil the above requirements are open to doubt and are, proportionately, of less relative value.
13. That in the absence of such exact control cases, the method suggested in paragraph 10 is the only feasible and practicable, and open to least errors, especially if a large number of observations are made, the larger the better.
14. That the treatment of every alternate case, as has been suggested, is liable to even greater objection than the method suggested in paragraph 10, as it would entail the waste of serum in moribund cases, cases that nothing short of a miracle could revive, as also its further waste upon convalescent and semi-convalescent patients who are already on the high road to recovery, and who require no further active treatment by drugs or by serum to complete their cure.

N. H. Choksy,
Medical Officer in Charge,
Arthur Road Hospital.

The following papers, viz., a Report by Captain Childe, I.M.S., on the treatment of plague cases in Bombay by Galeotti's serum, with lists of cases treated in the Mahratta Hospital, Bombay, and, from the 29th of January 1899 to the 27th March and in May 1899, in the Arthur Road Hospital, Bombay, have been forwarded to the Indian Plague Commission by the Government of Bombay.

REPORT

ON THE

TREATMENT OF PLAGUE IN BOMBAY BY Dr. GALEOTTI'S CURATIVE SERUM,

BY

Captain L. F. CHILDE, I.M.S.

Observations.—The total number of cases treated by Dr. Galeotti's serum, from February 2nd to March 27th, has been 148; of which 125 were treated at the Arthur Road Hospital, 12 were treated at the Marátha Hospital, and 11 were treated at their own houses.

By sex, these 148 persons consisted of 118 males and 30 females (11 being children); and

By caste, there were 117 Hindus, 18 Native Christians, 2 Pársis, and 4 Máhomedans.

With regard to duration of disease, it is found that—

30	were treated on the	1st	day of disease.
49	do.	2nd	do.
43	do.	3rd	do.
20	do.	4th	do.
4	do.	5th	do.
1	was do.	6th	do.
1	do.	7th	do
148			

so that 122 out of 148 patients were receiving the serum treatment on or before the 3rd day of plague.

Mortality.—Of the total 148 cases, 99 died, 43 recovered, and 6 are still under treatment.

Out of the 113 adult males, 78 died, 29 recovered, and 6 are under treatment.

Of the 24 adult females, 14 died, 10 recovered.

Of the 11 children, 7 died, 4 recovered.

Mortality by Caste.—Of the 117 Hindus, 80 died, 31 recovered, and 6 are under treatment.

Of the 18 Native Christians, 12 died, 6 recovered.

Of the 9 Pársis, 4 died, 5 recovered.

Of the 4 Máhomedans, 3 died, 1 recovered.

Percentage Mortality.—Of all the cases, the percentage of deaths to recoveries is 66·9, but if the 6 cases under treatment be excluded, the percentage mortality is 69·7.

By Sex.—Of the 113 adult males, the mortality is 69·1 per cent., but excluding the 6 cases under treatment, it amounts to 72·9 per cent.

Of the 24 adult females, the mortality is 58·3 per cent.

Of the 11 children, the mortality is 63·6 per cent.

By caste, the mortality is as follows:—

Hindus	68·4	per cent.
Máhomedans	75	do.
Native Christians	66·7	do.
Pársis	44·4	do.

1. *General Remarks.*—Selection of cases for treatment. In this enquiry an endeavour was made to confine the treatment to cases of average severity, that is to those who, on admission, seemed to have a reasonable chance of recovery; so that two classes of patients did not receive the serum, namely, those who were obviously moribund, and those whose symptoms were mild, and who were likely to recover, irrespective of treatment. It must be admitted that it is difficult to make a prognosis in any case of plague, as the most unexpected results occur; still, as far as could be judged from the symptoms on admission to hospital, the above selection of cases was made.

2. *Treatment in Addition to the Serum.*—In all cases at the Arthur Road Hospital a stimulant (rum) was given every two hours; and in cases showing symptoms of heart failure cardiac tonics were also administered, such as digitalis, strychnine, strophanthus, caffeine.

3. *Causes of Death.*—These were sudden heart failure, secondary pneumonia, bronchitis, œdema of the lungs, hæmorrhage, or exhaustion; in fact, death was due to the same causes as in plague patients not treated by the serum.

4. *Effects of the Serum Treatment.*—In many cases the temperature fell 1 or 2 degrees shortly after the injection of the serum, and with this fall there was a corresponding improvement in the patient's general condition; but this improvement was often only temporary, and would be observed after each subsequent injection; still, in those patients who died, the general course of the disease was steadily from bad to worse, and their symptoms were practically identical with those of plague patients treated in other ways.

5. *General Conclusions.*—From the evidence of this enquiry it cannot be said that Dr. Galeotti's serum treatment of plague shows any marked curative results; for of the 142 completed cases the mortality is 69·7 per cent., and the general death-rate of plague patients treated in a hospital is only a little higher. I am informed that this rate is 80 per cent., but have no means of verifying the statement; still, accepting this figure as correct, the most that can be claimed for Dr. Galeotti's treatment is a 10 per cent. reduction in mortality.

6. *Evidence of Early Cases.*—Of the 148 cases, 30 received the serum treatment on the first day of disease; and of these 30, 22 died and 8 recovered, so that in this series the death-rate was 73·3 per cent. Now it is an axiom in serum therapeutics that the earlier the treatment, the more favourable the prognosis; yet in this instance we find that the mortality of the patients treated on the first day is higher than that of all the patients who underwent the treatment; the respective figures being 73·3 and 69·7 per cent. This fact points to the same conclusion, namely, that treatment by Dr. Galeotti's serum has not much influence on the course of plague.

L. F. Childe, M.B., Captain, I.M.S.,
Acting First Physician, Sir J. J. Hospital.
Bombay, 4th April 1899.

STATEMENT OF WORK

DONE BY

Dr. GALEOTTI

WITH NEW SERUM

AT THE

MAHRATTA PLAGUE HOSPITAL, BOMBAY,

UP TO 28TH FEBRUARY 1899.

STATEMENT of WORK done by Dr. GALEOTTI with new SERUM at the MAHRATT

(Prepared by Dr. Dhargalker, Chief Medic

No.	Register No.	Name.	Sex.	Age.	Occupation.	Residence.	Date of Admission.	Date of Disease.
				Years.			1899.	
1	714	Narbada Luxuman -	Female	18	Mill hand	- - - - -	2nd February	1st February
2	697	Maroti Gopal - -	Male	20	Do. -	Dongri, Murári Sali's House.	2nd „	1st „
3	720	Sakoo Malji - -	Female	35	Cooly -	Segregation Camp, Mahratta Hospital.	3rd „	2nd „
4	753	Bhiku Ramji - -	Male	6	Nil -	Wari Bundar Segregation Camp.	3rd „	2nd „
5	731	Uma Mahadu - -	Female	15	Do. -	No. 2, Gilder Street -	3rd „	2nd „
6	767	Dugdoo Honaji - -	Male	30	Cooly -	127, Nowroji Hill, North.	4th „	3rd „
7	776	Sai Tukarm - -	Female	15	Do. -	Appa Telli's Chawl, House No. 9.	4th „	3rd „
8	797	Kalapa Basapa - -	Male	20	Do. -	84, Bhandari Street -	5th „	4th „
9	838	Babaji Narayan -	Do.	30	Mill hand	Thoba Wani's Chawl -	7th „	6th „
10	842	Narayan Bhanu - -	Do.	25	Cooly -	Kharwachi Wadi, Fire Brigade, Girgaum.	7th „	6th „
11	848	Ganu Kalu - -	Do.	30	Do. -	2nd Nagpada, Huzari Muhalla.	7th „	6th „
12*	—	Radha Hari - -	Female	20	—	—	11th March -	9th March -

* This statement corresponds to that referred to in Captain Childe's remarks, except that cas

GUE HOSPITAL, BOMBAY, up to 28th February 1899.

cer, Mahratta Plague Hospital.)

o.	Date of Serum injected.	Amount of Serum injected.										Remarks.
		Morning.	Evening.	Morning.	Evening.	Morning.	Evening.	Morning.	Evening.	Morning.	Evening.	
		1st Day.		2nd Day.		3rd Day.		4th Day.		5th Day.		
1	3rd February	18	15	15	15	19	19	19	—	—	—	Died 8.50 p.m., 6th February 1899.
2	2nd „	15	15	10	—	—	—	—	—	—	—	Recovered.
3	3rd „	20	—	10	—	10	—	—	—	—	—	Died 6 p.m., 6th February 1899. Vapour bath given.
4	3rd „	5	12	10	—	—	—	—	—	—	—	Died 4.30 p.m., 4th February 1899.
5	3rd „	—	10	10	15	10	—	—	—	—	—	Recovered.
6	4th „	19	19	18	19	—	—	—	—	—	—	Died 7.50 p.m., 5th February 1899.
7	6th „	15	—	—	—	—	—	—	—	—	—	Died 5 a.m., 7th February 1899. Vapour bath given.
8	5th „	15	19	19	—	19	—	—	—	—	—	Recovered.
9	7th „	10	19	—	19	—	19	—	—	—	—	Died 12.30 p.m., 11th February 1899.
10	7th „	—	19	19	—	19	15	—	10	—	—	Died 7 a.m., 11th February 1899.
11	7th „	—	15	—	19	—	—	—	—	—	—	Died 10 a.m., 10th February 1899.
12*	—	15	15	16	—	—	—	—	—	—	—	Died 11.30 a.m., 12th March 1899. Heart failure.

12, Radha Hari, appears only in Captain Childe's statement, and not in that of Dr. Dhargalker.

STATEMENT of PLAGUE CASES treated by Dr. GALEOTTI

No.	Name.	Date of Admission.	Age.	Sex.	Caste.	Day of Disease.	Date of Serum injected.
	Series.		Years.				
12/1	Girza Babu - 1st	29th January 1899 -	10	Child -	Hindu -	2 days -	29th January 1899 -
14/2	J. J. De Silva - do.	28th do. -	61	Male -	Christian	4 „ -	30th do. -
15/3	Unknown - - do.	31st do. -	42	Do. -	Hindu -	1 day -	31st do. -
16/4	Rama Raghu - do.	1st February 1899 -	20	Do. -	Do. -	4 days -	1st February 1899 -
17/5	Bhambu Raju - do.	1st do. -	20	Do. -	Do. -	4 „ -	1st do. -
18/6	Mahadu Ithu - do.	31st January 1899 -	10	Child -	Do. -	4 „ -	1st do. -
19/7	Shiva Issu - do.	1st February 1889 -	40	Male -	Do. -	2 „ -	1st do. -
20/8	Ganesh Khedu - do.	1st do. -	38	Do. -	Do. -	2 „ -	1st do. -
21/9	Biranji Ganesh - do.	1st do. -	25	Female	Do. -	3 „ -	1st do. -
22/10	Deuba Gainu - do.	2nd do. -	18	Male -	Do. -	3 „ -	2nd do. -
23/11	Esu Kondaji - do.	1st do. -	30	Do. -	Do. -	5 „ -	2nd do. -
24/12	Gannu Gosai - do.	2nd do. -	29	Do. -	Do. -	3 „ -	2nd do. -
25/13	Pandu Gopal - do.	2nd do. -	15	Do. -	Do. -	1 day -	2nd do. -
26/14	Shiva Hari - do.	4th do. -	30	Do. -	Do. -	4 days -	4th do. -
27/15	Abba Bala - do.	4th do. -	32	Do. -	Do. -	7 „ -	4th do. -
28/16	Govind Hiru - do.	6th do. -	25	Do. -	Do. -	3 „ -	6th do. -
29/1	Maruthi Jambu - 2nd	3rd do. -	25	Do. -	Do. -	6 „ -	3rd do. -
30/2	Jani Bhawu - do.	2nd do. -	20	Female	Do. -	3 „ -	3rd do. -
31/17	Ramchandra Narayan 1st	8th do. -	22	Male -	Do. -	4 „ -	8th do. -
32/3	J. A. Fernandes - 2nd	3rd do. -	18	Female	Christian	2 „ -	3rd do. -
33/4	Diva Lemba - do.	3rd do. -	28	Male -	Hindu -	4 „ -	3rd do. -
34/5	Marooti Santu - do.	3rd do. -	35	Do. -	Do. -	4 „ -	3rd do. -
35/6	Sambu Ganu - 2nd	4th do. -	2½	Child -	Hindu -	3 days -	4th do. -
36/7	Jumnabai Dulal - do.	4th do. -	6	Do. -	Do. -	2 „ -	4th do. -
37/8	Tribhowan Govind. do.	3rd do. -	18	Male -	Do. -	2 „ -	3rd do. -
38/9	Soma Nathu - do.	5th do. -	15	Do. -	Do. -	1 day -	5th do. -
39/10	Shivram Bhau - do.	5th do. -	12	Do. -	Do. -	2 days -	5th do. -
40/1	Dagru Janu - 3rd	7th do. -	10	Child -	Do. -	1 day -	7th do. -
41/2	Dhondu Bhairu - do.	7th do. -	30	Male -	Do. -	3 days -	7th do. -
42/3	Jai Tukaram - do.	7th do. -	50	Female	Do. -	4 „ -	7th do. -
43/11	Salvador Noronha - 2nd	5th do. -	40	Male -	Christian	3 „ -	5th do. -

NOTE.—From No. $\frac{13}{1}$ to $\frac{148}{13}$ he denominators represent the number of each case in the series of serum injections to which it Bombay, by Dr. Choksy on the 18th March, 1899, in so far as the same cases are included in both, for Dr. Choksy's tables In the cases shown in Dr. Choksy's tables the position of buboes and notes made by Dr. Choksy have been added to the table in this table.

t at the Arthur Road Hospital, Bombay.

Amount of Serum injected.												Result.	Remarks.
1st Day.		2nd Day.		3rd Day.		4th Day.		5th Day.		6th Day.			
Morning.	Evening.	Morning.	Evening.	Morning.	Evening.	Morning.	Evening.	Morning.	Evening.	Morning.	Evening.		
c.c.	c.c.	c.c.	c.c.	c.c.	c.c.	c.c.	c.c.	c.c.	c.c.	c.c.	c.c.		
—	18	—	10	—	—	—	—	—	—	—	—	Died 3.15 p.m., 31st January, 1899.	Temperature 106·4°, death from gradual heart failure; right cervical bubo.
20	—	—	—	—	—	—	—	—	—	—	—	Died 2.15 a.m., 31st January, 1899.	Injected on the 6th day; died 4 hours after injection; left inguinal and right femoral buboes.
—	15	—	15	—	15	—	—	—	—	—	—	Died 12 o'clock, 1st February, 1899.	Moribund; life kept up by stimulant injection; right inguinal bubo.
15	15	10	10	10	10	—	—	10	20	15	20	Recovered - -	Septic type; recovery without suppuration of bubo; right inguinal.
15	—	15	—	—	—	—	—	—	—	—	—	Died 10 p.m., 1st February, 1899.	Left axillary bubo; moribund; died 12 hours after admission.
10	10	5	5	5	10	10	10	5	5	5	5	Recovered - -	Septic type, profuse malaria, bubo left axillary, suppurated, good recovery, intestinal hæmorrhage.
—	15	15	15	15	15	10	15	—	15	15	15	Do. - - -	Bubo left axillary with infiltration; septic type; recovery with suppuration.
—	15	10	15	15	15	—	—	—	—	—	—	Died 7 a.m., 4th February, 1899.	Right axillary bubo; improvement and then relapse.
—	15	—	10	15	15	20	20	—	—	—	—	Died 6.50 p.m., 4th February, 1899.	Meningeal symptoms hæmatimesis; left inguinal bubo.
15	—	15	10	—	—	—	—	—	—	—	—	Died 9 p.m., 3rd February, 1899.	Multiple buboes; right axillary, right cervical infiltration.
—	15	—	—	—	—	—	—	—	—	—	—	Died 7 p.m., 3rd February, 1899.	Injected on the 2nd, died 2 hours after injection; right axillary and cervical buboes.
—	15	—	—	—	—	—	—	—	—	—	—	Died 12.15 p.m., 3rd February, 1899.	Left femoral, inguinal and iliac buboes; moribund; died 6 hours after injection.
—	15	18	10	15	15	—	—	—	—	—	—	Died 8 a.m., 5th February, 1899.	Semi-conscious on admission; right femoral inguinal and iliac buboes.
—	15	15	15	15	15	—	—	—	—	—	—	Died 9.15 p.m., 6th February, 1899.	Bubo, left inguinal; death from failure of heart.
—	20	15	20	20	20	15	15	—	—	—	—	Died 6.30 p.m., 12th February, 1899.	Buboes, left inguinal, femoral and iliac, right; lived for 8 days, death from œdema of lungs.
—	20	20	20	20	20	—	—	—	—	—	—	Died 9 p.m., 8th February, 1899.	Heart very feeble, very delirious, life prolonged by stimulant injections; right inguinal and iliac buboes.
10	10	—	—	—	—	—	—	—	—	—	—	Died 10 p.m., 6th February, 1899.	Tetanic injection stopped; right femoral bubo.
15	—	—	—	—	—	—	—	—	—	—	—	Died 12.50 noon, 3rd February, 1899.	Moribund; died 2 hours after injection; right femoral bubo.
—	20	20	20	20	15	15	15	15	—	—	—	Died 21st February, 1899.	Death from œdema of lung; right femoral bubo.
15	15	15	15	10	10	10	10	—	—	—	—	Recovered - -	Right axillary bubo; good recovery without suppuration of bubo.
—	15	15	15	—	—	—	—	—	—	—	—	Died 10.15 a.m., 5th February, 1899.	Unconscious; left femoral and iliac buboes; temperature 105°–106°; heart failure.
—	15	10	10	15	20	20	20	—	—	—	—	Died 10 a.m., 7th February, 1899.	Almost unconscious; left inguinal and axillary buboes; life prolonged by stimulant injections.
5	5	—	—	—	—	—	—	—	—	—	—	Died 6.45 a.m., 5th February, 1899.	Sudden failure of heart; right femoral bubo.
10	—	10	10	—	—	—	—	—	—	—	—	Died 8.30 p.m., 6th February, 1899.	Convulsions and œdema of lungs; right axillary bubo.
—	15	15	15	10	15	10	10	10	10	—	—	Recovered - -	Right femoral bubo; septic type; good recovery; bubo suppurated.
20	20	20	20	15	15	—	20	10	10	10	—	Do. - -	Right inguinal bubo; septic type; good recovery without suppuration.
15	15	15	—	—	—	—	—	—	—	—	—	Died 3.55, 7th February, 1899.	Sudden failure of the heart; left femoral and inguinal bubo.
—	20	20	20	—	—	—	—	—	—	—	—	Died 3 a.m., 8th February, 1899.	Heart failure; left femoral bubo.
—	20	20	15	15	20	20	15	—	—	—	—	Died 9.30 p.m., 17th February, 1899.	Died of exhaustion; right inguinal bubo.
—	20	20	15	—	—	—	—	—	—	—	—	Died 7.30 p.m., 6th February, 1899.	Right femoral and iliac, right axillary buboes.
—	20	20	20	20	20	10	—	10	10	10	—	Died, 23rd February, 1899.	Left femoral inguinal and iliac buboes; died of exhaustion from sloughing of iliac bubo.

elongs. This table, attached to Captain Childe's report, corresponds with tables submitted to the Municipal Commissioner, comprise only Series 1 2 3, 4, cases Nos. 1 to 6, 16 and 17 in Series 5, and Case No. 30 in Series 6 in Captain Childe's tables. published. References have been given to the Charts published in App. No. LXIII, where those Charts relate to cases entered

STATEMENT of PLAGUE CASES treated by DR. GALEOTTI'S

No.	Name.	Date of Admission.	Age.	Sex.	Caste.	Day of Disease.	Date of Serum injected.
	Series.		Years.				
44	Balu Laku - - 3rd.	8th February 1899 -	18	Male -	Hindu -	1 day -	8th February 1899 -
4							
45	Piri Bapuji - do.	7th do. -	20	Female	Do. -	2 days -	7th do. -
5							
46	Bhawansing Raising. do.	8th do. -	12	Male -	Do. -	2 „ -	9th do. -
6							
47	Dhondu Raghu - do.	8th do. -	19	Do. -	Do. -	4 „ -	9th do. -
7							
48	Kondi Babull - do.	10th do. -	12	Female	Do. -	1 day -	11th do. -
8							
49	Shamji Pitamber - do.	13th do. -	40	Male -	Do. -	3 days -	13th do. -
9							
50	R. M. De Silva - do.	9th do. -	22	Female	Christian	2 „ -	11th do. -
10							
51	Jiji Khandu - do.	14th do. -	12	Do. -	Hindu -	3 „ -	15th do. -
11							
52	Dhondu Sukaram - do.	13th do. -	25	Male -	Do. -	3 „ -	13th do. -
12							
53	Mr. M. (Private) - do.	9th do. -	25	Do. -	Parsi -	2 „ -	10th do. -
13							
54	Chemi Bapu do. - do.	13th do. -	40	Female	Hindu -	1 day -	13th do. -
14							
55	Mrs. S. P. do. - 1st	30th January 1899 -	16	Do. -	Parsi -	2 days -	13th January 1899 -
18							
56	Mrs. K. do. - 3rd	11th February 1899 -	46	Do. -	Do. -	4 „ -	11th February 1899 -
15							
57	Miss K. do. - do.	11th do. -	16	Do. -	Do. -	1 day -	11th do. -
16							
58	Joda Harsayal - 4th	21st do. -	40	Male -	Hindu -	4 days -	21st do. -
1							
59	Jiwa Kalu - - do.	21st do. -	18	Do.	Do. -	2 „ -	21st do. -
2							
60	Parbu Narsu - - do.	21st do. -	30	Do. -	Do. -	3 „ -	21st do. -
3							
61	Hiralal Sah - - do.	21st do. -	35	Do. -	Do. -	3 „ -	21st do. -
4							
62	Nama Rama - do.	24th do. -	35	Do. -	Do. -	2 „ -	24th do. -
5							
63	Ganu Chandria - do.	23rd do. -	22	Do. -	Do. -	2 „ -	24th do. -
6							
64	Gangaram Bhoda - do.	24th do. -	50	Do. -	Do. -	2 „ -	24th do. -
7							
65	Shanker Suka - - do.	24th do. -	5	Child -	Do. -	2 „ -	24th do. -
8							
66	Raghu Krishna - do.	24th do. -	32	Male -	Do. -	3 „ -	24th do. -
9							
67	Sukaram Bapu - do.	25th do. -	18	Do. -	Do. -	2 „ -	25th do. -
10							
68	Dhondu Vithu - do.	26th do. -	12	Do. -	Do. -	3 „ -	26th do. -
11							
69	Ganput Yessu - - do.	25th do. -	22	Do. -	Do. -	2 „ -	26th do. -
12							
70	Babukhan Husain 5th	24th do -	18	Do. -	Musalman	2 „ -	23rd do. -
1							
71	Rama Dagru - do.	26th do. -	25	Do. -	Hindu -	2 „ -	26th do. -
2							
72	Bhau Babaji - do.	26th do. -	19	Do. -	Do. -	2 „ -	26th do. -
3							
73	Reva Begla - - do.	26th do. -	30	Do. -	Do. -	2 „ -	26th do. -
4							
74	Mahadu Tanu - do.	27th do. -	27	Do. -	Do. -	3 „ -	27th do. -
5							
75	Yedu Rama - do.	26th do. -	6	Child -	Do. -	3 „ -	28th do. -
6							
76	Sawalia Dhondi - do.	2nd March, 1899 -	12	Male -	Do. -	5 „ -	2nd March, 1899 -
7							
77	Kondiba Maroti - do.	5th do. -	20	Female	Do. -	2 „ -	5th do. -
8							
78	Walji Tricum do.	5th do. -	10	Child -	Do. -	3 „ -	5th do. -
9							
79	Tricum Purshotam do.	5th do. -	30	Male -	Do. -	3 „ -	5th do. -
10							
80	Harjiwan Tricum - do.	5th do. -	13	Do.	Do. -	3 „ -	5th do. -
11							
81	Abba Bayaji - do.	1st do. -	30	Do.	Do. -	2 „ -	1st do. -
12							

at the ARTHUR ROAD HOSPITAL, BOMBAY—*continued.*

Amount of Serum injected.												Result.	Remarks.
1st Day.		2nd Day.		3rd Day.		4th Day.		5th Day.		6th Day.			
Morning.	Evening.	Morning.	Evening.	Morning.	Evening.	Morning.	Evening.	Morning.	Evening.	Morning.	Evening.		
c.c.	c.c.	c.c.	c.c.	c.c.	c.c.	c.c.	c.c.	c.c.	c.c.	c.c.	c.c.		
20	—	—	—	—	—	—	—	—	—	—	—	Died 6.15 p.m., 8th February, 1899.	Moribund; right axillary bubo; died 8 hours after injection; gradual heart failure.
—	20	20	15	—	15	15	15	15	15	10	—	Recovered - -	Right axillary bubo, resolved.
—	—	15	—	10	10	10	10	—	—	—	—	Do. - -	Left femoral bubo; septic type; good recovery without suppuration.
—	—	20	—	15	20	15	20	15	10	10	10	Do. - -	Left femoral and iliac bubo; septic type; good recovery with suppuration.
20	20	20	15	20	20	—	—	—	—	—	—	Died 7.20 a.m., 14th February, 1899.	Right axillary bubo.
20	—	—	—	—	—	—	—	—	—	—	—	Died 5 p.m., 13th February, 1899.	Sudden failure of heart; right femoral bubo, infiltration.
20	—	—	—	—	—	—	—	—	—	—	—	Recovered - -	Right posterior, cervical and left femoral buboes, resolved.
20	20	15	20	7	—	—	—	—	—	—	—	Do. - -	Left axillary bubo, with infiltration.
—	20	20	20	10	—	15	20	20	—	—	—	Died 1 a.m., 18th February, 1899.	From œdema of lungs; right femoral and inguinal bubo.
20	20	—	—	—	—	—	—	—	—	—	—	Recovered - -	Left axillary bubo, suppurated.
—	20	20	20	10	—	—	—	—	—	—	—	Died 5.30 p.m., 15th February, 1899.	Left femoral bubo.
15	15	15	15	15	15	10	5	5	—	—	—	Recovered - -	Bubo right femoral and iliac; had a relapse with iliac bubo on left side.
20	20	—	—	—	—	—	—	—	—	—	—	Died 2 a.m., 12th February, 1899.	Mitral regurgitation and diabetes; right femoral and inguinal bubo.
20	18	28	—	—	—	—	—	—	—	—	—	Recovered - -	Left femoral and inguinal bubo.
—	20	—	—	—	—	—	—	—	—	—	—	Died 9 p.m., 21st February, 1899.	Moribund; died 3 hours after injection; left femoral inguinal and iliac bubo.
20	—	—	—	—	—	—	—	—	—	—	—	Died 4 a.m., 22nd February, 1899.	Heart failure; right axillary bubo with infiltration.
—	20	20	20	—	—	—	—	—	—	—	—	Died 7.55 p.m., 22nd February, 1899.	Death from secondary pneumonia; left axillary bubo.
—	20	20	20	20	—	20	20	—	—	—	—	Died 4 p.m., 3rd March, 1899.	Death from secondary pneumonia; left femoral bubo.
20	20	20	20	20	20	—	—	—	—	—	—	Died 6 p.m., 27th February, 1899.	Right inguinal bubo.
30	30	20	20	20	20	—	—	—	—	—	—	Died 10 a.m., 27th February, 1899.	Left axillary bubo, with infiltration.
20	20	20	20	20	20	—	—	—	—	—	—	Died 7.15, 28th February, 1899.	Right femoral and inguinal bubo.
10	10	5	5	5	—	—	—	—	—	—	—	Recovered.	Left femoral bubo; recovered without suppuration.
—	20	20	20	20	—	—	—	—	—	—	—	Died 12.30, 26th February, 1899.	Right post cervical bubo.
20	20	20	—	—	—	—	—	—	—	—	—	Do.	Left axillary, left post cervical buboes.
15	—	—	—	—	—	—	—	—	—	—	—	Died 3.30 p.m., 26th February, 1899.	Moribund; left axillary bubo; died 5 hours after injection.
20	—	—	—	—	—	—	—	—	—	—	—	Died 5.30 p.m., 26th February, 1899.	Right, inguinal bubo; died 7 hours after injection.
—	15 at his house	20	20	15	20	15	15	—	—	—	—	Died 9.15 a.m., 2nd March, 1899.	Death from secondary pneumonia; septic type; left inguinal bubo.
—	20	20	10	—	—	—	—	—	—	—	—	Died 8.30 p.m., 27th February, 1899.	Heart failure; eptic type; left axillary bubo.
—	20	20	20	15	10	—	20	15	—	—	—	Died 3.45 a.m., 3rd March, 1899.	Septic type; heart failure; right femoral and iliac bubo.
—	20	20	15	10	15	—	—	—	—	—	—	Died 8 p.m., 28th February, 1899.	Died from œdema of lung; left femoral and inguinal bubo.
—	30	30	30	20	20	15	—	—	—	—	—	Recovered - -	Right posterior bubo, with infiltration, neck and chest—suppuration.
10	10	5	—	—	—	—	—	—	—	—	—	Do. - -	Right femoral bubo.
15	15	10	—	—	—	—	—	—	—	—	—	Do. - -	A mixed case of plague and remittent fever.
20	—	—	—	—	—	—	—	—	—	—	—	Died 7 p.m., 7th March, 1899.	Heart failure.
10	10	5	—	—	—	—	—	—	—	—	—	Died 3.40 a.m., 7th March, 1899.	Do.
20	20	10	—	—	—	—	—	—	—	—	—	Died 16th March, 1899.	Marasma and exhaustion.
20	15	10	—	—	—	—	—	—	—	—	—	Died 10.45 p.m., 6th March 1829.	Heart failure.
—	30	30	30	20	—	20	—	—	—	—	—	Died 8th March 1899.	Secondary pneumonia

STATEMENT of PLAGUE CASES treated by DR. GALEOTTI

No.	Name.	Date of Admission.	Age.	Sex.	Caste.	Day of Disease.	Date of Serum. injected.
	Series.		Years.				
82/17	Sonu Babaji - 3rd	21st February, 1899 -	30	Male -	Hindu -	3 days -	21st February, 1899 -
83/18	Mr. R. P. K. (Private) - - 4th	15th do. -	6	Child -	Parsi -	1 day -	15th do. -
84/13	Bhagwan Bhana - 5th	6th March, 1899 -	25	Male -	Hindu -	2 days -	6th March, 1899 -
85/14	Pundharinath Balashet - - do.	6th do. -	22	Do. -	Do. -	2 „ -	6th do. -
86/15	Vasan Govind - do.	7th do. -	30	Do. -	Do. -	2 „ -	7th do. -
87/16	Kashibai (Private) do.	27th February, 1899 -	27	Female	Do. -	1 day -	27th February, 1899 -
88/17	Mr. S. P. C. (Private) - - do.	28th do. -	40	Male -	Portuguese	1 „ -	28th do. -
89/1	Manecklal Lallu - 6th	4th March, 1899 -	36	Do. -	Hindu -	1 „ -	4th March, 1899 -
90/2	Rama Sackaram - do.	5th do. -	14	Do. -	Do. -	1 „ -	5th do. -
91/3	Binda Bapu - do.	7th do. -	32	Do. -	Do. -	3 days -	7th do. -
92/4	Chadulla B. Farid - do.	7th do. -	40	Do. -	Mahomedan.	4 „ -	7th do. -
93/5	Ganu Gabur - do.	8th do. -	20	Do. -	Hindu -	3 „ -	8th do. -
94/6	Gaffur Lalla - do.	7th do. -	7	Child -	Mahomedan.	2 „ -	7th do. -
95/7	Bhiku Ramchandra do.	7th do. -	20	Male -	Hindu -	1 day -	8th do. -
96/8	Ambu Govinda - do.	6th do. -	18	Do. -	Do. -	2 days -	8th do. -
97/9	Hurshunkar Buldev do.	8th do. -	45	Do. -	Do. -	3 „ -	8th do. -
98/10	Nathia Bhau - do.	9th do.	23	Do. -	Do. -	3 „ -	9th do. -
99/11	T. V. Mungabun - do.	9th do. -	30	Do. -	Native Christian.	2 „ -	9th do. -
100/12	Mr. J. B. J. (Private) do.	3rd do. -	11	Do. -	Portuguese	2 „ -	4th do. -
101/13	Bipati Mehge - - do.	11th do. -	20	Do. -	Hindu -	3 „ -	11th do. -
102/14	Dagdu Sadu - - do.	11th do. -	30	Do. -	Do. -	3 „ -	12th do. -
103/15	L. C. Sequera - - do.	12th do. -	45	Do. -	Christian	3 „ -	12th do. -
104/16	Pandu Janu - - do.	13th do. -	20	Do. -	Hindu -	2 „ -	13th do. -
105/17	Kessu Lalla - - do.	12th do. -	60	Do. -	Do. -	2 „ -	13th do. -
106/18	Bhawanishunker Hary do.	13th do. -	17	Do. -	Do. -	2 „ -	13th do. -
107/19	Nana Bhana - - do.	13th do. -	25	Do. -	Do. -	2 „ -	13th do. -
108/20	Francis X. Fernandes do.	13th do. -	20	Do. -	Christian	3 „ -	14th do. -
109/21	Gunia Sudu - - do.	11th do. -	35	Do. -	Hindu -	4 „ -	14th do. -
110/22	Apa Bhaukhan - do.	14th do. -	20	Do. -	Do. -	2 „ -	14th do. -
111/23	Sheva Kondaji - do.	14th do. -	35	Do. -	Do. -	2 „ -	14th do. -
112/24	Ciprian Fernandes - do.	14th do. -	25	Do. -	Christian	2 „ -	14th do. -
113/25	John De Costa - do.	15th do. -	30	Do. -	Do. -	3 „ -	15th do. -
114/26	Sukai Jahari - do.	15th do. -	30	Do. -	Hindu -	4 „ -	15th do. -
115/27	Deria Isra - - do.	15th do. -	23	Do. -	Do.	5 „ -	15th do. -
116/28	Mamiaji Yesram - do.	16th do. -	15	Do. -	Do. -	3 „ -	16th do. -
117/29	Cyprian Alfonso - do.	15th do. -	40	Do. -	Christian	1 day -	16th do. -
118/30	F. G. (Private) - do.	27th February, 1899 -	14	Do. -	Hindu -	4 days -	——
119/31	Mrs. H. S. (Private) do.	15th March, 1899 -	56	Female	Parsi -	3 „ -	15th March, 1899 -

RUM at the ARTHUR ROAD HOSPITAL, BOMBAY—*continued.*

o.	Amount of Serum injected.												Result.	Remarks.
	1st Day.		2nd Day.		3rd Day.		4th Day.		5th Day.		6th Day.			
	Morning.	Evening.	Morning.	Evening.	Morning.	Evening.	Morning.	Evening.	Morning.	Evening.	Morning.	Evening.		
	c.c.	c.c.	c.c.	c.c.	c.c.	c.c.	c.c.	c.c.	c.c.	c.c.	c.c.	c.c.		
2 7	—	40	—	—	—	—	—	—	—	—	—	—	Died 7 a.m., 22nd February, 1899.	Multiple buboes, left femoral, right axillary, right cervical; died 12 hours after injection.
3 3	49 c.c. total.												Recovered - -	Left femoral and inguinal bubo resolved.
4 3	20	10	—	—	—	—	—	—	—	—	—	—	Do. - -	—
5 4	—	15	10	—	—	—	—	—	—	—	—	—	Died 10 p.m., 7th March, 1899.	Heart failure.
6 5	15	10	10	10	10	—	—	—	—	—	—	—	Died 9.30 p.m., 9th March, 1899.	Secondary pneumonia.
7 6	110 c.c. total quantity.												Recovered - -	Right femoral bubo.
8 7	55 c.c. total quantity.												Died 2nd March, 1899.	Heart failure; left femoral inguinal bubo.
9 1	—	30	15	20	—	—	—	—	—	—	—	—	Died 5th March, 1899.	Do. *See* Chart 9, App. LXIII.
0 2	—	20	—	—	—	—	—	—	—	—	—	—	Died 6th March, 1899.	Moribund. *See* Chart 10, App. LXIII.
1 3	18	—	—	—	—	—	—	—	—	—	—	—	Died 7th March, 1899.	Do. *See* Chart 13, App. LXIII
2 4	17	—	20	—	15	—	—	—	—	—	—	—	Died 13th March, 1899.	Improvement and then relapse. *See* Chart 14, App. LXIII.
3 5	15	15	10	—	—	—	—	—	—	—	—	—	Died 9th March, 1899.	Heart failure. *See* Chart 17, App. LXIII.
4 6	10	10	7	7	—	10	—	—	—	—	—	—	Died 10th March, 1899.	Plague and measles; heart failure. *See* Chart 12, App. LXIII.
5 7	15	20	—	—	—	—	—	—	—	—	—	—	Died 9th March, 1899.	*See* Chart 15, App. LXIII.
6 8	—	20	10	10	—	—	—	—	—	—	—	—	Recovered - -	Septic type. *See* Chart 11, App. LXIII.
7 9	—	20	15	10	10	—	—	—	—	—	—	—	Do. - -	*See* Chart 16, App. LXIII.
8 0	15	10	—	—	—	—	—	—	—	—	—	—	Died 10th March, 1899.	Heart failure. *See* Chart 18, App. LXIII.
9 1	—	20	15	15	10	10	—	—	—	—	—	—	Died 13th March, 1899.	Do. *See* chart 19, App. LXIII.
0 2	—	—	—	—	—	—	—	—	—	—	—	—	Recovered - -	Septic type. *See* chart 7, App. LXIII.
1 3	—	20	15	15	10	10	10	10	10	—	—	—	Do. - -	*See* chart 20, App. LXIII.
2 4	—	20	15	10	20	20	—	—	—	—	—	—	Died 15th March, 1899.	Secondary pneumonia. *See* Chart 21, App. LXIII.
3 5	—	20	10	—	10	15	15	—	—	—	—	—	Do. - -	Fatty heart and do. *See* Chart 22, App. LXIII.
4 3	20	15	15	15	15	15	—	—	—	—	—	—	Recovered - -	*See* chart 24, App. LXIII.
5 5	15	15	15	15	10	10	—	—	—	—	—	—	Died 23rd March, 1899.	From exhaustion after 11 days. *See* chart 23, App. LXIII.
6 7	15	15	10	—	—	—	—	—	—	—	—	—	Recovered - -	*See* chart 25, App. LXIII.
7 3	—	20	20	20	—	—	—	—	—	—	—	—	Died 15th March, 1899.	Heart failure. *See* chart 26, App. LXIII.
8 1	20	20	20	15	10	—	—	—	—	—	—	—	Recovered - -	*See* chart 28, App. LXIII.
9 9	20	15	15	15	10	—	—	—	—	—	—	—	Do. - -	*See* chart 27, App. LXIII.
0 --	--	20	10	10	16	—	—	—	—	—	—	—	Do. - -	*See* Chart 29, App. LXIII.
1 2	—	20	20	20	15	20	20	—	15	—	—	—	Do. - -	*See* Chart 30, App. LXIII.
2 1	—	20	10	—	—	—	—	—	—	—	—	—	Do. - -	*See* Chart 31, App. LXIII.
3 3	20	20	—	—	—	—	—	—	—	—	—	—	Died 15th March, 1899.	Moribund. *See* Chart 33, App. LXIII.
4 4	20	20	—	—	—	—	—	—	—	—	—	—	Died 16th March, 1899.	Secondary pneumonia and heart failure. *See* Chart 32, App. LXIII.
5 5	—	20	10	10	—	—	—	—	—	—	—	—	Recovered - -	*See* Chart 34, App. LXIII.
6 3	20	20	—	—	—	—	—	—	—	—	—	—	Died 17th March, 1899.	Heart failure. *See* Chart 37, App. LXIII.
7 1	20	20	10	10	10	10	—	—	—	—	—	—	Died 18th March, 1899.	Do. *See* Chart 35, App. LXIII.
8 9	70 c.c. total quantity.												Recovered - -	Axillary bubo. *See* Chart 8, App. LXIII.
9	57	c.c.	—	—	—	—	—	—	—	—	—	—	Died 22nd March, 1899.	Heart failure and œdema lung. *See* Chart 36, App. LXIII.

STATEMENT of PLAGUE CASES treated by Dr. GALEOTTI's

No.	Name.	Date of Admission.	Age.	Sex.	Caste.	Day of Disease.	Date of Serum injected.
	Series.		Years.				
120	Bhagwandas Raghu do.	16th March 1899 -	30	Male -	Hindu -	4 days -	16th March 1899 -
32 121	Doming Caitan - do.	16th do. -	60	Do. -	Christian	1 day -	16th do. -
33 122	Krishna Vithu - do.	17th do. -	40	Do. -	Hindu -	3 days -	17th do. -
34 123	Cosme Damian - do.	17th do. -	23	Do. -	Christian	3 „ -	17th do. -
35 124	Walla Prema - do.	17th do. -	28	Do. -	Hindu -	4 „ -	17th do. -
36 125	Gandaram Karamchand. do.	16th do. -	30	Do. -	Do. -	4 „ -	17th do. -
37 126	Mrs. Moses - do.	17th do. -	30	Female	Christian	4 „ -	17th do. -
38 127	Babaji Zora - do.	17th do. -	40	Male -	Hindu -	3 „ -	18th do. -
39 128	Peter Fernandes - do.	18th do. -	30	Do. -	Christian	2 „ -	18th do. -
40 129	Maruti Ganu - do.	18th do. -	20	Female	Hindu -	3 „ -	18th do. -
41 130	Miss J. B. S. (Private) do.	17th do. -	16	Do. -	Parsi -	2 „ -	17th do. -
42 131	Sawatie Saurela - do.	18th do.	35	Do. -	Hindu -	2 „ -	18th do. -
43 132	Francis Periera - do.	18th do. -	30	Male -	Christian	1 day -	18th do. -
44 133	Antone Salvador Lobo. do.	18th do. -	60	Do. -	Do. -	3 days -	18th do. -
45 134	Shaik Madar - do.	18th do -	30	Do. -	Hindu -	3 „ -	18th do. -
46 135	Pedro Bustaw De Costa. 6th	18th do. -	42	Do. -	Christian	1 day -	18th do. -
47 136	Piran Amer - 7th	16th do. -	35	Do. -	Mahomedan.	2 days -	16th do. -
1 137	Sawla Mahado - do.	18th do. -	35	Do. -	Hindu -	1 day -	18th do. -
2 138	Dhondu Krishna - do.	19th do. -	30	Do. -	Do. -	3 days -	19th do. -
3 139	Sittia Balloo - do.	20th do. -	22	Do. -	Do. -	3 „ -	20th do. -
4 140	Michal Francis - do.	21st do. -	14	Do. -	Christian	2 „ -	21st do. -
5 141	Sonia Premia - do.	22nd do. -	12	Do. -	Hindu -	5 „ -	23rd do. -
6 142	Nama Rowji - do.	23rd do. -	25	Do. -	Do. -	1 day -	23rd do. -
7 143	Bhagla Puchya - do.	24th do. -	18	Do. -	Do. -	3 days -	24th do. -
8 144	Mahadeo Panga - do.	25th do. -	25	Do. -	Do. -	2 „ -	25th do. -
9 145	Bhagwan Hanuiman do.	25th do. -	18	Do. -	Do. -	2 „ -	26th do. -
10 146	Tatya Babaji - do.	25th do. -	25	Do. -	Do. -	1 day -	26th do. -
11 147	Ratan Dhana - do.	26th do. -	14	Do. -	Do. -	2 days -	27th do. -
12 148	Babaji Dhakoo - do.	27th do. -	30	Do. -	Do. -	3 „ -	27th do. -
13							

UM at the ARTHUR ROAD HOSPITAL, BOMBAY—*continued.*

	Amount of Serum injected.												Result.	Remarks
	1st Day.		2nd Day.		3rd Day.		4th Day.		5th Day.		6th Day.			
	Morning.	Evening.	Morning.	Evening.	Morning.	Evening.	Morning.	Evening.	Morning.	Evening.	Morning.	Evening.		
	c.c.	c c.	c.c.	c.c.	c.c.	c.c.	c.c.	c.c.	c.c.	c.c.	c.c.	c.c.		
0	—	10	10	10	10	—	10	—	—	—	—	—	Recovered - -	*See* Chart 38, App. LXIII.
1	—	20	15	10	10	—	—	—	—	—	—	—	Died 18th March, 1899.	Œdema lung and heart failure. *See* Chart 39, App. LXIII.
2	20	20	15	15	—	—	—	—	—	—	—	—	Died 19th March, 1899.	Heart failure. *See* Chart 41, App. LXIII.
3	30	20	20	15	20	—	—	—	—	—	—	—	Do. -	Do. *See* Chart 42, App. LXIII.
4	—	20	15	10	10	—	—	—	—	—	—	—	Died 20th March, 1899.	Œdema lung. *See* Chart 46, App. LXIII.
5	—	20	15	10	10	10	—	—	—	—	—	—	Do. - -	Septic type. *See* Chart 40, App. LXIII.
6	—	20	20	10	5	10	5	—	—	—	—	—	Recovered - -	*See* Chart 45, App. LXIII.
7	15	10	10	15	10	—	—	—	—	—	—	—	Died 26th March, 1899.	Secondary pneumonia. *See* Chart 43, App. LXIII.
8	20	10	10	—	—	—	—	—	—	—	—	—	Died 19th March, 1899.	Heart failure. *See* Chart 47, App. LXIII.
9	20	15	15	15	5	—	—	—	—	—	—	—	Recovered - -	*See* Chart 48, App. LXIII.
0	50	c.c.	—	—	—	—	—	—	—	—	—	—	Died 19th March, 1899.	Heart failure. *See* Chart 44, App. LXIII.
1	—	20	15	20	—	—	—	—	—	—	—	—	Died 20th March, 1899.	Bronchitis. *See* Chart 49, App. LXIII.
2	—	20	15	—	—	—	—	—	10	—	—	—	Recovered.	
3	—	15	20	—	—	—	—	—	—	—	—	—	Died 19th March, 1899.	Heart failure. *See* Chart 50, App. LXIII.
4	—	15	15	—	—	—	—	—	—	—	—	—	Do. - -	Do. *See* Chart 51 App. LXIII.
5	—	15	20	15	15	10	—	—	—	—	—	—	Died 22nd March, 1899.	Secondary pneumonia. *See* Chart 53 App. LXIII.
6	—	30	20	20	15	—	10	—	10	—	—	—	Recovered.	
7	—	30	20	—	—	—	—	—	—	—	—	—	Died 20th March, 1899.	Moribund and heart failure.
8	20	10	10	10	5	—	—	—	—	—	—	—	Recovered.	
9	15	15	15	15	—	15	10	15	—	—	—	—	Died 24th March, 1899.	
0	—	20	15	15	15	10	10	—	—	—	—	—	Do. - -	Heart failure.
1	15	15	15	10	10	10	7	5	—	—	—	—	?	
2	—	20	20	—	—	—	—	—	—	—	—	—	Died 24th March, 1899.	Heart failure.
3	20	15	15	15	10	15	—	15	—	—	—	—	?	
4	20	20	15	10	10	10	—	—	—	—	—	—	?	
5	20	20	15	15	15	—	—	—	—	—	—	—	?	
6	20	—	—	—	—	—	—	—	—	—	—	—	Died 27th March, 1899.	Secondary pneumonia.
7	15	15	10	—	—	—	—	—	—	—	—	—	?	
8	—	20	15	—	—	—	—	—	—	—	—	—	?	

The following STATEMENTS of CASES treated with GALEOTTI'S SERUM in the ARTHUR ROAD, HOSPITAL,
Letter dated

SERIES XI.—HORSE No. 1.

General No.	Serial No.	Date of Admission.	Names.	Age.	Sex.	Caste	Duration.
1	8	1st May	Matarba Ittu	35	Male	Hindu	4 days
2	9	2nd „	Chandri, wife of Anjia	28	Female	Do	5 „
3	10	2nd „	Dhondu Anjia	6	Male	Do	5 „
4	11	2nd „	Laxmi Hari	28	Female	Do	4 „
5	12	4th „	Khrishna Dhondi	35	Male	Do	3 „
6	13	6th „	Shivdatdas Sadu	35	Do.	Do.	2 „
7	14	7th „	Bhiwa Dada	35	Do.	Do.	2 „
8	15	8th „	Kavel Lama	20	Do.	Do.	8 „
9	16	10th „	Anusuya Dhondiba	6	Female	Do.	5 „
10	17	13th „	Valabh Nagar	30	Male	Do.	5 „
11	18	15th „	Chandrabai Balla	30	Female	Do.	2 „
12	19	23rd „	Kalidin Bind	30	Male	Do.	4 „
13	20	26th „	Anandi Bhowani	22	Female	Do.	3 „
14	21	27th „	Jayi Rama	30	Do.	Do.	2 „
15	22	30th „	Mulla Kanna	14	Male	Do.	4 „

SERIES XII.—HORSE

16	1	21st May	Mr. C. F. D.	43	Male	Parsi	4 days
17	2.	24th „	Mr. Kharagett	53	Do.	Do.	3 „

ıBAY, were submitted by DR. CHOKSY to the MUNICIPAL COMMISSIONERS, BOMBAY, with his
. June 1899.

d Bleeding) (Completed).

neral No.	Bubo.	Quantity injected.	Died.	Recovered.	Remarks.
1	Left femoral - -	60 c. c. -	—	Recovered.	
2	Right axillary - -	110 c. c. -	6th May 1899 -	—	Death from œdema of lung.
3	Over left lower ribs - -	30 c. c. -	—	Recovered.	
4	Right inguinal - -	50 c. c. -	—	Do.	
5	Right femoral and inguinal -	85 c. c. -	6th May 1899 -	—	Sudden heart failure.
6	Right inguinal - -	70 c. c. -	7th do. -	—	Sudden heart failure from diarrhœa.
7	Left femoral - - -	60 c. c. -	8th do. -	—	Acute œdema of lung, diarrhœa, heart failure.
8	Right axillary with infiltration	55 c. c. -	—	Recovered.	Suppuration of infiltration.
9	Left inguinal - - -	20 c. c. -	—	Do.	
10	Right axillary - - -	35 c. c. -	18th May 1899 -	—	Death from œdema of lung.
11	Right posterior cervical -	60 c. c. -	—	Recovered.	
12	Left femoral - -	40 c. c. -	24th May 1899 -	—	Pneumonic plague, death from heart failure.
13	Right inguinal - -	55 c. c. -	28th do. -	—	Heart failure, very delirious on admission, with failing pulse.
14	Right axillary supra trochlear	80 c. c. -	28th do. -	—	Pregnant, aborted next morning, admitted with œdema of lung.
15	Right femoral - -	35 c. c. -	—	Recovered.	Death from heart failure.

. 4 (2nd Bleeding).

16	Right inguinal - -	105 c. c. -	—	Recovered.	Plague and malaria.
17	Right axillary with extensive infiltration on chest and shoulder.	135 c. c. -	27th May 1899 -	—	Chyluria fatty heart. Heart failure gradual.

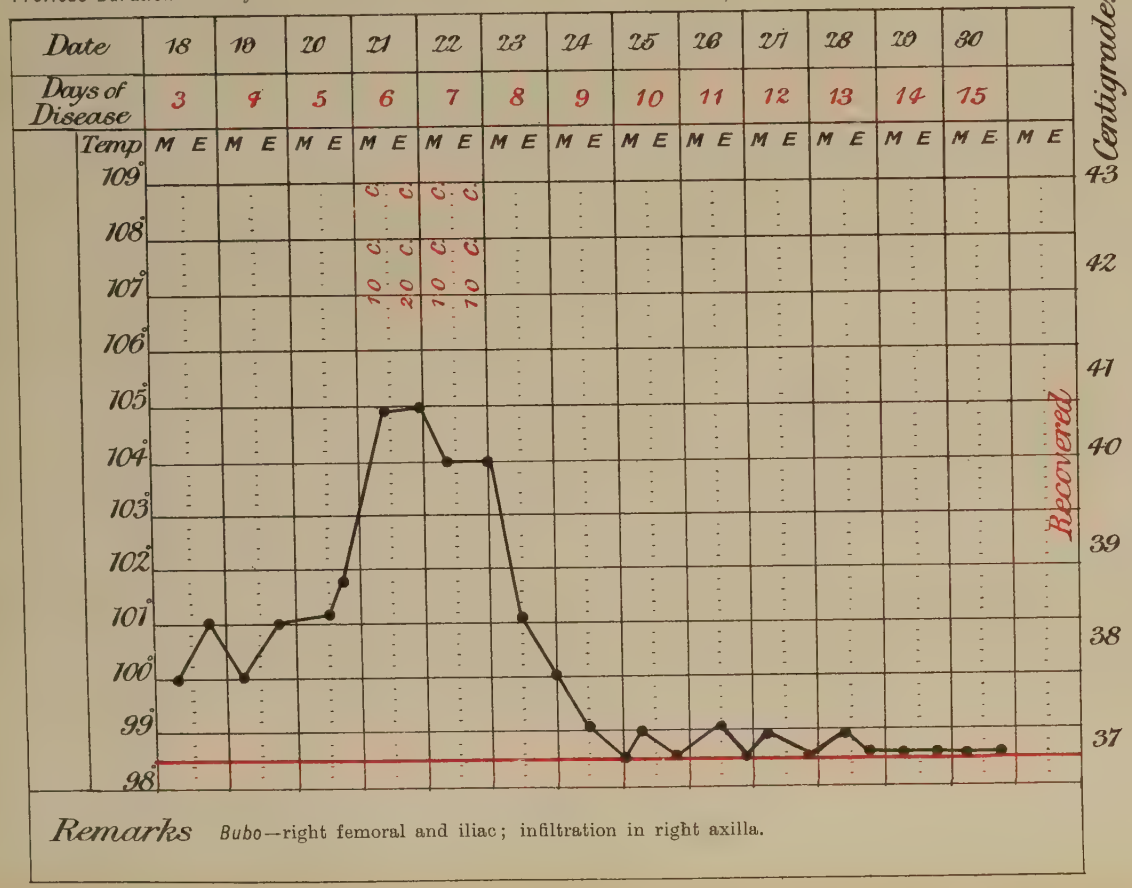
APPENDIX No. LXIII. *(See Question No. 20,938.)*

Municipal Hospital for Infectious Diseases, Arthur Road, Bombay.

(Dr. CHOKSY.)

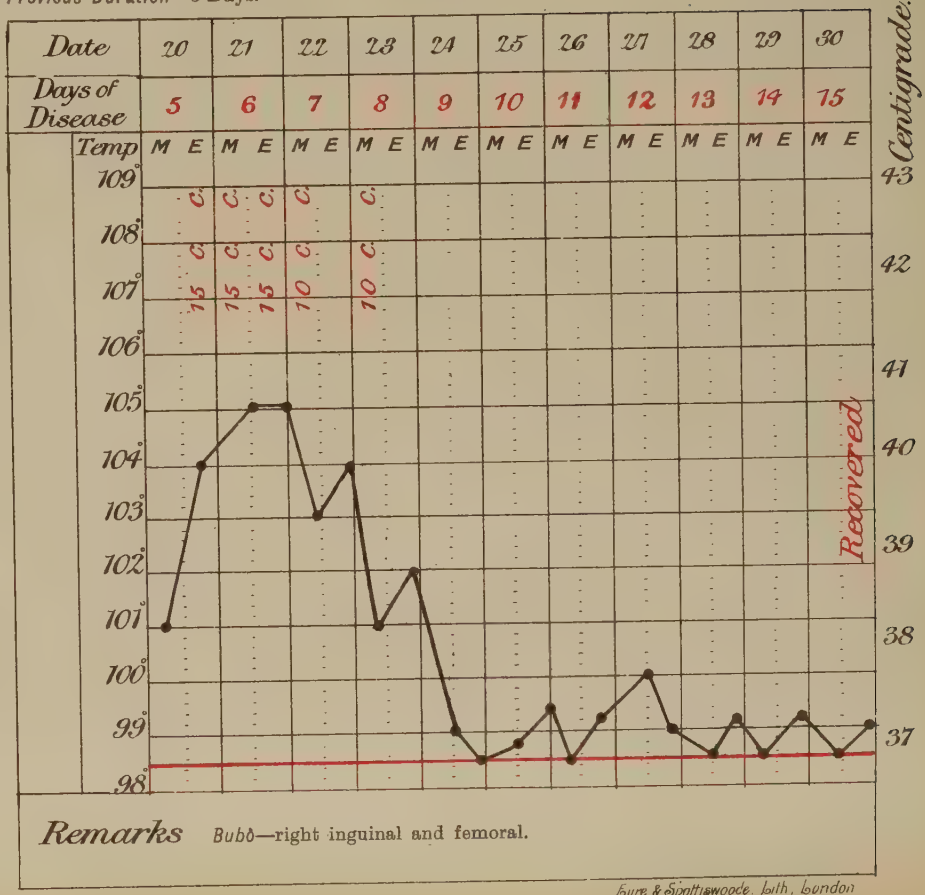
CLINICAL CHARTS OF PLAGUE CASES TREATED WITH LUSTIG'S SERUM.

1. *Date of Admission*—18th June 1897.
General Number—
Name—ARDAD ULLA. *DIAGNOSIS*—Plague.
Occupation—Fireman. *Sex*—Male. *Age*—30.
Previous Duration—3 Days. *Caste*—Muhammadan.



Remarks *Bubo*—right femoral and iliac; infiltration in right axilla.

2. *Date of Admission*—20th June 1897.
General Number—
Name—KASINATTI BALLA. *DIAGNOSIS*—Plague.
Occupation—Domestic Servant. *Sex*—Male. *Age*—30.
Previous Duration—5 Days. *Caste*—Hindu.



Remarks *Bubo*—right inguinal and femoral.

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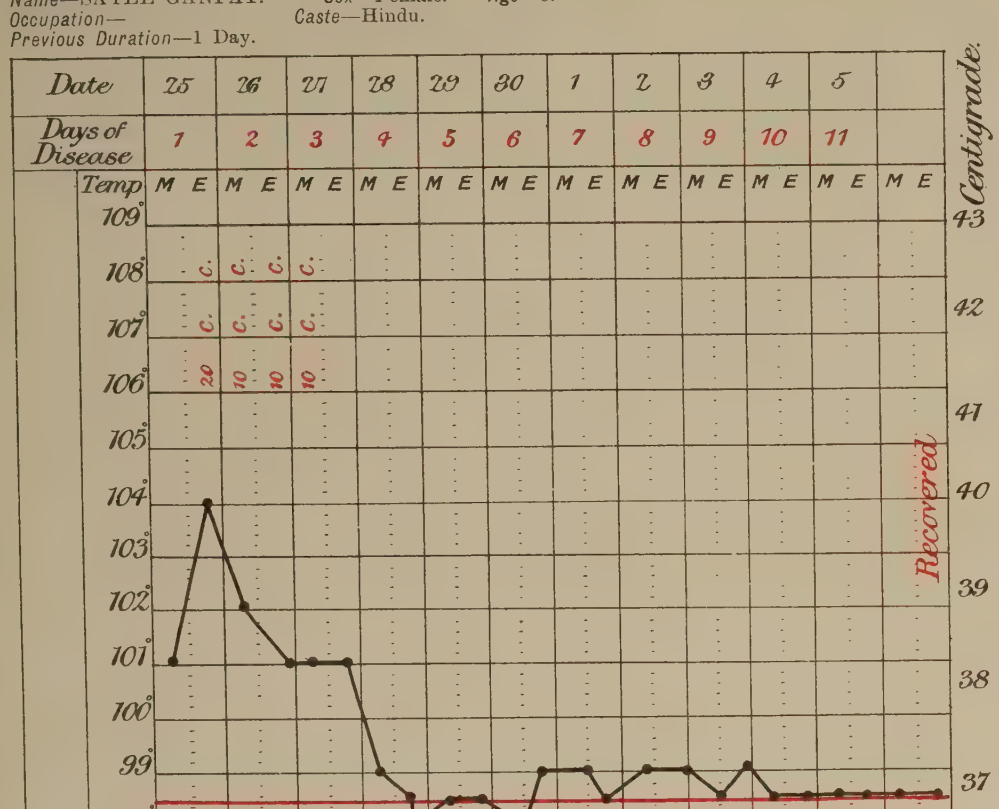
(Dr. CHOKSY.)

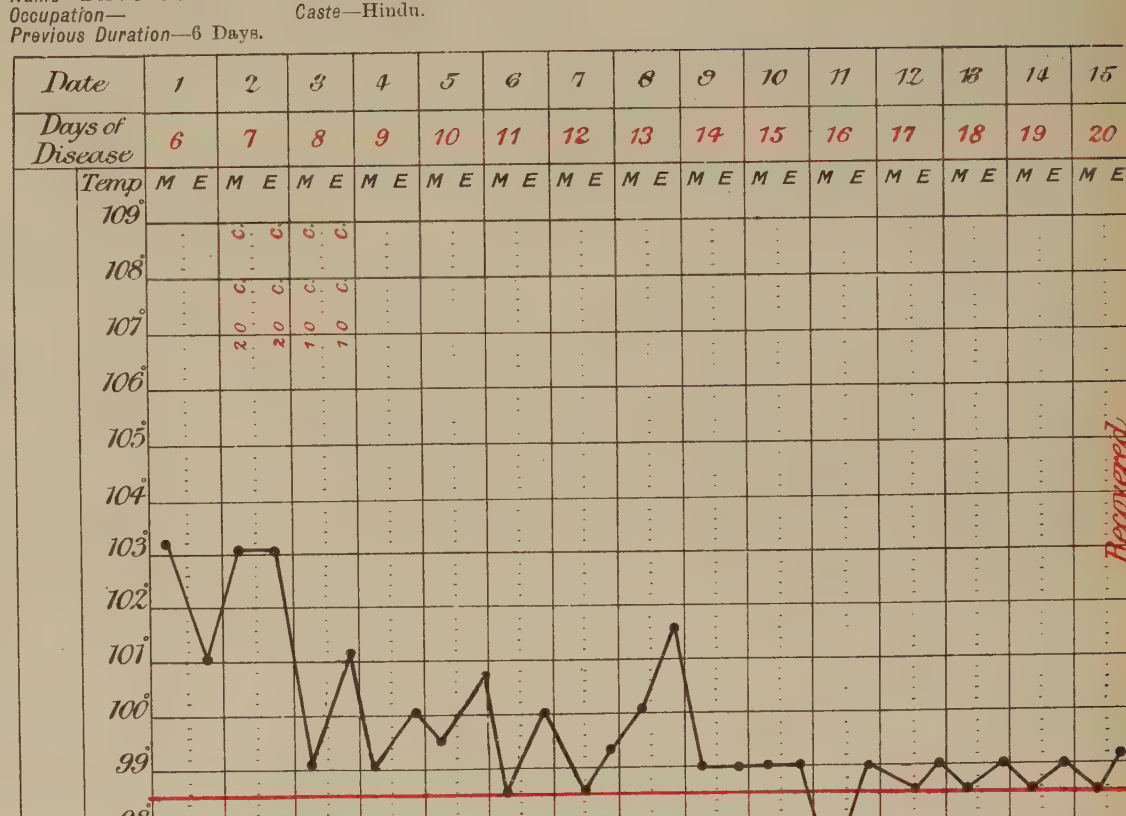
CLINICAL CHARTS OF PLAGUE CASES TREATED WITH LUSTIG'S SERUM.

3. *Date of Admission*—25th June 1897.
General Number— *DIAGNOSIS*—Plague.
Name—SAYEE GANPAT. *Sex*—Female. *Age*—8.
Occupation— *Caste*—Hindu.
Previous Duration—1 Day.



Remarks *Bubo*—left submaxillary with infiltration in the neck and below the chin.

4. *Date of Admission*—1st July 1897.
General Number— *DIAGNOSIS*—Plague.
Name—DAVU GOVIND. *Sex*—Female *Age*—16.
Occupation— *Caste*—Hindu.
Previous Duration—6 Days.



Remarks *Bubo*—right inguinal and femoral with pelvic infiltration.

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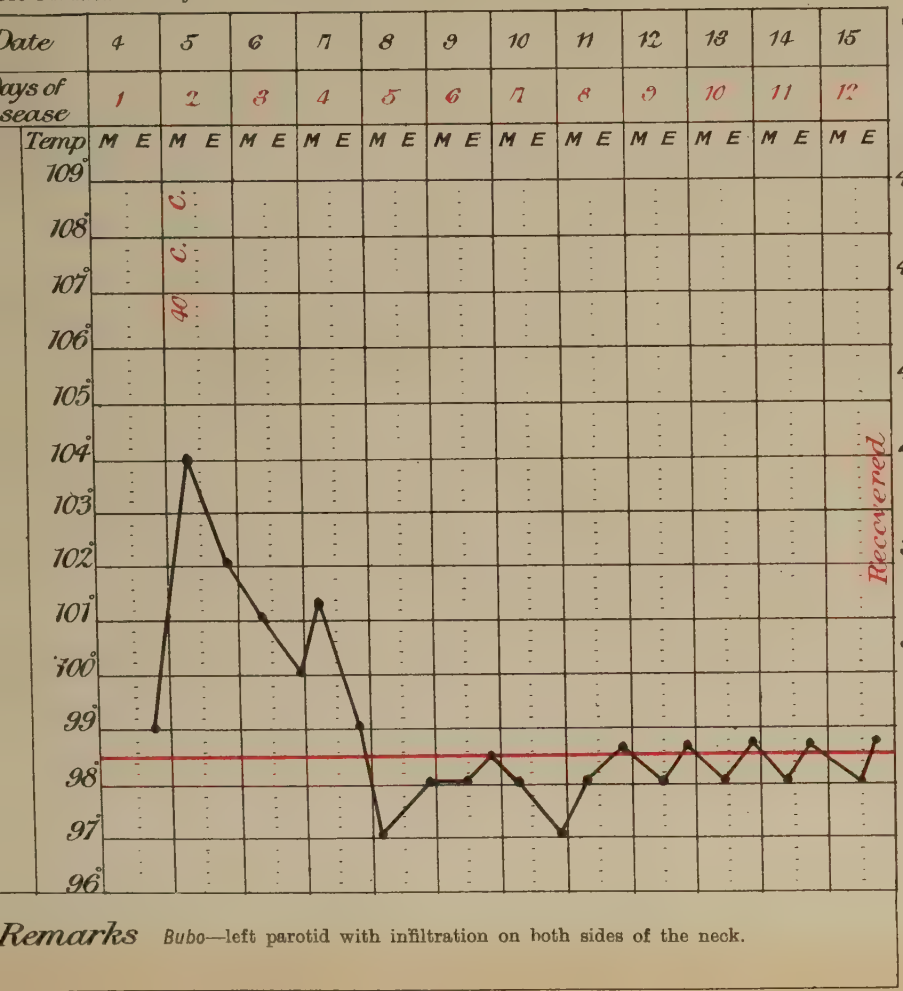
(Dr. CHOKSY,)

CLINICAL CHARTS OF PLAGUE CASES TREATED WITH LUSTIG'S SERUM.

of Admission—4th July 1897.
al Number—
—SAKDU TABIA. Sex—Male. Age—9.
pation—
ous Duration—1 Day. Caste—Hindu.

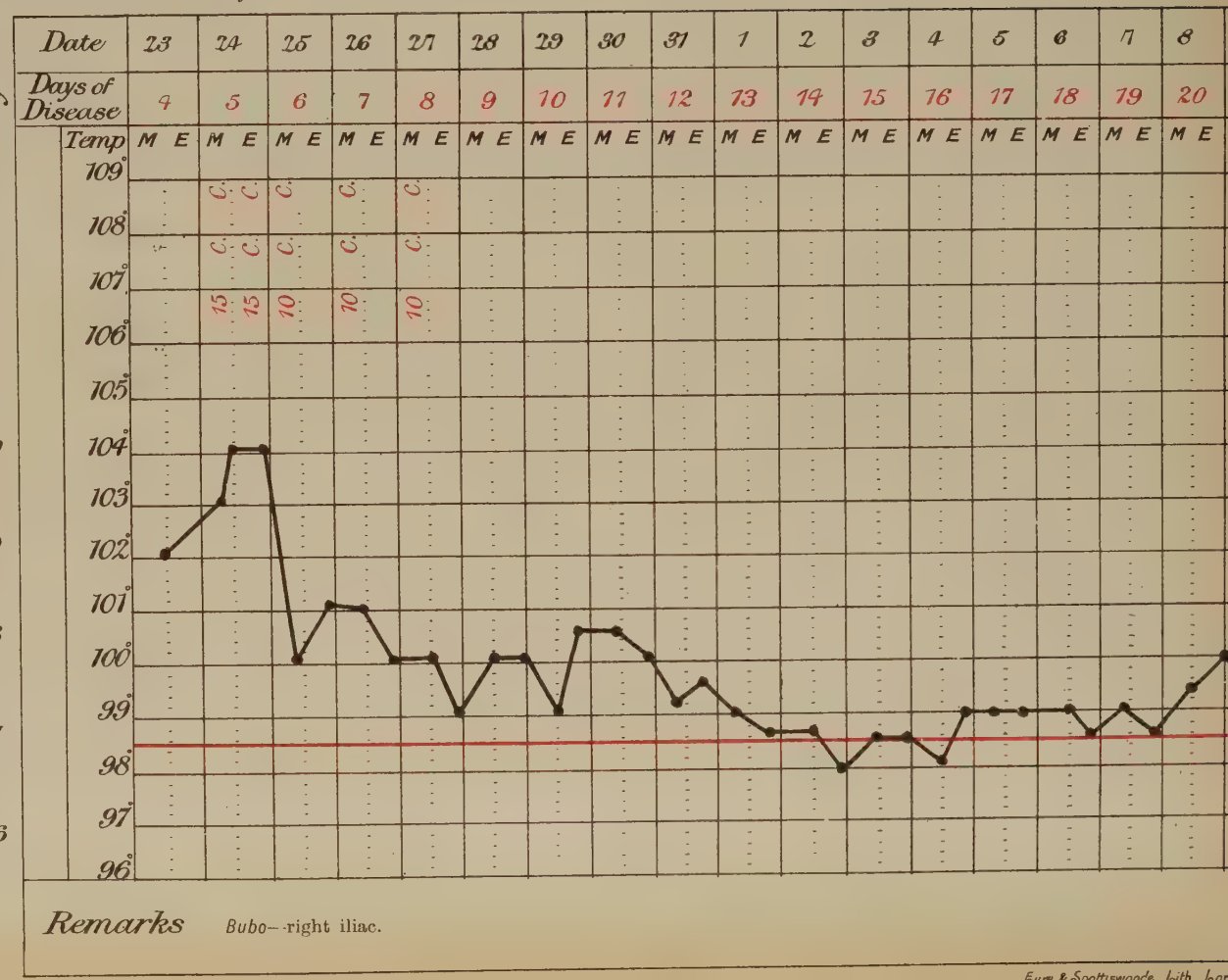
DIAGNOSIS—Plague.



Remarks Bubo—left parotid with infiltration on both sides of the neck.

6. Date of Admission—23rd July 1897.
General Number—
Name—GANGOO BALA. Sex—Male. Age—12.
Occupation— Caste—Hindu.
Previous Duration—4 Days.

DIAGNOSIS—Plague.



Remarks Bubo—right iliac.

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APPENDIX No. LXIV. (*See Question No.* 21,033.)

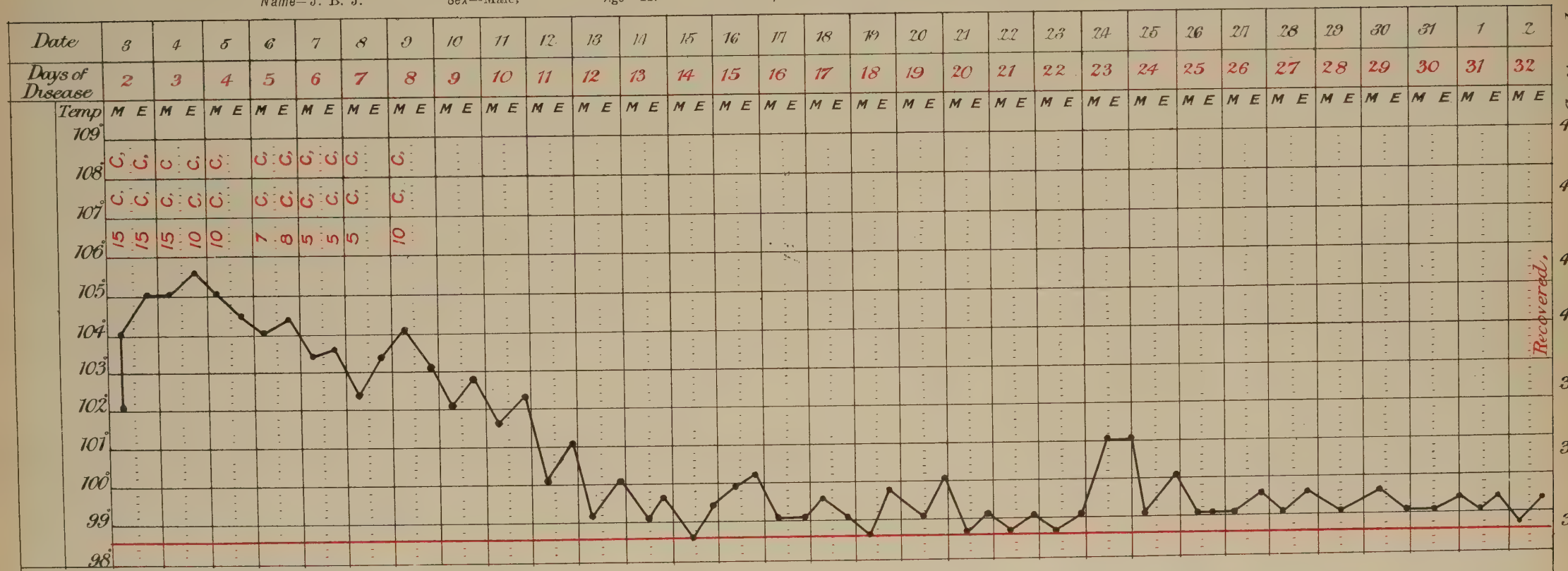
Municipal Hospital for Infectious Diseases, Arthur Road, Bombay.

(Dr. CHOKSY.)

CLINICAL CHARTS OF PLAGUE CASES TREATED WITH LUSTIG'S SERUM.

7. *Date of Admission*—3rd March 1899. *General Number*— *DIAGNOSIS*—Plague.

Name—J. B. J. *Sex*—Male, *Age*—11. *Occupation*—Student. *Caste*—Parsee. *Previous Duration*—2 Days.



Remarks *Bubo*—left femoral and left iliac; mesenteric glands secondarily infected; good recovery.

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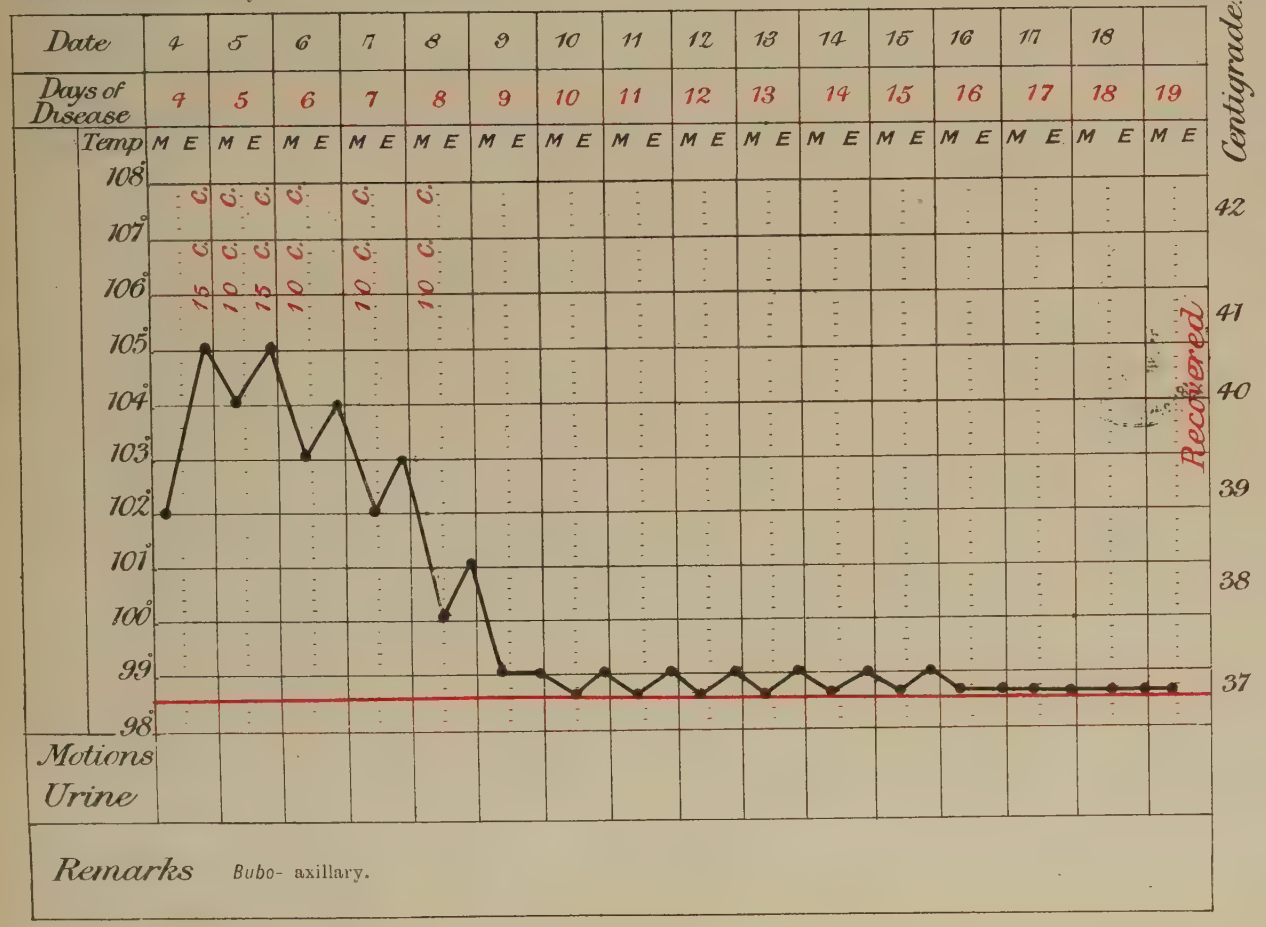
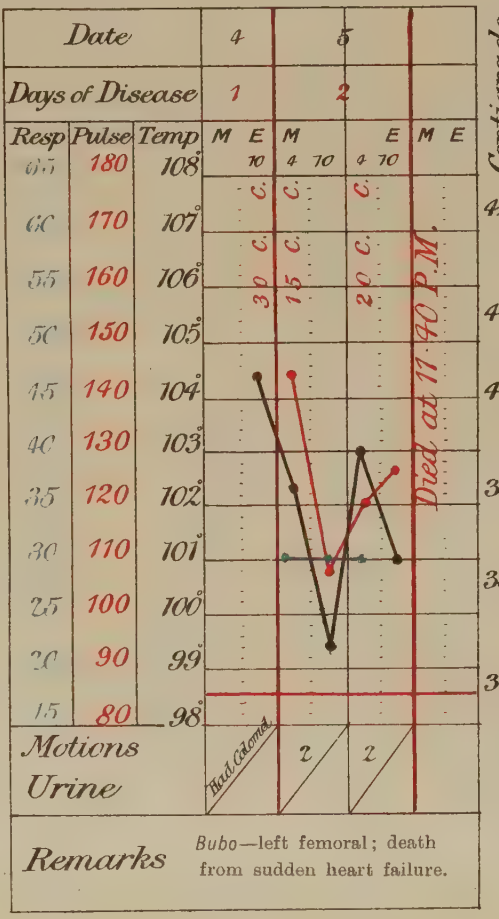
(Dr. CHOKSY.)

CLINICAL CHARTS OF PLAGUE CASES TREATED WITH LUSTIG'S SERUM.

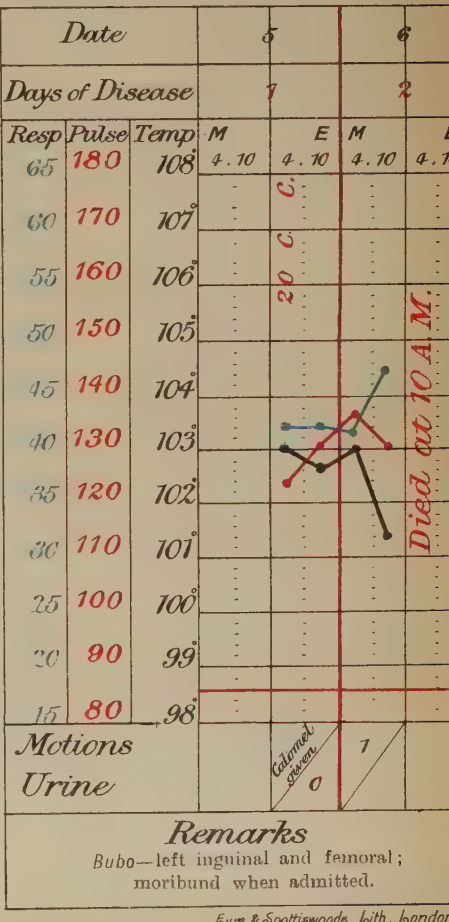
8. *Date of Admission*—27th February 1899.
General Number— *DIAGNOSIS*—Plague.
Name—F. G. *Sex*—Male. *Age*—14.
Occupation—Nil. *Caste*—Hindu.
Previous Duration—4 Days.



9. *Date of Admission*—4th March 1899.
General Number—850. *DIAGNOSIS*—Plague.
Name—MANEKLAL LALLOO. *Sex*—Male. *Age*—36.
Occupation—Milkman. *Caste*—Hindu.
Previous Duration—1 Day.


Date 4 5
Days of Disease 1 2
Resp Pulse Temp M E M E M E
65 60 55 50 45 40 35 30 25 20 15
180 170 160 150 140 130 120 110 100 90 80
108° 107° 106° 105° 104° 103° 102° 101° 100° 99° 98°
Centigrade 42 41 40 39 38 37
C. 30 15 20
Died at 11.40 P.M.
Motions Urine
Bad. Calomel 2 2
Remarks Bubo—left femoral; death from sudden heart failure.

10. *Date of Admission*—5th March 1899, at 1.20 p.m
General Number—863. *DIAGNOSIS*—Plag
Name—RAMA SUCCARAM. *Sex*—Male. A
Occupation—Table Boy. *Caste*—Hindu.
Previous Duration—1 Day.


Date 5 6
Days of Disease 1 2
Resp Pulse Temp M E M
65 60 55 50 45 40 35 30 25 20 15
180 170 160 150 140 130 120 110 100 90 80
108° 107° 106° 105° 104° 103° 102° 101° 100° 99° 98°
Centigrade 42 41 40 39 38 37
C. 20
Died at 10 A.M.
Motions Urine
Calomel Green 1
0
Remarks
Bubo—left inguinal and femoral; moribund when admitted.

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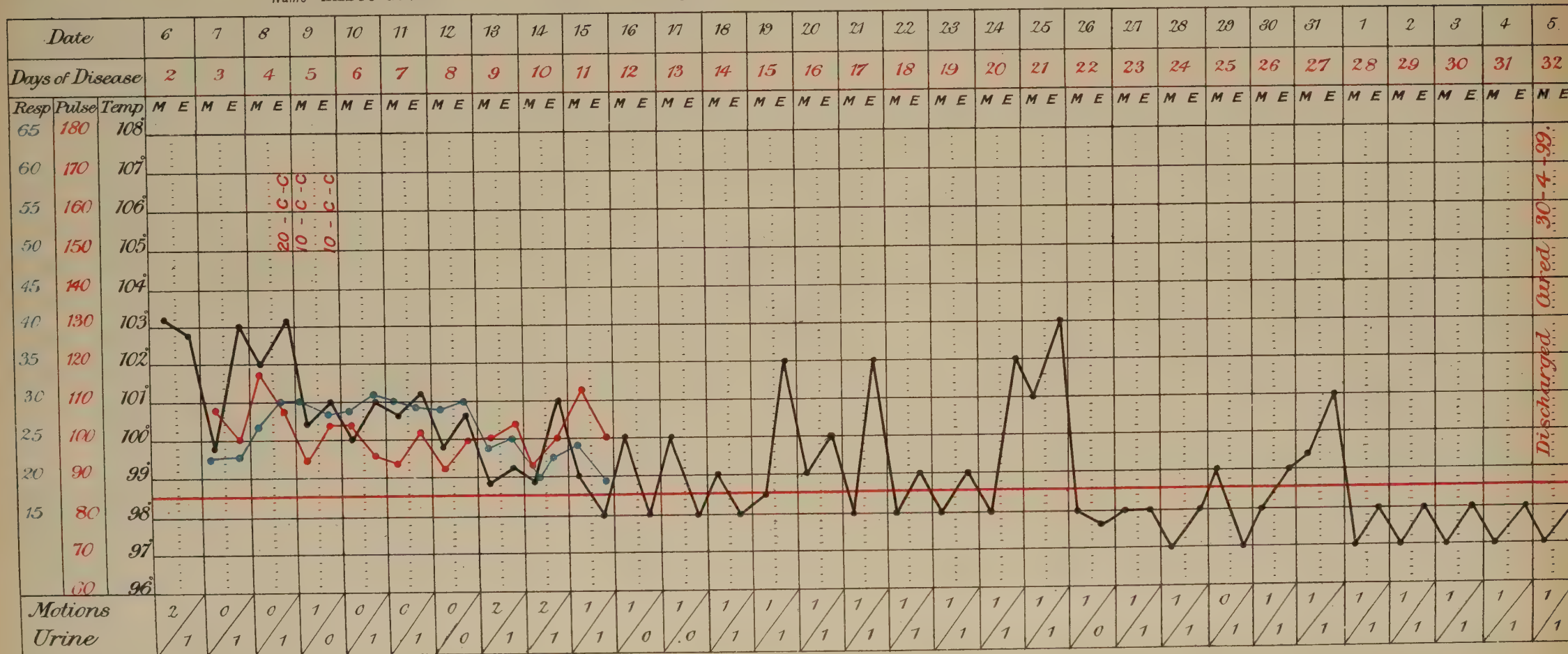
(Dr. CHOKSY.)

CLINICAL CHARTS OF PLAGUE CASES TREATED WITH LUSTIG'S SERUM.

11. *Date of Admission*—6th March 1899, at 11.15 a.m. *General Number*—883. *DIAGNOSIS*—Plague.

Name—AMBOO GOVINDA. *Sex*—Male. *Age*—18. *Occupation*—Table Boy. *Caste*—Hindu. *Previous Duration*—2 Days.



Remarks. *Bubo*—double inguinal and right femoral.

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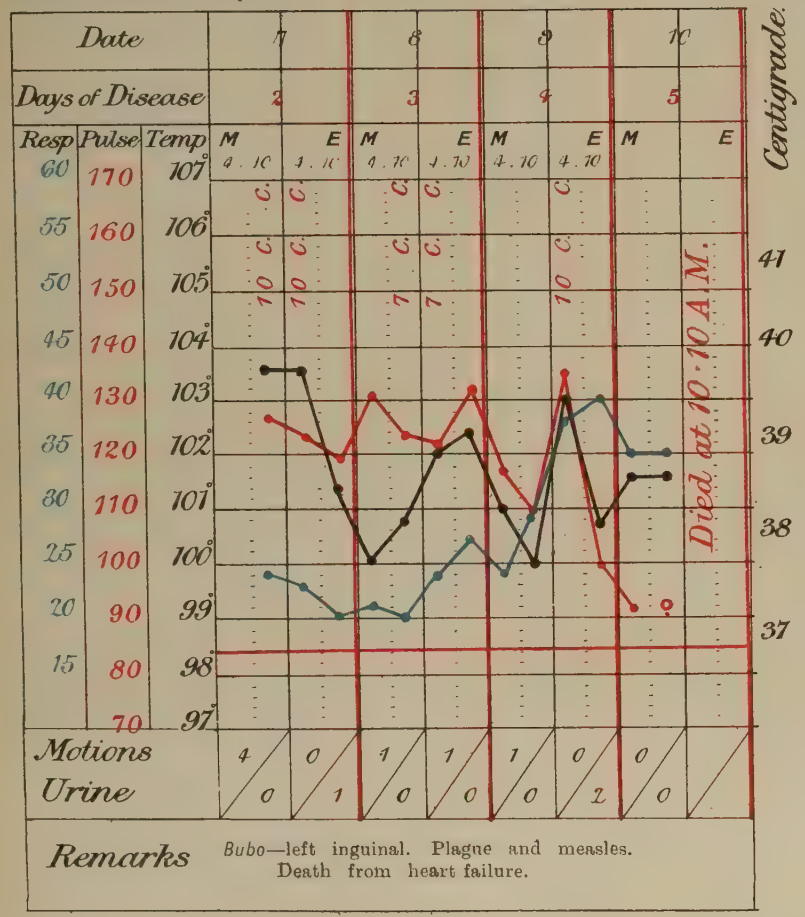
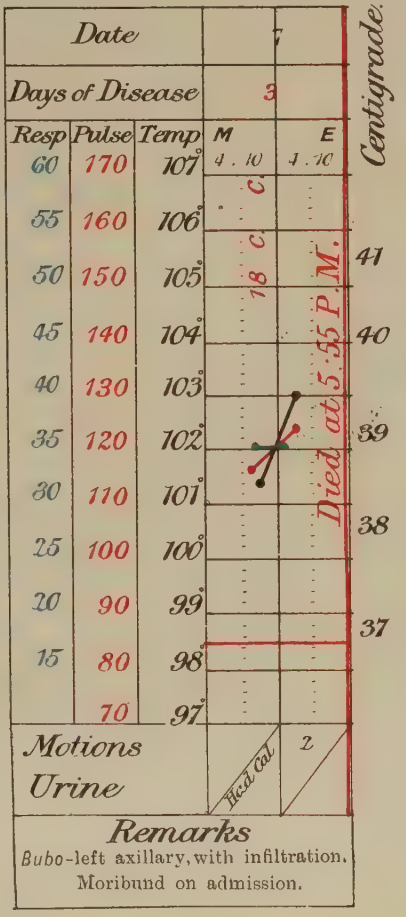
(Dr. CHOKSY.)

CLINICAL CHARTS OF PLAGUE CASES TREATED WITH LUSTIG'S SERUM.

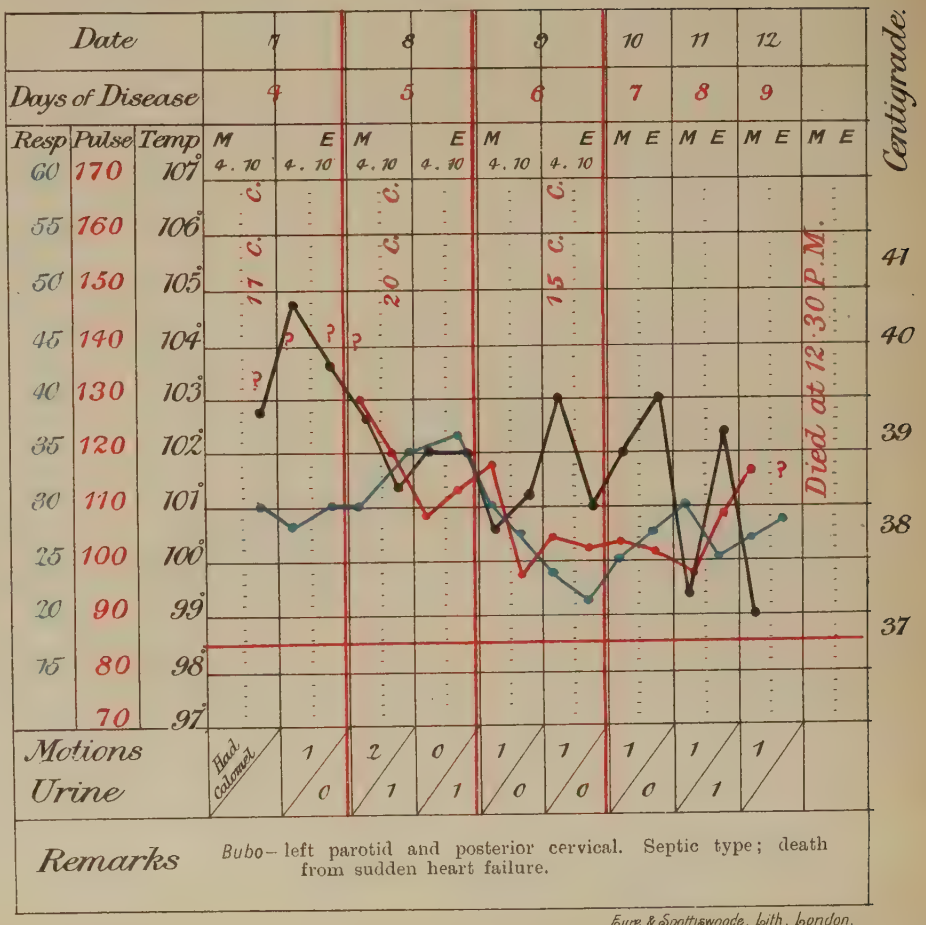
12. *Date of Admission*—7th March 1899, at 8.15 a.m.
General Number—897. *DIAGNOSIS*—Plague.
Name—GAFFOOR LALLA. *Sex*—Male. *Age*—7.
Occupation—Nil. *Caste*—Musalman.
Previous Duration—2 Days.



13. *Date of Admission*—7th March 1899, at 9 a.m.
General Number—899. *DIAGNOSIS*—Plague.
Name—BINDA BAPOO. *Age*—32.
Occupation—Mill Hand. *Caste*—Hindu.
Previous Duration—3 Days. *Sex*—Male.



14. *Date of Admission*—7th March 1899, at 9.50 a.m.
General Number—903. *DIAGNOSIS*—Plague.
Name—CHADULLA B. FARID. *Sex*—Male. *Age*—40.
Occupation—Labourer. *Caste*—Musalman.
Previous Duration—4 Days.



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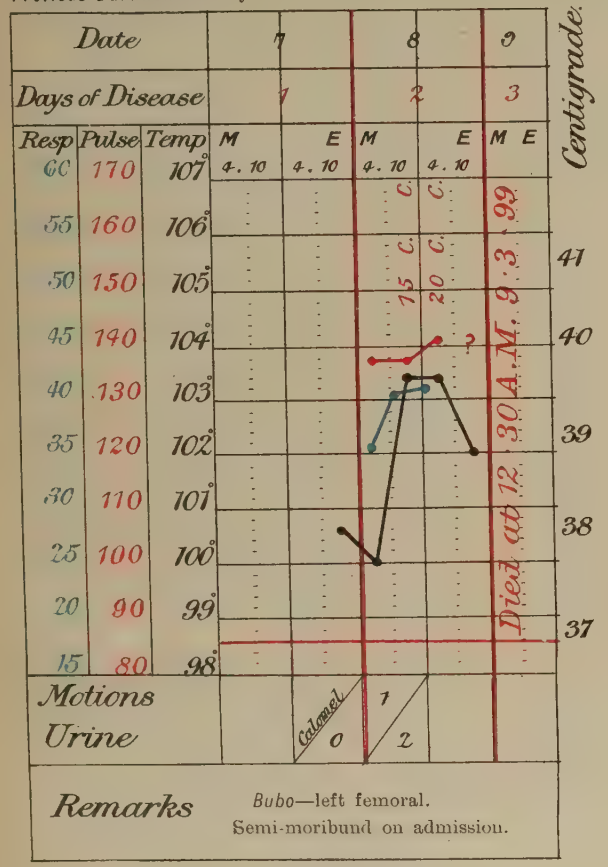
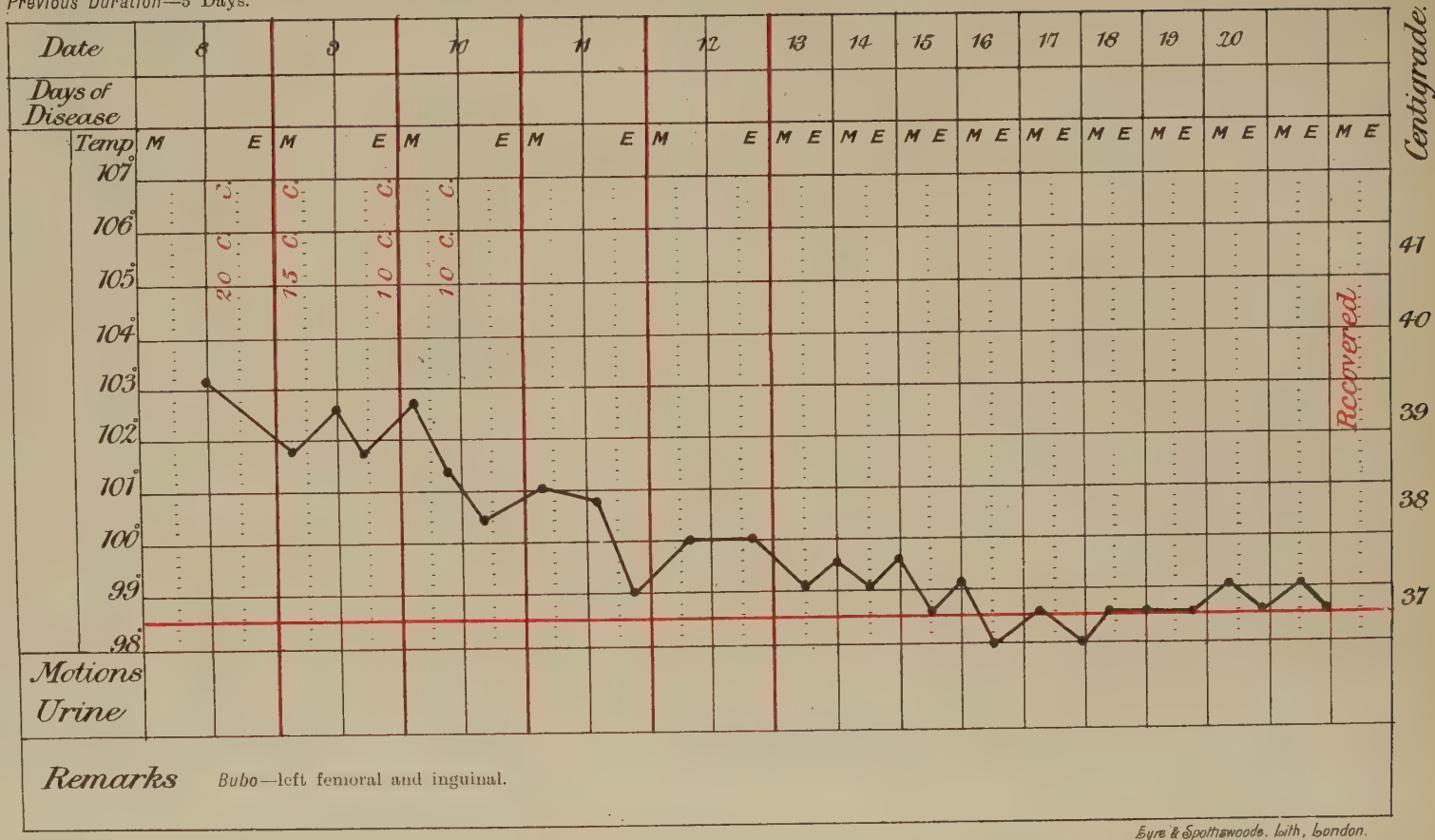
CLINICAL CHARTS OF PLAGUE CASES TREATED WITH LUSTIG'S SERUM.

Date of Admission—7th March 1899, at 8.30 p.m.
General Number—921. *DIAGNOSIS*—Plague.
Name—BHICOO RAMCHANDRA. *Sex*—Male. *Age*—20.
Occupation—Mill Hand. *Caste*—Hindu.
Previous Duration—1 Day.



16. *Date of Admission*—8th March 1899.
General Number—936. *DIAGNOSIS*—Plague.
Name—HARSHANKAR BULDEV. *Sex*—Male. *Age*—45.
Occupation—Ramoosee. *Caste*—Hindu.
Previous Duration—3 Days.



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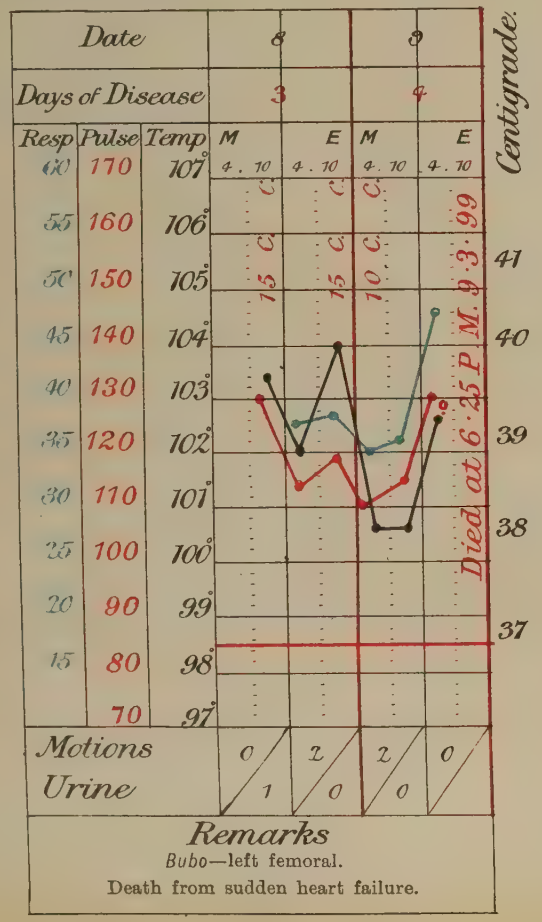
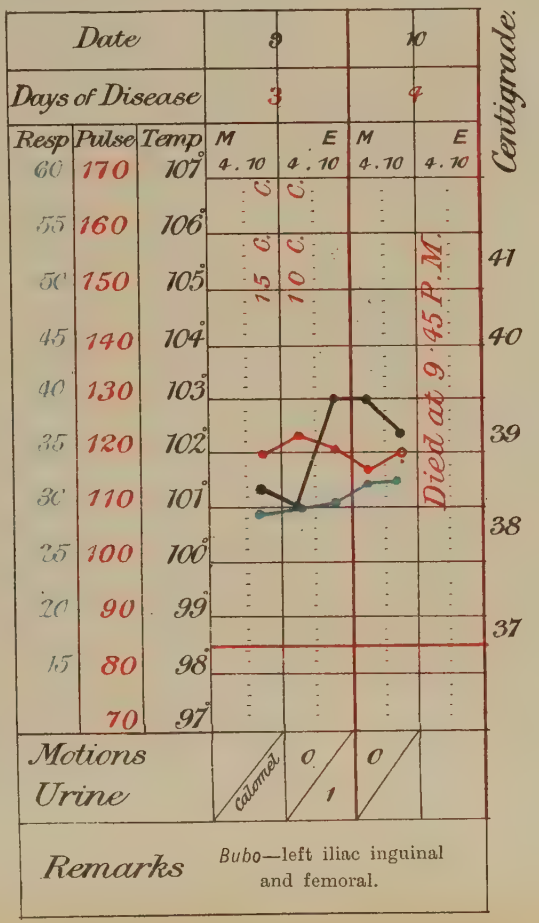
(Dr. CHOKSY.)

CLINICAL CHARTS OF PLAGUE CASES TREATED WITH LUSTIG'S SERUM.

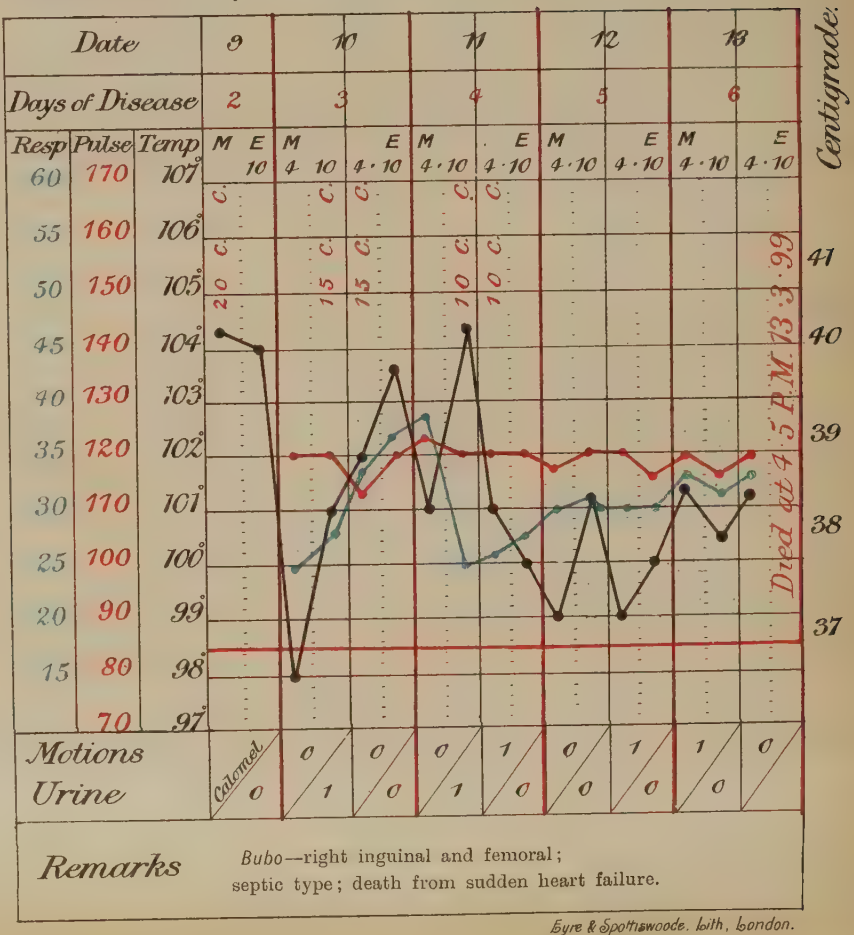
17. *Date of Admission*—8th March 1899, at 9.30 a.m.
General Number—924. *DIAGNOSIS*—Plague.
Name—GANOO GABOOR. *Sex*—Male. *Age*—20.
Occupation—Cook. *Caste*—Hindu.
Previous Duration—3 Days.



18. *Date of Admission*—9th March 1899, at 9.55 a.m.
General Number—955. *DIAGNOSIS*—Plague.
Name—NATHIA BHAOO. *Sex*—Male. *Age*—28.
Occupation—Labourer. *Caste*—Hindu.
Previous Duration—3 Days.



19. *Date of Admission*—9th March 1899, at 5.45 p.m.
General Number—969. *DIAGNOSIS*—Plague.
Name—T. V. MUNGABEEN. *Sex*—Male. *Age*—30.
Occupation—Clerk. *Caste*—Christian.
Previous Duration—2 Days.



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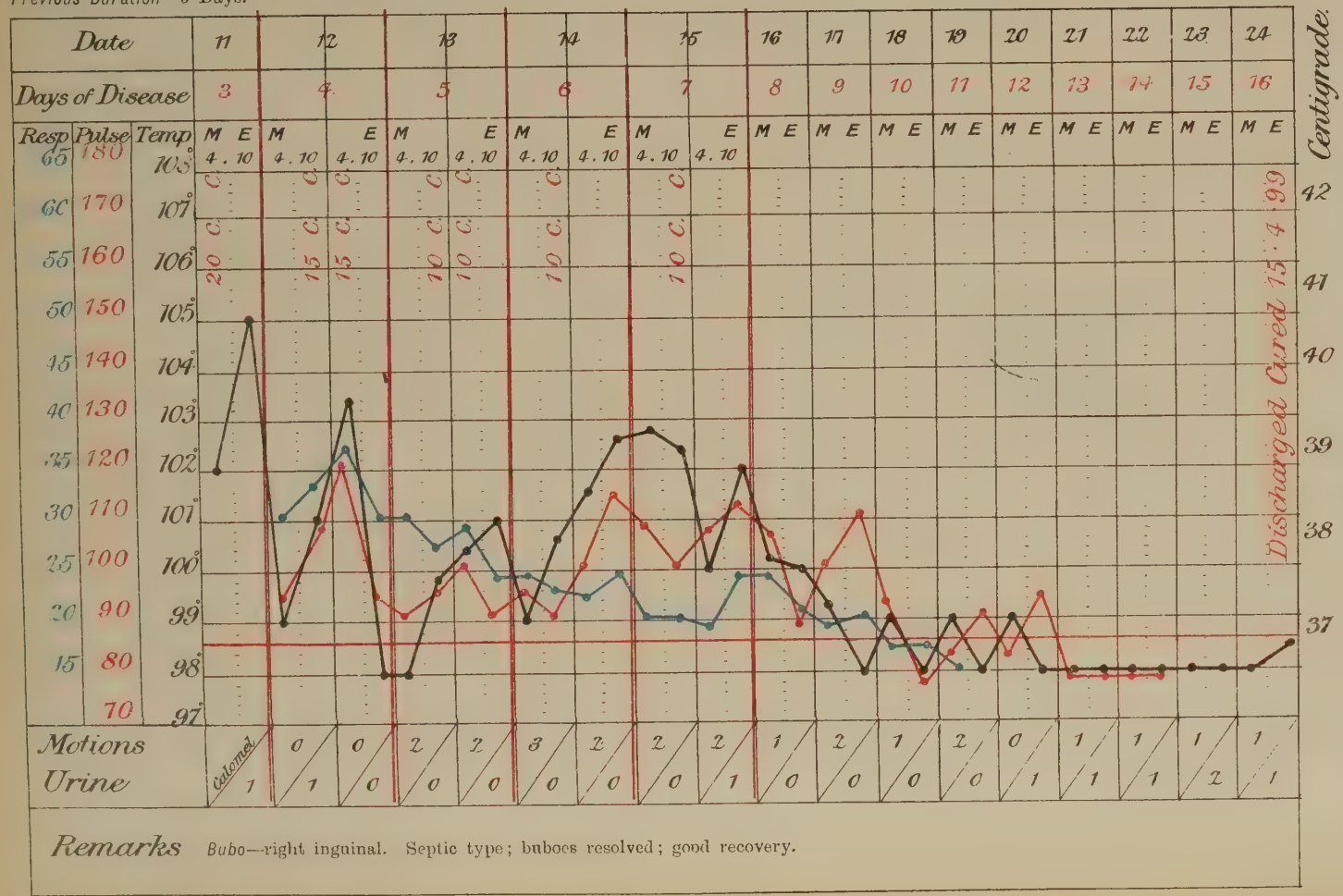
(Dr. CHOKSY.)

CLINICAL CHARTS OF PLAGUE CASES TREATED WITH LUSTIG'S SERUM.

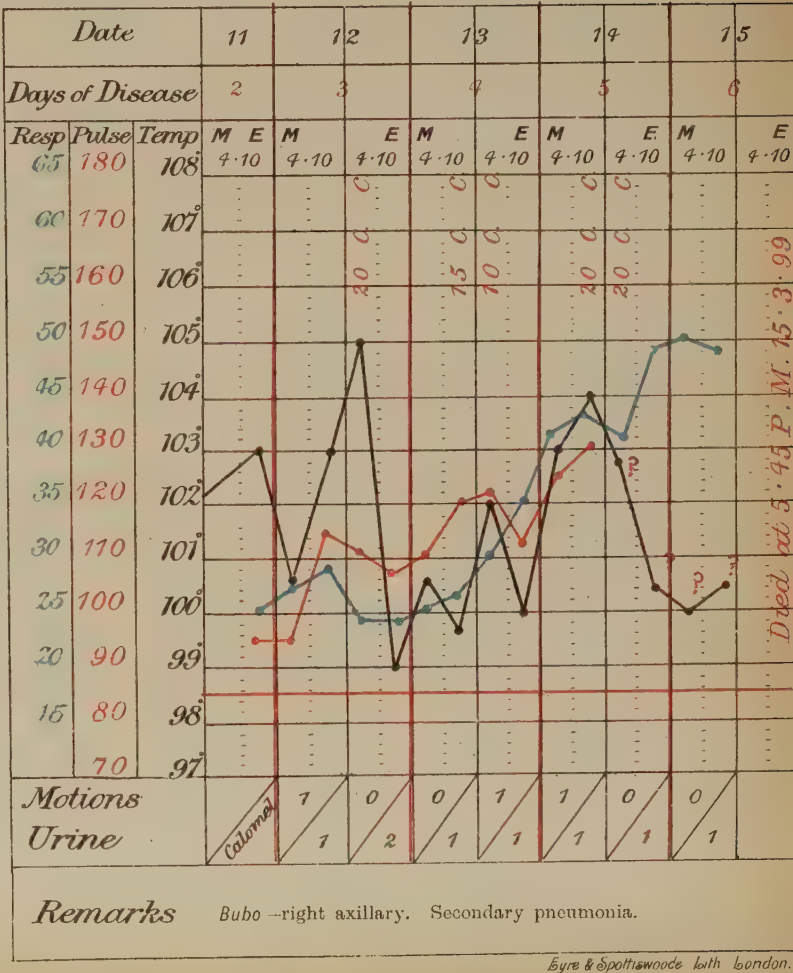
Date of Admission—11th March 1899, at 4.10 p.m.
General Number—999 *DIAGNOSIS*—Plague.
Name—BIPTEE MEHEGE. *Sex*—Male. *Age*—20.
Occupation—Labourer. *Caste*—Hindu.
Previous Duration—3 Days.



Remarks *Bubo*—right inguinal. Septic type; buboes resolved; good recovery.

21.

Date of Admission—11th March 1899, at 6.45 p.m.
General Number—1003. *DIAGNOSIS*—Plague.
Name—DAGDOO SADOO. *Sex*—Male. *Age*—30.
Occupation—Labourer. *Caste*—Hindu.
Previous Duration—2 Days.



Remarks *Bubo*—right axillary. Secondary pneumonia.

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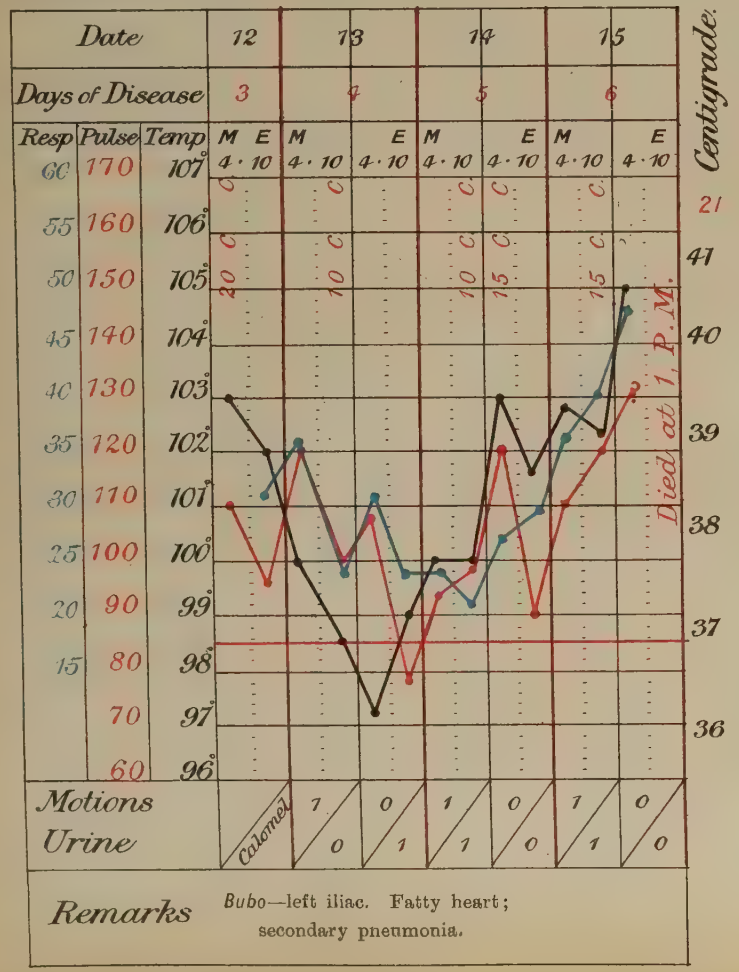
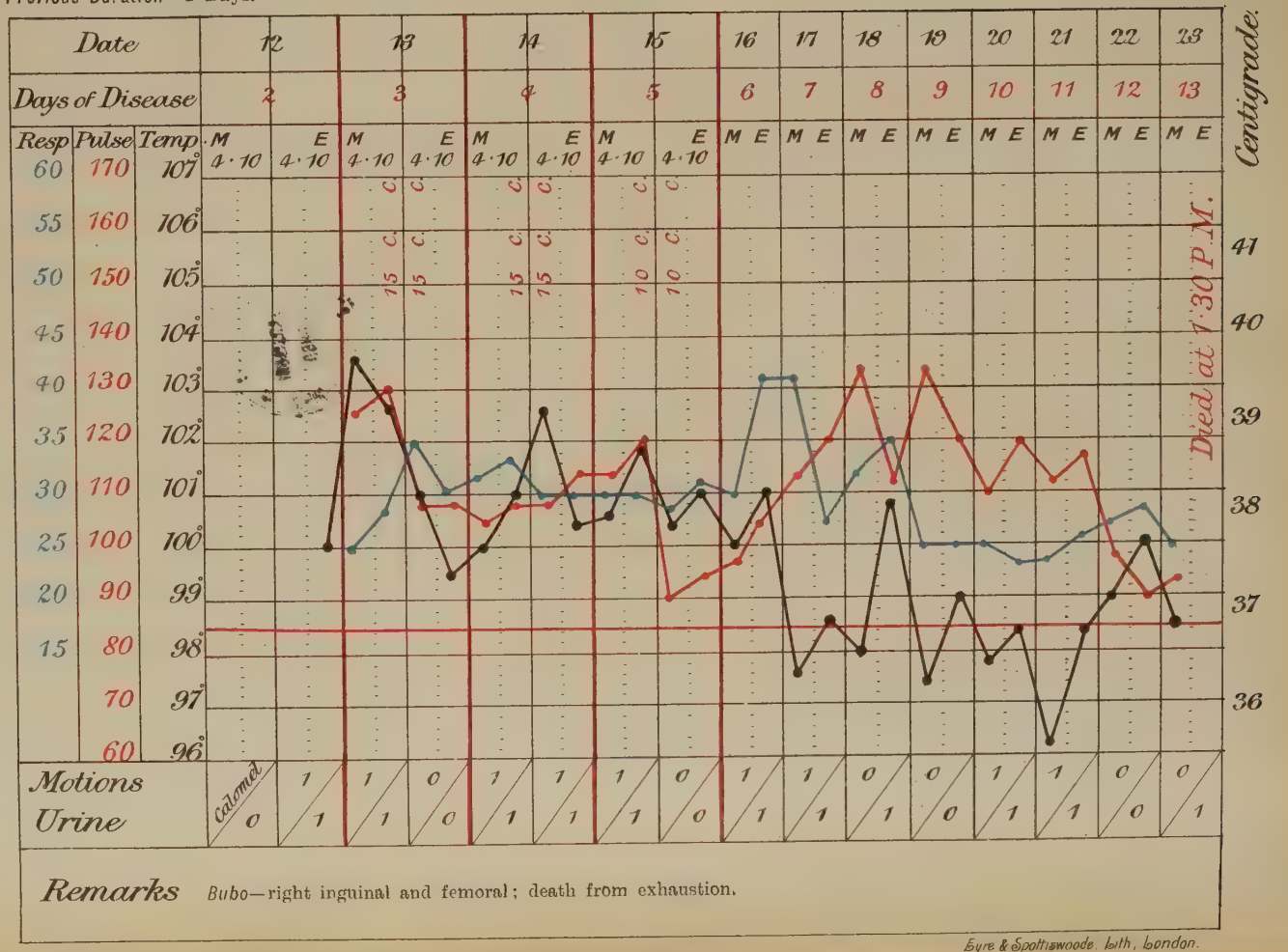
(Dr. CHOKSY.)

CLINICAL CHARTS OF PLAGUE CASES TREATED WITH LUSTIG'S SERUM.

22. *Date of Admission*—12th March 1899, at 3.30 p.m.
General Number—1013. *DIAGNOSIS*—Plague.
Name—LAWRENCE CAITAN. *Sex*—Male. *Age*—45.
Occupation—Clerk. *Caste*—Christian.
Previous Duration—3 Days.



23. *Date of Admission*—12th March 1899, at 10.25 p.m.
General Number—1017. *DIAGNOSIS*—Plague
Name—KESOO LALLA. *Sex*—Male. *Age*—60.
Occupation—Domestic Servant. *Caste*—Hindu.
Previous Duration—2 Days.



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APPENDIX No. LXIII. (*See Question No.* 20,938.)

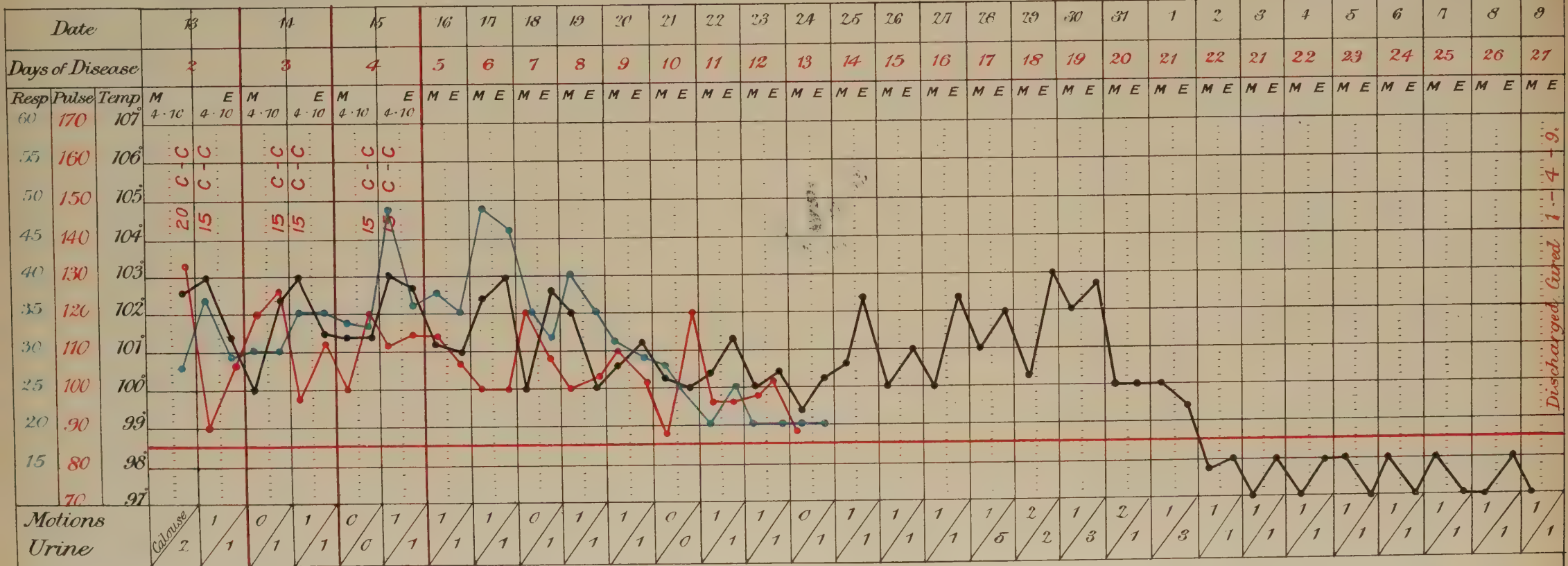
Municipal Hospital for Infectious Diseases, Arthur Road, Bombay.

(Dr. CHOKSY.)

CLINICAL CHARTS OF PLAGUE CASES TREATED WITH LUSTIG'S SERUM.

24. *Date of Admission*—13th March 1899, at 9 a.m. *General Number*—1019. *DIAGNOSIS*—Plague.

Name—PANDOO JANOO. *Sex*—Male. *Age*—20. *Occupation*—Labourer. *Caste*—Hindu. *Previous Duration*—2 Days.



Remarks *Bubo*—right inguinal; plague bronchitis; arthritis; buboes resolved; good recovery.

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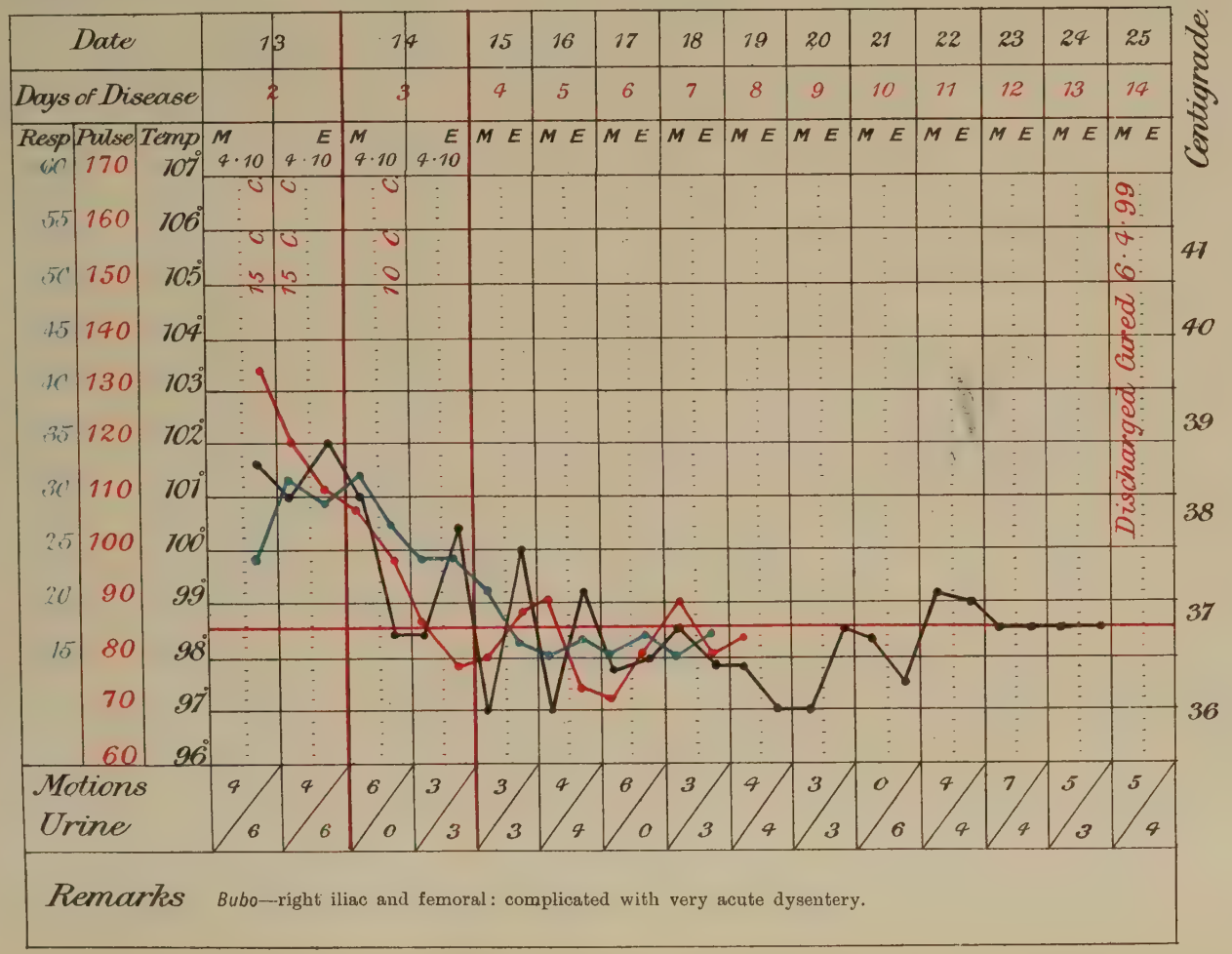
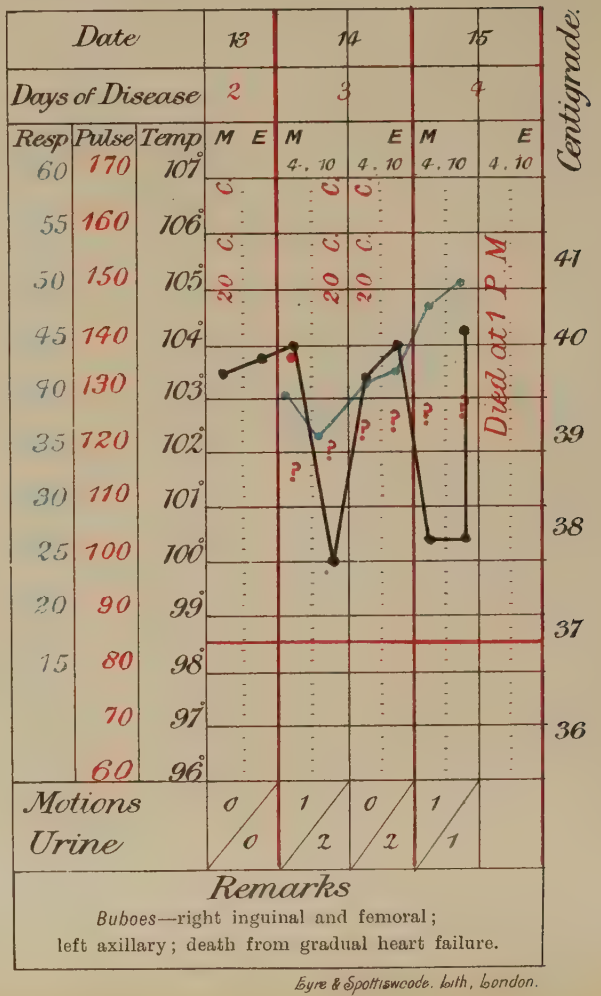
(Dr. CHOKSY.)

CLINICAL CHARTS OF PLAGUE CASES TREATED WITH LUSTIG'S SERUM.

25. *Date of Admission*—13th March 1899, at 9.55 a.m.
General Number—1023. *DIAGNOSIS*—Plague.
Name—BHOWANISHANKER H. *Sex*—Male. *Age*—17.
Occupation—Bookseller. *Caste*—Hindu.
Previous Duration—2 Days.



26. *Date of Admission*—13th March 1899, at 5 p.m.
General Number—1034. *DIAGNOSIS*—Plague.
Name—NANA BHANA. *Sex*—Male. *Age*—25.
Occupation—Syce. *Caste*—Hindu.
Previous Duration—2 Days.



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APPENDIX No. LXIII. *(See Question No. 20,938.)*

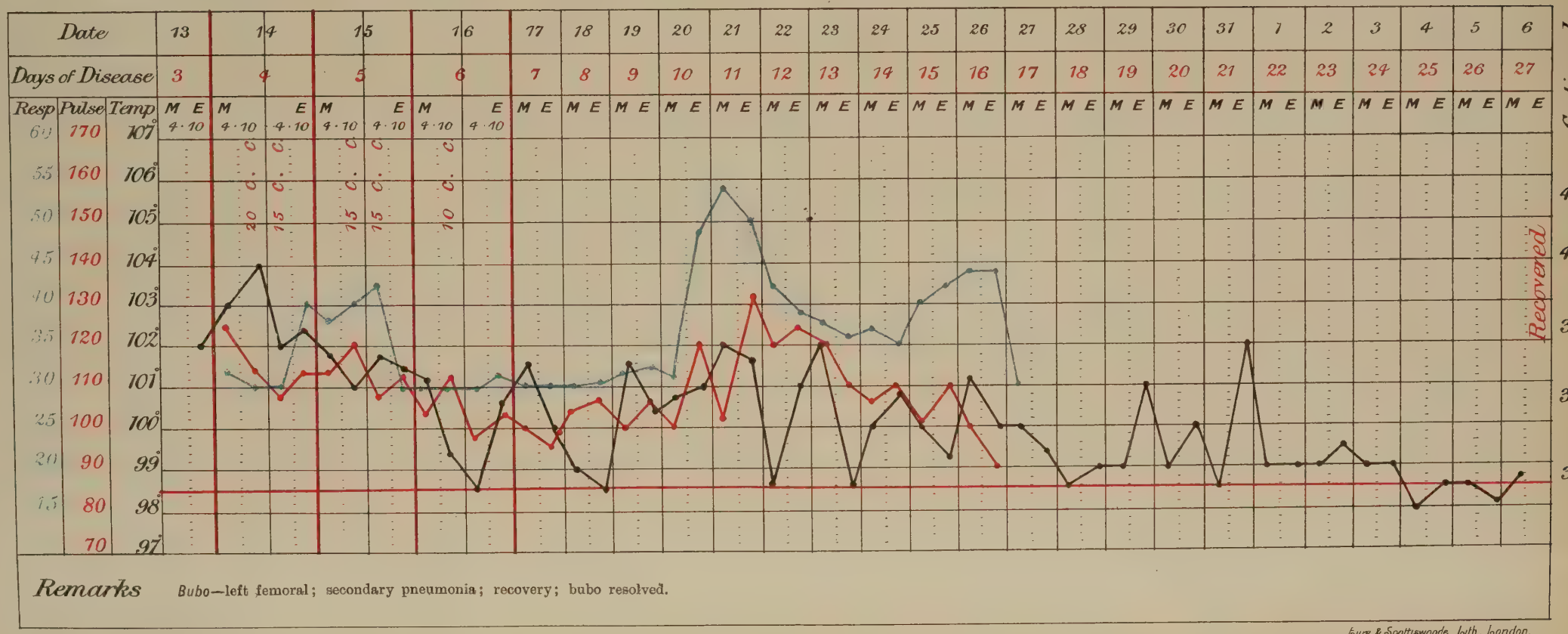
Municipal Hospital for Infectious Diseases, Arthur Road, Bombay.

(Dr. CHOKSY.)

CLINICAL CHARTS OF PLAGUE CASES TREATED WITH LUSTIG'S SERUM.

27. *Date of Admission*—13th March 1899, at 7.15 p.m. *General Number*—1035. *DIAGNOSIS*—Plague.

Name—GUNIA SADOO. *Sex*—Male. *Age*—35. *Occupation*—Beggar. *Caste*—Hindu. *Previous Duration*—3 Days.



Remarks Bubo—left femoral; secondary pneumonia; recovery; bubo resolved.

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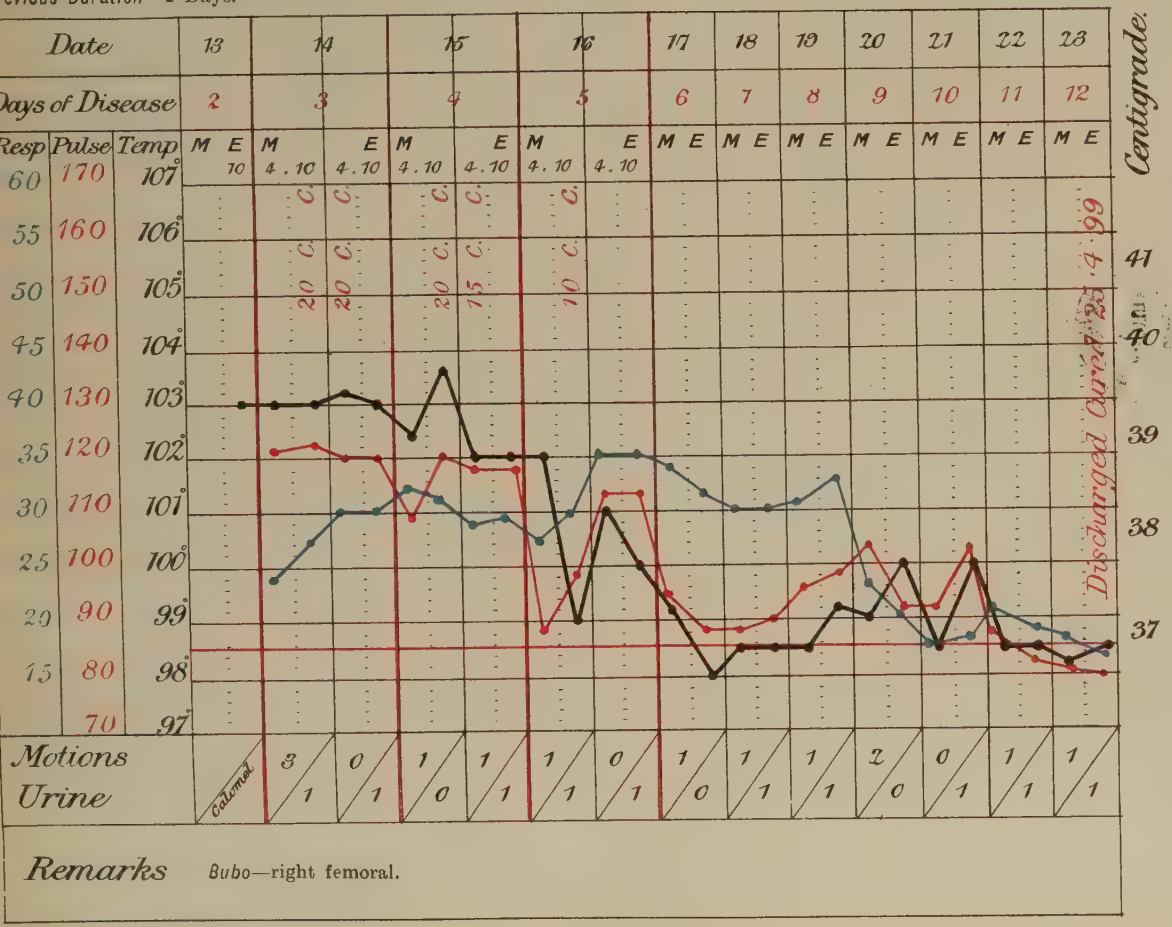
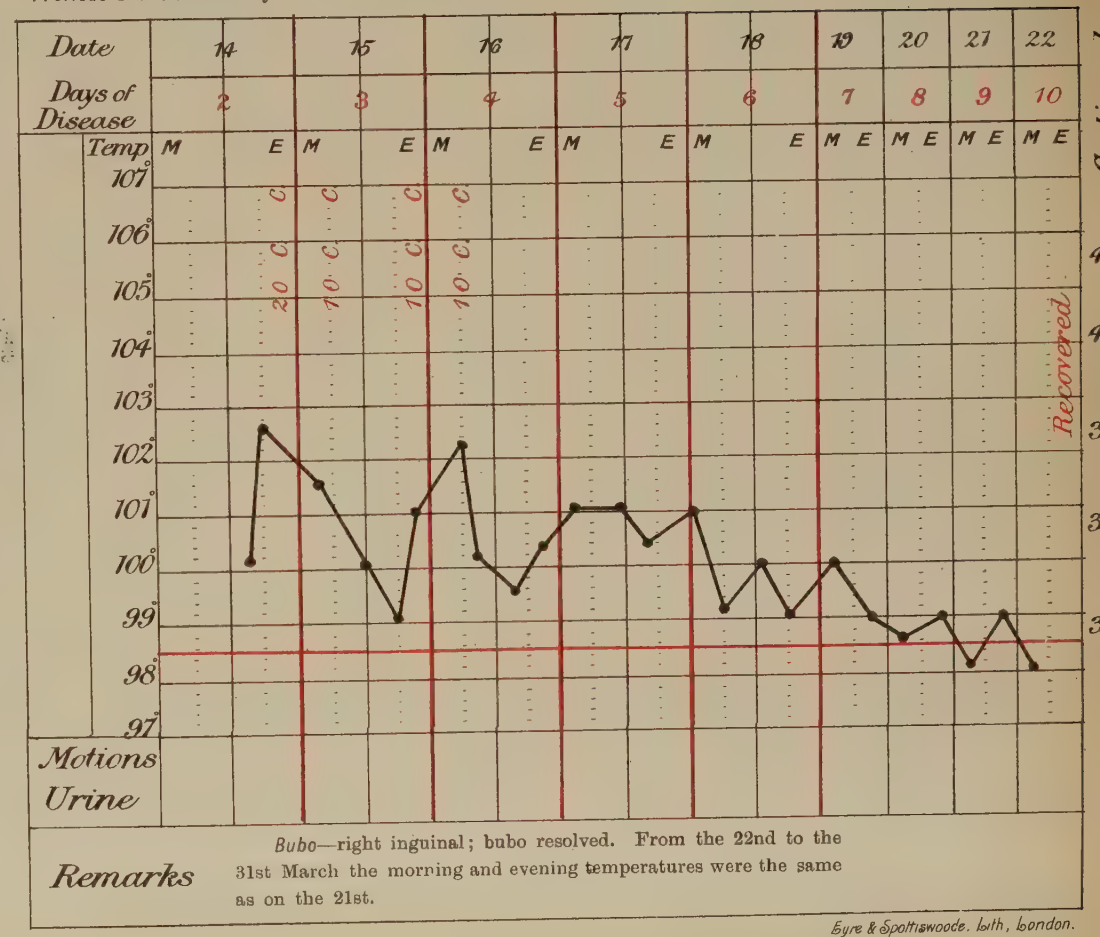
(Dr. CHOKSY.)

CLINICAL CHARTS OF PLAGUE CASES TREATED WITH LUSTIG'S SERUM.

ate of Admission—13th March 1899, at 9 p.m.
eneral Number—1036. *DIAGNOSIS*—Plague.
ame—FRANCIS XAVIER F. *Sex*—Male. *Age*—20.
ccupation—Fitter. *Caste*—Christian.
revious Duration—2 Days.



29. *Date of Admission*—14th March 1899.
General Number—1048. *DIAGNOSIS*—Plague.
Name—APPA BHAU. *Sex*—Male. *Age*—20.
Occupation—Mill Hand. *Caste*—Hindu.
Previous Duration—2 Days.



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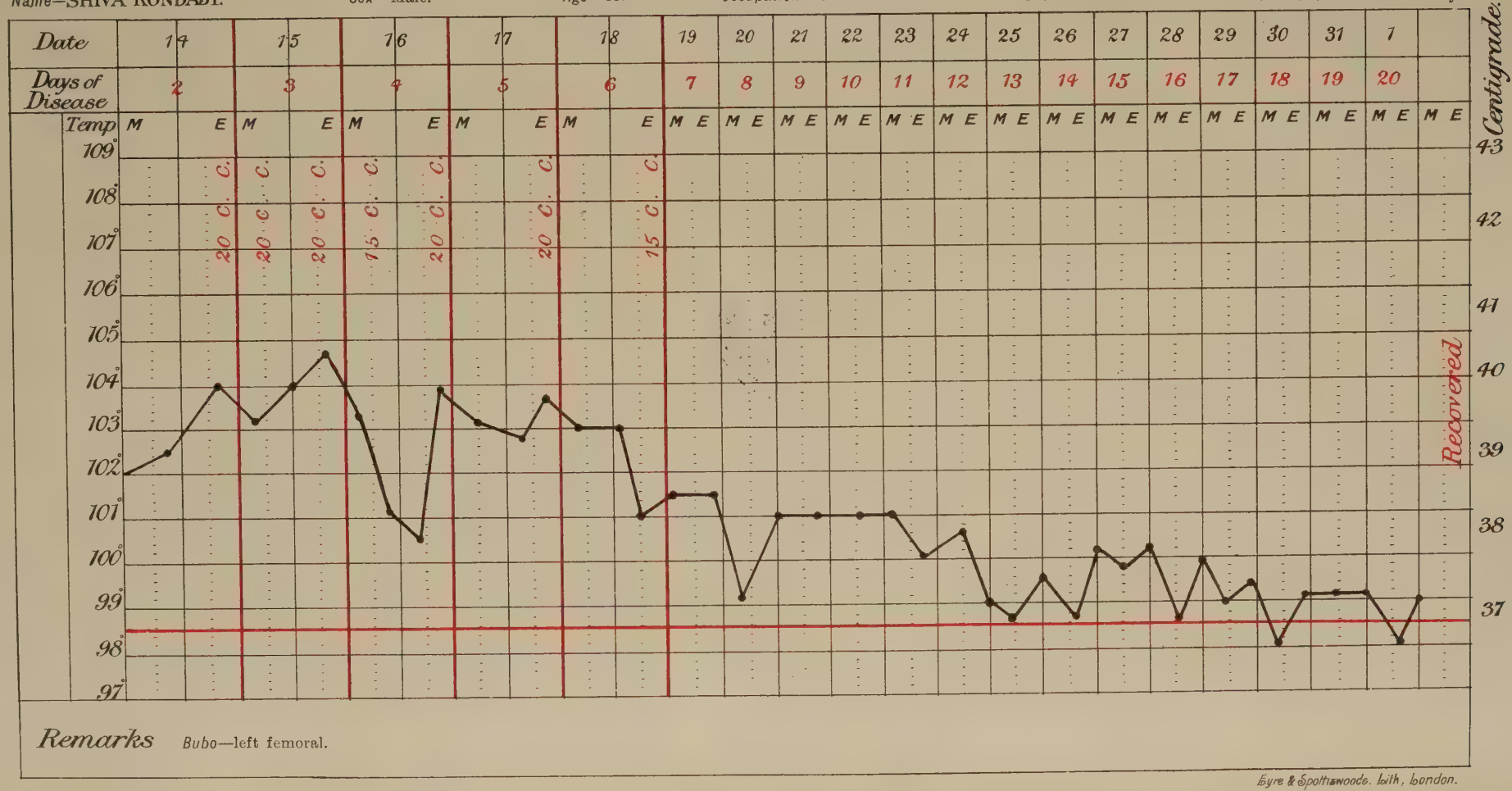
CLINICAL CHARTS OF PLAGUE CASES TREATED WITH LUSTIG'S SERUM.

30. *Date of Admission*—14th March 1899. *General Number*—1050. *DIAGNOSIS*—Plague.

Name—SHIVA KONDAJI. *Sex*—Male. *Age*—35. *Occupation*—Coolie. *Caste*—Hindu. *Previous Duration*—2 Days.



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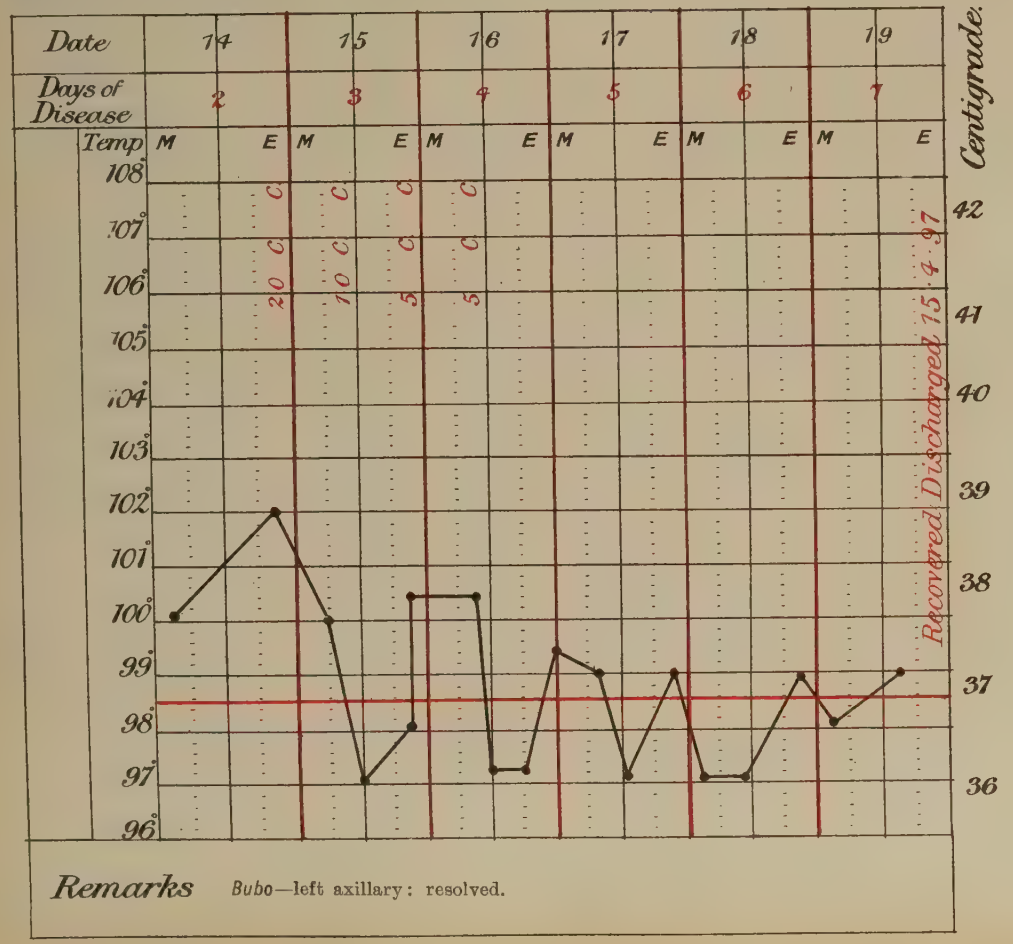
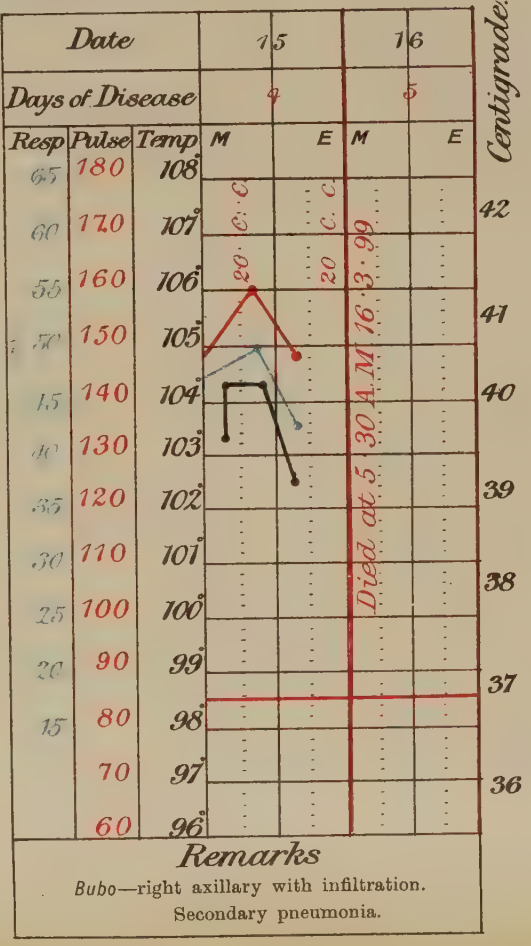
CLINICAL CHARTS OF PLAGUE CASES TREATED WITH LUSTIG'S SERUM.

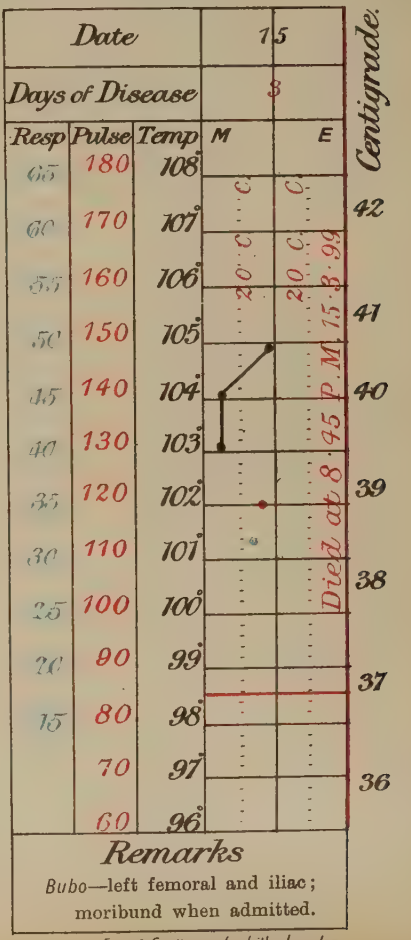
31. *Date of Admission*—14th March 1899.
General Number—1053. *DIAGNOSIS*—Plague.
Name—CYPRIAN F. *Sex*—Male. *Age*—25.
Occupation—Cook. *Caste*—Christian.
Previous Duration—2 Days.



32. *Date of Admission*—15th March 1899, at 10.20 a.m.
General Number—1064. *DIAGNOSIS*—Plague.
Name—SOOKAI JAHIRI. *Sex*—Male. *Age*—30.
Occupation—Labourer. *Caste*—Hindu.
Previous Duration—4 Days.



33. *Date of Admission*—15th March 1899, at 10.25 a.m.
General Number—1065. *DIAGNOSIS*—Plague.
Name—JOHN DE COSTA. *Sex*—Male. *Age*—30.
Occupation—Cook. *Caste*—Christian.
Previous Duration—3 Days.



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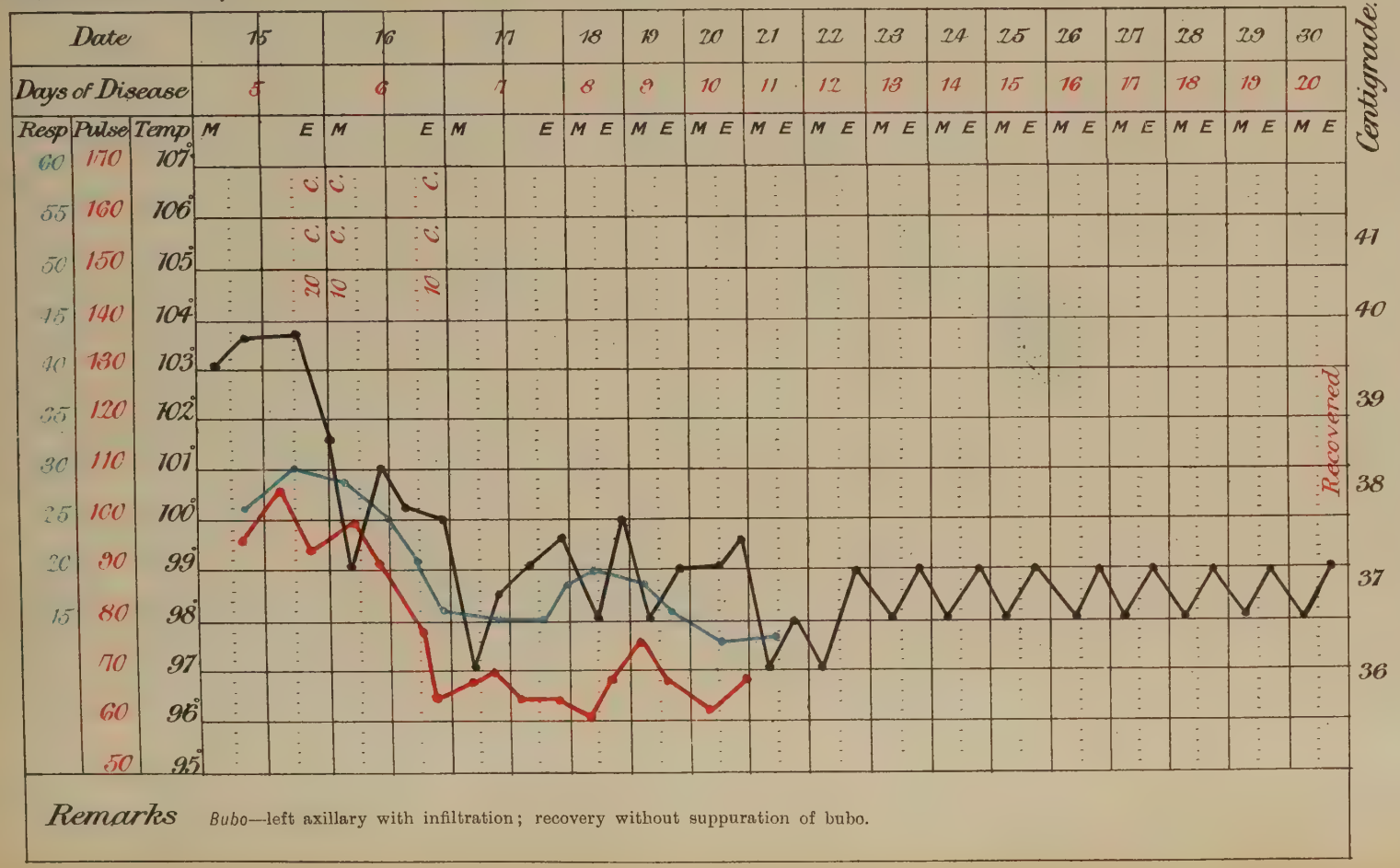
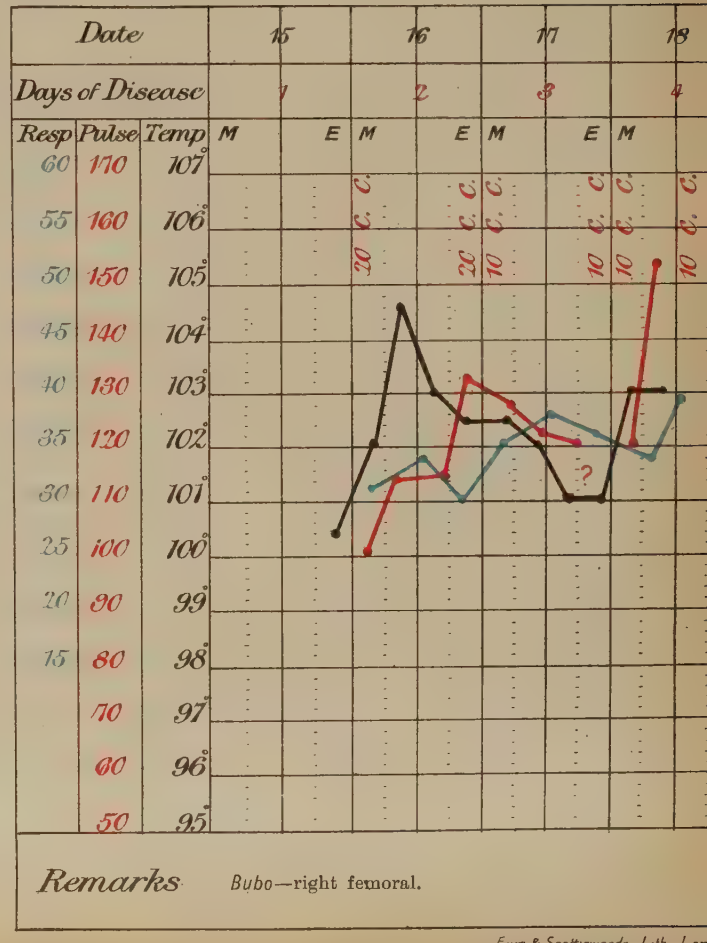
CLINICAL CHARTS OF PLAGUE CASES TREATED WITH LUSTIG'S SERUM.

34. *Date of Admission*—15th March 1899, at 10.45 a.m.
General Number—1066. *DIAGNOSIS*—Plague.
Name—DERIA ISRA. *Sex*—Male. *Age*—23.
Occupation—Domestic Servant. *Caste*—Hindu.
Previous Duration—5 Days.



Remarks Bubo—left axillary with infiltration; recovery without suppuration of bubo.

35. *Date of Admission*—15th March 1899, at 8.50 p.m.
General Number—1080. *DIAGNOSIS*—Plague.
Name—CYPRIAN ALFONSO. *Sex*—Male. *Age*—40.
Occupation—Broker. *Caste*—Christian.
Previous Duration—1 Day.



Remarks Bubo—right femoral.

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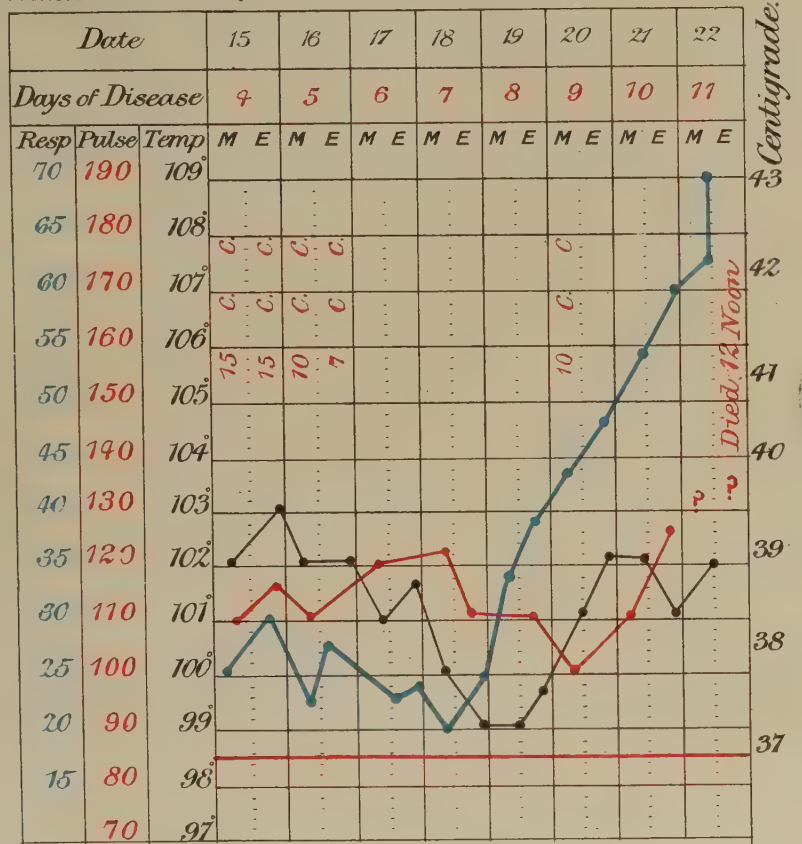
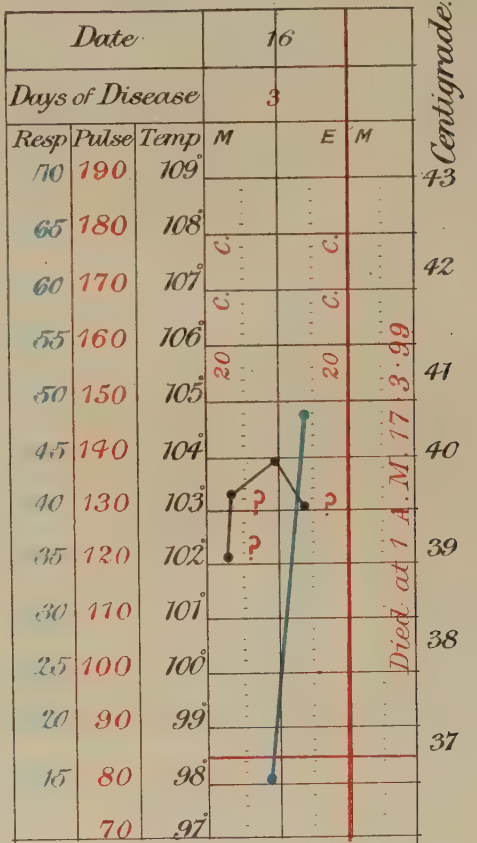
CLINICAL CHARTS OF PLAGUE CASES TREATED WITH LUSTIG'S SERUM.

36. *Date of Admission*—15th March 1899, at
General Number— *DIAGNOSIS*—Plague.
Name—MRS. S. *Sex*—Female. *Age*—56.
Occupation— *Caste*—Parsee.
Previous Duration—4 Days.



37. *Date of Admission*—16th March 1899, at 7.15 a.m.
General Number—1082. *DIAGNOSIS*—Plague.
Name— MAMIAJI YESHRAM. *Sex*—Male. *Age* 15.
Occupation—Labourer. *Caste*—Hindu.
Previous Duration—3 Days.



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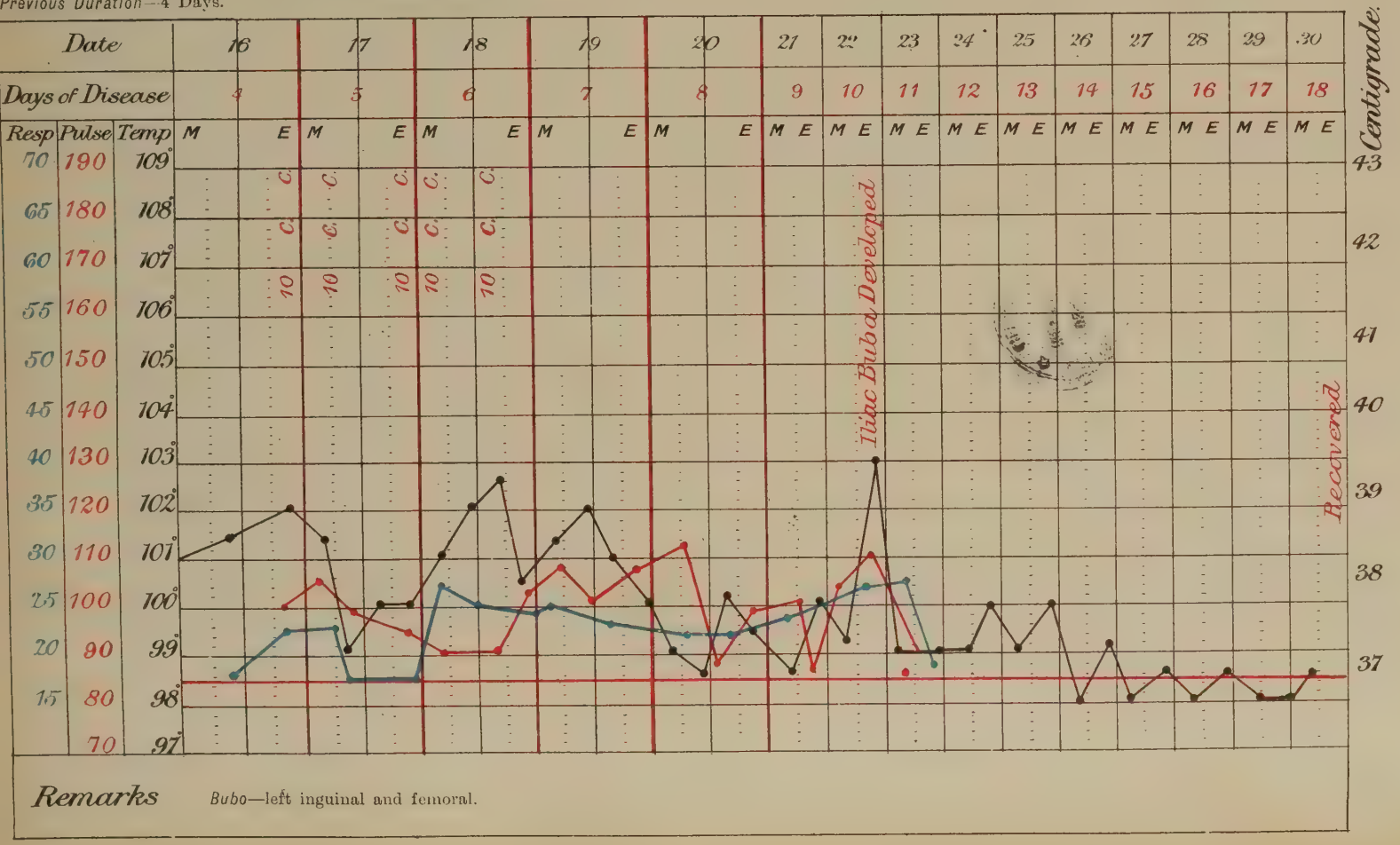
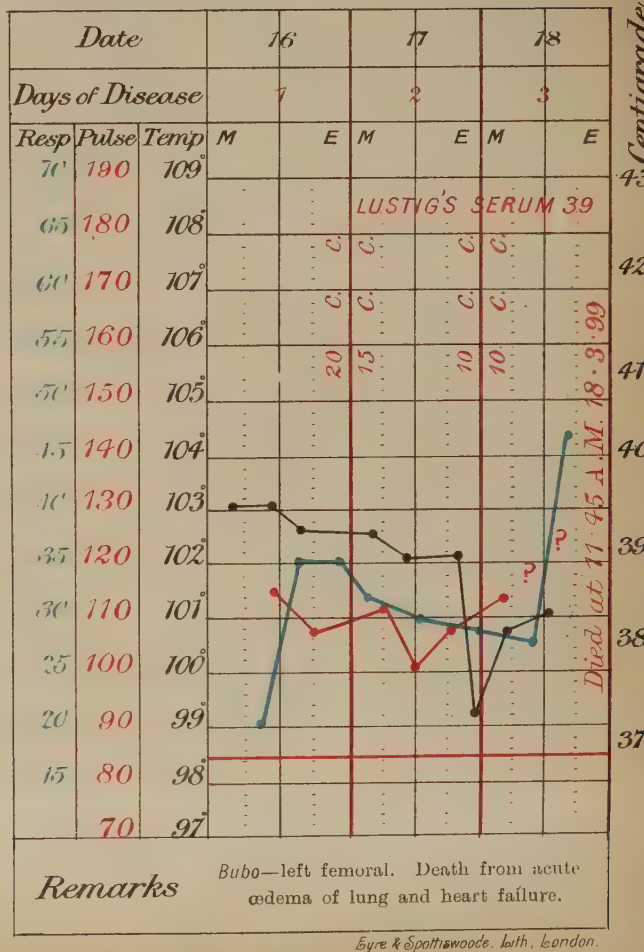
CLINICAL CHARTS OF PLAGUE CASES TREATED WITH LUSTIG'S SERUM.

Date of Admission—16th March 1899, at 10.20 a.m.
General Number—1088. *DIAGNOSIS*—Plague.
Name—BHAGWANDAS RAGHOODAS. *Sex*—Male. *Age*—30.
Occupation—Beggar. *Caste*—Hindu.
Previous Duration—4 Days.



Remarks Bubo—left inguinal and femoral.

39. *Date of Admission*—16th March 1899, at 11.30 a.m.
General Number—1094. *DIAGNOSIS*—Plague.
Name—DOMINGO CAETAN. *Sex*—Male. *Age*—60.
Occupation—Cook. *Caste*—Christian.
Previous Duration—1 Day.



Remarks Bubo—left femoral. Death from acute œdema of lung and heart failure.

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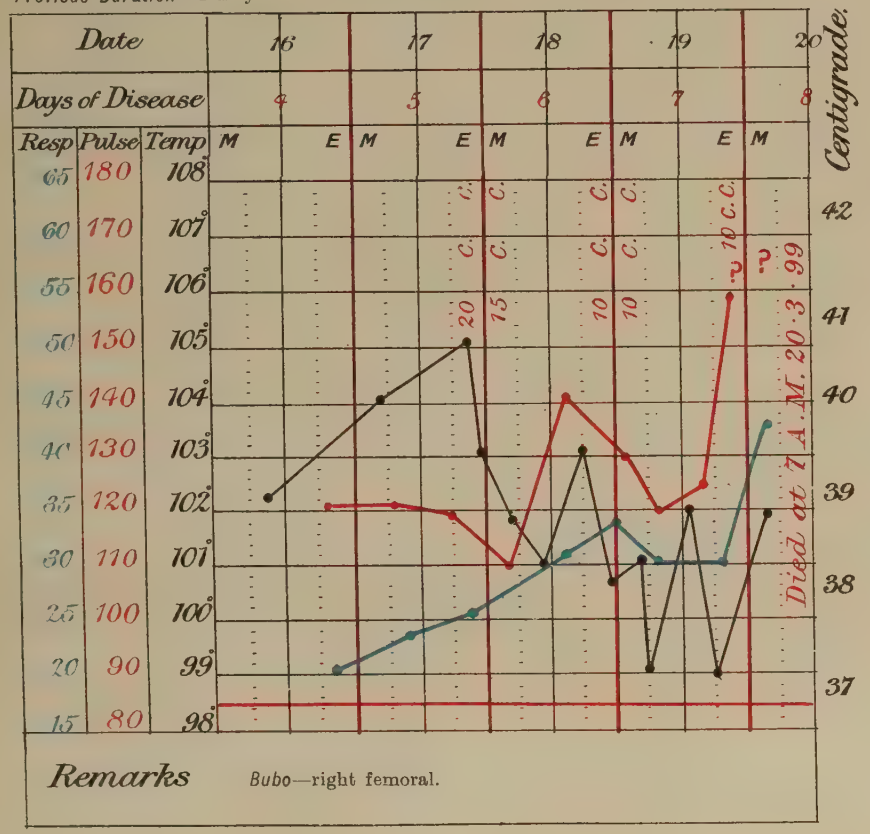
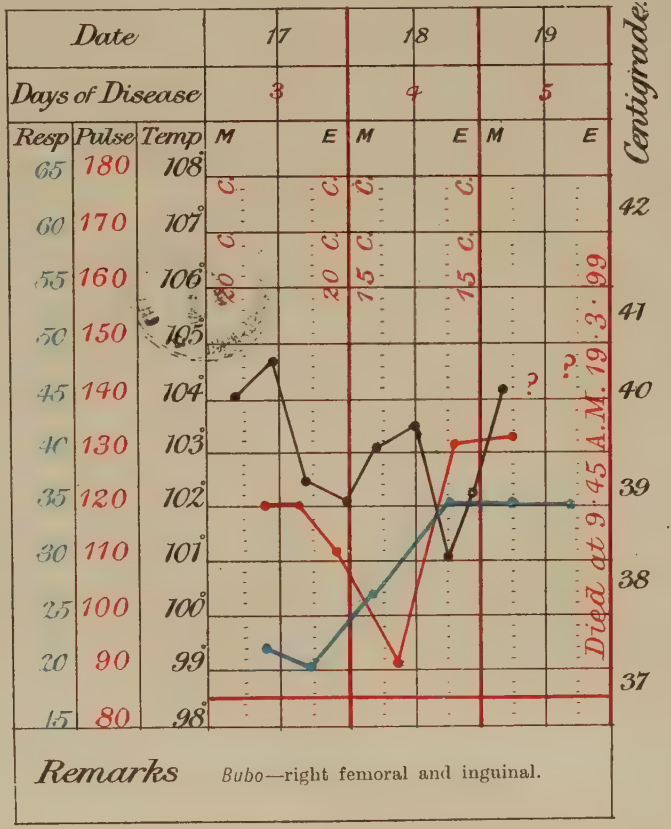
CLINICAL CHARTS OF PLAGUE CASES TREATED WITH LUSTIG'S SERUM.

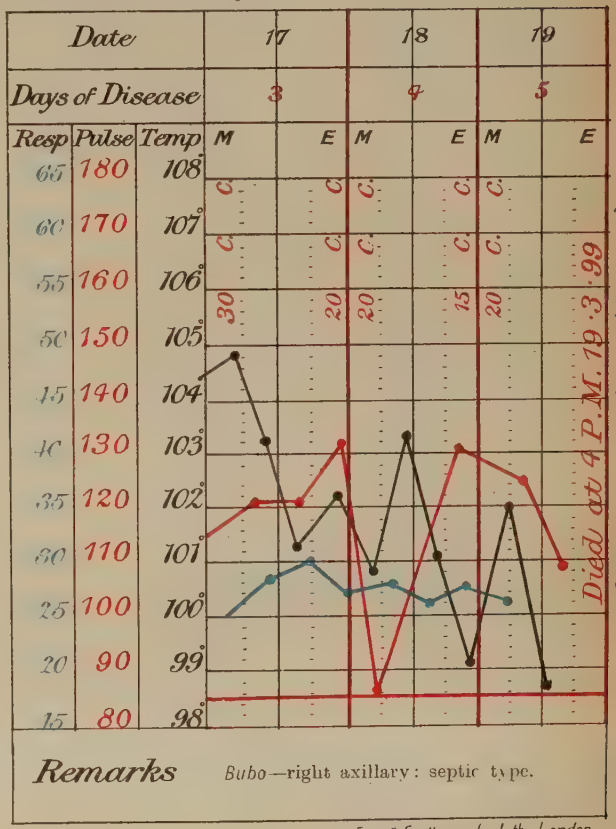
40. *Date of Admission*—16th March 1899, at 10.10 p.m.
General Number—1099. *DIAGNOSIS*—Plague.
Name—GANDARAM KARAMCHAND. *Sex*—Male. *Age*—30.
Occupation—Merchant. *Caste*—Hindu.
Previous Duration—4 Days.



41. *Date of Admission*—17th March 1899, at 8 a.m.
General Number—1101. *DIAGNOSIS*—Plague.
Name—KRISHNA VITHU *Sex*—Male *Age*—40.
Occupation—Domestic Servant. *Caste*—Hindu.
Previous Duration—3 Days.



42. *Date of Admission*—17th March 1899, at 9.10 a.m.
General Number—1103. *DIAGNOSIS*—Plague.
Name—COSME DAMIEN DE SA. *Sex*—Male. *Age*—23.
Occupation—Billiard Marker. *Caste*—Christian.
Previous Duration—3 Days.



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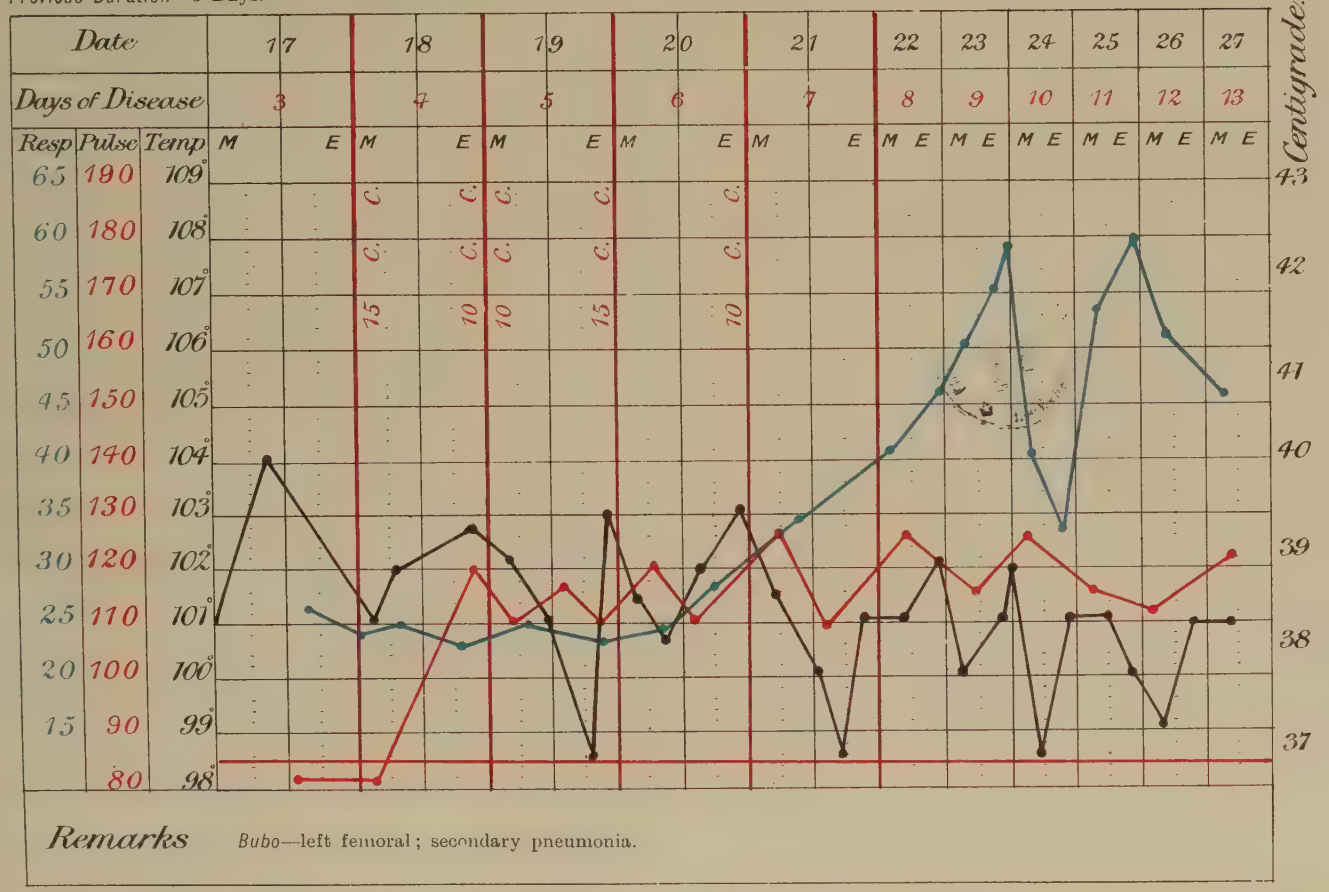
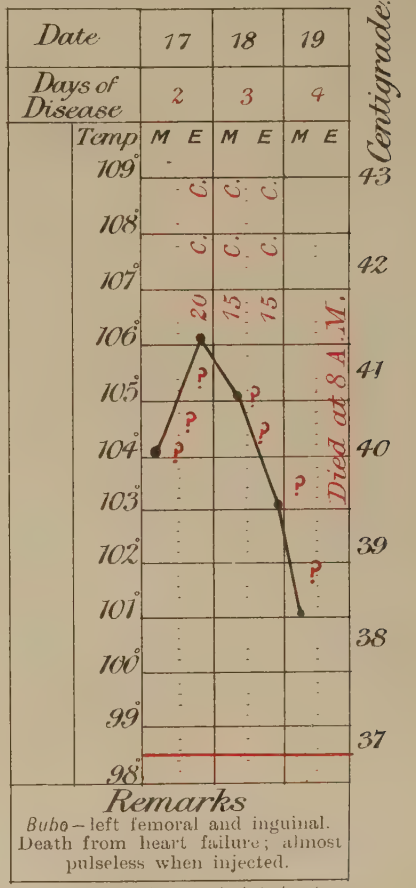
CLINICAL CHARTS OF PLAGUE CASES TREATED WITH LUSTIG'S SERUM.

43. *Date of Admission*—17th March 1899, at 10.10 a.m.
General Number—1104. *DIAGNOSIS*—Plague.
Name—BABAJI ZORA. *Sex*—Male. *Age*—40.
Occupation—Barber. *Caste*—Hindu.
Previous Duration—3 Days.



Remarks *Bubo*—left femoral; secondary pneumonia.

44. *Date of Admission*—17th March 1899.
General Number— *DIAGNOSIS*—Plague.
Name—Miss J. B. S. *Sex*—Female. *Age* 16.
Occupation— *Caste*—Parsee.
Previous Duration—2 Days.



Remarks
Bubo—left femoral and inguinal.
Death from heart failure; almost pulseless when injected.

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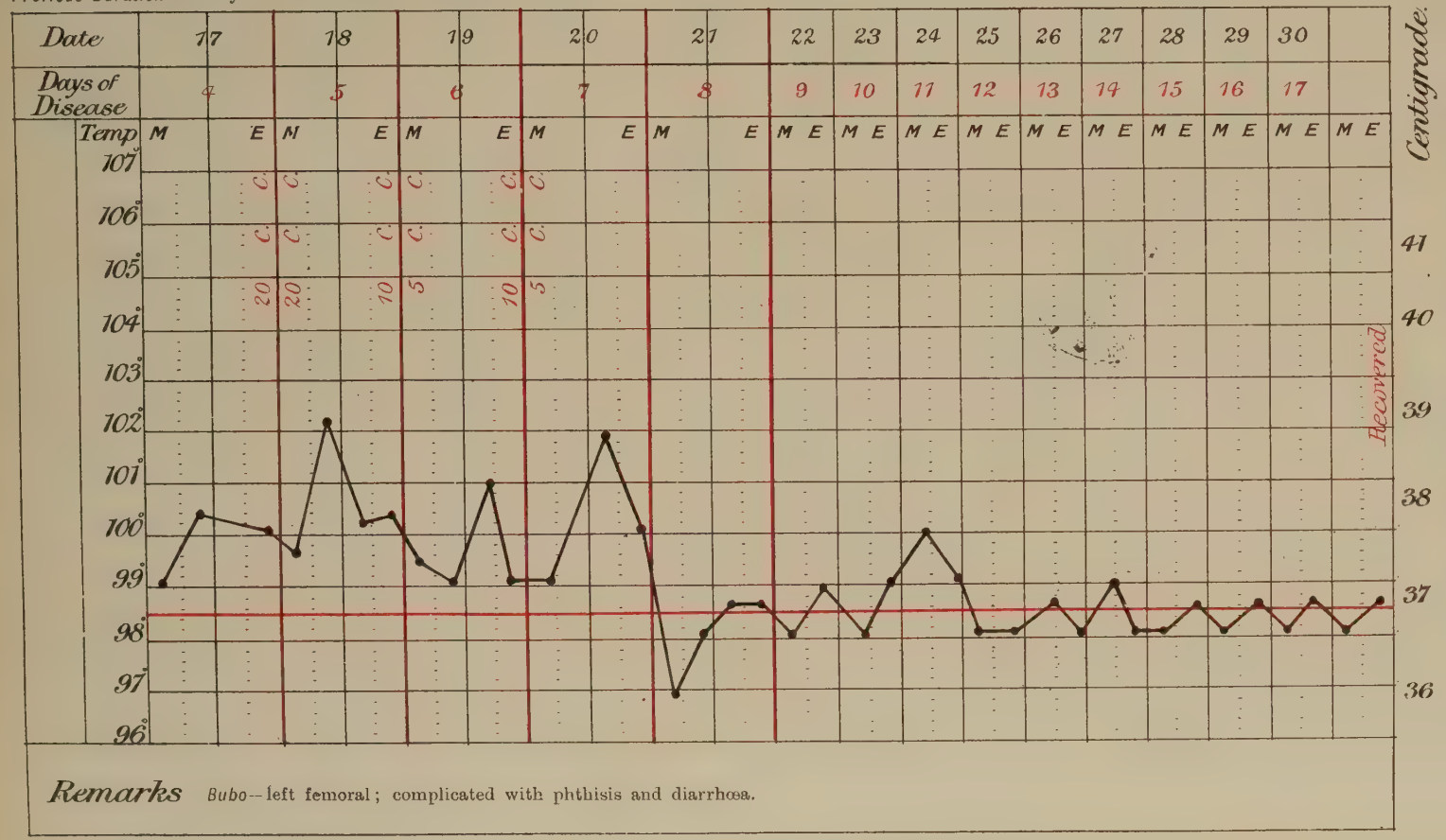
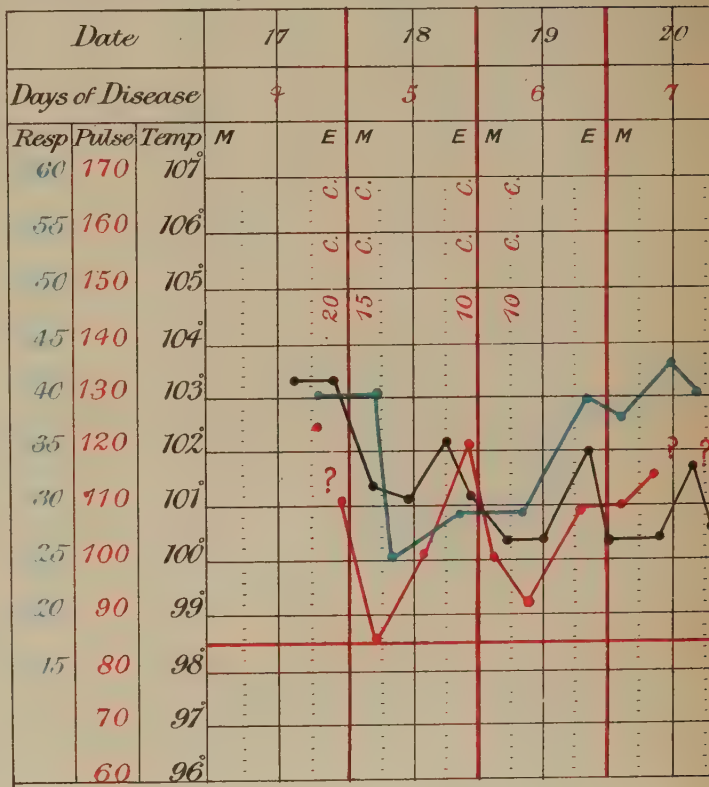
CLINICAL CHARTS OF PLAGUE CASES TREATED WITH LUSTIG'S SERUM.

5. *Date of Admission*—17th March 1899.
General Number—1112. *DIAGNOSIS*—Plague.
Name—Mrs. MOSES. *Sex*—Female. *Age*—30.
Occupation—Nurse. *Caste*—Christian.
Previous Duration—4 Days.



Remarks Bubo—left femoral; complicated with phthisis and diarrhœa.

46. *Date of Admission*—17th March 1899, at 6.10 p.m.
General Number—1116. *DIAGNOSIS* Plague.
Name—WALLABH PREMA. *Sex*- Male. *Age*—40.
Occupation—Groom. *Caste*- Hindu.
Previous Duration—4 Days.



Remarks Bubo—right inguinal.

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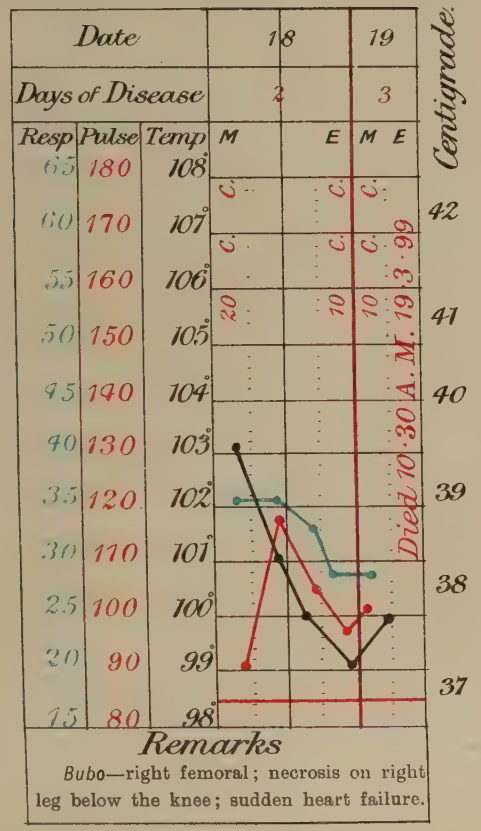
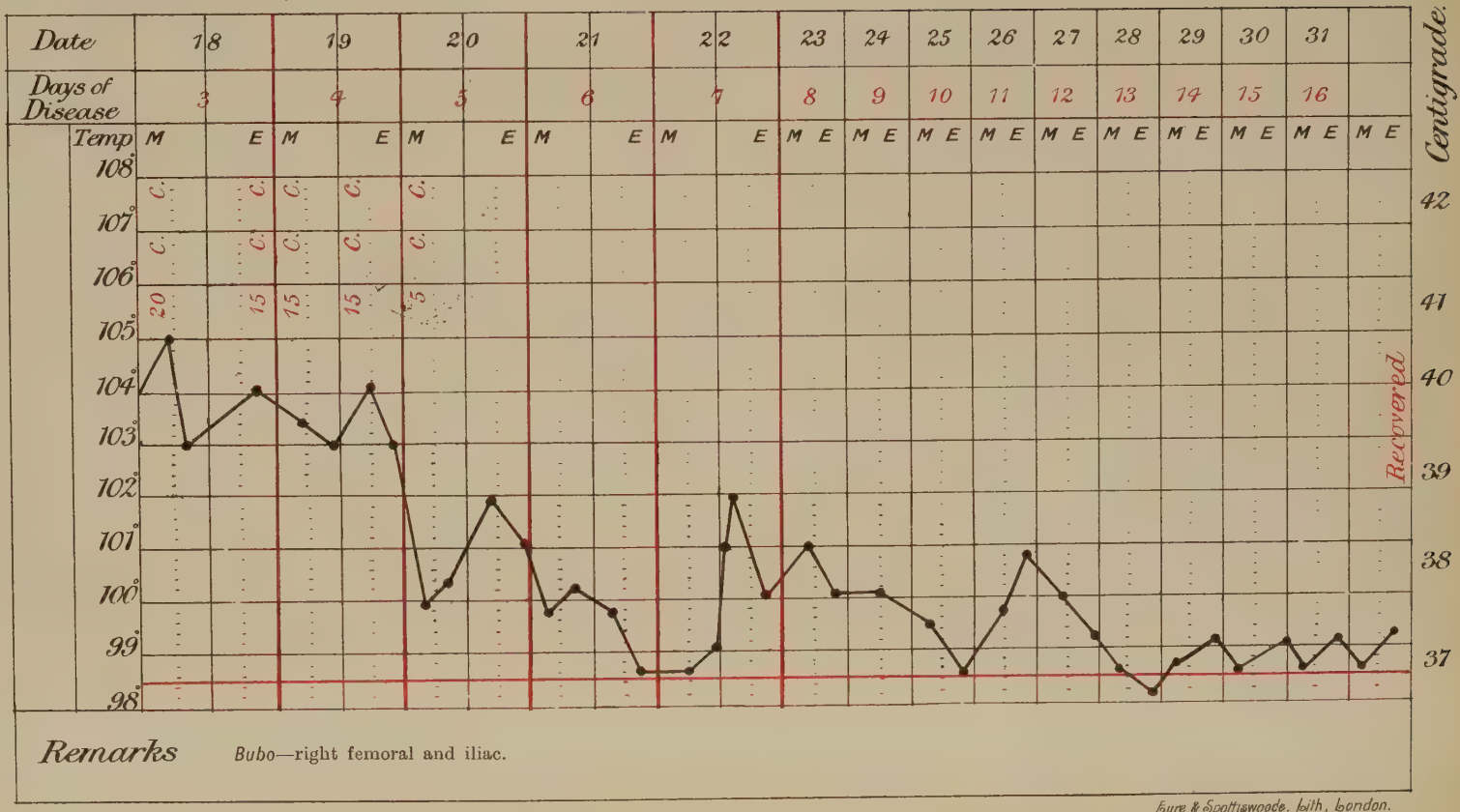
CLINICAL CHARTS OF PLAGUE CASES TREATED WITH LUSTIG'S SERUM.

47. *Date of Admission*—18th March 1899.
General Number—1117. *DIAGNOSIS*—Plague.
Name—PETER FERNANDES. *Sex*—Male. *Age*—30.
Occupation—Butler. *Caste*—Christian.
Previous Duration—2 Days.



48. *Date of Admission*—18th March 1899.
General Number—1118. *DIAGNOSIS*—Plague.
Name—MARUTI GANOO. *Sex*—Female. *Age*—20.
Occupation—Nil. *Caste*—Hindu.
Previous Duration—3 Days.



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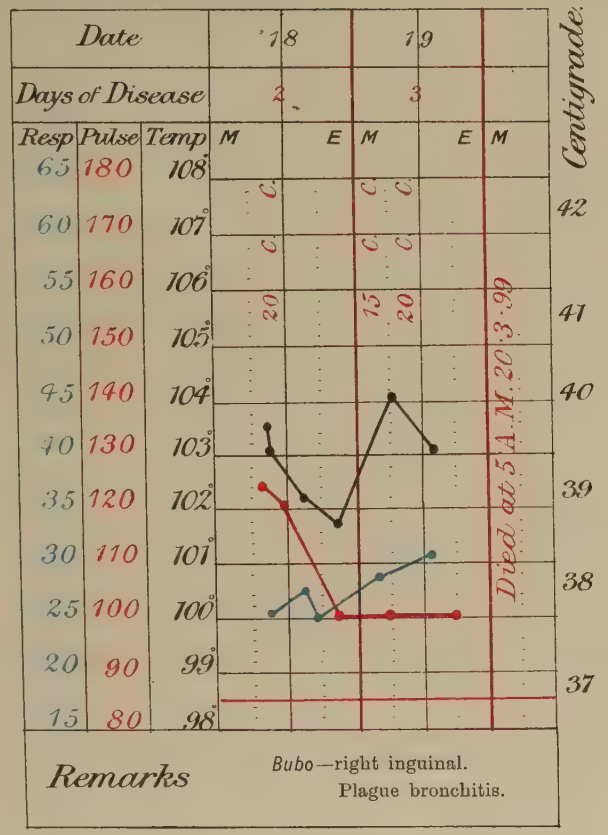
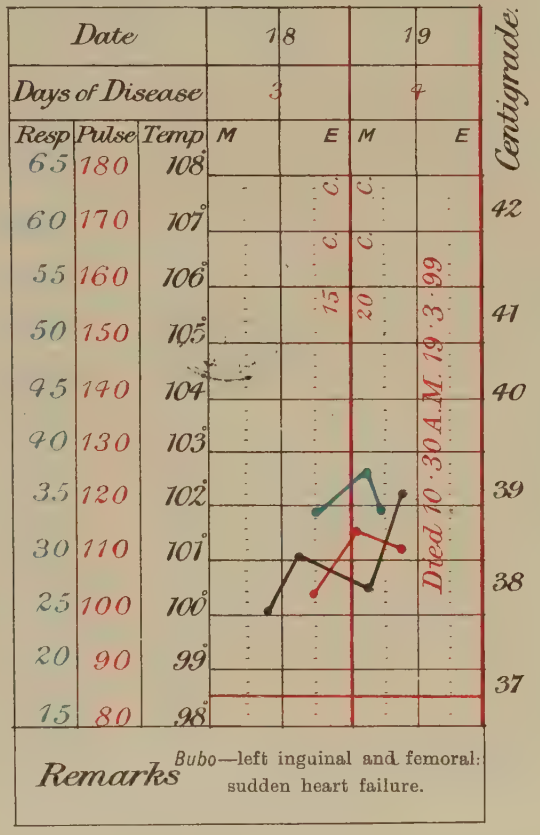
CLINICAL CHARTS OF PLAGUE CASES TREATED WITH LUSTIG'S SERUM.

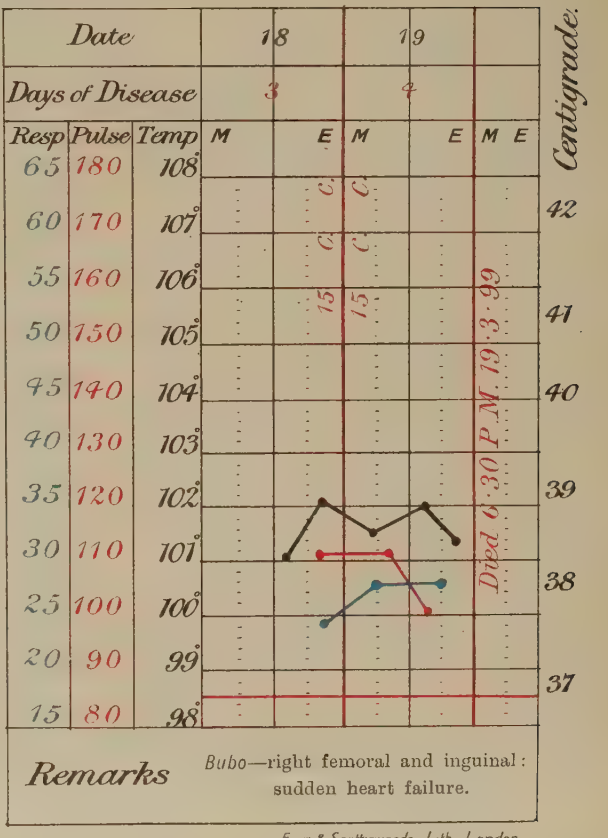
49. *Date of Admission*—18th March 1899.
General Number—1126. *DIAGNOSIS*—Plague.
Name—SAVATRI SAMDA. *Sex*—Female. *Age*—25.
Occupation—Nil. *Caste*—Hindu.
Previous Duration—2 Days.



50. *Date of Admission*—18th March 1899.
General Number—1127. *DIAGNOSIS*—Plague.
Name—A. S. LOBO. *Sex*—Male. *Age*—60.
Occupation—Cook. *Caste*—Christian.
Previous Duration—3 Days.



51. *Date of Admission*—18th March 1899.
General Number—1128. *DIAGNOSIS*—Plague.
Name—SHEICK MADAR. *Sex*—Male. *Age*—30.
Occupation—Malee. *Caste*—Muhammadan.
Previous Duration—3 Days.



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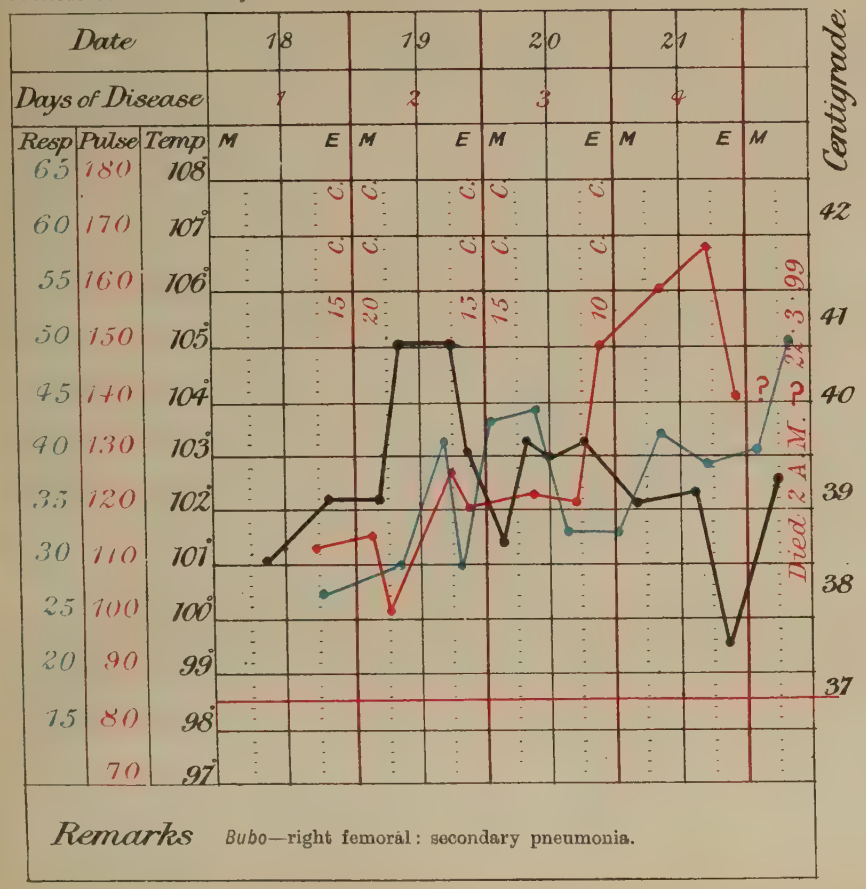
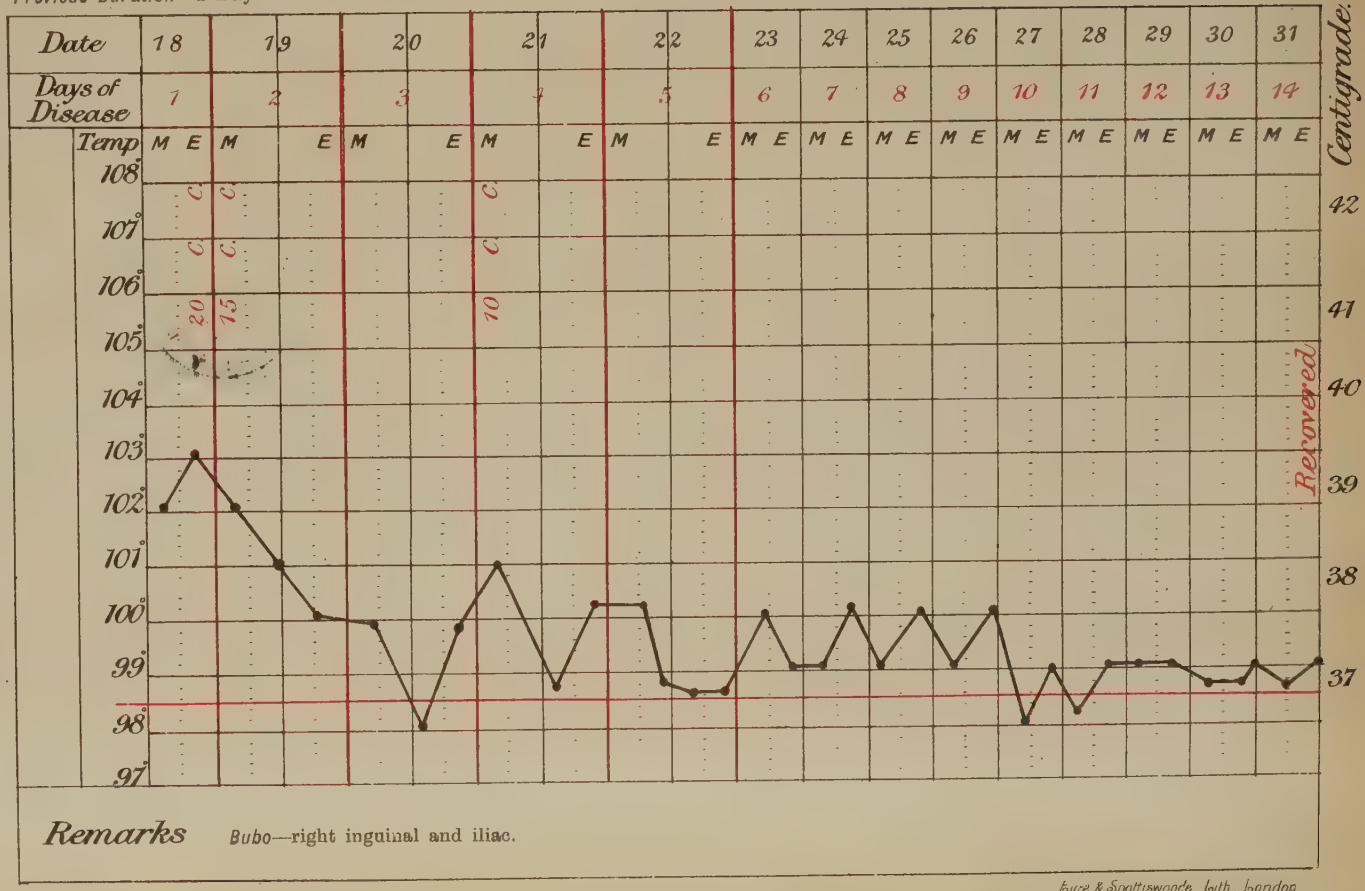
(Dr. CHOKSY.)

CLINICAL CHARTS OF PLAGUE CASES TREATED WITH LUSTIG'S SERUM.

52. *Date of Admission*—18th March 1899.
General Number—1135. *DIAGNOSIS*—Plague.
Name—P. S. DE COSTA. *Sex*—Male. *Age*—42.
Occupation—Baker. *Caste*—Christian.
Previous Duration—1 Day.



53. *Date of Admission*—18th March 1899.
General Number—1132. *DIAGNOSIS*—Plague.
Name—G. F. G. *Sex*—Male. *Age*—35.
Occupation—Nil. *Caste*—Christian.
Previous Duration—1 Day.



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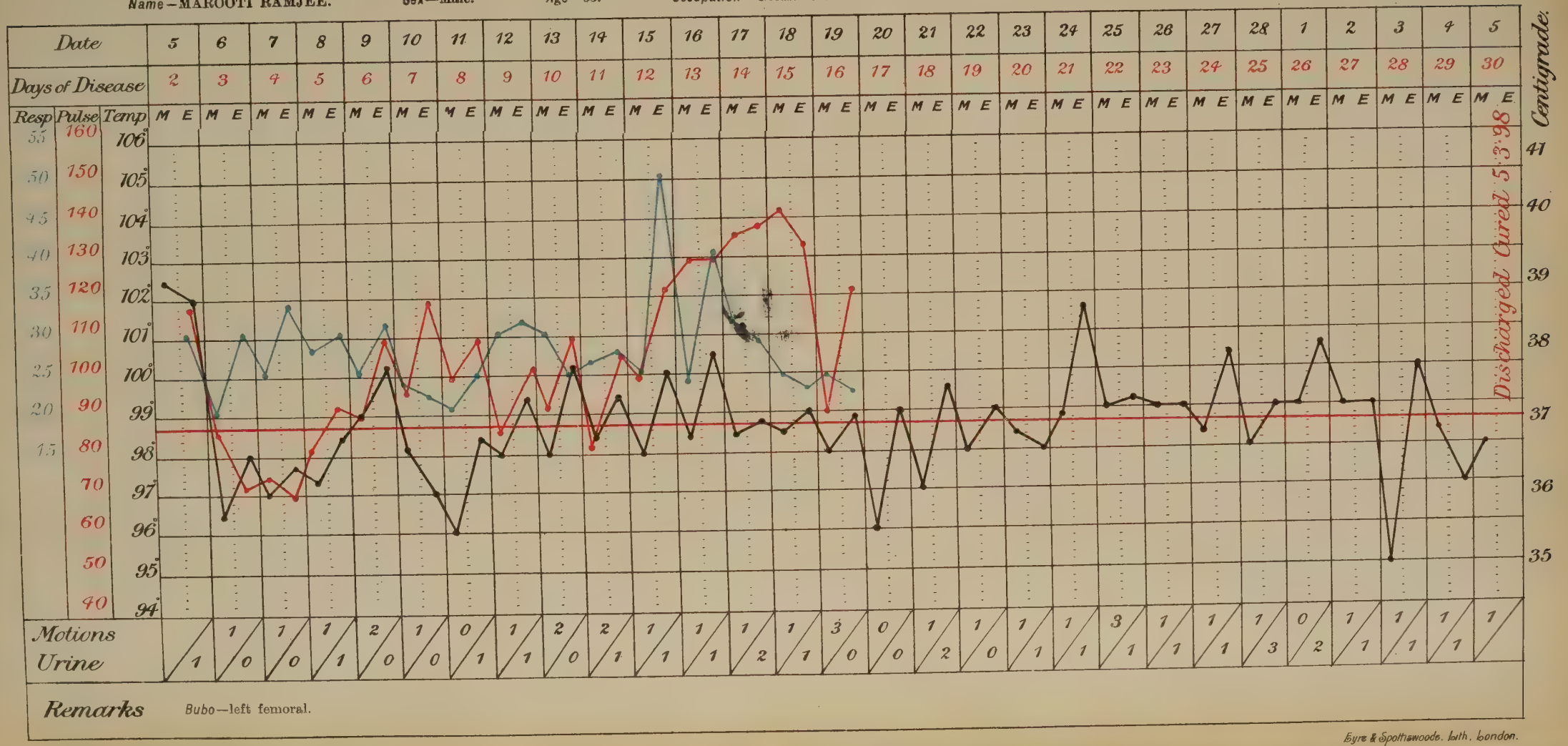
(Dr. CHOKSY.)

CLINICAL CHARTS OF PLAGUE CASES.

1. Date of Admission—5th February 1898, at 12.35 p.m. General Number—1011. DIAGNOSIS—Plague.

Name—MAROOTI RAMJEE. Sex—Male. Age—35. Occupation—Goldsmith. Caste—Hindu. Previous Duration—2 Days.



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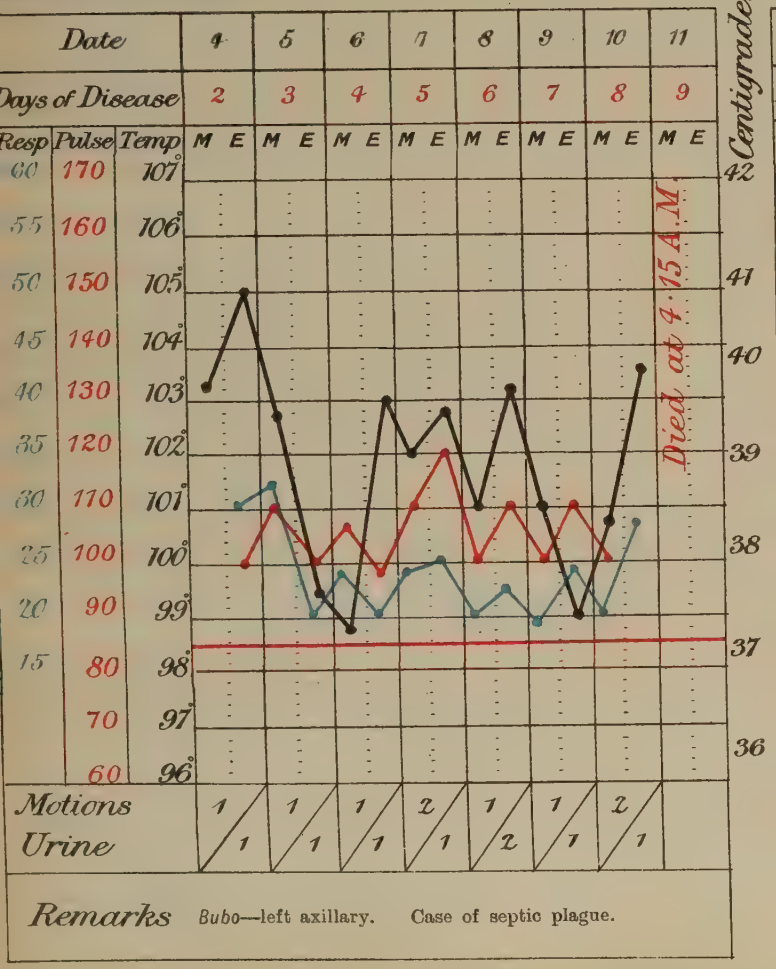
(Dr. CHOKSY.)

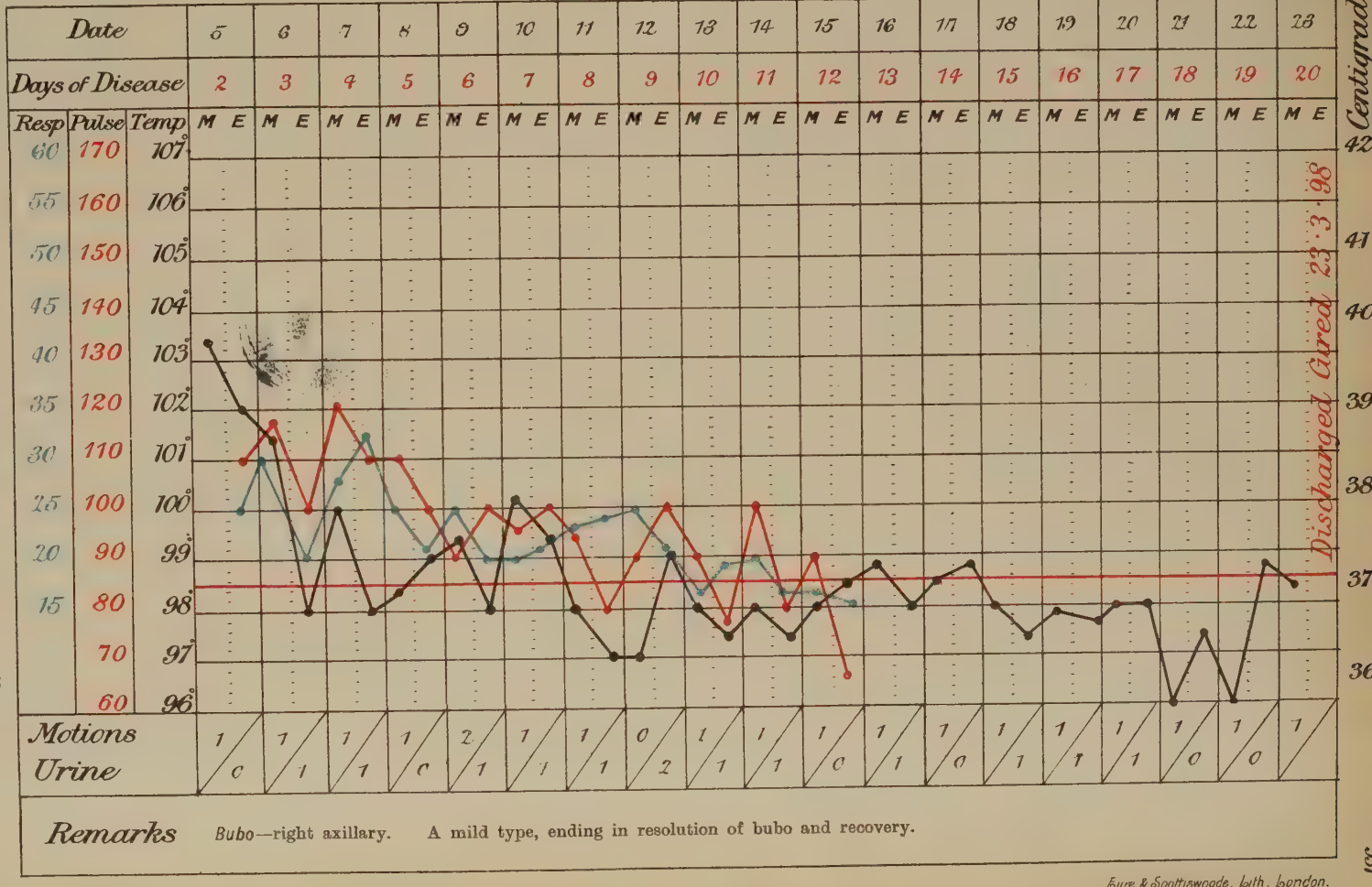
CLINICAL CHARTS OF PLAGUE CASES.

ate of Admission—4th March 1898, at 9.55 a.m.
eneral Number—1922. *DIAGNOSIS*—Plague.
ame—YESSOO, wife of Govind. *Sex*—Female. *Age*—20 years.
ccupation—Nil. *Caste*—Hindu.
revious Duration.—2 Days.



Remarks *Bubo*—left axillary. Case of septic plague.

3. *Date of Admission*—5th March 1898, at 10.25 a.m.
General Number—1957. *DIAGNOSIS*—Plague.
Name—IZABEL, d. of Rosario. *Sex*—Female *Age*—5 years.
Occupation—Nil. *Caste*—Christian.
Previous Duration—2 Days.



Remarks *Bubo*—right axillary. A mild type, ending in resolution of bubo and recovery.

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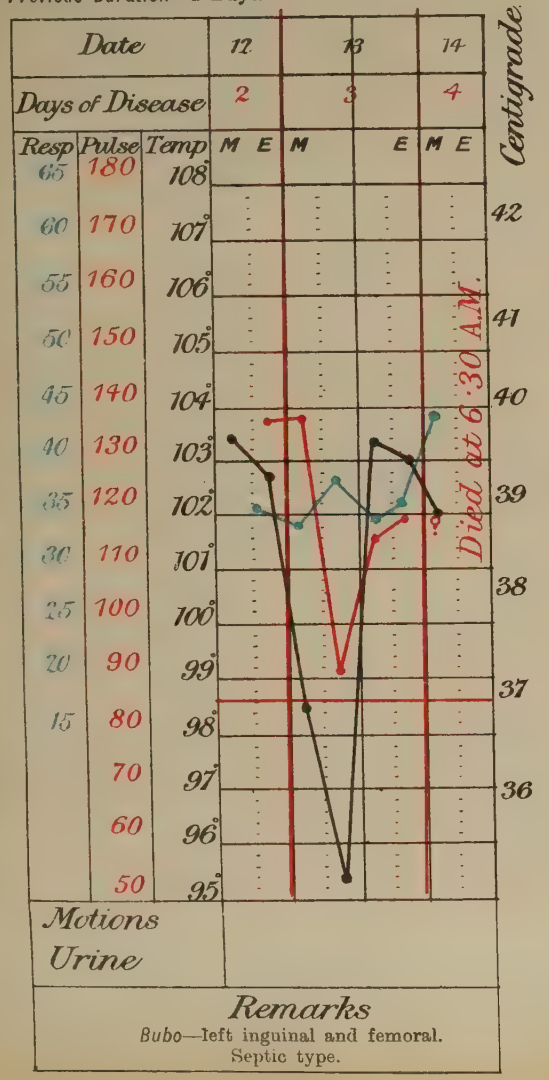
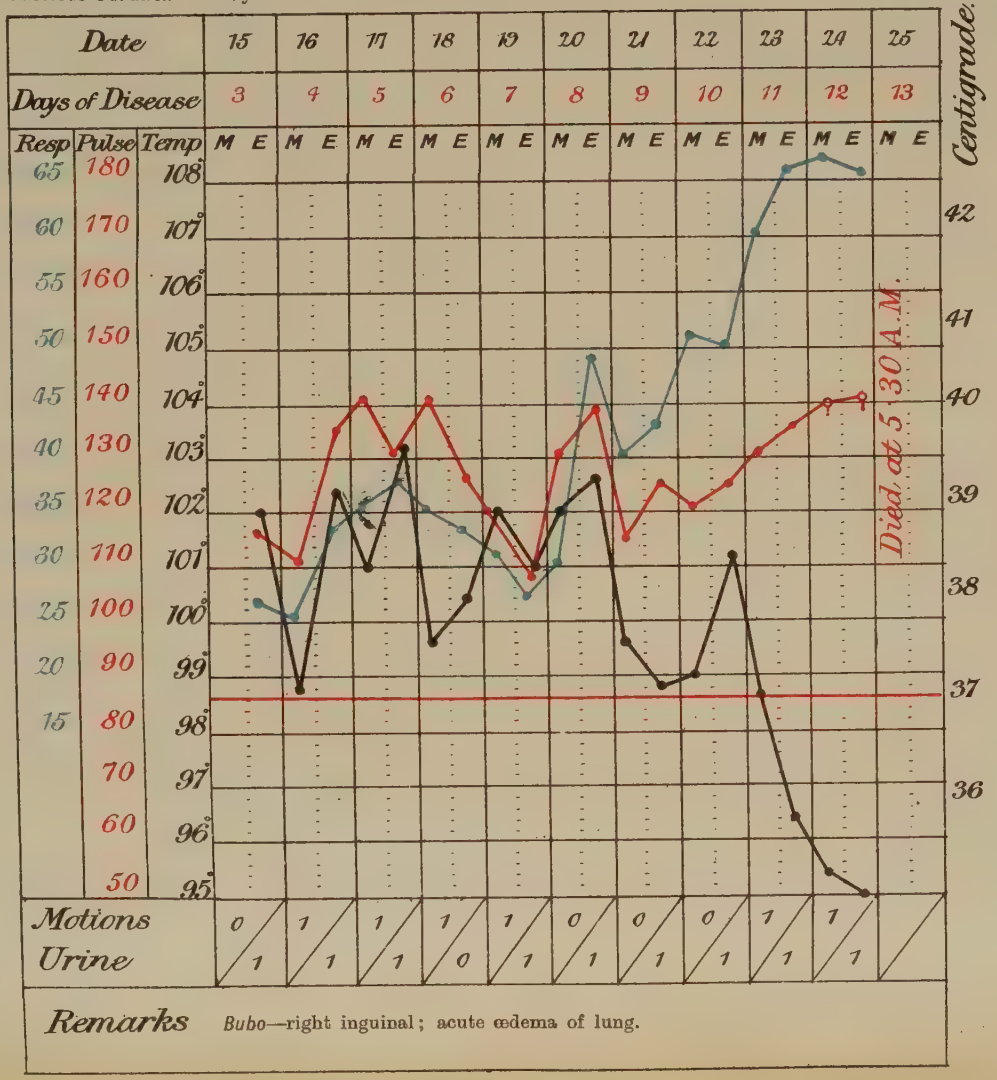
CLINICAL CHARTS OF PLAGUE CASES.

4. *Date of Admission*—12th March 1898, at 10.40 a.m.
General Number—2114. *DIAGNOSIS*—Plague.
Name—NARSOO BURMAJI. *Sex*—Male. *Age*—45.
Occupation—Biscuit Seller. *Caste*—Hindu.
Previous Duration—2 Days.



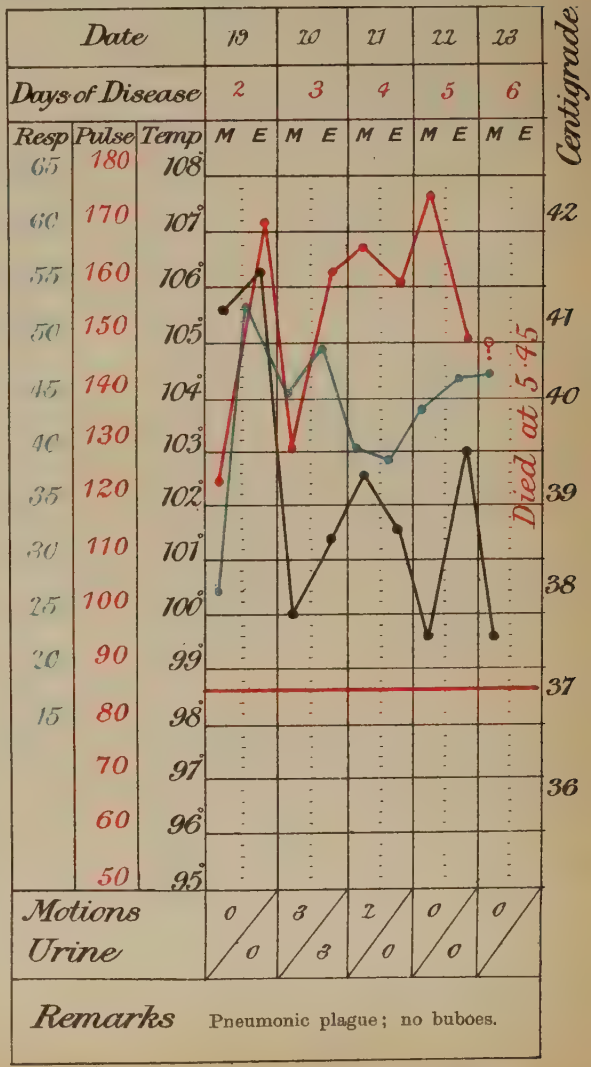
Remarks
Bubo—left inguinal and femoral.
Septic type.

5. *Date of Admission*—15th March 1898, at 1.45 p.m.
General Number—2205. *DIAGNOSIS*—Plague.
Name—JAMNABAI, wife of WAMON. *Sex*—Female. *Age*—25.
Occupation—Nil. *Caste*—Hindu.
Previous Duration—3 Days.



Remarks *Bubo*—right inguinal; acute œdema of lung.

6. *Date of Admission*—19th March 1898, at 11 a.m.
General Number—2287. *DIAGNOSIS*—Plague.
Name—RITA FERNANDEZ. *Sex*—Female. *Age*—15.
Occupation—Nil. *Caste*—Christian.
Previous Duration—2 Days.



Remarks Pneumonic plague; no buboes.

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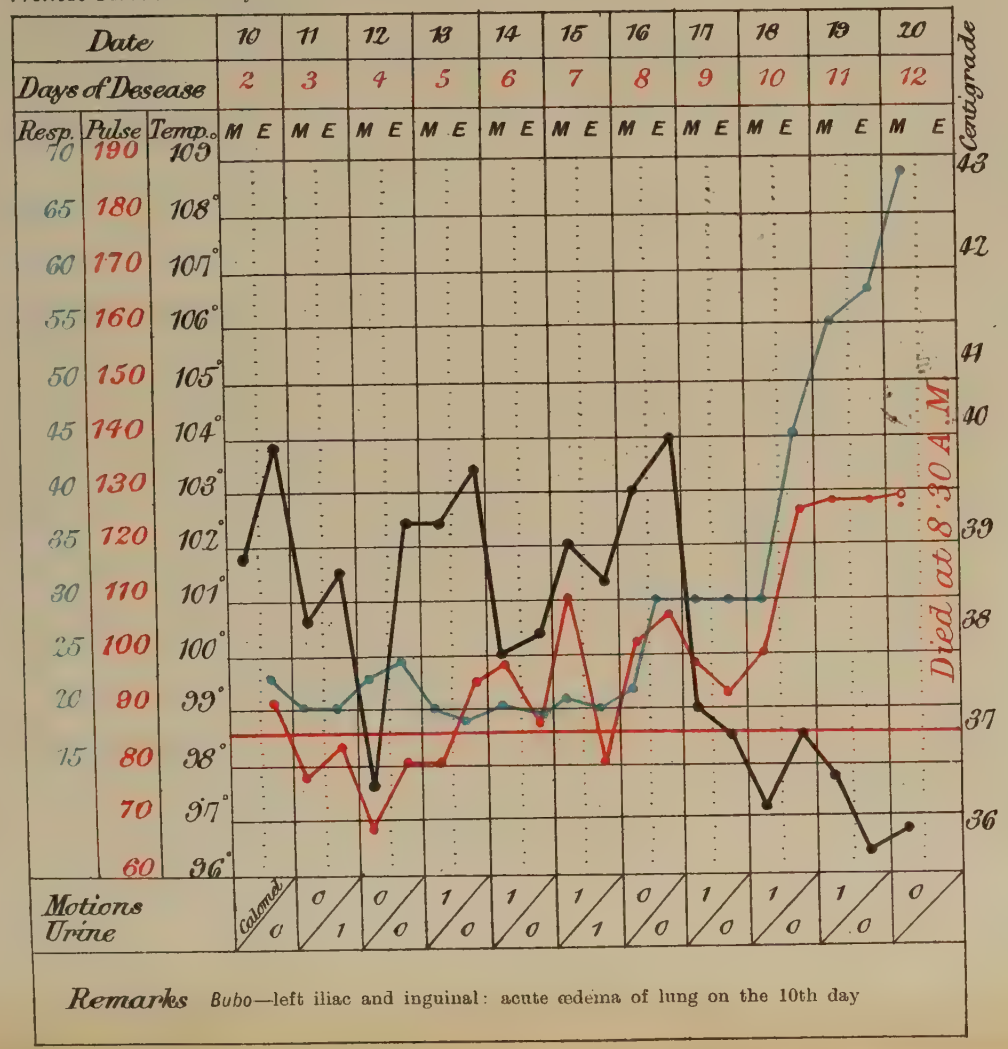
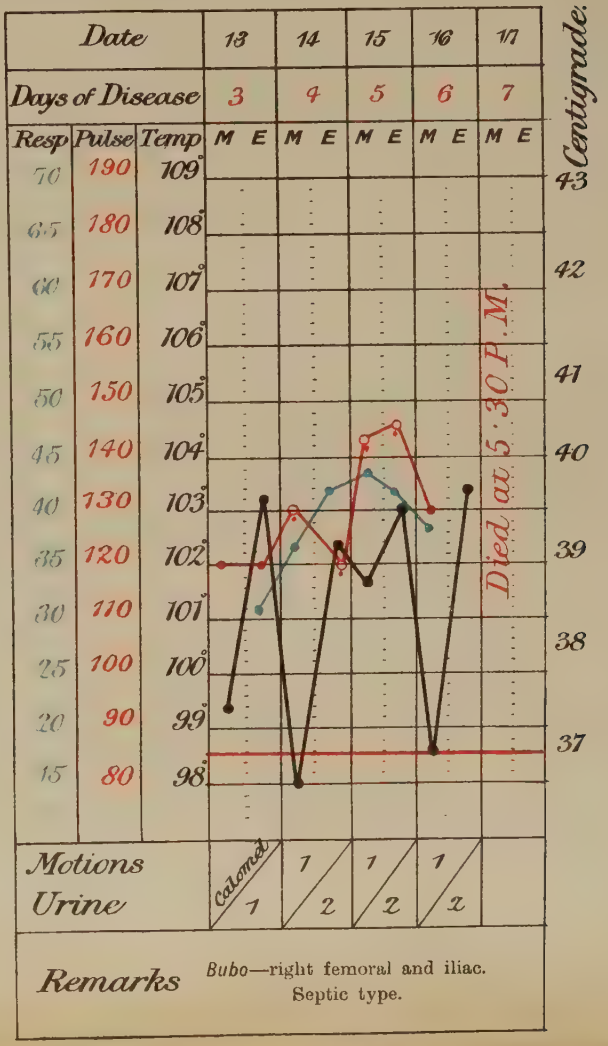
CLINICAL CHARTS OF PLAGUE CASES.

7. *Date of Admission*—10th February 1899, at 9.40 a.m.
General Number—424. *DIAGNOSIS*—Plague.
Name—BHIKHA VITHOO. *Sex*—Male. *Age*—20.
Occupation—Labourer. *Caste*—Hindu.
Previous Duration—2 Days.



Remarks *Bubo*—left iliac and inguinal: acute œdema of lung on the 10th day

8. *Date of Admission*—13th February 1899, at 9.5 a.m.
General Number—473. *DIAGNOSIS*—Plague.
Name—DHONDI, wife of MAIMAJI. *Sex*—Female. *Age*—22.
Occupation—Labourer. *Caste*—Hindu.
Previous Duration—3 Days.



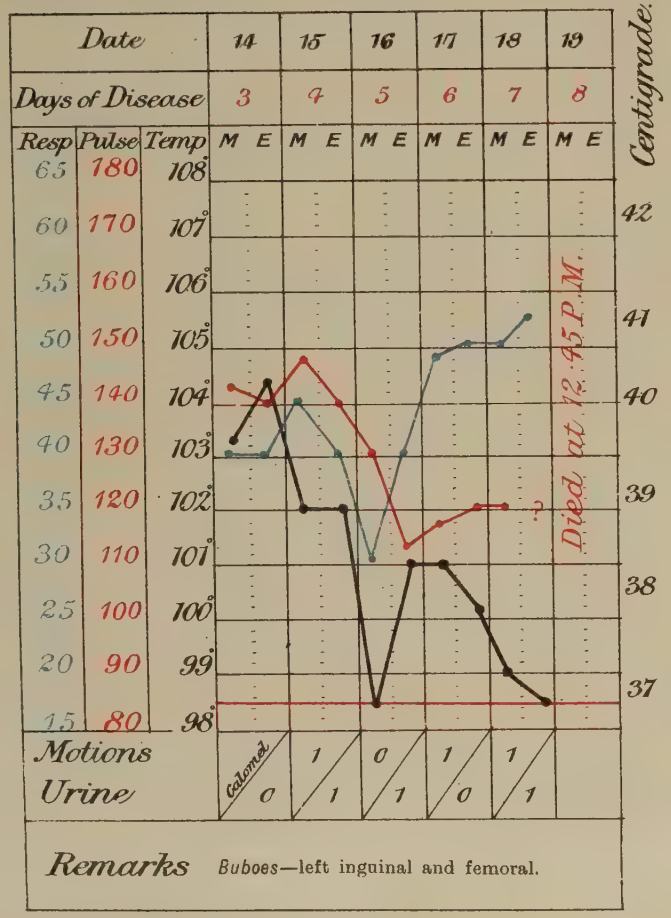
Remarks *Bubo*—right femoral and iliac. Septic type.

Municipal Hospital for Infectious Diseases, Arthur Road, Bombay.

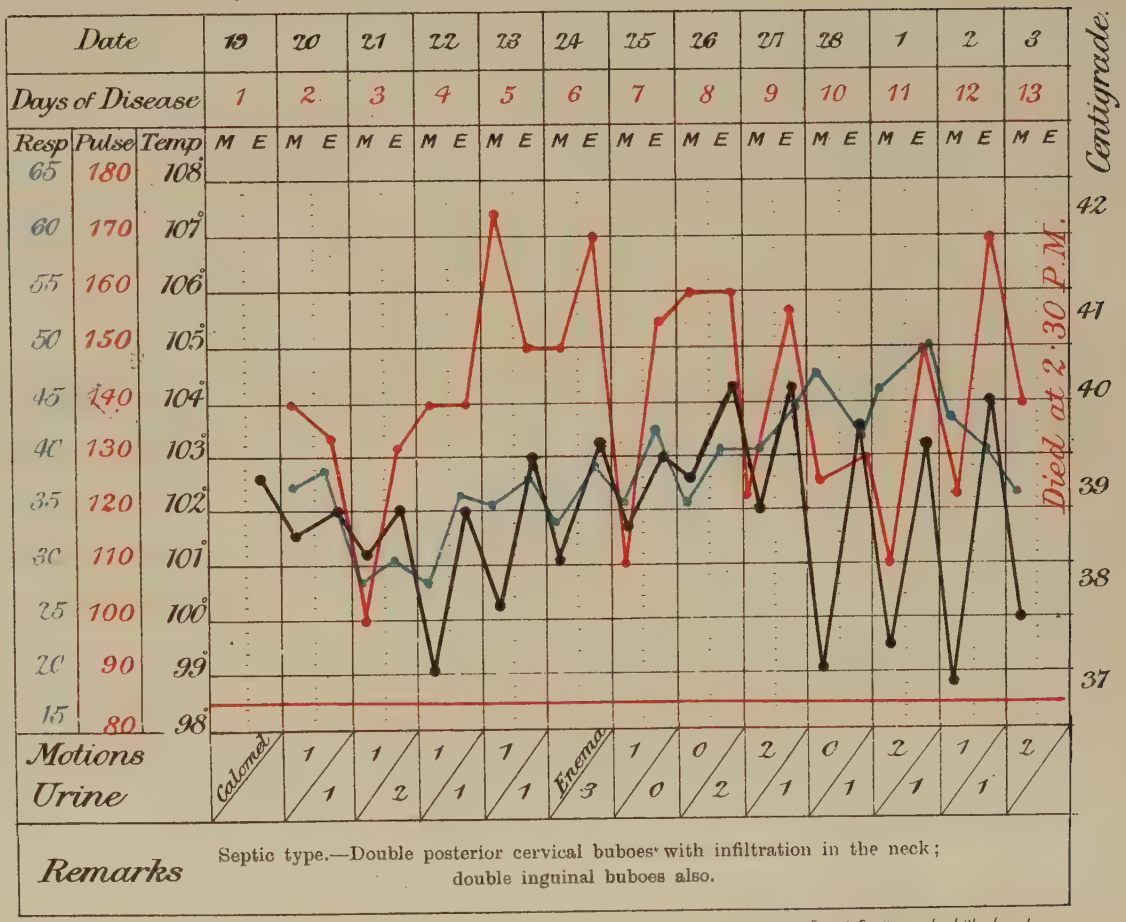
(Dr. CHOKSY.)

CLINICAL CHARTS OF PLAGUE CASES.

9. *Date of Admission*—14th February 1899, at 11.55 a.m.
General Number—493. *DIAGNOSIS*—Plague.
Name—LUXMI, widow of NAMA. *Sex*—Female. *Age*—35.
Occupation—Labourer. *Caste*—Hindu.
Previous Duration—3 Days.



10. *Date of Admission*—19th February 1899, at 7.55 a.m.
General Number—594. *DIAGNOSIS*—Plague.
Name—RUTH DE CRUZ. *Sex*—Female *Age*—7.
Occupation—Nil. *Caste*—Christian.
Previous Duration—1 Day.


Date
19 20 21 22 23 24 25 26 27 28 1 2 3
Days of Disease
1 2 3 4 5 6 7 8 9 10 11 12 13
Resp Pulse Temp
M E M E M E M E M E M E M E M E M E M E M E M E M E
65 180 108°
60 170 107°
55 160 106°
50 150 105°
45 140 104°
40 130 103°
35 120 102°
30 110 101°
25 100 100°
20 90 99°
15 80 98°
Centigrade.
42 41 40 39 38 37
Died at 2.30 P.M.
Motions
Urine
Calomel
1 / 1
1 / 2
1 / 1
1 / 1
Enema
3
1 / 0
0 / 2
2 / 1
0 / 1
2 / 1
1 / 1
2
Remarks
Septic type.—Double posterior cervical buboes with infiltration in the neck; double inguinal buboes also.

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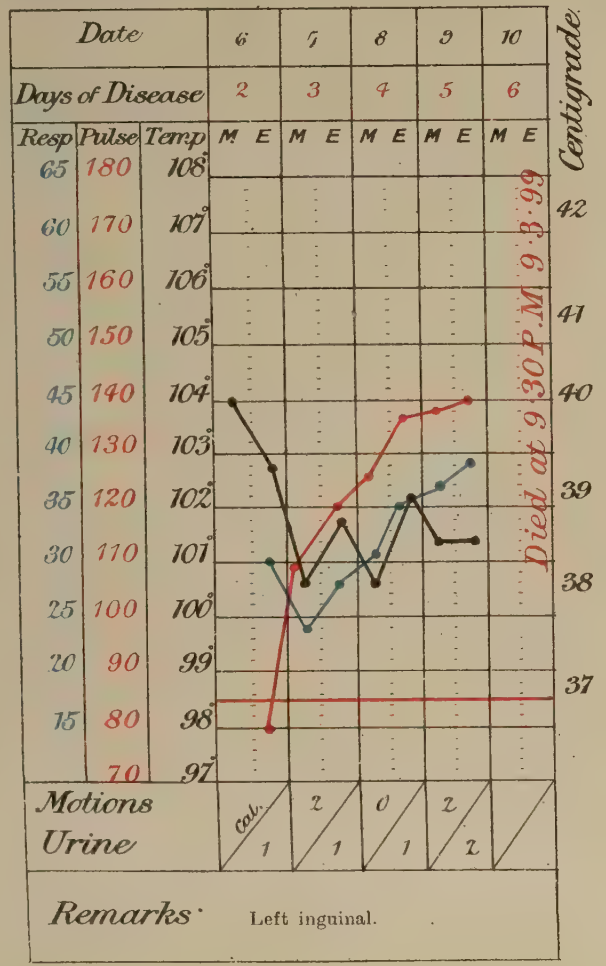
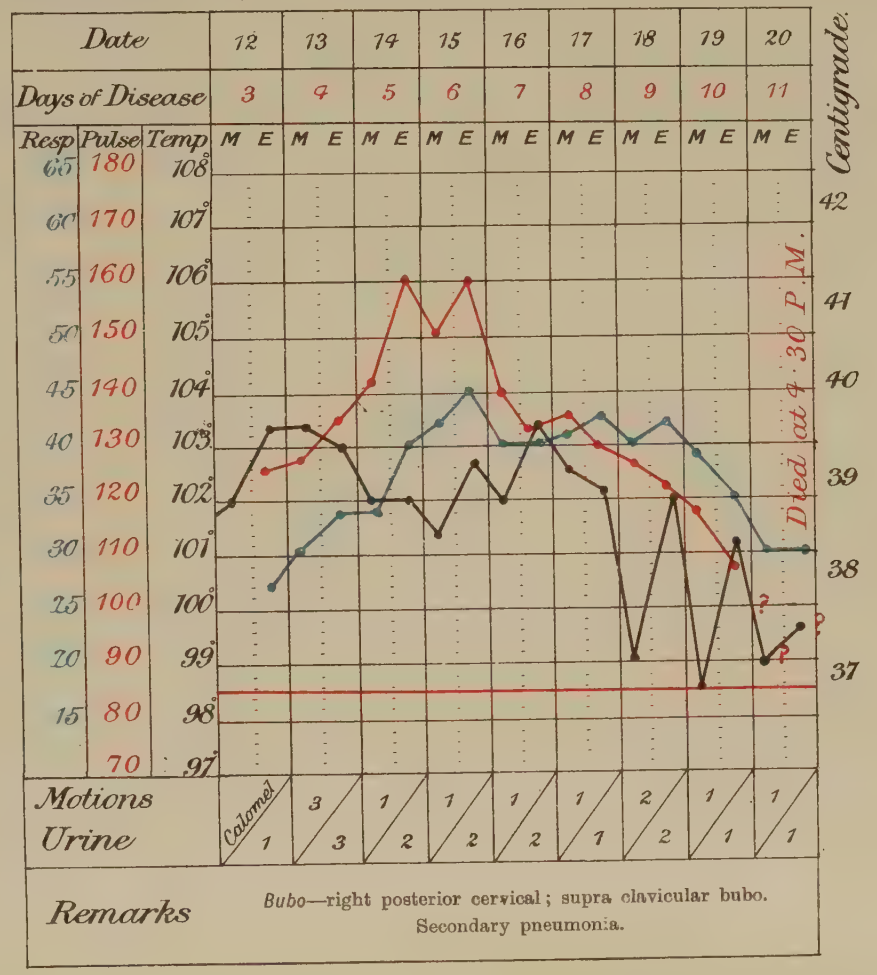
CLINICAL CHARTS OF PLAGUE CASES.

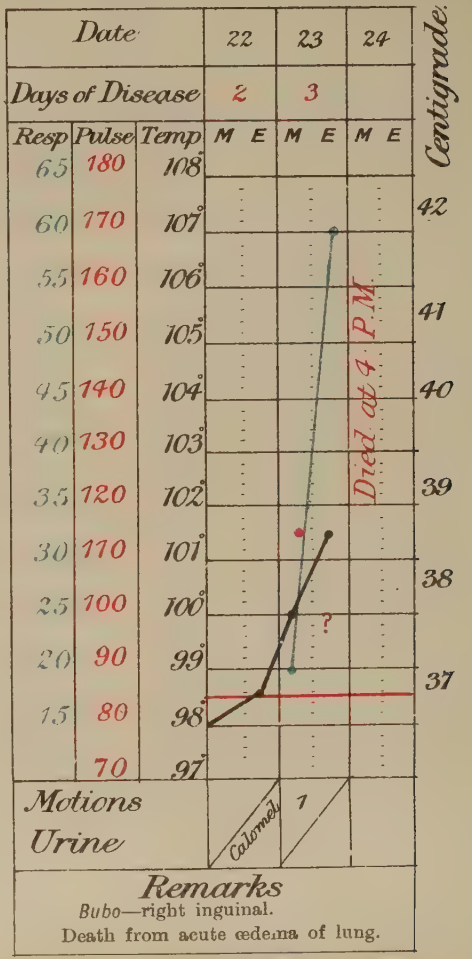
11. *Date of Admission*—6th March 1899, at 10.45 a.m.
General Number—881. *DIAGNOSIS*—Plague.
Name—RAMA KEDARI. *Sex*—Male. *Age*—22.
Occupation—Mill Hand. *Caste*—Hindu.
Previous Duration—2 Days.



12. *Date of Admission*—12th March 1899, at 9.15 a.m.
General Number—1007. *DIAGNOSIS*—Plague.
Name—VISNOO RAOJEE. *Sex*—Male. *Age*—18.
Occupation—Butler. *Caste*—Hindu.
Previous Duration—3 Days.



13. *Date of Admission*—22nd March 1899, at 5.30 p.m.
General Number—1204. *DIAGNOSIS*—Plague.
Name—FRANCIS DE SILVA. *Sex*—Male. *Age*—30.
Occupation—Tailor. *Caste*—Christian.
Previous Duration—2 Days.



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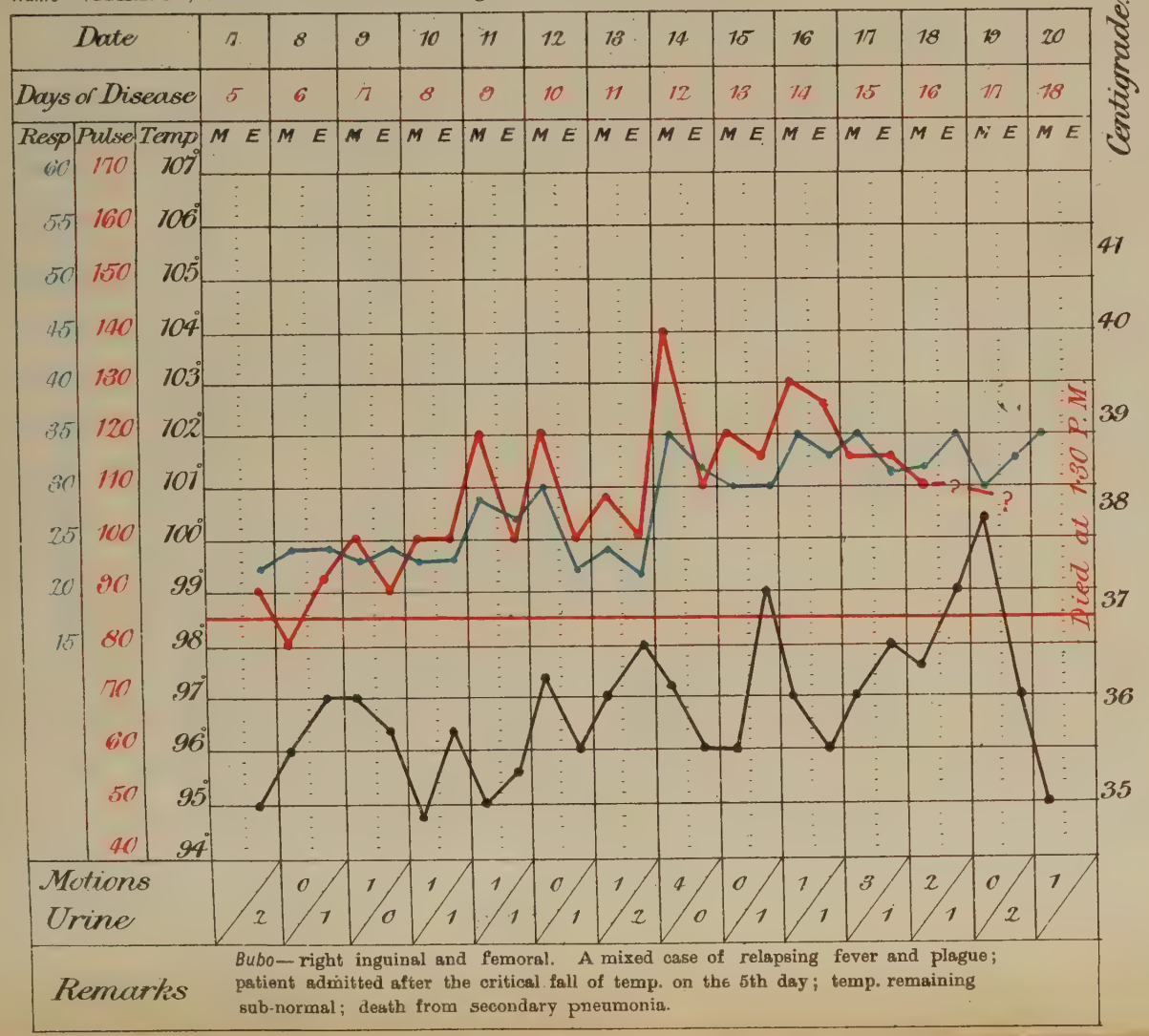
(Dr. CHOKSY.)

CLINICAL CHARTS OF CASES OF PLAGUE AND RELAPSING FEVER.

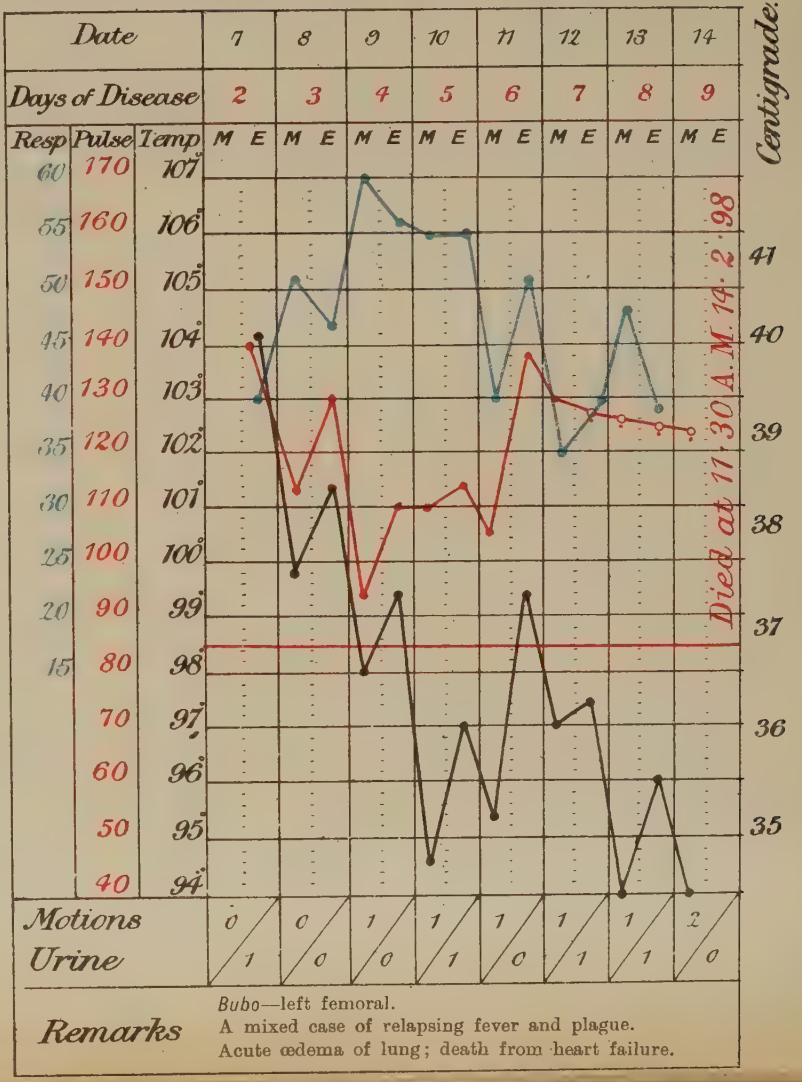
14. *Date of Admission*—7th February 1897. *General Number*—1109.

Name—VITHABAI, wife of ATMARAM. Female. *Age*—70. Hindu. *Occupation*—Nil.


Date
7 8 9 10 11 12 13 14 15 16 17 18 19 20
Days of Disease
5 6 7 8 9 10 11 12 13 14 15 16 17 18
Resp Pulse Temp
M E M E M E M E M E M E M E M E M E M E M E M E M E
Centigrade.
41 40 39 38 37 36 35
Died at 1.30 P.M.
Motions
Urine
2 1 0 1 1 1 1 0 1 2 4 0 0 1 1 3 1 2 1 0 2 1
Remarks
Bubo—right inguinal and femoral. A mixed case of relapsing fever and plague; patient admitted after the critical fall of temp. on the 5th day; temp. remaining sub-normal; death from secondary pneumonia.

15. *Date of Admission*—7th February 1898. *General Number*—1088.

Name—RADHA ATMARAM. Female. *Age*—24. Hindu. *Occupation*—Nil.


Date
7 8 9 10 11 12 13 14
Days of Disease
2 3 4 5 6 7 8 9
Resp Pulse Temp
M E M E M E M E M E M E M E M E
Centigrade.
41 40 39 38 37 36 35
Died at 11.30 A.M. 14.2.98
Motions
Urine
0 1 0 0 1 0 1 1 1 0 1 1 1 1 2 0
Remarks
Bubo—left femoral.
A mixed case of relapsing fever and plague.
Acute œdema of lung; death from heart failure.

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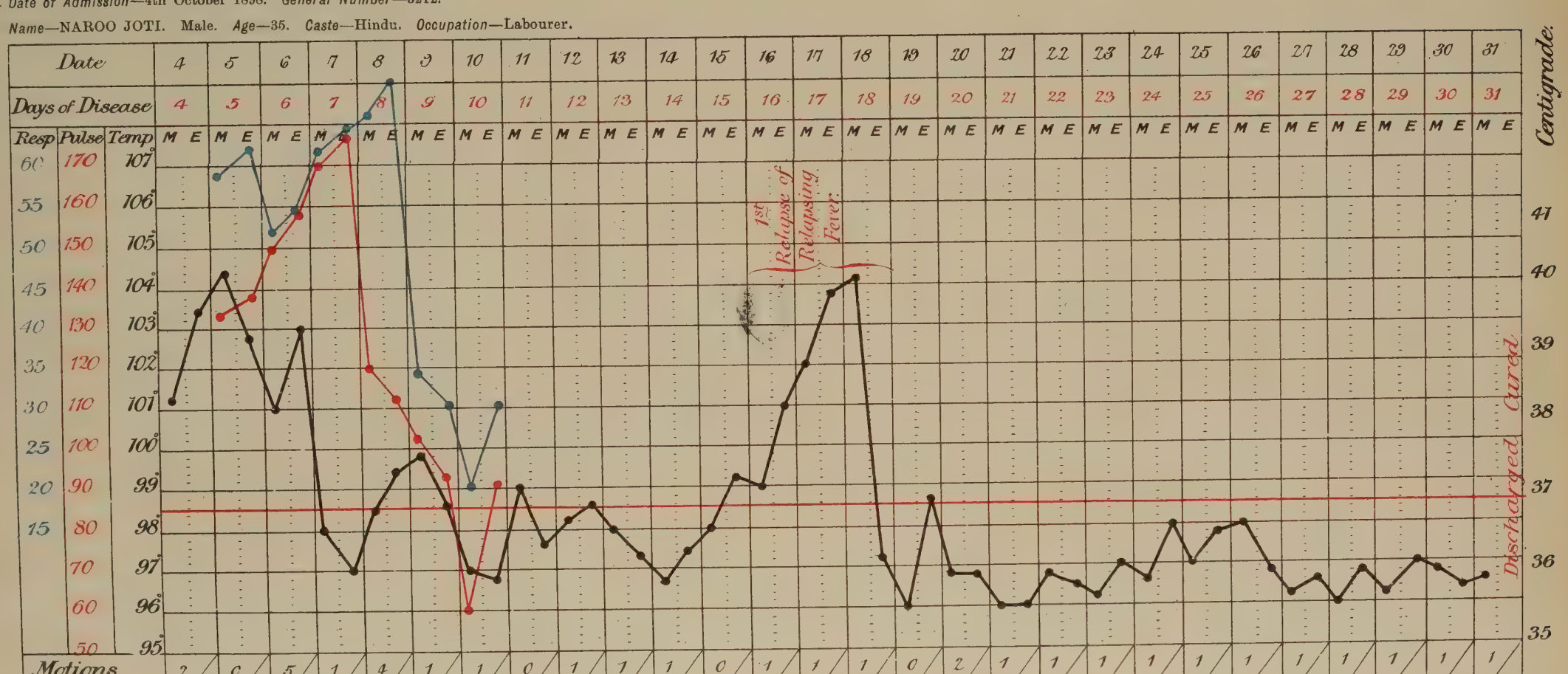
(Dr. CHOKSY.)

CLINICAL CHARTS OF CASES OF PLAGUE AND RELAPSING FEVER.

16. *Date of Admission*—4th October 1898. *General Number*—3212.

Name—NAROO JOTI. Male. *Age*—35. *Caste*—Hindu. *Occupation*—Labourer.



Remarks *Bubo*—left axillary with infiltration; recovery without suppuration of bubo. A mixed case of relapsing fever and plague.

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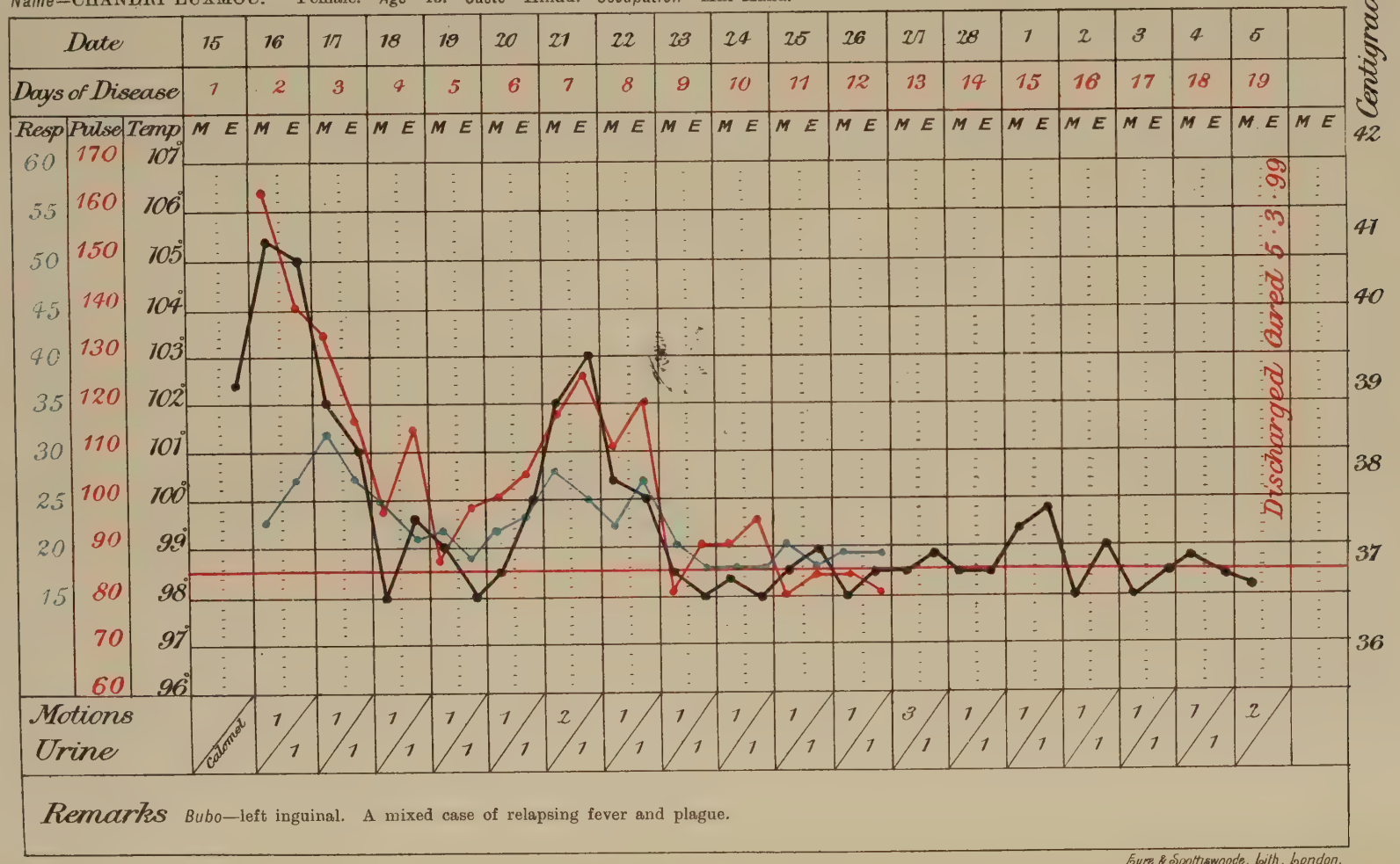
(Dr. CHOKSY.)

CLINICAL CHARTS OF CASES OF PLAGUE AND RELAPSING FEVER.

17. *Date of Admission*—15th February 1899. *General Number*—522.

Name—CHANDRI LUXMOU. Female. *Age*—13. *Caste*—Hindu. *Occupation*—Mill Hand.



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(See Question No. 21,034)

SPHYGMOGRAPHIC TRACINGS

PATIENTS TREATED IN THE ARTHUR ROAD HOSPITAL, BOMBAY.

6—4—99. RAMA BALU.

24—4—99. BALTHAZAR DE SOUZA.

26—4—99. BALA KUSABA.

26—4—99. BHANA KHUSHAL.

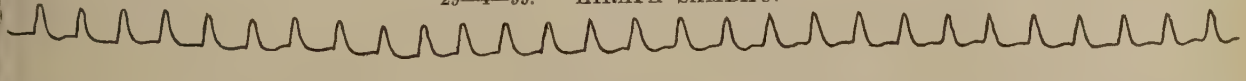
26—4—99. SAMBHU GOPAL.

26—4—99. No. 1,564.

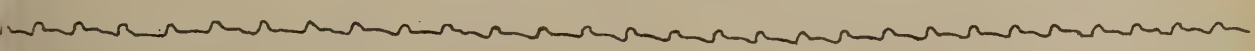
27—4—99. MAHADU BHANU.

28—4—99. KRISHNABAI, WIFE OF RAGHU.

29—4—99. HIRAPA SAMBHU.



29—4—99. UPASHA KONDAJI.



30—4—99. CHANDIPARSHAD GANGAPARSHAD.

WEEK ENDING 6 A.M.		PLAGUE ATTACKS	PLAGUE DEATHS	AVERAGE DAILY ATTACKS	AVERAGE DAILY DEATHS
JULY 1897	10	13	1	4·3	3
	17	24	18	3·4	2·6
	24	55	27	7·8	3·8
	31	69	43	9·8	6·1
AUG 1897	7	86	75	12·3	10·8
	14	156	117	22·3	16·7
	21	207	156	29·5	22·3
	28	249	204	35·5	29·1
SEP 1897	4	204	161	29·0	23·0
	11	126	97	18	13·8
	18	103	80	14·7	11·4
	25	46	37	6·5	5·3
OCT 1897	2	34	27	4·8	3·8
	9	21	11	3·0	1·5
	16	19	16	2·7	2·3
	23	12	7	1·7	1·0
	30	9	9	1·3	1·3
NOV 1897	6	8	6	1·1	0·8
	13	1	1		
	20	5	3		
	27	4	6		
DEC 1897	4	0	0		
	11	4	3		
	18	1	1		
	25	1	1		
JAN 1898	1	2	2		
	8	1	1		
	15	1	1		
	22	0	0		
	29	1	0		

Chart shewing the progress of the Plague in Karad Town, 1897-98. Includes all attacks and deaths occurring in Karad Municipal area.

Attacks red, deaths black.

PLAGU	
ATTACKS	DEATH
13	1
20	10
38	11
60	12
69	15
137	51
198	103
216	128
182	102
116	63
88	54
38	13
28	12
17	7
14	9
6	4
7	7
4	3
0	0
3	2
1	0
0	0
1	1
1	1
1	1
0	0
1	1
0	0
0	0
0	0

CHARTS RELATING TO THE PLAGUE IN KARAD IN 1897-8.

(See Question No 21088.)

Chart showing the entries into Karad Town through the Observation Camp and by means of "periodical examination," less those leaving the Town through the Observation Camp. It is not accurate as showing the actual entries into the Town, but shows very well the trend of the population. The red line shows the period when there were more departures from than entries into the Town.

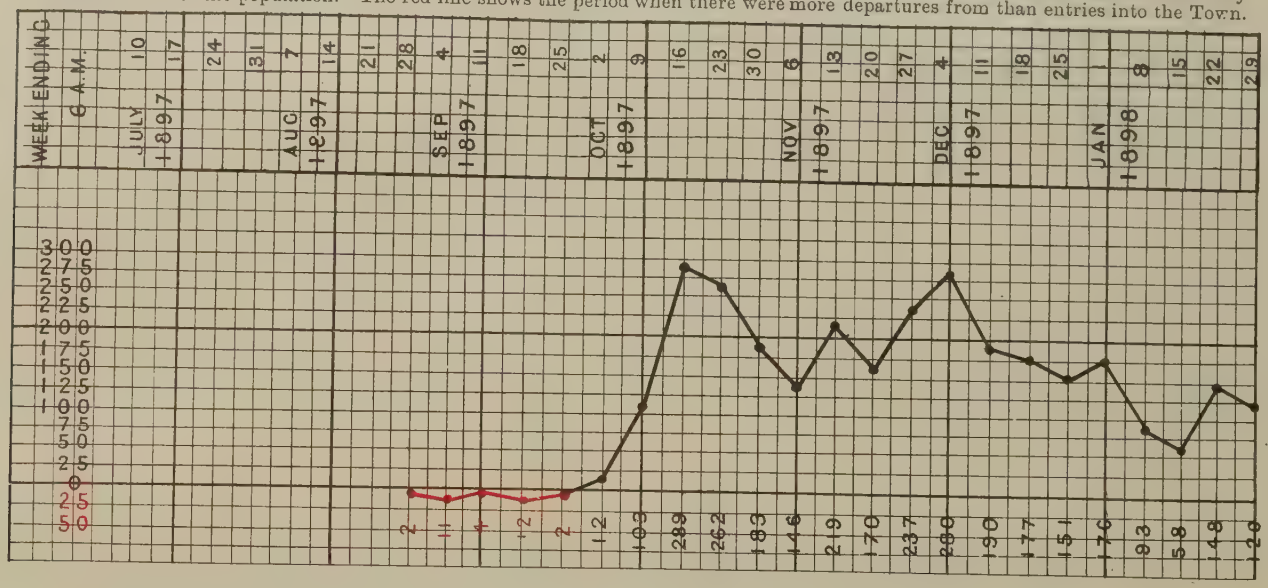


Chart shewing progress of Plague in Karad Town, 1897-98. Includes only the attacks and deaths in the Town proper. Attacks red, deaths black.

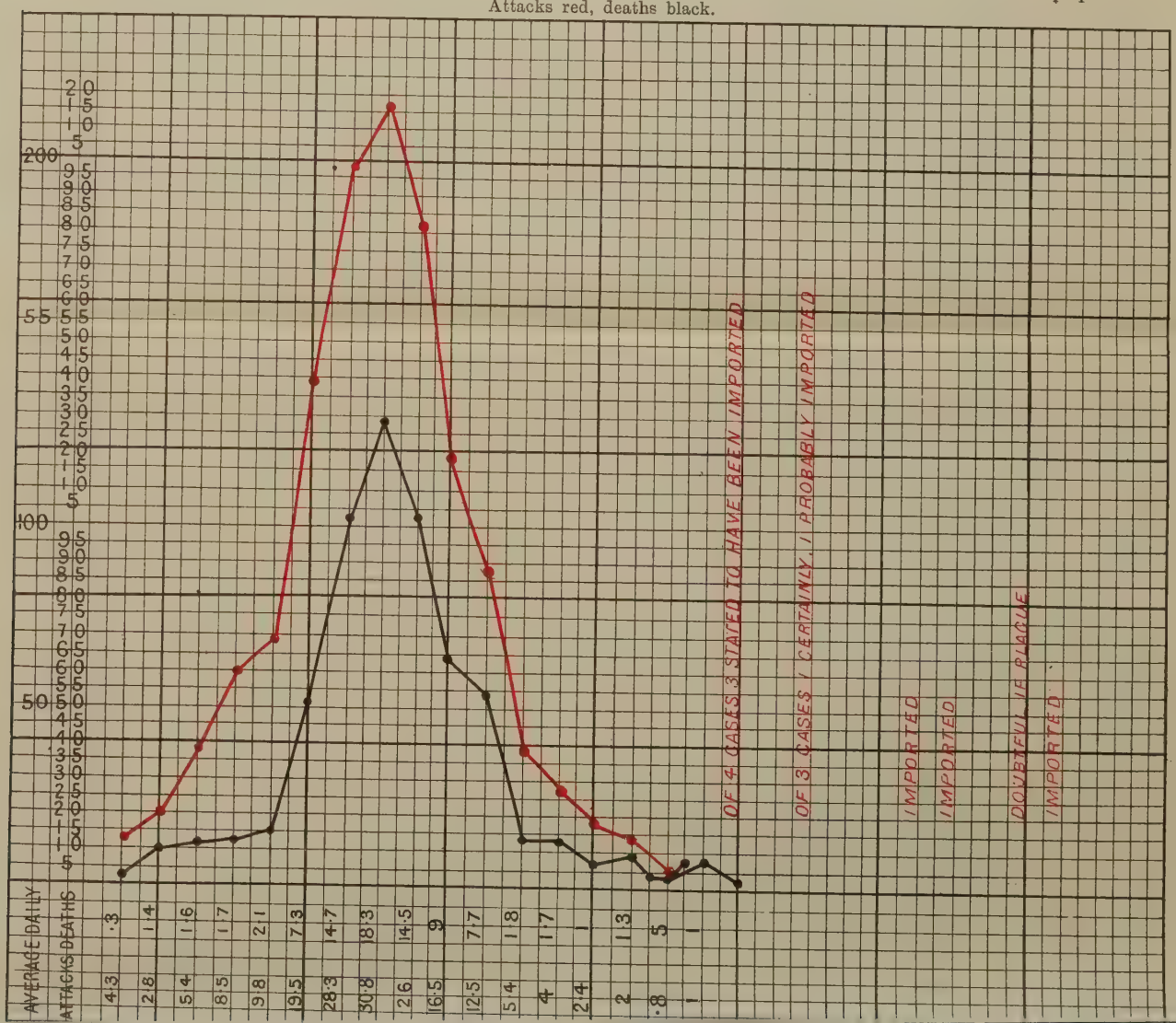
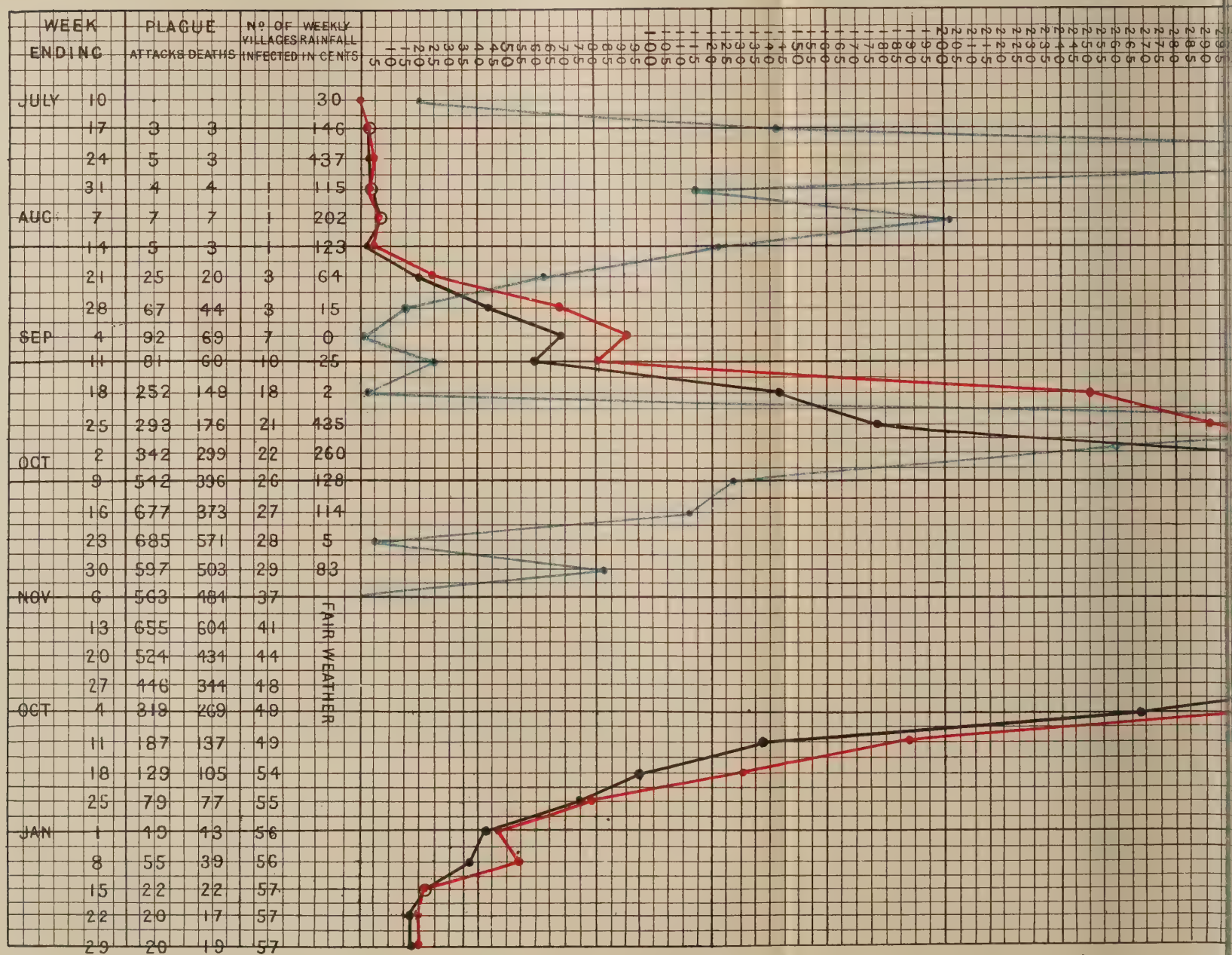


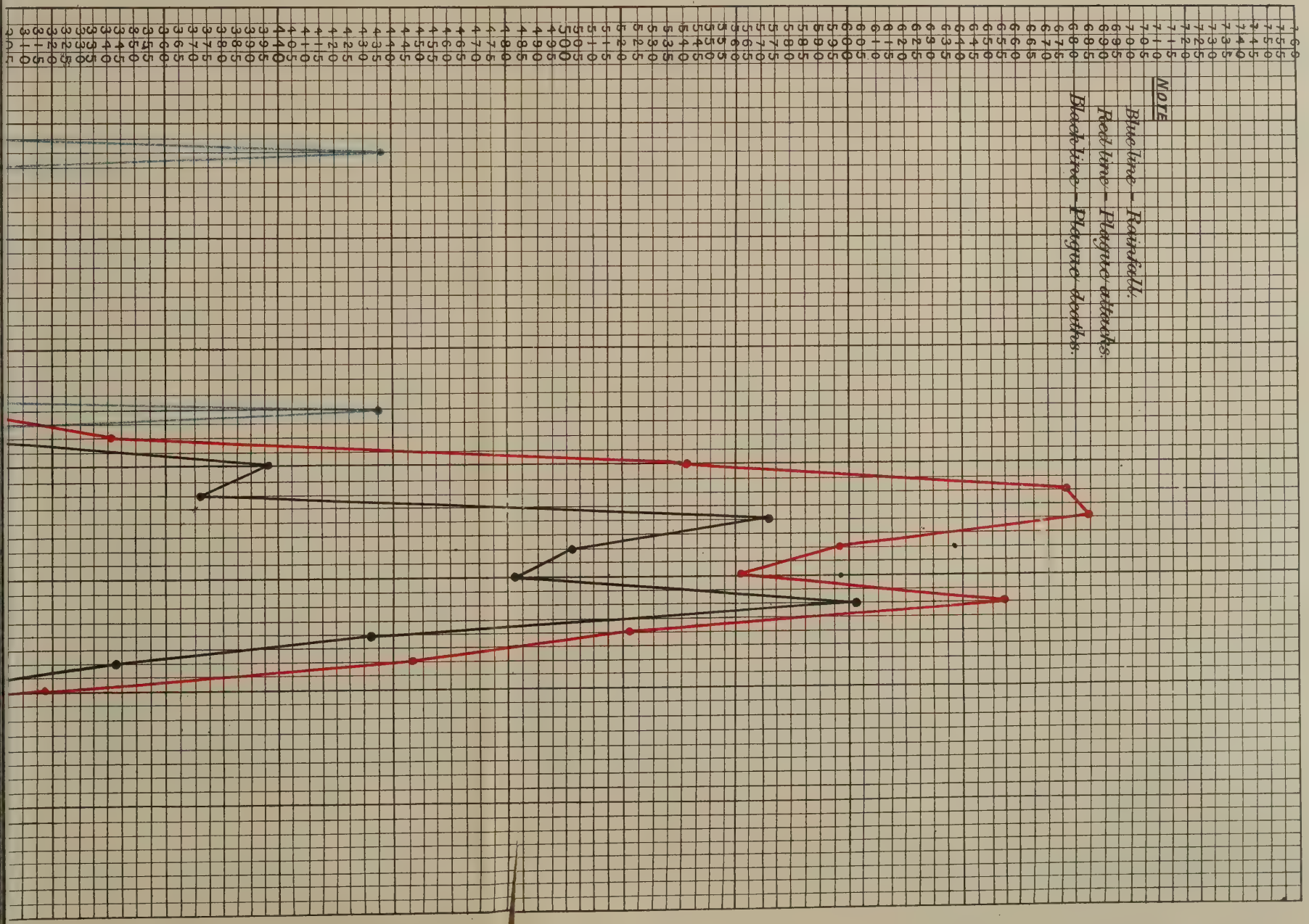
CHART OF THE PLAGUE EPIDEMIC IN THE

WEEK ENDING		PLAGUE ATTACKS	DEATHS	No. OF VILLAGES INFECTED	WEEKLY RAINFALL IN CENTS
JULY	10	1	1		30
	17	3	3		146
	24	5	3		437
	31	4	4	1	115
AUG	7	7	7	1	202
	14	5	3	1	123
	21	25	20	3	64
	28	67	44	3	15
SEP	4	92	69	7	0
	11	81	60	10	25
	18	252	149	18	2
	25	293	176	21	435
OCT	2	342	299	22	260
	9	542	396	26	128
	16	677	373	27	114
	23	685	571	28	5
	30	597	503	29	83
NOV	6	563	481	37	
	13	655	604	41	
	20	524	434	44	
	27	446	341	48	
OCT	4	318	269	49	
	11	187	137	49	
	18	129	105	54	
	25	79	77	55	
JAN	1	49	43	56	
	8	55	39	56	
	15	22	22	57	
	22	20	17	57	
	29	20	19	57	



B. (See Question Nº 21088.)

ARAD VILLAGES DURING THE RAINS OF 1897.



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ARTS OF THE PLAGUE EPIDEMIC IN 4 TOWNS IN THE KHANDESH DISTRICT, BOMBAY PRESIDENCY.

GAON

15,000

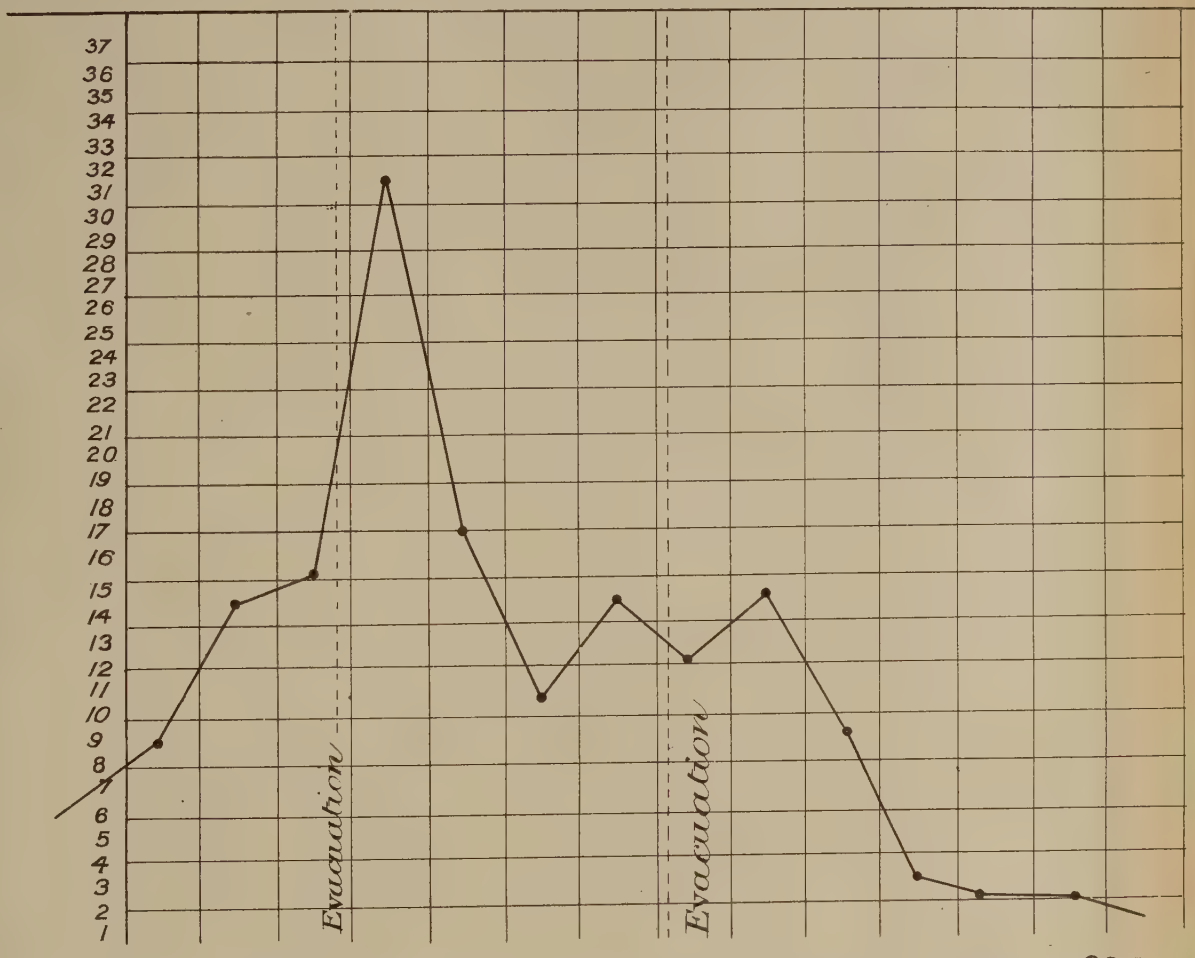
CHART OF PLAGUE ATTACKS

WEEK ENDING (N.B. 8 DAYS TO A WEEK!)

attacks Unrecorded 40 + Recorded 115 = 155

A

1897·8. 22 NOV, 30 NOV, 7 DEC, 15 DEC, 23 DEC, 31 DEC, 7 JAN, 13 JAN, 22 JAN, 31 JAN, 7 FEB, 15 FEB, 23 FEB, 28 FEB,



N.B. 5/6 Town was evacuated between 7 & 15 Dec. 1897.

Y.3905. Ev, 110 1/6 " " " 13 & 22 Jan. 1898.

BHOLANA

Pop. 800

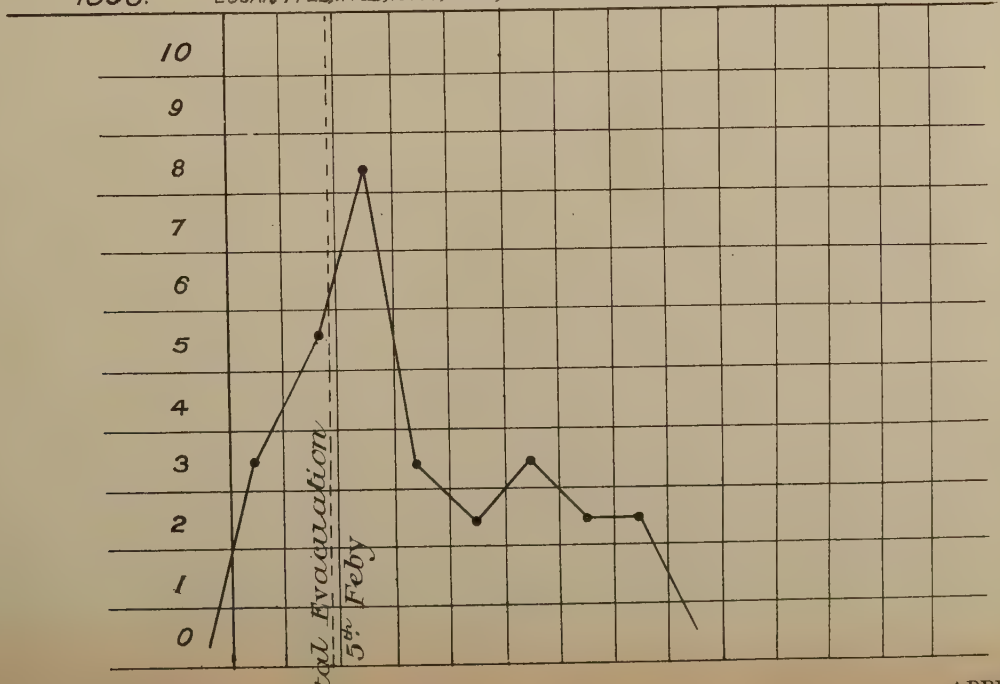
CHART OF PLAGUE ATTACKS

WEEK ENDING.

B

otal attacks 30

1898. 28 JAN, 4 FEB, 11 FEB, 18 FEB, 25 FEB, 4 MAR, 11 MAR, 18 MAR, 25 MAR,



INSTITUTE

CHART OF PLAGUE ATTACKS

NASWADI
p, 269
al attacks 25
1898·9

WEEK ENDING

C

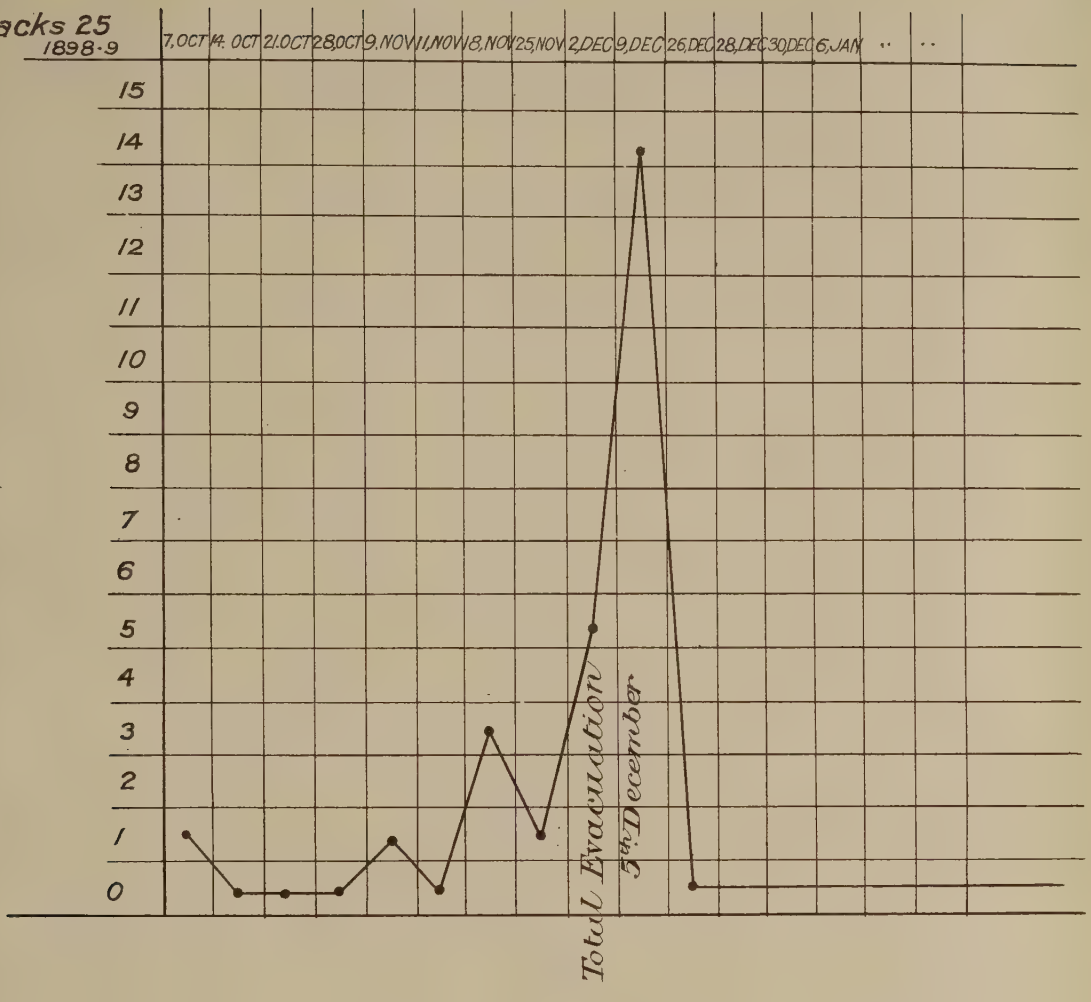
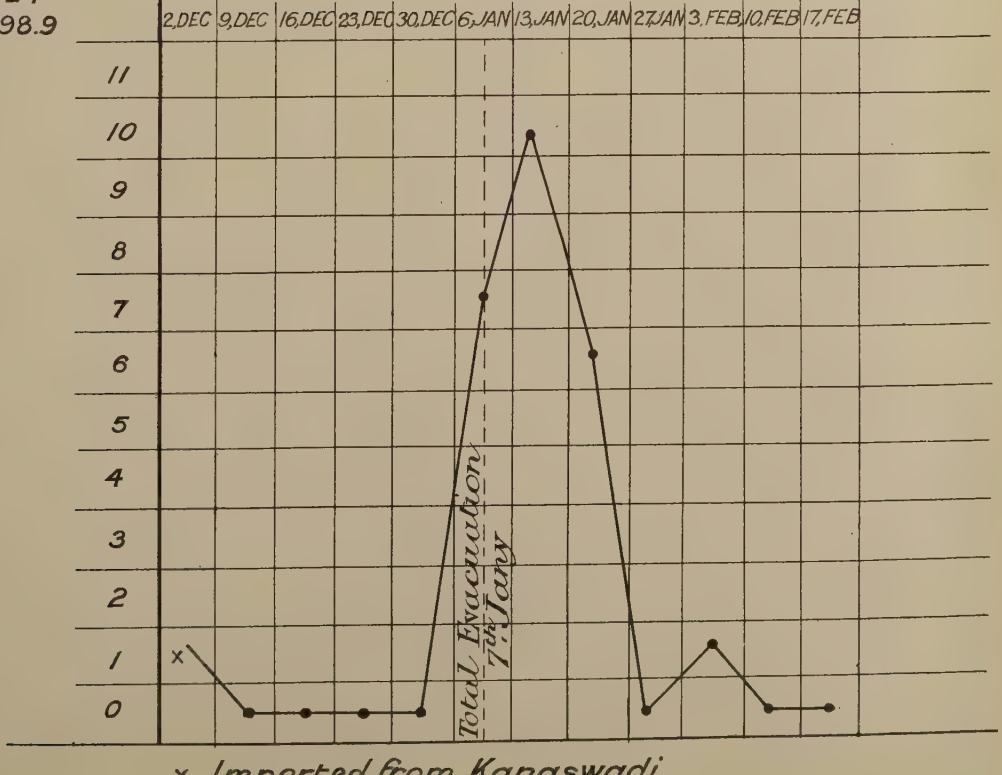


CHART OF PLAGUE ATTACKS

GOZORA
op, 950
tal attacks 24
1898.9

WEEK ENDING

D



x Imported from Kanaswadi

APPENDIX No. LXVIII.

REPORT by the PLAGUE COMMISSIONER, HYDERABAD, to the GOVERNMENT OF INDIA,

regarding the

DISSEMINATION of the INFECTION of PLAGUE through the Medium of RAW COTTON.

PLAGUE—HYDERABAD STATE.

No. 999 M., dated Camp Mandalgira, the 12th January 1899.

From A. H. STEVENS, Esq., Deputy Plague Commissioner, to Lieutenant-Colonel E. LAWRIE, M.B., I.M.S., Plague Commissioner, Hyderabad State.

I HAVE the honour to report that, from personal ervation in plague infected and adjacent areas, I e come to the conclusion that the infection of plague isseminated through the medium of raw cotton.

ly reasons for arriving at this conclusion are as ows:—

have reported the difficulties that have from time time been experienced in evacuating houses, and sequently in disinfecting them (by digging up and ning the floor surfaces by the kiln process) owing to verandahs and outer rooms (which are almost ariably the sleeping places in village houses) being cked up with bales of unpressed cotton (the usual e of each bale, which is sewn up in rough gunny th, is about 6 × 3 × 3). I have noticed these bales otton in almost all the Kopbal infected villages as l as, in a few instances, in Lingsugur, and in the ected villages round Sholapur. I also remember my istant Mr. Venkatesha Rao reporting the same state affairs last year at the infected village of Pallaskheda r the Khandesh frontier.

The danger of plague infection being propagated ough the medium of cotton bales was most forcibly pressed upon my mind a few days ago, when I saw ne hundreds of bales of cotton despatched by rail m Bannikop Station on the Southern Mahratta ilway. On inquiry, I found that this cotton had n brought the same evening from the infected age of Bannikop, which is only one mile from the lway station, and in which over 400 people have hin the past two months died of plague. I forthh rode to the village, which is being disinfected. I saw the last lot of bales of cotton belonging to the signment taken out of the verandahs of two houses which I had some weeks before seen plague corpses ng, as well as the sick suffering from plague. The es were loaded on carts, taken to the railway station, l lifted direct from the carts into closed cotton vans, interval that elapsed between the bales leaving the ected premises and being packed in the closed ton waggon was considerably under one hour. The ne was 4.30 p.m., when there was little or no sun to troy the bacillus or infection during transit; and I no reason why each bale of cotton thus sent should be a medium for spreading the disease into whatr city or town it is imported; whether taken to a ton press for pressing, or to a mill for manuturing, all the conditions surrounding it, and its ure treatment, would be favourable to the propaion of plague by means of the infection contained in bale.

At present, as far as I understand, very little is own as to the actual manner in which plague infecn is propagated, but I believe I have hit upon one y in which it may be, by means of the railway, seminated far and wide into the very centres of ies and towns supposed to be safeguarded and hedged und by the strictest quarantine and other plague gulations.

It is noteworthy how persistently plague has stuck the cotton manufacturing towns and the cotton procing districts, and no explanation of the reason of s has, I believe, been brought forward.

Possibly cotton itself may, under certain conditions, be a medium favourable for preserving the plague bacillus alive for a long time, and this may explain some of the hitherto unaccounted for outbreaks of plague at various places, the origin of which have never been traced out.

The subject is one that is, I think, worthy of scientific investigation, such as I am unable to bring to bear upon it, and is one that may be advisable for the Plague Commission to inquire into.

With reference to the case in point—the Bannikop cotton—you may ask why I permitted it to be exported? I am not aware that I have any legal power to stop it. The consignments of country produce from these districts are made in fulfilment of contracts with large mercantile firms such as Ralli Brothers and others, and any interference on my part might lead to a law suit. I shall, therefore, be much obliged if my legal powers in the direction of interfering with the cotton and other trades may be exactly defined for my future guidance.

No. 472, dated Hyderabad, the 25th January 1899.

From Lieutenant-Colonel E. LAWRIE, Plague Commissioner, Hyderabad, to the FIRST ASSISTANT RESIDENT, Hyderabad.

I HAVE the honour to enclose a copy of a letter from Mr. A. H. Stevens, Deputy Plague Commissioner, Hyderabad State, on the subject of the dissemination of the infection of plague through the medium of raw cotton.

On receipt of Mr. Stevens' letter, I telegraphed to him to forward specimens of the cotton he suspected to contain the infection to me for bacterioscopic examination: and specimens were accordingly received from him on the 22nd instant.

Cultures in sterilised broth were made from these specimens by Dr. Mullanah the same day. On the 23rd instant, the broth had become cloudy and contained a growth. Preparations from this growth showed a variety of organisms, among which a polar-stained bacillus, morphologically resembling the bacillus of plague, was prominent. At 2 o'clock p.m., that day three rabbits, which had been inoculated with a culture of the organisms found in Haffkine's fluid on the 28th of December 1898, and the 1st and 4th of January 1899, were inoculated with ½, 1, and 2 c.c. respectively of the broth culture made from the organisms in the suspected cotton. The temperature of the first rabbit rose rapidly to 105, and it died on the morning of the 25th instant, about 36 hours after the inoculation. *Post mortem* examination showed that it died of plague, and the plague bacillus was found in the glands, in the lungs, and in the spleen. A few were also found in the blood. A control experiment made with ordinary raw cotton bought in the bazar shows that it contains no organisms such as were found in the infected cotton.

The importance of Mr. Stevens' discovery—that the infection of plague can be disseminated through the medium of raw cotton—cannot be over-estimated, and I am of opinion that it ought to be communicated at once to the Government of India and to the Indian Plague Commission.

No. 11–C., dated Hyderabad Residency, the 27th January 1899.

From Sir TREVOR CHICHELE-PLOWDEN, K.C.S.I., Resident at Hyderabad, to the SECRETARY TO THE GOVERNMENT OF INDIA, Home Department.

I HAVE the honour to forward, for the information of the Government of India, a copy of a letter, No. 472, dated the 25th January 1899, from the Plague Commissioner, Hyderabad, with a copy of its enclosure, regarding the dissemination of the infection of plague through the medium of raw cotton.

No. 485, dated Calcutta, the 6th February, 189

From R. NATHAN, Esq., Under-Secretary to the Go ment of India, to the SECRETARY TO THE I PLAGUE COMMISSION.

I AM directed to forward, for the informati the Indian Plague Commissioners, a copy of a l from the Resident at Hyderabad, No. 11–C., dated 27th January, 1899, regarding the disseminatio the infection of plague through the medium of cotton.

APPENDIX No. LXIX.

MEMORANDUM ON EXPERIMENTS TO DETERMINE THE PROPHYLACTIC EFFECT OF HAFFKINE'S FLUID ON RABBITS,

Performed by Order of the Plague Commissioner, Hyderabad,

by

Lieut.-Colonel E. Lawrie, M.B., I.M.S., and Mr. Mullannah, M.B.

Summary.

Class A.

Animals protected with re-sterilised Haffkine's fluid.

Recovered after inoculation with plague	9
Died	1
Died from the protective	0
Total	10

Of the animals experimented on in Class A., two had one large dose of the protective (20 c.c.), one had two large doses (20 c.c. each), four had one small dose (2 c.c.), and two had two medium doses (6¼ c.c.). The rabbit that died had two small doses of the prophylactic of 1 c.c. each.

Class B.

Animals protected with contaminated Haffkine's fluid.

Recovered after inoculation with plague	20
Died	10
Died from the protective	8
Total	38

Of the animals experimented on in Class B., two of those that recovered had one large dose of the protective (20 c.c.), one had one small dose (2 c.c.), 15 had two large doses (20 c.c.), and two had two small doses (6¼ c.c.). Of those that died in this class, four had one small dose (1 to 6 c.c.), and six had two large doses (20 c.c.) of the protective. One animal that died of the effects of the protective alone died of plague.

It must be noted that for the purposes of the experiments the doses of both the prophylactic, and of the plague cultures, administered by inoculation were unusually large. This was especially the case with the inoculations with the contaminated fluid.

TABLE.

No. of Rabbits.	Haffkine's Fluid.							Plague.												Remarks.
	Number of Brew.	Contaminated.	Resterilised.	First Inoculation.		Second Inoculation.		First Inoculation.		Second Inoculation.		Third Inoculation.		Fourth Inoculation.		Fifth Inoculation.		Sixth Inoculation.		
				Date.	Dose.	Date.	Dose.	Date.	Dose.	Date.	Dose.	Date.	Dose.	Date.	Dose.	Date.	Dose.	Date.	Dose.	
2	1713	Contaminated	—	7.8.98	1 c.c.	—	—	10.8.98	3 c.c.	—	—	—	—	—	—	—	—	—	—	Died of plague.
3	1713	„	—	7.8.98	3 c.c.	—	—	10.8.98	3 c.c.	—	—	—	—	—	—	—	—	—	—	„
4	1713	„	—	7.8.98	6 c.c.	—	—	10.8.98	3 c.c.	—	—	—	—	—	—	—	—	—	—	„
6	3250	„	—	16.8.98	6 c.c.	22.8.98	10 c.c.	27.8.98	1 c.c.	11.9.98	1 c.c.	14.9.98	1 c.c.	29.9.98	3 c.c.	8.11.98	1 c.c.	—	—	Alive.
7	3250	„	—	16.8.98	9 c.c.	22.8.98	15 c.c.	27.8.98	1 c.c.	11.9.98	1 c.c.	14.9.98	1 c.c.	29.9.98	3 c.c.	8.11.98	1 c.c.	—	—	„
8	3250	„	—	16.8.98	12 c.c.	22.8.98	20 c.c.	27.8.98	1 c.c.	11.9.98	1 c.c.	14.9.98	1 c.c.	29.9.88	3 c.c.	8.11.98	1 c.c.	—	—	„
10	1713	„	—	19.8.98	9 c.c.	22.8.98	15 c.c.	27.8.98	1 c.c.	11.9.98	1 c.c.	14.9.98	1 c.c.	29.9.98	3 c.c.	8.11.98	1 c.c.	—	—	„
11	1713	„	—	19.8.98	12 c.c.	22.8.98	20 c.c.	27.8.98	1 c.c.	11.9.98	1 c.c.	14.9.98	1 c.c.	—	—	—	—	—	—	Died of plague.
43	3250 (1) / 3962 (2)	„	—	7.10.98	20 c.c.	21.10.98	20 c.c.	7.11.98	1 c.c.	—	—	—	—	—	—	—	—	—	—	„
44	3250 / 3962	„	—	7.10.98	20 c.c.	21.10.98	20 c.c.	7.11.98	1 c.c.	—	—	—	—	—	—	—	—	—	—	Alive.
45	3250 / 3962	„	—	7.10.98	20 c.c.	21.10.98	20 c.c.	7.11.98	1 c.c.	—	—	—	—	—	—	—	—	—	—	„
46	3250 / 3962	„	—	7.10.98	20 c.c.	21.10.38	20 c.c.	—	—	—	—	—	—	—	—	—	—	—	—	Died of peritonitis.
47	3250 / 3962	„	—	7.10.96	20 c.c.	21.10.98	20 c.c.	7.11.98	1 c.c.	—	—	—	—	—	—	—	—	—	—	Alive.
48	3250 / 3962	„	—	7.10.98	20 c.c.	21.10.98	20 c.c.	7.11.98	1 c.c.	—	—	—	—	—	—	—	—	—	—	„
52	4096	„	—	25.10.98	20 c.c.	2.11.98	20 c.c.	7.11.98	1 c.c.	—	—	—	—	—	—	—	—	—	—	„
71	4168 (*a*)	„	—	19.11.98	20 c.c.	—	—	21.1.99	1 c.c.	—	—	—	—	—	—	—	—	—	—	„
72	4168 (*a*)	„	—	19.11.98	20 c.c.	1.12.98	20 c.c.	6.12.98	One loopful.	12.12.98	1 c.c.	—	—	—	—	—	—	—	—	Died of enteritis.
73	4168 (*a*)	„	—	19.11.98	20 c.c.	1.12.98	20 c.c.	6.12.98	„	—	—	—	—	—	—	—	—	—	—	Died of tetanus.
74	4168 (*a*)	„	—	19.11.98	20 c.c.	1.12.98	20 c.c.	6.12.98	„	12.12.98	1 c.c.	—	—	—	—	—	—	—	—	Alive.
75	4168 (*a*)	„	—	19.11.98	20 c.c.	1.12.98	20 c.c.	—	—	—	—	—	—	—	—	—	—	—	—	Died of toxæmia.
76	4168 (*a*)	„	—	19.11.98	20 c.c.	1.12.98	20 c.c.	6.12.98	One loopful.	12.12.98	1 c.c.	—	—	—	—	—	—	—	—	Alive.
77	4168 (*a*)	„	—	19.11.98	20 c.c.	1.12.98	20 c.c.	6.12.98	„	12.12.98	1 c.c.	—	—	—	—	—	—	—	—	„
78	4168 (*a*)	„	—	19.11.98	20 c.c.	1.12.98	20 c.c.	—	—	—	—	—	—	—	—	—	—	—	—	Died of toxæmia. No organisms found, and no growth occurred in media.
79	4168 (*a*)	„	—	19.11.98	20 c.c.	1.12.98	20 c.c.	6.12.98	One loopful.	12.12.98	1 c.c.	—	—	—	—	—	—	—	—	Alive.
96	4168 (*a*)	„	—	14.12.98	20 c.c.	—	—	—	—	—	—	—	—	—	—	—	—	—	—	Died of plague.
97	4168 (*a*)	„	—	14.12.98	20 c.c.	—	—	—	—	—	—	—	—	—	—	—	—	—	—	Died of enteritis.

98	4168 (*a*)	Contaminated	—	14.12.98	20 c.c.	22.1.99	20 c.c.	29.1.99	1 c.c.	—	—	—	—	—	—	—	—	—	—	Died of plague.
99	4168 (*a*)	„	—	14.12.98	20 c.c.	—	—	—	—	—	—	—	—	—	—	—	—	—	—	Died of toxæmia. No organisms found, and no growth occurred.
100	4168 (*a*) / 4168	„	—	14.12.98	20 c.c.	—	—	—	—	—	—	—	—	—	—	—	—	—	—	Died of toxæmia. A few diplo bacilli motile seen in spleen.
101	4168	„	—	14.12.98	20 c.c.	—	—	—	—	—	—	—	—	—	—	—	—	—	—	Died of toxæmia. No organisms. No growth.
104	5324	„	—	16.12.98	20 c.c.	—	—	—	—	—	—	—	—	—	—	—	—	—	—	Alive.
105	4168 / 4168 (*a*)	—	Resterilised	23.12.98	20 c.c.	27.12.98	20 c.c.	1.1.99	1 c.c.	—	—	—	—	—	—	—	—	—	—	„
106	4168 (*a*)	Contaminated	—	23.12.98	20 c.c	22.1.99	20 c.c.	29.1.99	1 c.c.	—	—	—	—	—	—	—	—	—	—	„
111	5338	„	—	28.12.98	20 c.c.	—	—	21.1.99	1 c.c.	—	—	—	—	—	—	—	—	—	—	„
112	3962	„	—	28.12.98	20 c.c.	—	—	—	—	—	—	—	—	—	—	—	—	—	—	„
122	5315	—	Resterilised	2.1.99	6½ c.c.	—	—	10.1.99	1 c.c.	—	—	—	—	—	—	—	—	—	—	„
123	5315	—	„	2.1.99	6½ c.c.	6.1.99	6½ c.c.	10.1.99	1 c.c.	—	—	—	—	—	—	—	—	—	—	„
124	5315	—	„	2.1.99	6½ c.c.	6.1.99	6½ c.c.	10.1.99	1 c.c.	—	—	—	—	—	—	—	—	—	—	„
127	5338	—	„	7.1.99	20 c.c.	—	—	21.1.99	1 c.c.	—	—	—	—	—	—	—	—	—	—	„
128	5315	Contaminated	—	7.1.99	6½ c.c.	—	—	10.1.99	1 c.c.	—	—	—	—	—	—	—	—	—	—	Died of plague.
136	5315	„	—	14.1.99	6½ c.c.	21.1.99	6½ c.c.	29.1.99	1 c.c.	—	—	—	—	—	—	—	—	—	—	Alive.
137	5315	„	—	14.1.99	6½ c.c.	21.1.99	6½ c.c.	29.1.99	1 c.c.	—	—	—	—	—	—	—	—	—	—	„
140	5835	—	Resterilised	19.1.99	1 c.c.	21.1.99	1 c.c.	29.1.99	1 c.c.	—	—	—	—	—	—	—	—	—	—	Died of plague.
141	5835	Contaminated	—	19.1.99	1 c.c.	21.1.99	1 c.c.	29.1.99	1 c.c.	—	—	—	—	—	—	—	—	—	—	„
152	4096	—	Resterilised	1.2.99	12½ c.c.	—	—	8.2.99	1 c.c.	—	—	—	—	—	—	—	—	—	—	Alive.
153	4096	Contaminated	—	1.2.99	12½ c.c.	—	—	8.2.99	1 c.c.	—	—	—	—	—	—	—	—	—	—	„
161	5385	Sterile	—	13.2.99	2 c.c.	—	—	22.2.99	1 c.c.	—	—	—	—	—	—	—	—	—	—	„
162	5430	Contaminated	—	13.2.99	2 c.c.	—	—	22.2.99	1 c.c.	—	—	—	—	—	—	—	—	—	—	„
163	5385	—	Resterilised	14.2.99	2 c.c.	—	—	22.2.99	1 c.c.	—	—	—	—	—	—	—	—	—	—	„
164	5430	—	„	14.2.99	2 c.c.	—	—	22.2.99	1 c.c.	—	—	—	—	—	—	—	—	—	—	„
166	4839	Contaminated	—	26.2.99	2 c.c.	—	—	—	—	—	—	—	—	—	—	—	—	—	—	„
167	5300	„	—	26.2.99	2 c.c.	—	—	—	—	—	—	—	—	—	—	—	—	—	—	„
168	5344	„	—	26.2.99	2 c.c.	—	—	—	—	—	—	—	—	—	—	—	—	—	—	„
169	5843	„	—	26.2.99	2 c.c.	—	—	—	—	—	—	—	—	—	—	—	—	—	—	„
170	4839	—	Resterilised	27.2.99	2 c.c.	—	—	—	—	—	—	—	—	—	—	—	—	—	—	„
171	5300	—	„	27.2.99	2 c.c.	—	—	—	—	—	—	—	—	—	—	—	—	—	—	„
172	5344	—	„	27.2.99	2 c.c.	—	—	—	—	—	—	—	—	—	—	—	—	—	—	„
173	5843	—	„	27.2.99	2 c.c.	—	—	—	—	—	—	—	—	—	—	—	—	—	—	„

Remarks.

1. Haffkine's fluid is presumably a culture in peptone of the plague bacillus; the bacillus having been killed off the products remain.

2. The fluid is of uncertain strength. As received by us originally it was, without exception, contaminated with organisms of putrefaction. Latterly it has been much purer. It did not occur to us that a fluid sent out from a bacteriological laboratory for inoculation could be contaminated from inefficient or careless preparation or putting up, and we naturally supposed that the organisms we found in the fluid constituted part of its normal composition (one of us, Dr. Mullanah, dissents). The presence of the organisms in the fluid being adventitious or accidental they have no bearing on the question of the value of Haffkine's inoculation against plague, though it is of the last importance to insist on the necessity of preventing contamination of any kind from occurring in the fluid. It is probable that the opposition to vaccination in England is due to accidental contamination of the lymph ordinarily employed having given rise to occasional cases of septic intoxication which have been ascribed, by the ignorant public, to the vaccine lymph itself instead of to its impurities. In this way vaccination has been brought into such discredit that the Vaccination Acts of Great Britain have been practically repealed. There is very little doubt that in a similar manner injury has been done to the cause of Haffkine's inoculation against plague in India.

3. The effects of the contaminated fluid are two-fold. It produced:—

(*a*) Septic intoxication.

(*b*) The so called "reactionary" fever.

4. When employed by inoculation in rabbits—animals which are peculiarly resistant to ordinary septic processes, but are the best of all for experimental purposes with plague—it was found that large doses of the contaminated fluid protected against the effects of subsequent inoculation with plague. Small doses afforded little or no protection. But it was also found that eight animals died of the effects of the protective inoculation alone, while the protection against the effects of subsequent inoculation with plague was so limited that 33·4 °/₀ of the animals experimented upon died of plague.

5. The effects of the Haffkine fluid when resterilised are very different from the above. When employed experimentally in rabbits it was found that large doses are not always required. Moderate or even small doses afford adequate protection. No animals died of the protective inoculation alone, and the maximum of protection is afforded by the fluid against the effect of subsequent inoculation with plague.

6. The effects of inoculation of the sterilised fluid in rabbits, and also, as far as our limited experience goes, in man, are simple. It produces "reactionary" fever alone without, as a rule, any evil consequences. The question is, what is this "reactionary" fever due to? There can be little doubt that it is due to plague. (One of us, Mullanah, says it is due to the plague toxins.) Plague is a disease which shows itself in many forms. In its virulent forms it is highly infectious, in its mild aspects it is hardly infectious at all. To make inoculation universally acceptable what is now wanted is that Haffkine's fluid shall be made of uniform strength, so that with a minimum dose the reactionary fever shall be of known intensity and protective power, and not under any circumstances contagious.

APPENDIX No. LXX.

REPORT

ON A

Bacteriological Investigation in the Purity or otherwise of M. Haffkine's Plague Prevention Inoculation Fluid,

BY

Capt. C. A. Johnston, I.M.S.

From Captain C. A. Johnston, M.B., D.P.H., I.M.S., Medical Officer, 3rd Madras Lancers, to the President, Indian Plague Commission.

Sir, Secunderabad, 9th January 1899.

I have the honour to forward herewith as requested by the Indian Plague Commission, a report on a bacteriological investigation into the purity, or otherwise, of Haffkine's plague prevention inoculation fluid, accompanied with stained preparations of some of the organisms found.

The report has had to be drawn up rather prematurely this day, as I have to accompany my regiment out of Secunderabad on "Long reconnaissance," and shall be absent from the station for a week. I therefore regret that it cannot be so fully drawn up as it otherwise might have been.

Bacteriological Examination of Haffkine's Fluid.

Seven bottles of Haffkine's fluid, with the following numbers, taken at random from about 50 others, were examined:—

Brew No. 4168 (A) received at Hyderabad on 19th November 1898.
Brew No. 5324 received at Hyderabad on 16th December 1898.
Brew No. 5338 received at Hyderabad on 16th December 1898.
Brew No. 3962 received at Hyderabad on 20th October 1898.
Brew No. 4096 received at Hyderabad on 20th October 1898.
Brew No. 5349 received at Hyderabad on 16th December 1898.
Brew No. 5315 received at Hyderabad on 16th December 1898.

General Plan of Investigation.

Seven tubes of slant agar were inoculated with the above seven fluids, by allowing 10 to 15 minims of the fluid to flow in each case over the surface of the agar. The necks of the bottles and their mouths and corks were sterilized, and the corks were extracted with sterilized forceps, so as to allow no air or organisms to contaminate their contents and nullify the investigations. Two control tubes of the same preparation of agar were kept, into which tubes no Haffkine's fluid was poured, but whose cotton wool plugs were removed for a few seconds, to imitate the process of pouring fluid in, and then re-applied.

All these nine tubes were kept at the ordinary temperature of the room in a special glass case, and allowed to remain for one day, when they were examined, and it was found that there were growths of living organisms in every one of them except the controls:—Nos. 4168 (A), 5324, 3962, and 4096 showed luxuriant growths, the other three less, and No. 5315 showed least growth of all.

Details of the Experiments.

No. 1 Haffkine's Fluid: Brew No. 3962.

Ten to 15 minims poured over agar slant gave, after 24 hours, yellow white and ivory white discrete colonies all growing on the surface of the agar. There was no cloudiness, gas formation, or liquefaction. Microscopic examination revealed staphylococci in the white colonies, and bacilli and cocci mixed in the yellowish white colonies. The bacilli are short thick rods joined together in places forming chains, some short, others longer, and some are oval like cocci, and do not take the stain uniformly. The bacilli are actively motile and the threads lazily motile—decolorized by Gram. The first subculture in agar was made of the bacillary colonies (yellowish white), giving as a result yellowish white coalesced colonies, and a few isolated white colonies—these latter consisting of staphylococci and the former of a mixture of bacilli and cocci. A second subculture was therefore made giving also a mixture, but the cocci were fewer in number, so an attenuated third subculture was made with broth, and thrown into a petri dish which gave, in 24 hours a few faint bluish white colonies consisting of bacilli only. This was transferred as a fourth subculture on to agar slant tubes, which gave a pure growth of bacilli only—the growth consisting of a dirty white transparent film. Stained and hanging drop preparations of this film showed the following:—bacilli, fat, straight rods with rounded ends uniformly stained, some single, others joined end to end in pairs, freely motile, the single ones have a wriggling movement, the jointed ones an eel-like movement. The jointed ones appear to be the result of fission in older ones. The bacilli are all decolorized by Gram.

A rabbit (No. 126) was inoculated in the thigh with an emulsion in sterilized broth (4 c.c.) of these bacilli, and a fifth subculture in agar was made and kept. The rabbit (No. 126) had a slight fever (maximum 104·2) for two days, and is now well.

No. 2 Haffkine's Fluid: Brew No. 4096.

Fifteen minims of this was thrown over the surface of an agar slant tube and gave, after 24 hours, lemon yellow confluent colonies, white isolated colonies, and white moulds. No cloudiness of agar, nor liquefaction, nor odour were observed.

Microscopical examination revealed staphylococci in the yellow and white colonies—and mucor mucedo in the mould. No bacilli were present.

No. 3 Haffkine's Fluid: Brew No. 4168A.

Ten to 15 minims were poured over the surface of an agar slant tube, and within 24 hours there was a feathery greyish white growth—and after 48 hours a luxuriant growth of greyish white and yellow expansion. On using a magnifier the colonies are small, circular, and discrete, some being whitish and some yellowish white—at the bottom of the tube a distinct clump of ivory white colonies was seen, small, circular, and coalescing. No liquefaction of agar—all growing on surface.

Microscopic Examination.—By cover glass, stained with gentian violet, the microscope showed staphylococci, micrococci, diplococci, and some bacilli. Hanging drop preparation shows Brownian movement and very feeble motility.

1st Subculture.—A little of the culture was mixed with sterilized broth attenuated and spread over a petri dish of agar, and gave as a result, within 24 hours, separate lemon yellow colonies as well as small pearl-like discrete ones, and a clump of greyish white colonies. Cover glass preparations showed staphylococci in the lemon yellow colonies, cocci (mono. and diplo.) in the grey white clumps, and bacilli in the

pearly blue ones. A second subculture of these pearly blue colonies was made into agar slant tubes with the result that luxuriant growth took place in 24 hours, of small pearly blue colonies, which ccalesced. Cover slip and hanging drop preparations show a short fat coccoid bacillus, straight, very feebly motile, not of uniform thickness, not staining uniformly, with rounded ends, and no general true polar staining; decolorized by Gram's method, some of the bacilli appear like oval cocci.

A rabbit (No. 109) was inoculated with 3 c.c. of a mixture of these bacilli in sterilized broth into the subcutaneous tissue of the thigh, and as a result had slight fever—maximum temperature 103·6° for two days —no other signs of illness, and is now quite well.

A third subculture of the pearly blue colonies of the first subculture (petri dish) was made into agar slant and left for three days. When examined the same pearly blue coalesced colonies were seen, which on microscopic examination showed fields of cocco-bacilli which do not stain uniformly and are very feebly motile. A rabbit (No. 110) was inoculated in the thigh with 4 c.c. of this subculture mixed with sterilized broth, and had a rise of temperature to 105·2° for a day, then lysis to normal.

No. 4. Haffkine's Fluid: Brew No. 5315.

Ten to 15 minims thrown over agar slant gave within 48 hours a faint growth of isolated clear colonies —which on microscopic examination revealed staphylococci. No bacilli were found. Rabbits (Nos. 122, 123, and 124) were inoculated subcutaneously with this fluid, re-sterilized by fractional method to 55° C. for one hour for three consecutive days and gave with No. 122 a maximum temperature of 104·6° the same evening, and fever the following two days to 104·2°, then fell to normal. Rabbit No. 123 gave 105° same day, next day 106°, then lysis to normal. No. 124 gave 105° same evening and 105° next day, then lysis to normal. A rabbit No. 128 was inoculated with the same quantity of the same fluid unsterilized, and gave 104° the same evening and 104·4° next day.

These Haffkinized rabbits are to be tested as regards their immunity against an inoculation of plague.

No. 5. Haffkine's Fluid: Brew No. 5324.

Ten to 15 minims of this fluid were thrown over the surface of an agar slant tube. Within 24 hours it showed a thick luxuriant growth of greyish white colonies, some the size of a small pin's head; in other parts of the tube a whitish film. All the colonies are growing on the surface of the agar, without liquefaction or odour. Microscopic examination of these colonies by stained cover glass and hanging drop preparations show staphylococci and long bacilli, varying in length, with rounded ends, and straight rods. Some of the rods show swelling in the middle and do not stain uniformly. Hanging drop preparation shows these bulgings very distinctly and the bacilli are feebly motile. The bacilli are decolorized by Gram's method, but the bulging portion retains a little of the stain. They were mainly found in the whitish films of the culture, so the first subculture from this film was made on an agar slant tube which showed a luxuriant growth of whitish expansion as well as a yellow expansion. On microscopical examination these are found to consist of bacilli as well as cocci, so an attenuated second subculture mixed with sterilixed broth was thrown on to a petri dish. This showed three distinct forms of growth—a yellow brain-like expansion consisting mainly of staphylococci, a white brain-like expansion consisting mainly of cocci, and isolated grey white colonies consisting mainly of bacilli. A third subculture was therefore made of these isolated grey white colonies on to agar slant tubes. This third subculture shows a pure growth of bacilli which are straight rods, short and fat, somewhat like oval cocci, most of them non-motile, occasionally some of them, joined end to end, show motility.

[N.B.—A subculture of this has been sent to the Plague Commission.]

Rabbit No. 119 was inoculated with 4 c.c. of this third subculture in sterilized broth into the thigh. It gave a rise of temperature to 105° F. for one day and the temperature is 104° F. now.

A subculture was made of the yellow brain-like colonies of staphylococci from the second subculture of the petri dish to obtain a pure growth of the organism, and rabbit No. 121 was inoculated in the ear with 1 c.c. of the growth in sterilized broth, giving as a result pyrexia to 104° F. with local œdema, swelling, and congestion, and later an abscess and ulceration.

No. 6. Haffkine's Fluid: Brew No. 5338.

Ten to 15 minims of the fluid were poured over the surface of an agar slant tube, and after 48 hours a growth appeared as a thin dewy veil, with a few isolated colonies like dewdrops, and a dozen or so of raised round whitish colonies about the size of a small pin's head. No clouding or liquefaction or odour of agar.

Cover glass preparations (hanging drop and stained) were made and showed from the dewy veil and dew-drop colonies mainly cocci, while that from the white colonies showed bacilli.

The bacilli were short rods with rounded ends, in length about half the diameter of a blood corpuscle, some are in chains and a few are smaller like oval cocci—they stain fairly uniformly, but are decolorized by Gram's method. They have a very feeble movement in hanging drop—some are motionless. A first subculture of these whitish colonies was made on agar slant giving a fairly luxuriant growth of a yellowish white expansion which shows a mixture of staphylococci and bacilli. A further attenuation by a second subculture (mixed with sterilized broth) was made on a petri dish. The petri dish gave within 36 hours bluish white colonies containing long chains of bacilli—which were at once transferred by a third subculture on to an agar slant tube—which gave a growth of yellowish white colonies consisting only of bacilli. These bacilli varied in size, some were small and coccoid, others were in length about half the diameter of a blood corpuscle, and many were seen joined end to end (chiefly in pairs) giving short chains—their ends were rounded and they were rod shaped—showing feeble eel-like movements. A fourth subculture of this was made in agar slant and the remainder of the third subculture was mixed with 4 c.c. of sterilized broth and injected into the flank of a healthy rabbit (No. 120) on the 1st of January. Early on the morning of the 3rd January, within three days of inoculation, the rabbit died—the signs previous to death were firstly rise of temperature to 103·4°, then a sudden fall to sub-normal with giddiness and slight tremors; no diarrhœa. A *post mortem* examination was held the same morning with the following results:—

On reflecting the skin from the abdomen the blood in the vessels was seen to be very dark in colour and fluid, with patches of hæmorrhages on the subcutaneous surface of the abdomen and thorax. No enlarged glands were present. *Thoracic organs*—fluid in the pericardium—venous engorgement of the heart—lungs normal in appearance—heart blood dark and fluid. *Abdomen*—petechial hæmorrhages in abdominal wall. Large intestines very dark green in colour—spleen not enlarged but very dark. In the regions of the gall bladder, the liver, and adjacent surface of the stomach (which is distended with food) there is bile staining, the kidneys are highly congested and very dark in colour. Agar slant tubes were inoculated with a drop of the heart's blood, peritoneal fluid, spleen blood, and pulp—one pure agar slant tube being kept as control.

The blood, spleen, liver, and peritoneal fluid of the cadaver were examined microscopically, and bacilli were found in all in abundance, except the peritoneal fluid. The bacilli in all appear to stain uniformly (one or two in a field show polar staining) varying in length, but generally the length of half the diameter of a blood corpuscle, rod shaped, with rounded ends, some smaller and more oval and some joined end to end, giving a fission-like appearance. These bacilli are very similar morphologically to the bacilli in the subculture that was used for inoculation.

A fifth subculture was made from the fourth subculture, and another rabbit was inoculated.

Rabbit (No. 125) was inoculated with 4 c.c. of an emulsion of this subculture in sterilized broth, and had a temperature of 104·4 for one day and then lysis to normal.

The cultures in the agar tubes of the blood, &c., of rabbit No. 120, all show luxuriant bacillian growths except the control which shows none. These bacilli are found to be decolorized by Gram's method. The

growth appears to be of the following character:—greyish colonies, the size of pin's heads, circular, coalescing to form a greyish layer—that from the blood shows other kind of colonies raised small circular pearl-like rising above the greyish layer. The growth from the peritoneal fluid seems the purest. On microscopic examination of these growths, the one from the blood shows bacilli, some long rods, rounded ends, which are motile—others smaller, somewhat oval in form, which are not so motile; all show irregular staining, and are thicker at the ends than in the centre. The growth from the peritoneal fluid gave small rod-shaped bacilli, with rounded ends; and oval bacilli which are very feebly motile and do not stain uniformly. When treated first with alcohol and then stained they show polar staining. The culture from the liver shows two forms of bacilli, one set are polar stained, and the other, a longer rod, do not stain at poles. These bacilli are similar morphologically to some of the bacilli found in the subculture that was inoculated into the rabbit, and their culture growths are almost identical. These growths are to be further tested by inoculation into other rabbits, rats, fowls, and pigeons. A subculture of the culture from the rabbit's blood, when put into sterilized broth, made the broth cloudy and colonies fell to the bottom as a grey mass. Rabbit No. 125 had a rise of temperature to 104·5 the day following inoculation, and it is now falling towards normal.

A rabbit (No. 129) was inoculated with a subculture made from the peritoneal fluid of rabbit No. 120 that died, and gave a rise of temperature next day to 106·4°.

A pigeon was inoculated with a subculture of peritoneal fluid of rabbit No. 120 but only gave a rise of temperature to about one degree.

Two fowls were also inoculated subcutaneously and by injection into the mouth of subculture of peritoneal fluid and spleen of rabbit No. 120, but no pathogenic results followed.

The efficacy of re-sterilizing (by fractional method) and inoculation of this brew of Haffkine's fluid was tried on the following rabbits:—

Rabbit No. 111 was inoculated subcutaneously with 20 c.c. of the unsterilized fluid and showed a temperature of 106° (maximum) the second day, then lysis to normal, fever lasting four days. Rabbit No. 125 inoculated with the same quantity of re-sterilized H. F. 5338, subcutaneously, showed temperature of 104·8 (maximum) for a day.

No. 7. Haffkine's Fluid, No. 5349.

Ten minutes of this fluid thrown over agar slant gave within forty-eight hours isolated greyish white colonies and a whitish scum over the agar—no liquefaction or odour—later a few isolated white colonies appeared.

Microscope revealed only micrococci and staphylococci in the isolated white colonies. No bacilli were found.

Conclusions arrived at as the Result of the Experiments up to Date.

The samples of Haffkine's fluid examined bacteriologically all show contamination with living micro-organisms; in some cases—Nos. 5315, 5349 and 4096—the contamination seems to be with cocci alone, in the others, with a mixture of cocci and bacilli. Although the samples taken show the above contamination, other bottles of the same brew show contamination with other forms of micro-organisms. As far as the investigations have gone it will be seen that the micro-organisms isolated from the fluids are many of them distinctly pathogenic, some more virulently so than others, in one case (No. 5338) the microbe was so pathogenic as to cause death (*vide* rabbit 120). Rabbit No. 121 shows the effect of the contamination with cocci, which was the staphylococcus aureus, producing local disease (abscess and ulceration) and a pyæmia. If the temperature charts of the inoculations with one of the brews and the organisms isolated from that brew be compared, it will be seen that the inoculation with the fluid, as received from Bombay, gives a greater rise of temperature in most cases than the pyrexia produced by the isolated organisms; therefore, if the living organisms were extracted from the fluid before inoculation, a lower temperature ought to be the result of inoculating such a fluid. This idea seems to be confirmed when the temperature charts of rabbits 101 and 105 are compared. No. 101 is the result of inoculation of Haffkine's fluid No. 4168, giving a rise of temperature to 106°, followed by death; whereas No. 105 shows the result of inoculation of the same of Haffkine's fluid re-sterilized giving a resulting temperature of 104·5 as the maximum and, instead of death, recovery. This is one example of the effect of re-sterilizing Haffkine's fluid, but others are now being carried out. It would appear, therefore, that re-sterilizing does away with a severe form of septic fever and complications which arise from the contaminating organisms; but the question whether this re-sterilizing destroys the efficacy of the original Haffkine's fluid remains to be considered, this is also being done—*vide* temperature chart of rabbit No. 105 (Haffkinized with brew No. 4168). One c.c. of plague culture was inoculated on the 1st January, giving a slight fever only, whereas the control rabbit No. 115, inoculated on the same day with the same dose of the same culture, gave a rise of temperature to 107 and death. The re-sterilized Haffkine's fluid was tested as regards the presence of any living organisms at the same time as inoculation was done, but no living organisms were found to grow on the agar tube. Temperature charts of rabbits Nos. 122, 123, 124, 128 and 130 can be compared also, with regard to the question of re-sterilizing, but it ought to be borne in mind that this brew of Haffkine's fluid showed only slight contamination of cocci.

The following deductions may be drawn from the experiments that have been done so far:—

1stly, that in bottling the Haffkine's fluid from the retorts sufficient care does not seem to have been exercised in preventing contamination.

2ndly, allowing that same contamination is unavoidable, due to the enormous out-turn of fluid necessary to meet the demand, all the bottles on being filled and before being sent away should be re-sterilized to 60° C. for an hour, and the corks should be replaced by proper fitting glass stoppers. This probably would not destroy the chemical products of putrefaction. If this were done, and if it was proved that re-sterilizing does not injure the protective influence of the inoculation fluid, then I feel sure there would be less number of cases of septic poisoning among the inoculated subjects, for the fluid which is received now from Bombay is undoubtedly a "putrescent organic liquid," as stated before the Commission by the Plague Commissioner, Hyderabad.

Further investigations and experiments with reference to the inoculation fluids and their isolated living organisms are being carried out by the authorities in charge of the laboratory in Hyderabad in continuation of the above work, and if so required, a further report at a later date can be furnished by them.

My thanks are due to Lieut.-Col. E. Lawrie, I.M.S., for allowing me to use the laboratory; and to his staff for their kind assistance during the above work.

PLAGUE—HYDERABAD STATE.

INVESTIGATIONS TABULATED.

Bacterioscopic Examination of Haffkine's Fluid.

No. 1.

Agar slant.
+
H. F. No. 4168 A.

Luxuriant growth, a mixture of cocci and bacilli.

1st subculture.
Attenuated in broth; and a petri dish inoculated.

3 separate colonies.
A. Lemon yellow=staphylococci.
B. Grey white =cocci {mono— diplo—
C. Pearly blue =bacilli.

C

2nd Subculture agar slant.
One day's growth.

Bacilli, pure growth of pearly blue circular colonies coalesced.
Bacilli, short fat (cocco-bacilli) feebly motile.

Rabbit No. 109 was inoculated with 3 c.c. of this growth mixed with broth.
Result, fever slight (103·6° max.) for 3 days—now alive and well.

3rd Subculture agar slant.
Three days' growth.

Bacilli, pure growth, pearly white coalesced colonies, mainly cocco-bacilli.

Rabbit No. 110 inoculated. Result, fever 4 days max. temp. 105·2°, now alive and well.

No. 2.

Agar slant.
+
H. F. No. 5324.

Luxuriant growth of Staphylococci and bacilli.
1st subculture.

Attenuated by subculture in agar slant.

2nd subculture with broth (control of broth, done 3 Jan. 1899.)
Attenuation on to petri dish.

Three kinds of growth.
A. Yellow brain-like growth =cocci.
B. White do. do. =cocci.
C. Isolated grey-white colonies=bacilli.

C. Baccilli.

3rd Subculture into agar slant.
One day's growth.

Luxuriant pure growth of whitish grey film of bacilli.
Bacilli=short fat coccus-shaped, spore formation seen, motile at times, but chiefly non-motile.

(A special subculture of this was sent to the Plague Commission.)

Rabbit No. 119 inoculated with 4 c.c. of this pure subculture:
Result, fever, max. temp. 105°.

A. and B. Cocci.

Subculture in agar of pure staphylococci.

Rabbit's ear (121) inoculated with above, gave local abscess and ulceration.

No. 3.

Agar slant.
+
H. F. No. 5338.

Growth, mixture of cocci and bacilli.

Subculture in agar slant.

Luxuriant growth of cocci and bacilli.

Attenuated 2nd subculture in petri dish.

Yellow and white colonies of staphylococci.
Blue white colonies of bacilli.
Bacilli=irregular size, some thin, others thick, jointed and oval (motile).

* A subculture (4th) kept, and a rabbit No. 120 inoculated with 4 c.c. of culture in broth.

Death within 60 hours, temp. sub-normal.

Cultures in agar were made of spleen liver and peritoneal fluid of this rabbit, giving growth of bacilli similar to the bacilli in the subculture.

* The subculture shows thickish bacilli motile growing by fission

No. 4.

Agar slant.
+
H. F. No. 3962.

Luxuriant growth, mixture of cocci and bacilli; cocci=mono, diplo, and staphylococci.

Subculture in agar slant.

A. Yellow coalesced colonies=bacilli and cocci.

B. White isolated colonies =staphylococci

2nd Subculture in agar.

Bacilli chiefly, also a few cocci.

3rd. Attentuated subculture with broth on to petri dish—shows gossamer veil expansion over which a thick yellow white expansion is present.

4th. Subculture on agar slant gives luxuriant growth of yellow white colonies of bacilli similar morphologically to bacilli from 5338.

Subculture on agar slant of liquid yellowish expansion.

Subculture on agar slant.

Rabit No. 126 inoculated with 4 c.c. of bacilli subcutaneously, result, slight fever, now well.

No. 5.

Agar slant.
+
H. F. No. 4096.

Luxuriant growth of—
Lemon yellow colonies=Staphylococci.
White =Staphylococci.
Moulds whitish =Mucor mucedo.
No bacilli found.

No. 6.

Agar slant.
+
H. F. No. 5349.

Growth of isolated grey white colonies of micro-cocci only.
One or two white isolated colonies of staphylococci.

No. 7.

Agar slant.
+
H. F. No. 5315.

Faint growth of clear hyaline-like colonies of staphylococci.
No bacilli.

APPENDIX No, LXXI (i).

APPENDIX No. LX

PLAGUE CHAR

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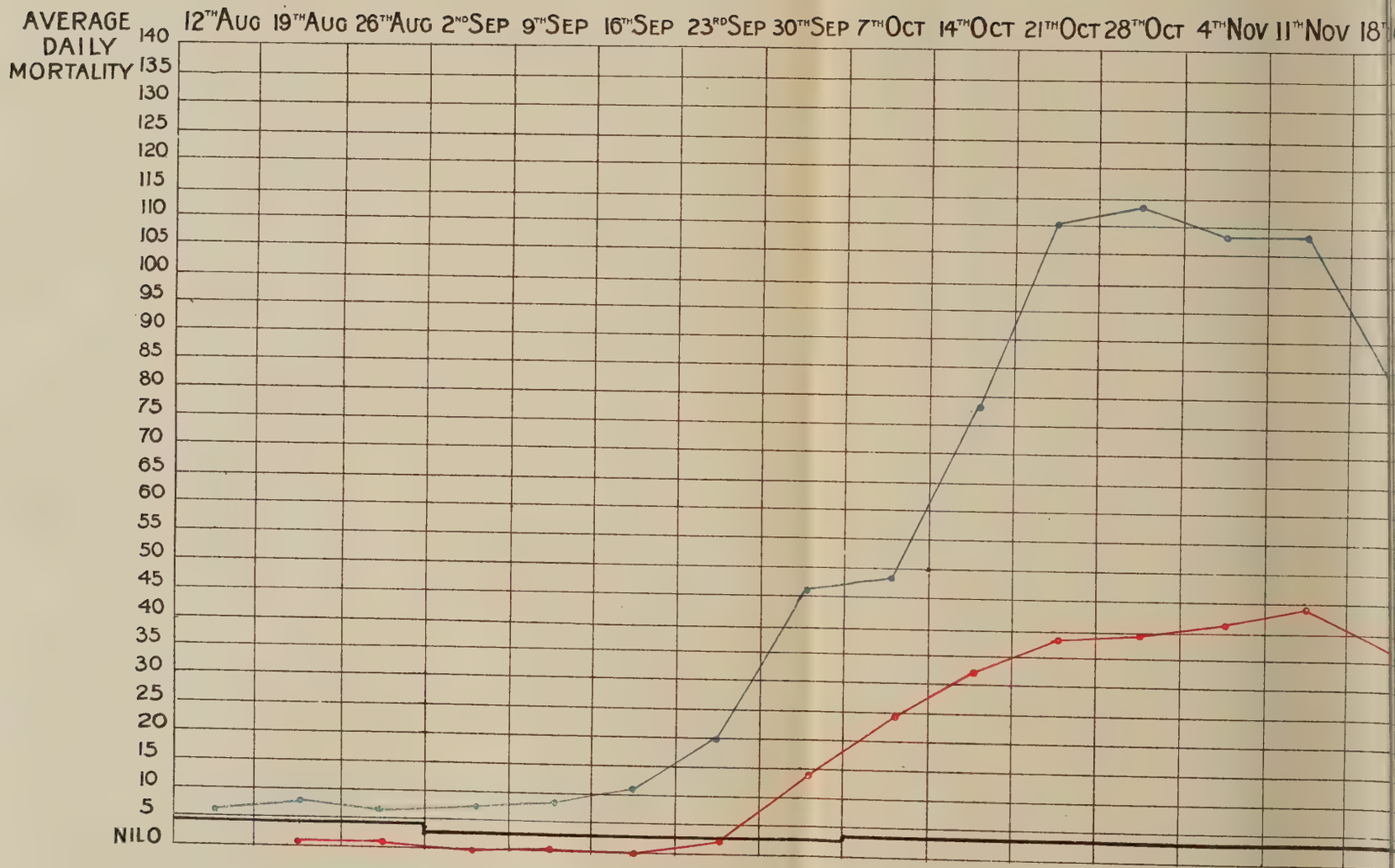

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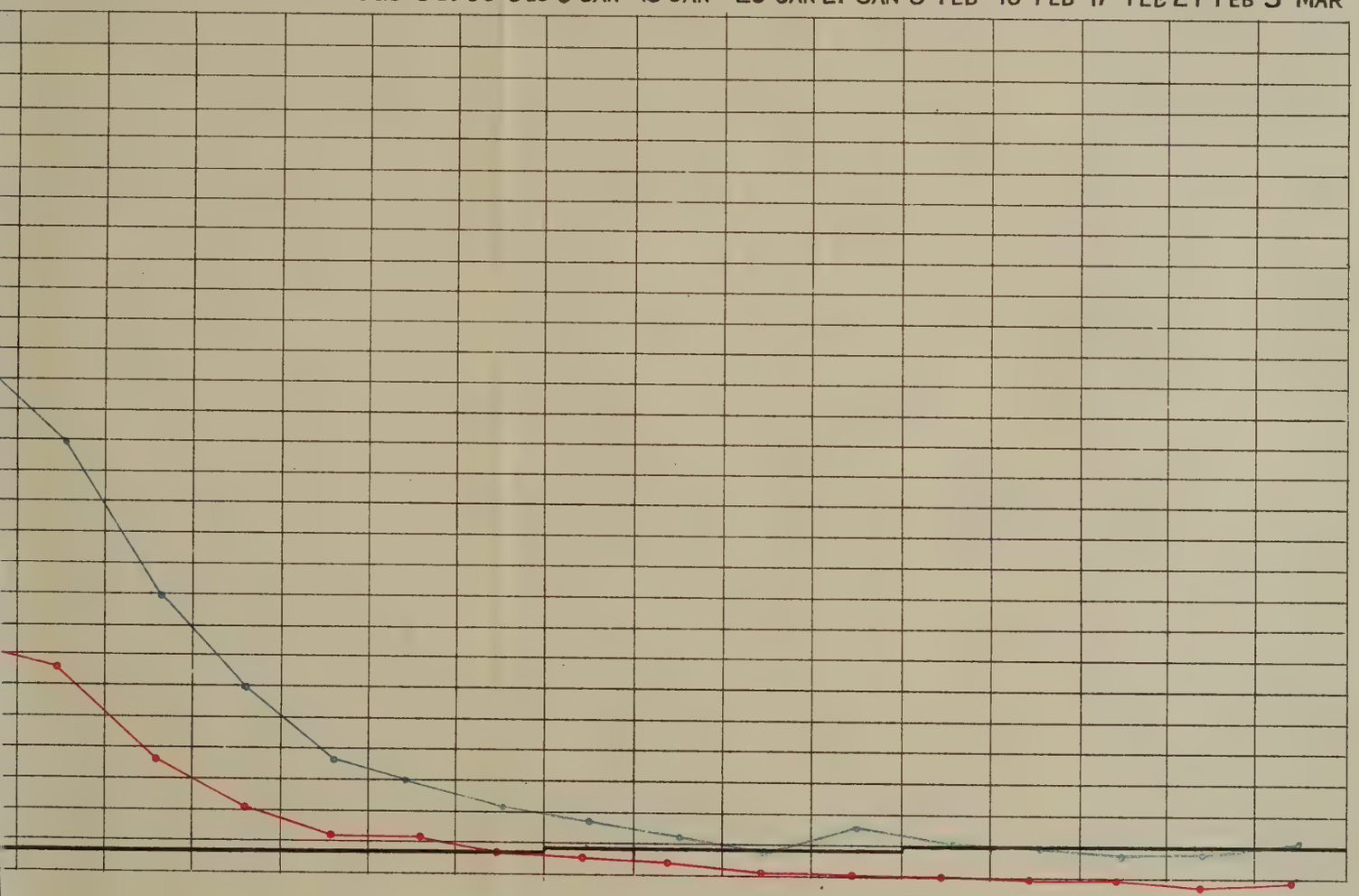
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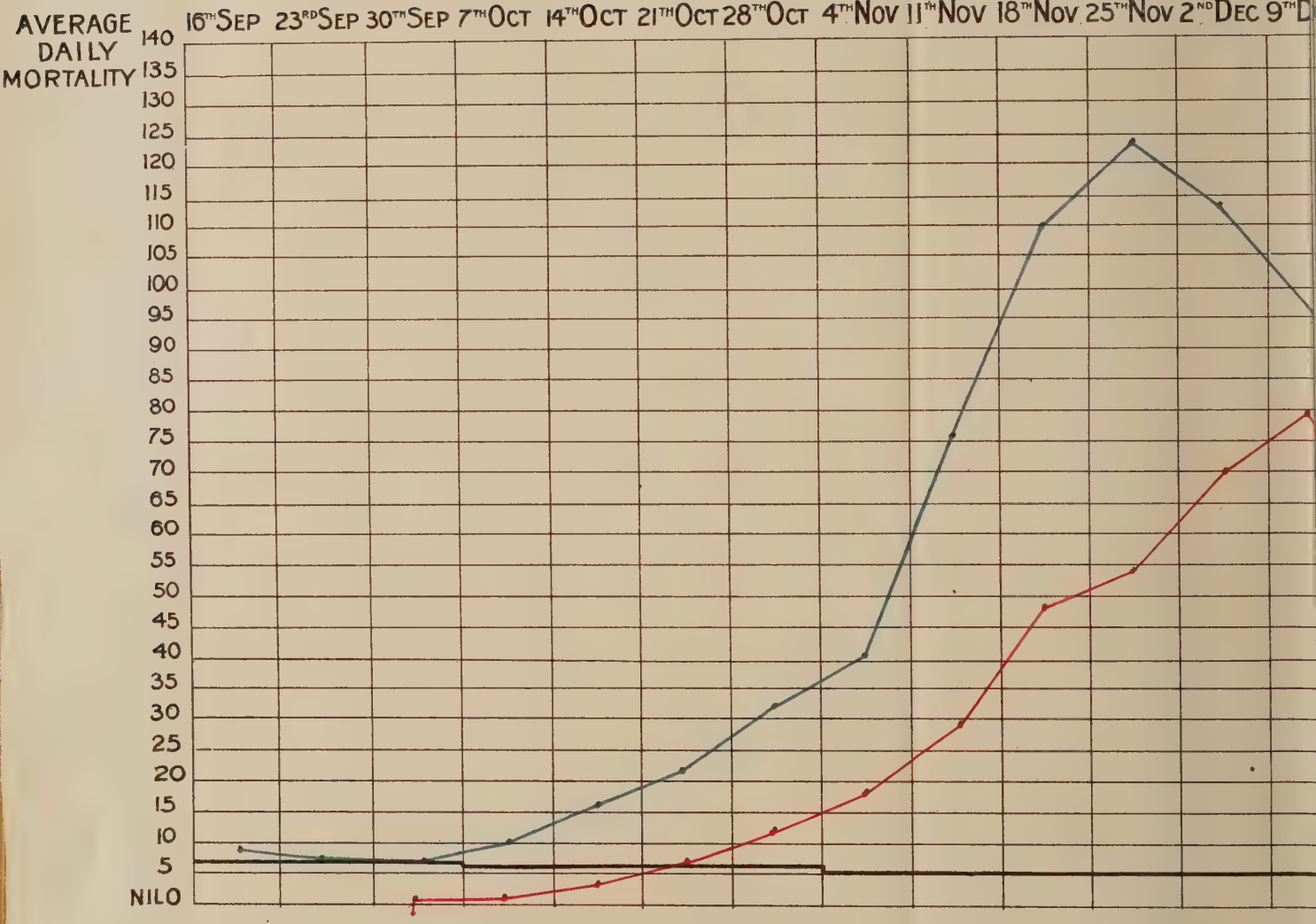

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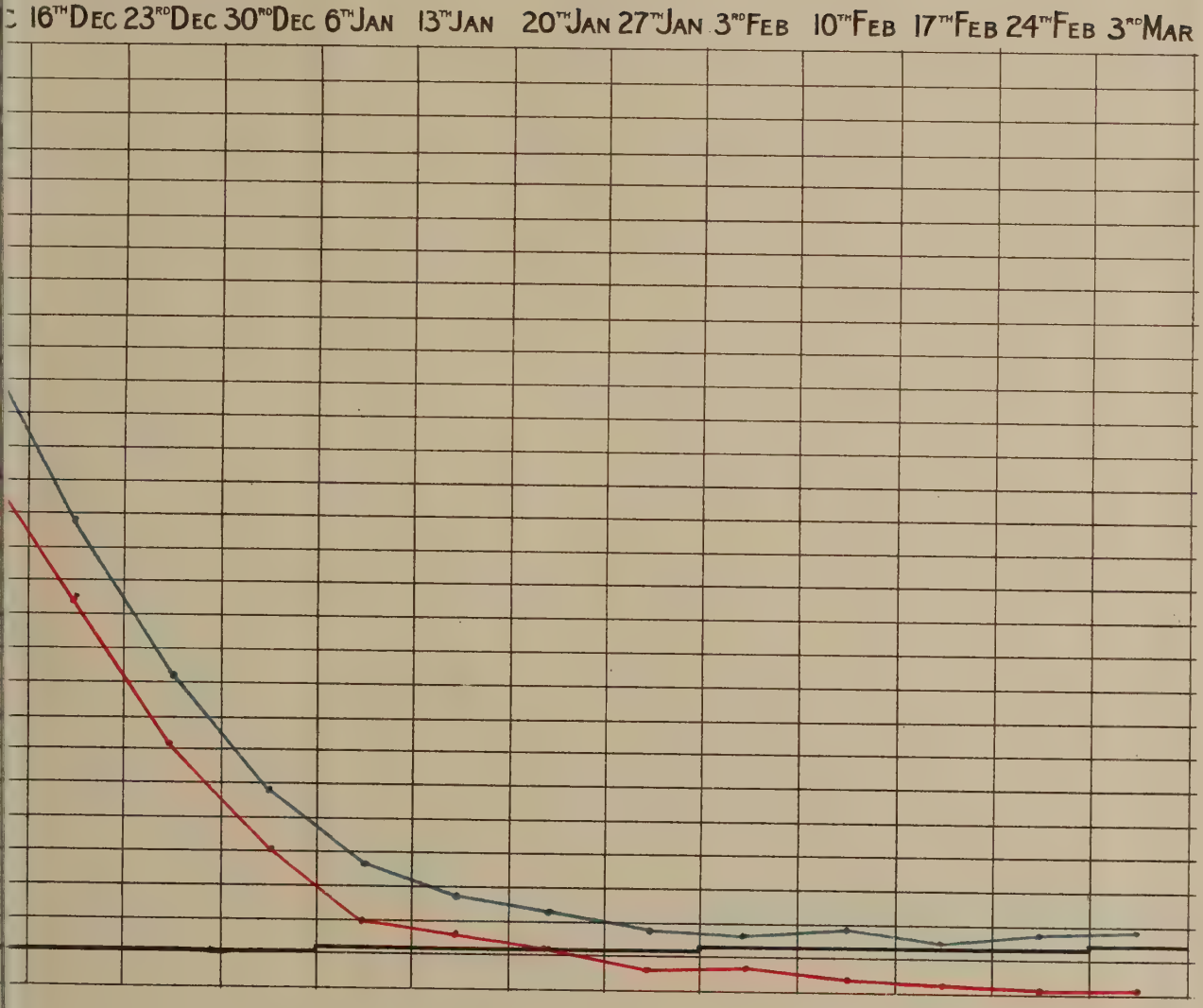

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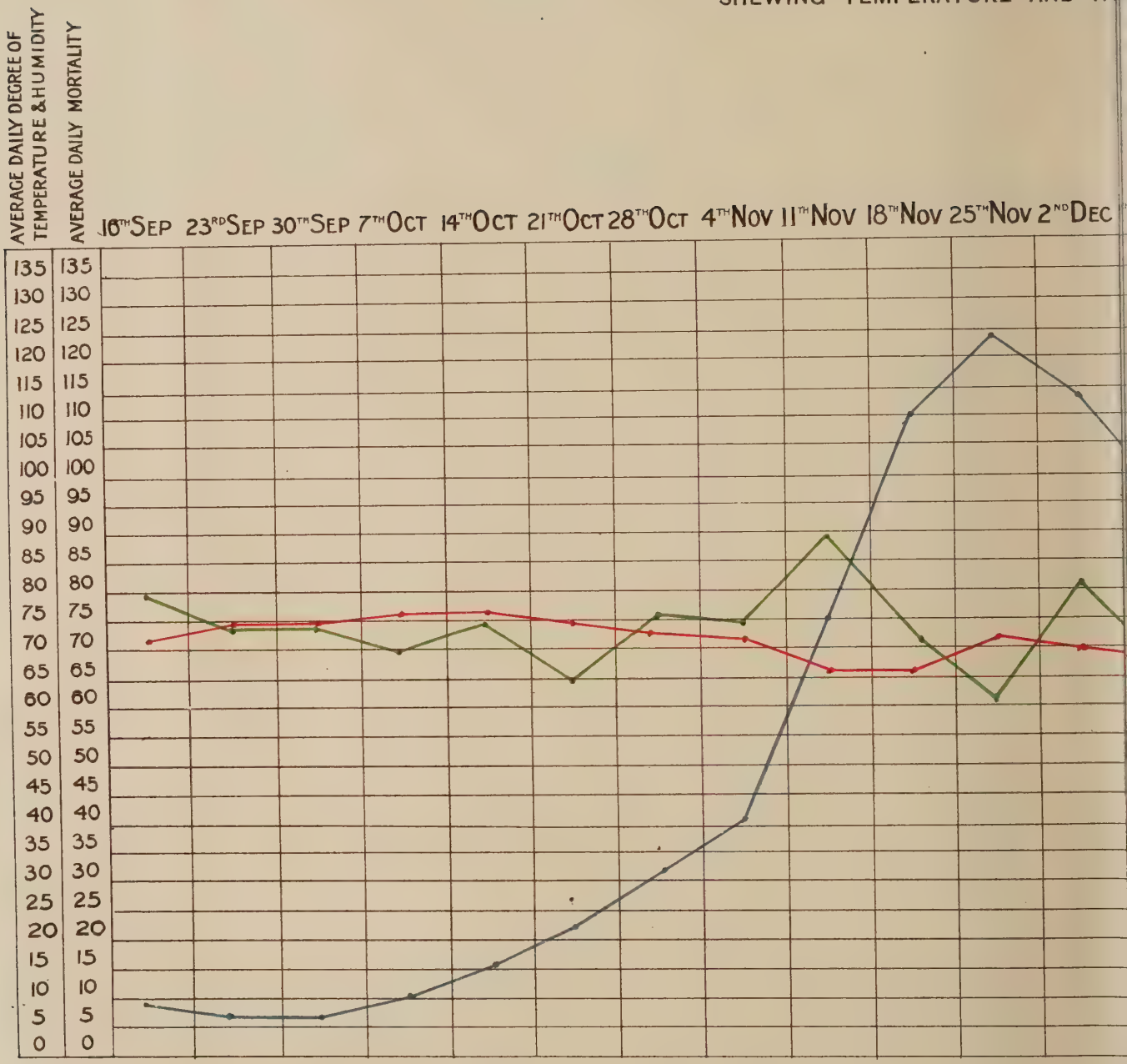

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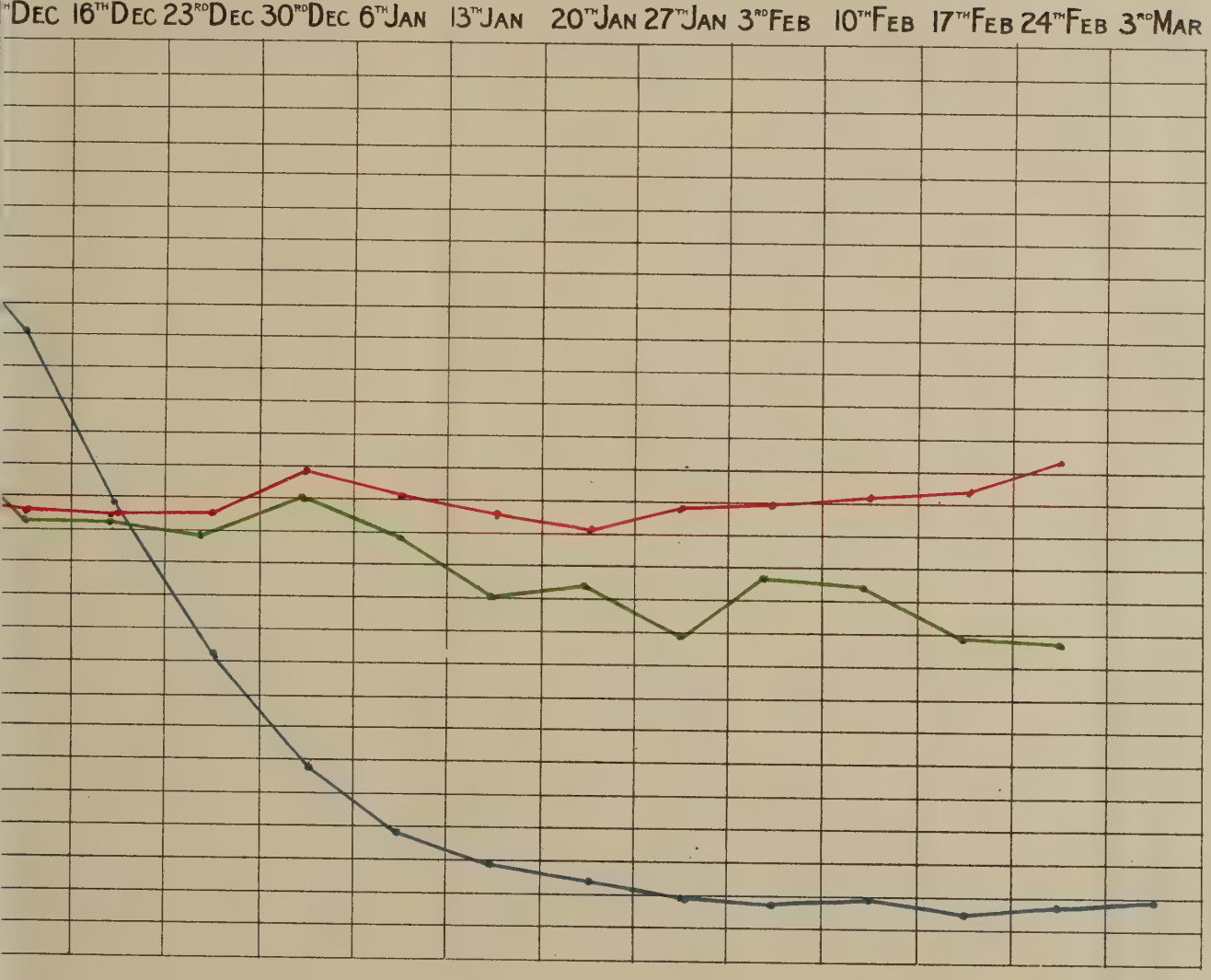

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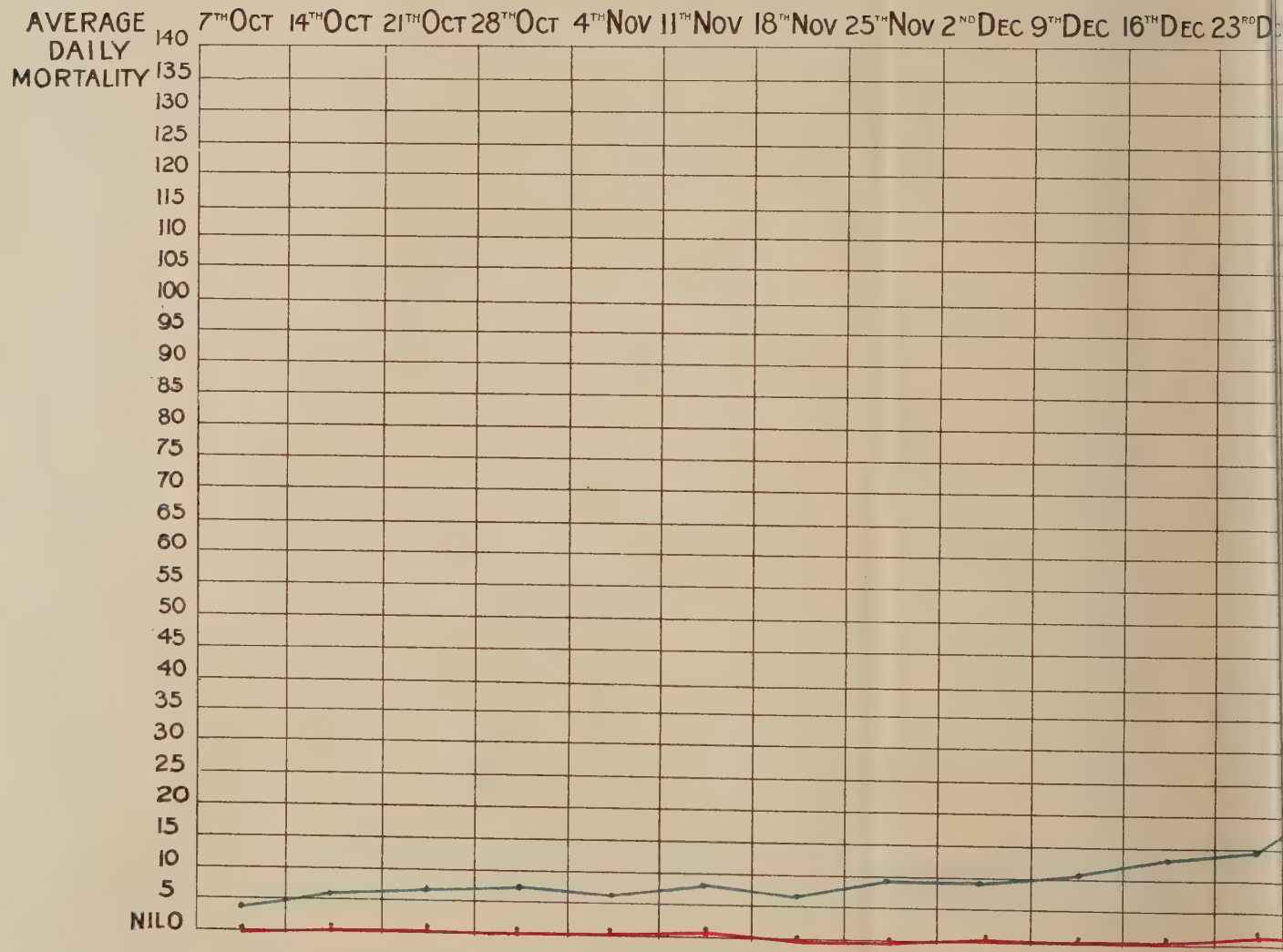

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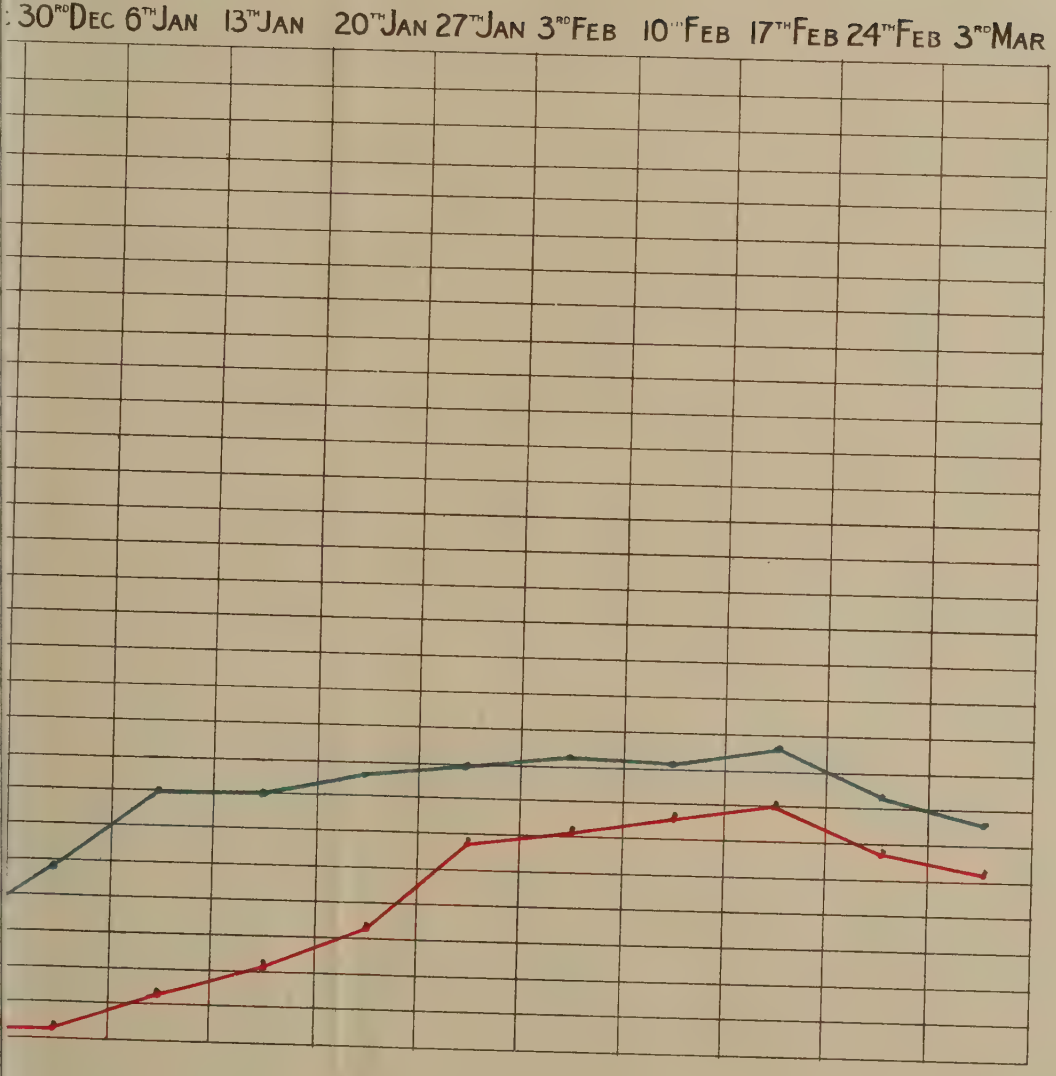

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No. 6.

Agar slant.
+
H. F. No. 5349.

Growth of isolated grey white colonies of micro-cocci only.
One or two white isolated colonies of staphylococci.

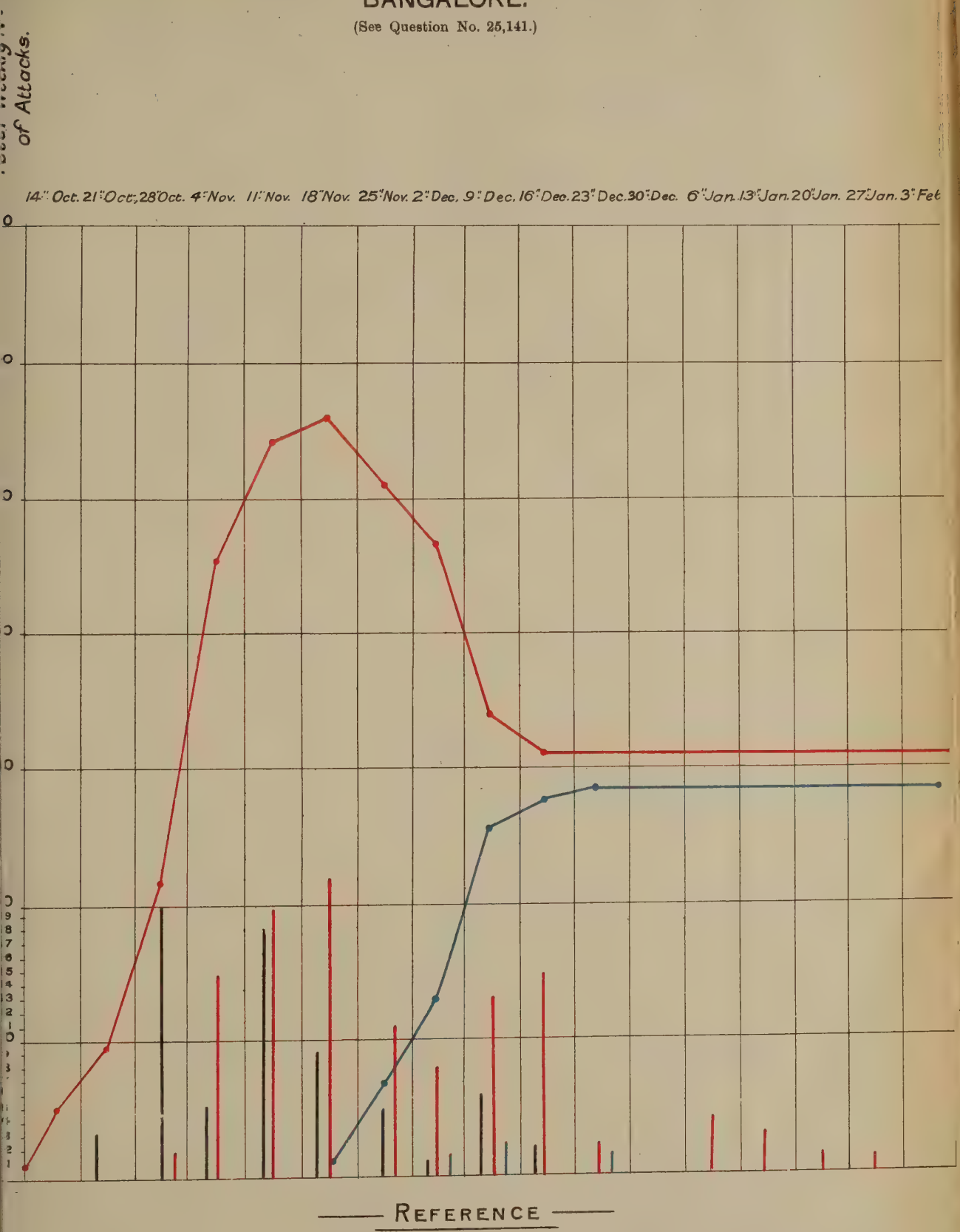
No. 7.

Agar slant.
+
H. F. No. 5315.

Faint growth of clear hyaline-like colonies of staphylococci.
No bacilli.

PLAGUE AND INOCULATION AMONG MILITARY POPULATION, BANGALORE.

(See Question No. 25,141.)



REFERENCE

RED CURVED LINE SHEWS TOTAL NUMBER ONCE INOCULATED WEEK BY WEEK

BLUE—Dº——Dº——Dº——Dº——Dº——TWICE——Dº——Dº——Dº—

BLACK VERTL Dº——Dº——SEIZURES AMONG UNINOCULATED

RED——Dº——Dº——Dº——Dº——Dº——Dº—ONCE INOCULATED.

BLUE—Dº——Dº——Dº——Dº——Dº—TWICE——Dº——

USHER
INSTITUTE
LIBRARY

HUBLI

IN THE DHARWAR DISTRICT OF BOMBAY,

TO ILLUSTRATE
THE OUTBREAK OF PLAGUE.

(See Question No. 1649.)

3	8–12–97
4	13–12–97
5	14–12–97
6	20–12–97
7	31–12–97
8	9– 1–98
9	10– 1–98
10	18– 1–98
11	30– 1–98
12	1– 2–98
13	3– 2–98
14	7– 2–98
15	4– 3–98
16	5– 3–98
17	Do.
18	7– 3–98
19	8– 3–98
20	12– 3–98
21	22– 3–98
22	11– 4–98
23	13– 4–98
24	14– 4–98
25	Do.
26	18– 4–98
27	Do.
28	21– 4–98
29	26– 4–98
30	29– 4–98
31	2– 5–98
32	6– 5–98
33	10– 5–98
34	14– 5–98
35	Do.
36	15– 5–98
37	17– 5–98
38	18– 5–98
39	Do.
40	Do.
41	23– 5–98
42	24– 5–98
43	26– 5–98
44	27– 5–98
45	Do.
46	Do.
47	Do.
48	29– 5–98
49	Do.
50	Do.

PROGRESS OF PLAGUE

Outbreak	Date
1	13th August 1898
2	14th d°
3	16th d°
4	16th d°
5	20th d°
6	21st d°
7	21st d°
8	22nd d°
9	27th d°
10	31 d°
11	1st Sepr 1898
12	3rd d°
13	3rd d°
14	3rd d°
15	20th d°
16	20th d°
17	7th October 1898

NOTE. (I) Town outbreak
(II) Suburb outbreak from (8)

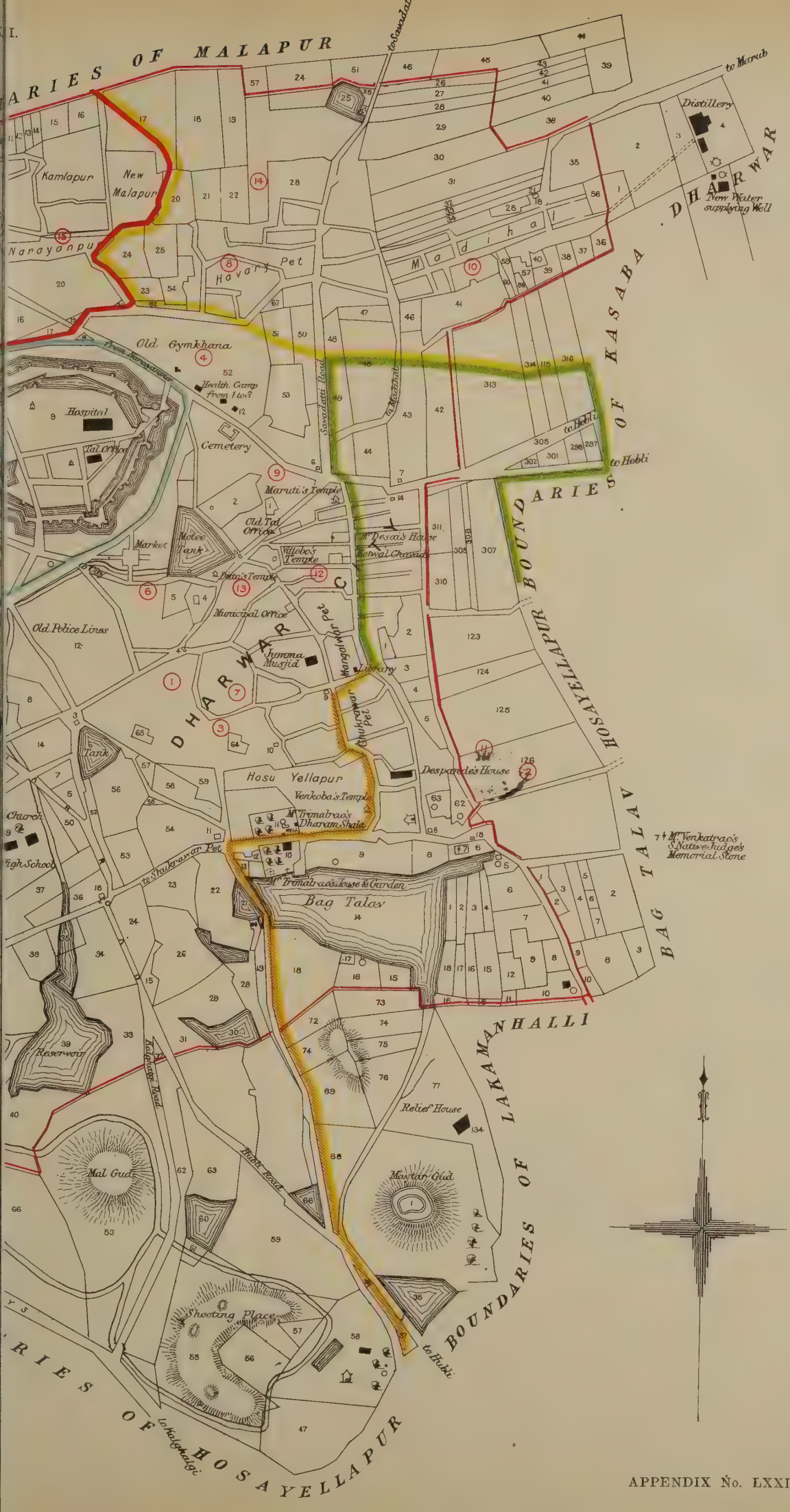
MAP OF
DHARWAR CITY
IN THE BOMBAY PRESIDENCY,
TO ILLUSTRATE THE OUTBREAK OF PLAGUE.

(See Question No. 1649.)

Scale of 20 Chains to an Inch

10 5 0 10 20 30 40 50 60 70 80 90 100 110 120

Police Chowkies from 1 to 13.
Octroi Posts from 14 to 21.
Red line shews the Municipal Limits


ARIES OF MALAPUR
to Sawadatti
to Maroob
Distillery
New Water supplying Well
Kamlapur
New Malapur
Narayanpur
Havary Pet
Madiha
DHARWAR
KASABA
Old Gymkhana
From Narayanpur
Health Camp From Hoti
Hospital
Tal. Office
Cemetery
Sawadatti Road
to Mundhal
to Hebli
to Hebli
BOUNDARIES OF
Maruti's Temple
Old Tal Office
Molee Tank
Market
Pir's Temple
Municipal Office
Old Police Lines
Jumma Musjid
Mangalwar Pet
Library
Chukrawar Pet
Mr Desai's House
Motwal Chavadi
Villoba's Temple
DHARWAR
Tank
Hosu Yellapur
Venkoba's Temple
Mr Tromatrao's Dharam Shala
Despande's House
HOSAYELLAPUR
TALAV
BAG
Mr Venkatrao's Native Judge's Memorial Stone
Church
High School
to Shukrawar Pet
Mr Tromatrao's House & Garden
Bag Talav
LAKAMANHALLI
Reservoir
Kalghatgi Road
Relief House
Hubli Road
Mal Gud
Mastar Gud
Shooting Place
to Hubli
BOUNDARIES OF
HOSAYELLAPUR
to Kalghatgi

APPENDIX No. LXXV.

STATISTICS regarding VESSELS which left BOMBAY from 1st December 1898 to 15th March 1899.

(In continuation of the figures published in App. No. X. in Vol. I.)

1898-99.	Vessels left for Aden, Red Sea, and Europe.					Vessels left for Ports out of India, other than Aden, Red Sea, and Europe.					Vessels left for Indian Ports, including Burma.				
	No. of Vessels.	No. of Crew.	No. of Passengers.	Total No. of Crew and Passengers.	No. rejected.	No. of Vessels.	No. of Crew.	No. of Passengers.	Total No. of Crew and Passengers.	No. rejected.	No. of Vessels.	No. of Crew.	No. of Passengers.	Total No. of Crew and Passengers.	No. rejected.
Dec. 1898 - -	21	2,350	2,185	4,535	59	57	2,550	1,204	3,754	49	6,308	48,766	56,352	105,118	1,327
Jan. 1899 - -	31	2,807	4,645	7,452	36	81	2,968	1,271	4,239	35	6,484	47,896	58,698	106,594	1,724
Feb. „ - -	29	4,550	2,209	6,759	48	71	2,419	835	3,254	44	6,510	46,613	64,287	110,900	3,135
Mar. 1 to 15, 1899	16	1,616	2,884	5,500	23	24	1,013	512	1,525	60	3,648	26,524	38,732	65,256	1,673
Totals for 1897 -	233	27,388	29,060	56,448	181	375	18,350	14,512	32,862	206	39,957	316,322	305,082	621,404	2,935
„ „ 1898 -	381	30,991	26,997	57,988	552	402	20,742	12,963	33,705	547	52,609	430,249	399,886	830,135	3,812
Jan. to 15 Mar. 1899.	76	8,973	9,738	18,711	107	176	6,400	2,618	9,018	139	16,642	121,033	161,717	212,750	6,532
Grand Total	690	67,352	65,795	133,147	840	953	45,492	30,093	75,585	892	109,208	867,604	866,685	1,734,2	23,279

Total number of vessels examined - - - -	110,851
„ „ „ crew „ - - - -	980,448
„ „ „ passengers „ - - - -	962,573
„ „ „ persons rejected - - - -	25,011

APPENDIX No. LXXVI.

NOTES ON THE TRAFFIC

between BOMBAY and MADAGASCAR

with special Reference

to the OUTBREAK of PLAGUE in TAMATAVE.

I. The Island of Madagascar, situate off the East Coast of Africa, forms the Eastern boundary of the Mozambique Channel. At the northern extremity of the Island is situate Dioga Saurez, the nearest port to Bombay, and much further down its eastern coast, the Port of Tamatave. On the northern half of the western face of the Island are situated Majunga and Nossi Bé: and between these latter and the Mozambique and Zanzibar portion of the opposing African shore the Islands of Mayotta and Comoro which, like Madagascar, belong to the French. All these places have at one time or other been called at by vessels leaving Bombay, and will be referred to again in what follows.

II. It is important to note that the minimum duration of the voyage from Bombay to the nearest port in Madagascar, viz., Dioga Saurez, for the class of steamers that run in that direction, is 15 or 16 days, and, for country craft, under the most favourable conditions of wind and weather, a month.

III. The outbreak of plague at Tamatave, and the subsequent statement of Dr. Proust that it had been proved that the disease had been carried to the Island, not by native craft, but by a ship laden with rice from Bombay, led to a careful investigation, having for its object the drawing up of a precise statement of the nature and amount of traffic, if any, between this port and Madagascar. The lines on which this investigation were carried out were as follows:—

Carefully kept registers of all ships leaving Bombay were scrutinised, and those leaving, not only for Madagascar, but for all Islands in the Indian Ocean lying west of India, and for all ports on the African coast were carefully tabulated. Such of the official logs of these ships as were available were then carefully examined, and the owners or agents of all ships in the list were interviewed.

IV. The information obtained was as follows:—

(1.) STEAMERS.

(*a.*) The British India Steam Navigation Company have not despatched any vessel, directly or indirectly, to any port in Madagascar probably for the last 10 and certainly for the last six years; nor have they booked cargo or passengers for trans-shipment to vessels bound for any port in the French Colony. They frequently send cargo to Zanzibar, and it may be that Zanzibar merchants may, and probably do, send portions of it on to Malagise ports, but the connection is a very indirect one indeed, the cargo having first to be landed and lie for some time in stores before being reshipped.

(*b.*) The German East Africa Company's steamers ply between Bombay and the East Coast of Africa, others of their vessels run up and down the African coast. The former never by any chance call at Bourbon, Mauritius, or Madagascar. The latter do call at Malagise ports. The Company have never sent either passengers or cargo from Bombay for trans-shipment on to their African steamers for Madagascar.

(*c.*) The Persian Steam Navigation Company have had no ships running to Madagascar since the end of 1897. Prior to this date (end of 1897) they did despatch an occasional vessel with cargo and passengers from Bombay and had intended to keep up a regular traffic with the Island, but a heavy tax on British borne goods, especially rice, and a heavy annual poll-tax on British subjects, natives of India, for the privilege of living in the Colony imposed by the French Government, has completely choked off the trade. The last of these vessels that had called at Madagascar was the s.s. *Mohammadi*. She left Bombay on the 9th of August 1897 with 691 passengers and a cargo composed largely of rice; ran direct to Delagoa Bay, where she discharged the greater number of her passengers and a portion of her cargo, and then proceeded to Madagascar, where she discharged the remainder of her passengers (no more than 50 in all (chief officer's statement)) and the following quantities of rice:—

Majunga	about	600 bags.
Nossi Bé	,,	800 ,,
Dioga Saurez	,,	2,900 ,,
Tamatave	,,	300 ,,

On this voyage the official log-book shows but one case of sickness, that of a native cook who was certified by the ship's surgeon to have died of "paralysis." This death took place between Dioga Saurez and Tamatave, 41 days after leaving Bombay, and after an illness of 30 days.

(*d.*) Mr. Haji Kasim has had steamers running from Calcutta, False Point, Balasore and other ports on the east coast of India to Bourbon, and perhaps Mauritius. These steamers may have been chartered from these places for Madagascar, but he is unable to give the details without reference to his agents on the other side of India. During the whole of 1898 and 1899 up to date, he has not sent any steamers from Bombay or any port on the west coast of India, directly or indirectly, to any port in Madagascar; nor has he, *at any time*, sent passengers or cargo to the Colony. During 1897 some of his Bombay ships did go to Madagascar but all these were chartered from Mauritius, left the Island in ballast and returned with bullocks. These steamers may have on rare occasions carried cargo to Madagascar, but this was Mauritius and not Indian cargo. The cessation of traffic with Madagascar, he stated, was due to the extraordinary high rate of duty charged on goods carried under the British flag.

(2.) COUNTRY CRAFT.

The pre-monsoon country craft traffic for 1898 began with a departure on January 8th and ended with one on the 7th of March. Between these two dates 17 vessels left Bombay for various ports in Madagascar, but none of these were bound for Tamatave. So that if they did ultimately go to that port they must have done so in the coasting trade of the Island. It is important to note here that Dr. Proust states that it has been distinctly proved that the plague was not carried to Madagascar by these vessels.

The post-monsoon traffic of 1898 opened with a departure on the 12th of December. From this date till the end of the year five vessels took out their port clearances and bills of health for Madagascar. Here again, not only were none of these vessels bound for Tamatave, but, to quote from a letter from the French Consul at Bombay, they would have arrived at their destinations after Tamatave was declared an infected port.

The following are precise details of this traffic:—

PRE-MONSOON.

ates.	Name of Ship.	Port bound for.
898.		
n. 8	Batel Sawai	Nossi Bé.
„ 8	Bugla Sahula	Mayotta.
„ 15	Gunja Fatch Salamat	Nossi Bé.
„ 15	„ Ali Madat	Nossi Bé.
„ 15	„ Daria Dowlat	Nossi Bé.
„ 24	Batel Jairam Parsad	Majunga.
„ 29	Gunja Hedree	Nossi Bé.
„ 31	Batel Daulat Parsad	Majunga.
b. 1	Gunja Daulat Parsad	Comoro.
„ 4	„ Kulter Parsad	Mayotta.
„ 4	„ Duruka Parsad	Majunga.
„ 4	„ Fatch Halbat	Mayotta.
„ 5	„ Sugar Parsad	Comoro.
„ 15	„ Fatch Karim	Majunga.
ar. 2	„ Ali Madat	Majunga.
„ 7	„ Fateh Salamat	Mayotta.
„ 7	Batel Sanklee	Comoro.

POST-MONSOON.

ec. 12	Gunja Fateh Salamat	Nossi Bé.
„ 16	„ Ali Madat	Dioga Saurez.
„ 17	„ Naran Parsad	Mayotta.
„ 19	„ Luckmi Parsad	Mayotta.
„ 24	Batel Husseinie	Mayotta.

V. The known facts of the case may thereupon be summarised as follows:—

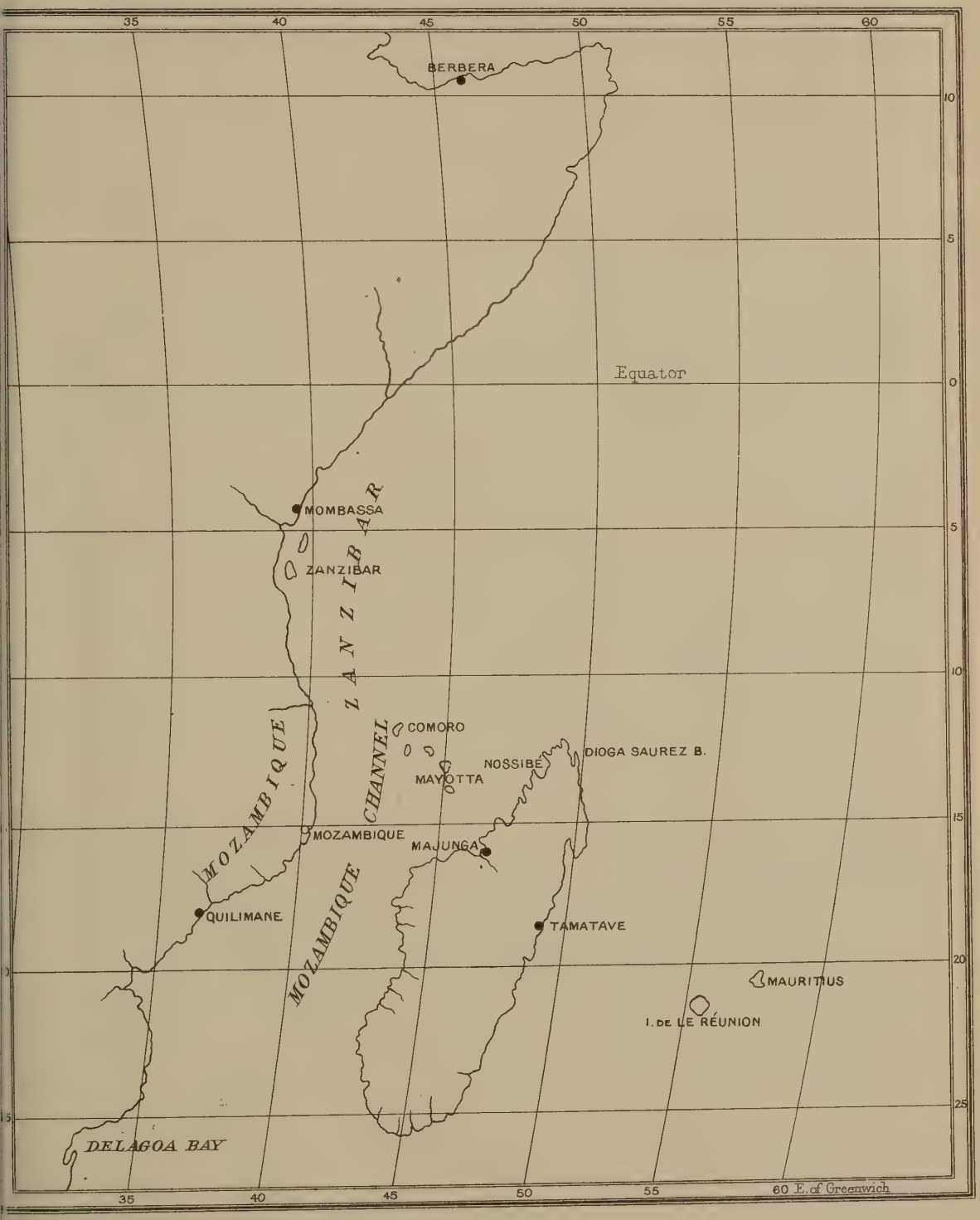
1. Tamatave is said to be infected about or after the 14th November last. It is not known that any other port in the French Colony was, either previously or at the same time, infected with the disease.
2. For fully 12 months prior to the declaration that the disease had made its appearance at Tamatave there had been, so far as steamers are concerned, no connexion whatever, either by vessels in ballast, by cargo, or by passengers, either directly or indirectly, between Bombay and any port in the French Colony of Madagascar.
3. For fully seven months prior to Tamatave being declared an infected port, the traffic by country craft from Bombay had ceased for the monsoons, and moreover, none of the native vessels which left Bombay in the early part of 1898 were bound for Tamatave.
4. It is important to vote that all vessels leaving Bombay carry bills of health, stating the exact number of crew and passengers on the date of departure, and that the minimum period occupied on the voyage is, for steamers, 15 or 16 days, and, for native craft, within the most favourable condition of wind and weather, a month.

S. EVANS,
Assistant to Port Health Officer,
Bombay.

(Signed) J. CRIMMIN, V.C., Major, I.M.S
Port Health Office, Bombay.
6th March 1899.

MAP ILLUSTRATING NOTES ON TRAFFIC BETWEEN BOMBAY AND MADAGASCAR.

(See Question No. 25,494.)



JULY 1898 — 2443

AUGUST 1898 — 3299

SEPTEMBER 1898 — 3645

OCTOBER 1898 — 3607

NOVEMBER 1898 — 2653

DECEMBER 1898 — 3350

POPULATION 829,000

POPULATION 8,85000

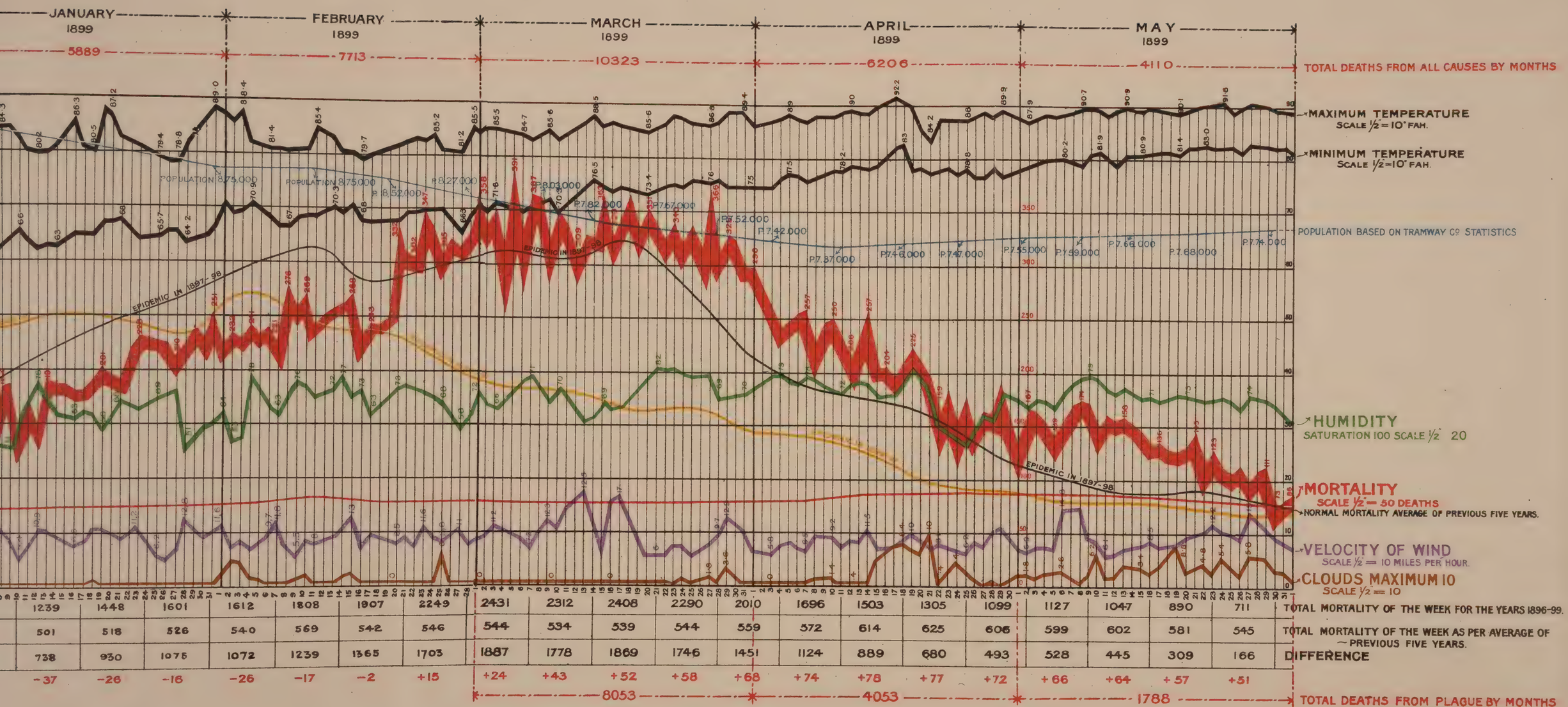
POPULATION 920,000

POPULATION 9,50,000

EPIDEMIC IN 1897-98

440	523	568	583	615	655	743	765	811	869	823	748	864	1002	930	828	711	654	645	604	606	616	679	688	798	742	894
501	532	565	627	669	640	681	618	604	574	589	612	594	592	557	559	559	550	537	546	556	560	460	470	470	469	494
					15	62	147	207	295	234	136	270	410	433	269	152	104	108	58	50	56	219	218	328	273	400
					+5	-6	-14	-25	-23	-24	-15	-24	-24	-29	-34	-40	-45	-49	-54	-63	-60	-54	-55	-55	-56	-58

12 13 +14 +17 +17



1239	1448	1601	1612	1808	1807	2249	2431	2312	2408	2290	2010	1696	1503	1305	1099	1127	1047	890	711	TOTAL MORTALITY OF THE WEEK FOR THE YEARS 1896-99.
501	518	526	540	569	542	546	544	534	539	544	559	572	614	625	606	599	602	581	545	TOTAL MORTALITY OF THE WEEK AS PER AVERAGE OF ~ PREVIOUS FIVE YEARS.
738	930	1075	1072	1239	1365	1703	1887	1778	1869	1746	1451	1124	889	680	493	528	445	309	166	DIFFERENCE
-37	-26	-16	-26	-17	-2	+15	+24	+43	+52	+58	+68	+74	+78	+77	+72	+66	+64	+57	+51	

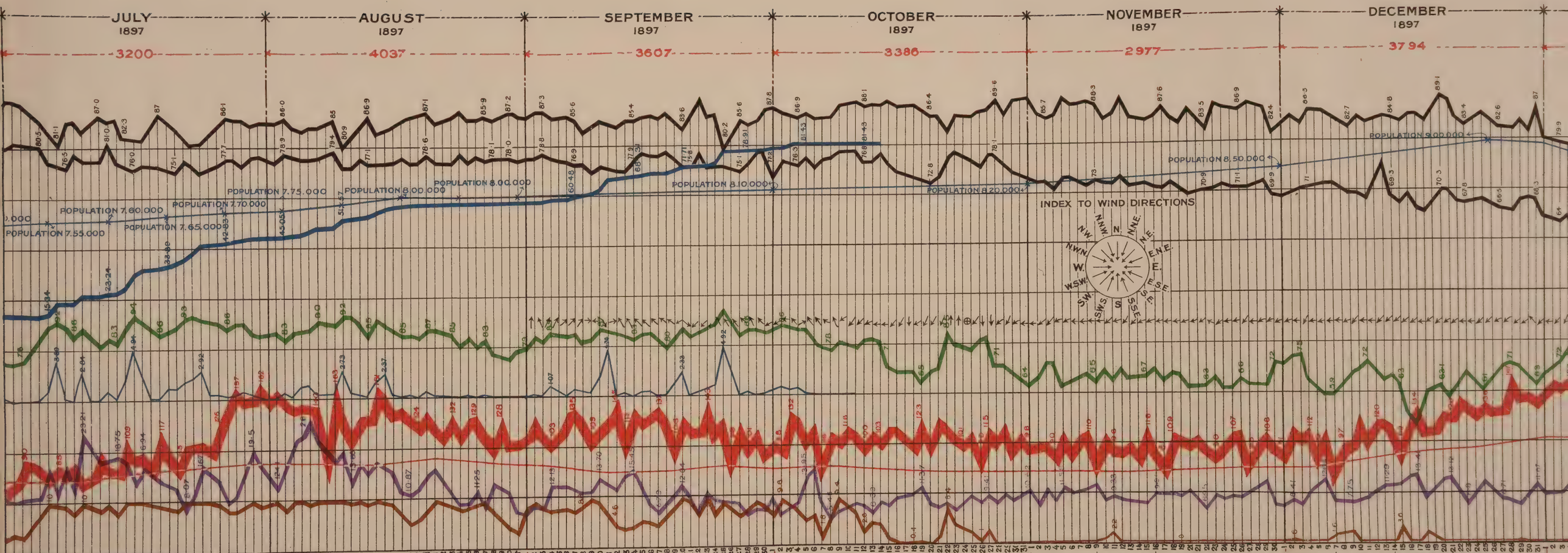
APPENDIX No. LXXVII.
(See Question No. 26,076, sqq.)

APPENDIX No.
(See Question No.

BOMBAY PLAGUE VISITAT

DIAGRAM SHEWING MORTALITY AGAINST TEMPERATU

Prepared by Mr. C. C. JAMES, *M.I.C.E., Deputy Execu*



550	577	878	770	1071	965	978	866	814	838	676	869	823	776	750	776	754	741	701	700	682	704	706	785	835	975	1061
500	491	522	548	585	558	552	600	562	552	514	518	546	531	541	514	491	496	493	495	502	514	511	575	629	652	724
BUBONIC CASES 7	4	9	14	18	8	21	13	25	33	27	35	60	44	29	46	44	44	53	47	66	70	83	95	158	200	337
+57 CHOLERA CASES *	+53	+54	+53	+53	+45	+37	+35	+33	+33	+28	+27	+27	+24	+22	+21	+19	+18	+15	+11	+7	+2.	-4	-13	-23	-34	-41
	1086				1697				1463				1176					883				1002				
15	35	32	84	220	173	143	117	76	37	41	31	29	32	12	20	22	13	11	7	8	4	4	5	4	4	4

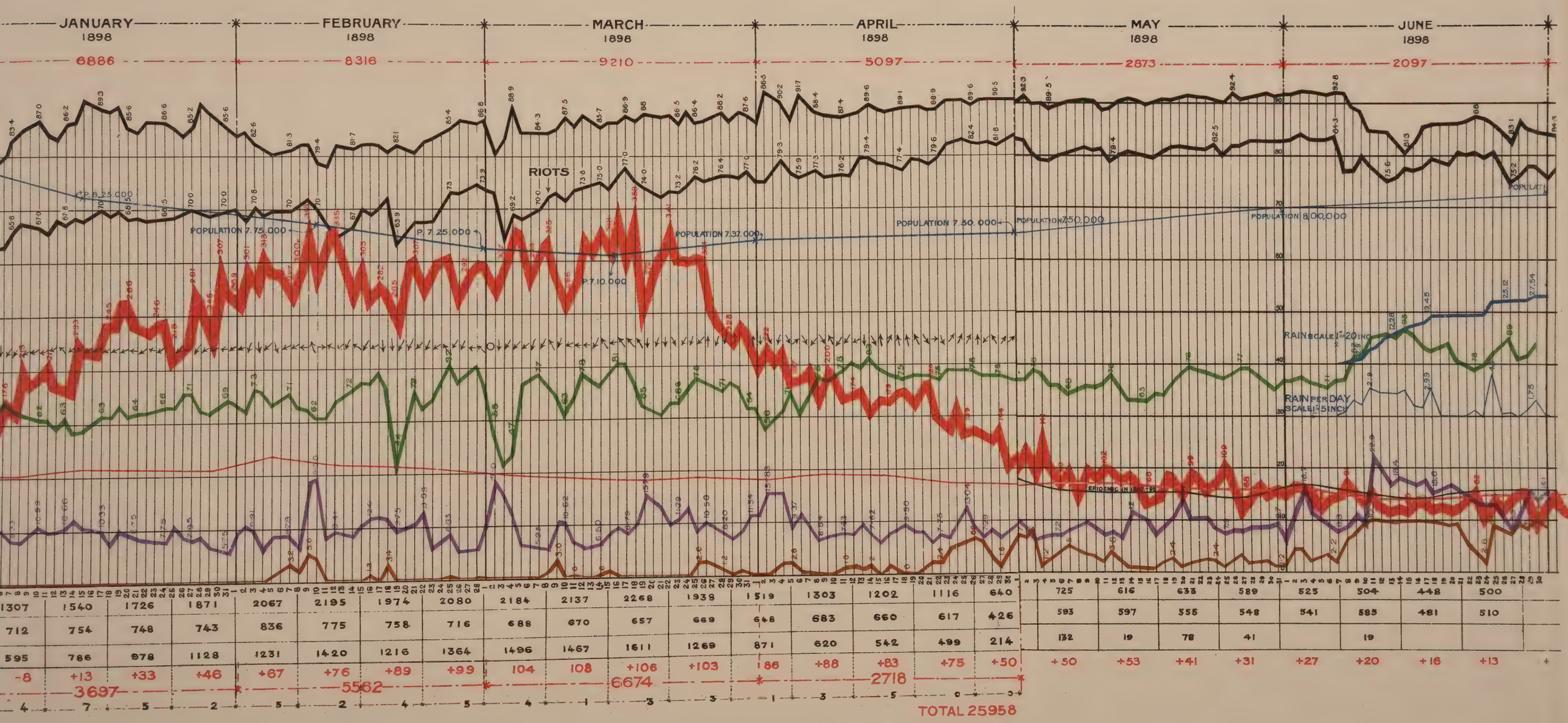
LXXVII.

26,076, sqq.)

ION OF 1896-99

RE, HUMIDITY, WIND-VELOCITY, CLOUD & RAIN.

ive Engineer, Drainage, Bombay Municipality.



1307	1540	1726	1871	2067	2195	1974	2080	2184	2137	2268	1938	1519	1303	1202	1116	640
712	754	748	743	836	775	758	716	688	670	657	669	648	683	660	617	426
595	786	978	1128	1231	1420	1216	1364	1496	1467	1611	1269	871	620	542	499	214
-8	+13	+33	+46	+67	+76	+89	+99	104	108	+106	+103	186	+88	+83	+75	+50
4	7	5	2	5	2	4	5	4	1	3	3	1	3	5	0	3

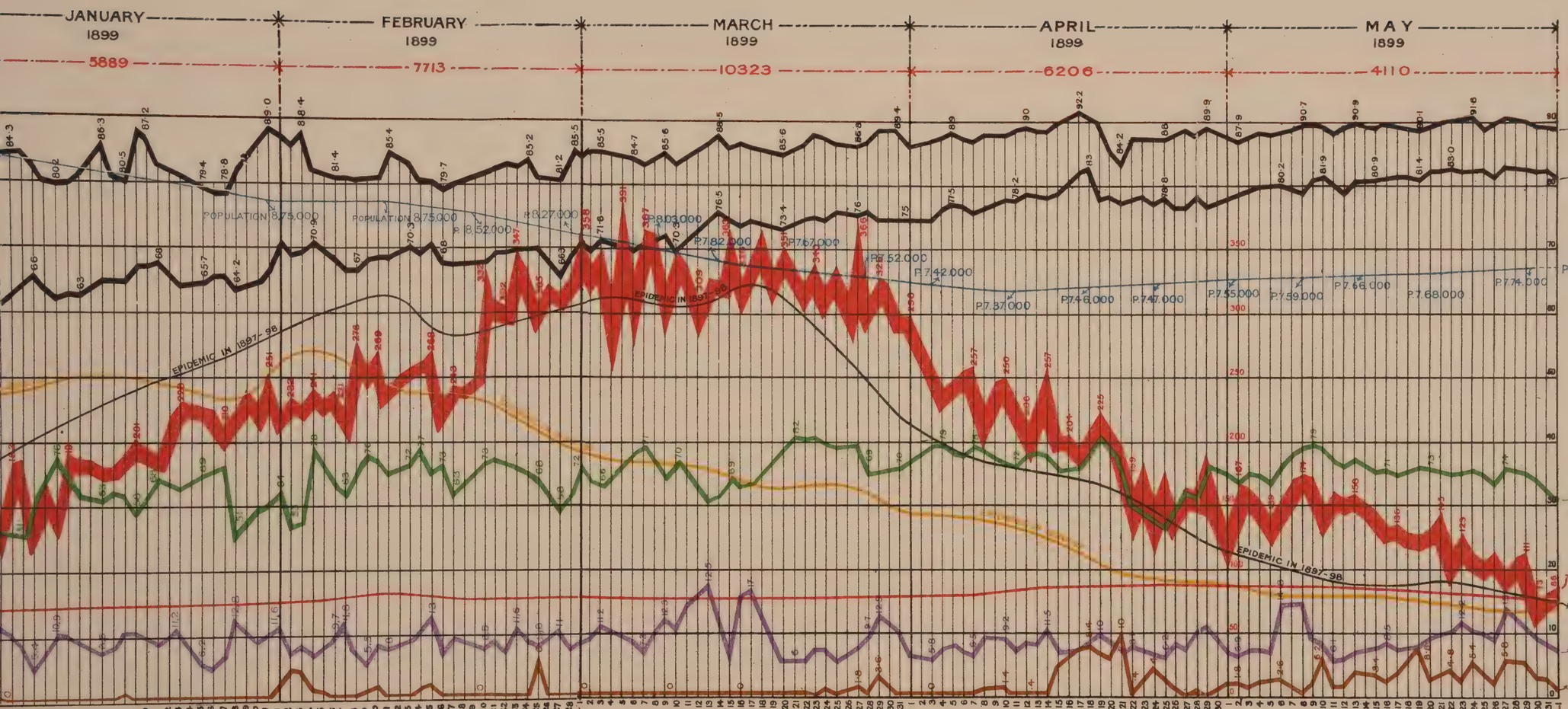
725	616	633	589	525	504	448	500	
593	597	555	548	541	585	481	510	
132	19	78	41		19			
+50	+53	+41	+31	+27	+20	+16	+13	+

JULY 1898	AUGUST 1898	SEPTEMBER 1898	OCTOBER 1898	NOVEMBER 1898	DECEMBER 1898
2443	3299	3645	3607	2853	3350

POPULATION 829,000 — POPULATION 835,000 — POPULATION 835,000 — POPULATION 920,000 — POPULATION 950,000

EPIDEMIC IN 1897-98 — EPIDEMIC IN 1897-98

440	523	568	585	615	655	743	765	811	869	823	748	864	1002	930	828	711	654	645	604	606	616	679	688	798	742	894	1111
501	532	565	627	669	640	681	618	604	574	589	612	594	592	557	559	559	550	537	546	556	560	460	470	470	469	494	48[illegible]
					15	62	147	207	295	234	136	270	410	433	269	152	104	108	58	50	56	219	218	328	273	400	62[illegible]
12	-13	+14	+17	+17	+5	-6	-14	-25	-23	-24	-15	-24	-24	-29	-34	-40	-45	-49	-54	-53	-60	-54	-53	-55	-56	-58	-4[illegible]



	1239	1448	1601	1612	1808	1907	2249	2431	2312	2408	2290	2010	1696	1503	1305	1099	1127	1047	890	711	TOTAL MORTALITY OF THE WEEK FOR THE YEARS 1896-99.
+	501	518	526	540	569	542	546	544	534	539	544	559	572	614	625	606	599	602	581	545	TOTAL MORTALITY OF THE WEEK AS PER AVERAGE OF — PREVIOUS FIVE YEARS.
	738	930	1075	1072	1239	1365	1703	1887	1778	1869	1746	1451	1124	889	680	493	528	445	309	166	DIFFERENCE
	-37	-28	-16	-26	-17	-2	+15	+24	+43	+52	+58	+68	+74	+78	+77	+72	+66	+64	+57	+51	

8053 — 4053 — 1788 — TOTAL DEATHS FROM PLAGUE BY MONTHS

APPENDIX No. LXXVII.
(See Question No. 26,076, sqq.)

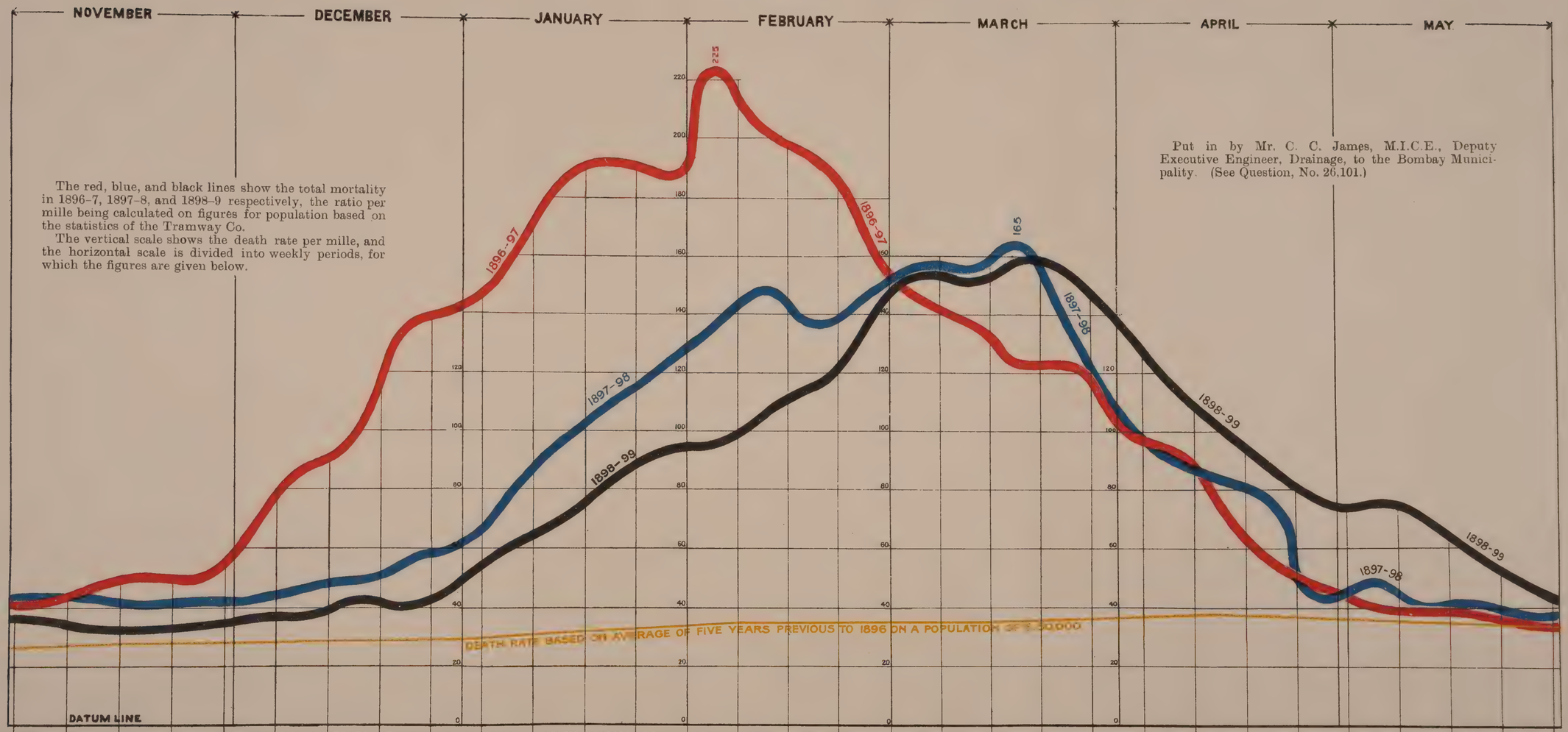
CHART SHOWING WEEKLY DEATH RATE PER 1000 PER ANNUM ON ACTUAL POPULATION

DURING THE PLAGUE EPIDEMIC IN BOMBAY CITY IN 1896-7, 1897-8, AND 1898-9.

The red, blue, and black lines show the total mortality in 1896–7, 1897–8, and 1898–9 respectively, the ratio per mille being calculated on figures for population based on the statistics of the Tramway Co.

The vertical scale shows the death rate per mille, and the horizontal scale is divided into weekly periods, for which the figures are given below.

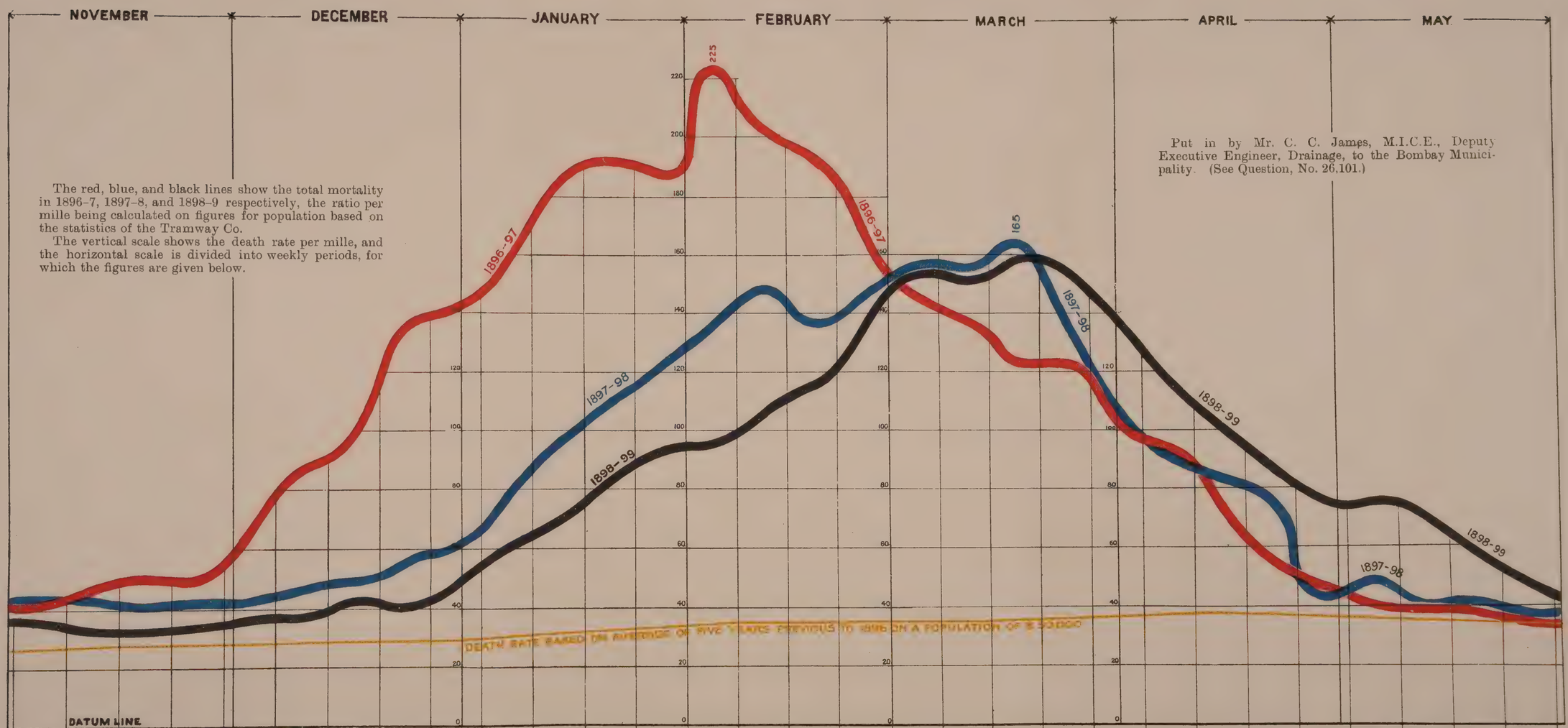
Put in by Mr. C. C. James, M.I.C.E., Deputy Executive Engineer, Drainage, to the Bombay Municipality. (See Question, No. 26,101.)



IN 1896-97	POPULATION	7,65,500	7,74,500	7,83,500	7,92,500	7,97,000	7,70,000	7,32,500	7,01,000	6,21,000	5,31,000	4,83,500	4,62,000	4,52,000	4,42,000	4,37,000	4,37,000	4,52,000	4,67,000	4,71,000	4,75,000	4,80,000	4,95,000	5,32,500	5,67,500	6,17,500	6,80,000	7,04,000	7,12,000	7,20,000	7,28,000
	DEATH RATE	42	47	50	51	68.5	88.5	100	137	143	160,5	189	194	189	225	205.5	196	171	148	139	125	124	106	94.5	77	56.5	49	41	40	39	35.5
IN 1897-98	POPULATION	823,750	8,31,250	8,38,750	8,46,250	856250	8,68,750	881250	9,00,000	8,90,000	8,57,500	8,25,000	8,12,500	7,97,500	7,82,500	7,66,650	7,50,150	7,33,500	7,21,250	7,13,750	7,15,400	7,26,200	7,31,600	7,40,250	7,43,500	7,46,750	7,50,000	7,61,290	7,72,580	7,83,870	7,95,170
	DEATH RATE	43	43,5	42	43	43	47	49	56	62	79	97	110.5	122	137	149	157	147	157,5	156	165	158.5	108	91.5	84	78	44	49.5	41.5	42	385
IN 1898-99	POPULATION	924,000	9,31,000	9,38,000	9,45,000	9,50,000	9,50,000	9,50,000	9,50,000	9,50,000	9,33060	9,16,120	8,99,180	8,82,240	8,75000	8,75,000	8,52,000	8,39,000	8,15,000	7,92,500	7,74,500	7,59,500	7,47,000	7,38,500	7,41,500	7,46,500	7,49,000	7,57,000	7,62,500	7,67,000	7,71,000
	DEATH RATE	36	34	33.5	34	37	37.5	43.5	40.5	49	62	70	84	94	96	107	116	139	155	151.5	161.5	157	140	119	105	91	76	77.5	71.5	60	48

CHART SHOWING WEEKLY DEATH RATE PER 1000 PER ANNUM ON ACTUAL POPULATION

DURING THE PLAGUE EPIDEMIC IN BOMBAY CITY IN 1896-7, 1897-8, AND 1898-9.



The red, blue, and black lines show the total mortality in 1896–7, 1897–8, and 1898–9 respectively, the ratio per mille being calculated on figures for population based on the statistics of the Tramway Co.

The vertical scale shows the death rate per mille, and the horizontal scale is divided into weekly periods, for which the figures are given below.

Put in by Mr. C. C. James, M.I.C.E., Deputy Executive Engineer, Drainage, to the Bombay Municipality. (See Question, No. 26,101.)

IN 1896-97	POPULATION	7,65,500	7,74,500	7,83,500	7,92,500	7,97,000	7,70,000	7,32,500	7,01,000	6,21,000	5,31,000	4,83,500	4,62,000	4,52,000	4,42,000	4,37,000	4,37,000	4,52,000	4,67,000	4,71,000	4,75,000	4,80,000	4,95,000	5,32,500	5,67,500	6,17,500	6,80,000	7,04,000	7,12,000	7,20,000	7,28,000
	DEATH RATE	42	47	50	51	68.5	88.5	100	137	143	160,5	189	194	189	225	205.5	196	171	148	139	125	124	106	94.5	77	56.5	49	41	40	39	35.5
IN 1897-98	POPULATION	8,23,750	8,31,250	8,38,750	8,46,250	8,56,250	9,68,750	8,81,250	9,00,000	8,90,000	8,57,500	8,25,000	8,12,500	7,97,500	7,82,500	7,66,650	7,50,150	7,33,500	7,21,250	7,13,750	7,15,400	7,26,200	7,31,600	7,40,250	7,43,500	7,46,750	7,50,000	7,61,290	7,72,580	7,83,870	7,95,170
	DEATH RATE	43	43,5	42	43	43	47	49	56	62	79	97	110.5	122	137	149	137	147	157,5	156	165	158.5	108	91.5	84	78	44	49.5	41.5	42	38.5
IN 1898-99	POPULATION	924,000	9,31,000	9,38,000	9,45,000	9,50,000	9,50,000	9,50,000	9,50,000	9,50,000	9,53060	9,16,20	8,99,180	8,82,240	8,75000	8,75,000	8,52,000	8,39,000	8,15,000	7,92,500	7,74,500	7,59,500	7,47,000	7,39,500	7,41,500	7,46,500	7,49,000	7,57,000	7,62,500	7,67,000	7,71,000
	DEATH RATE	36	34	33.5	34	37	37.5	43.5	40.5	49	62	70	84	94	96	107	116	139	155	151.5	161.5	157	140	119	105	91	76	77.5	71.5	60	48

APPENDIX No. LXXIX.

NOTES OF CASES OF PLAGUE

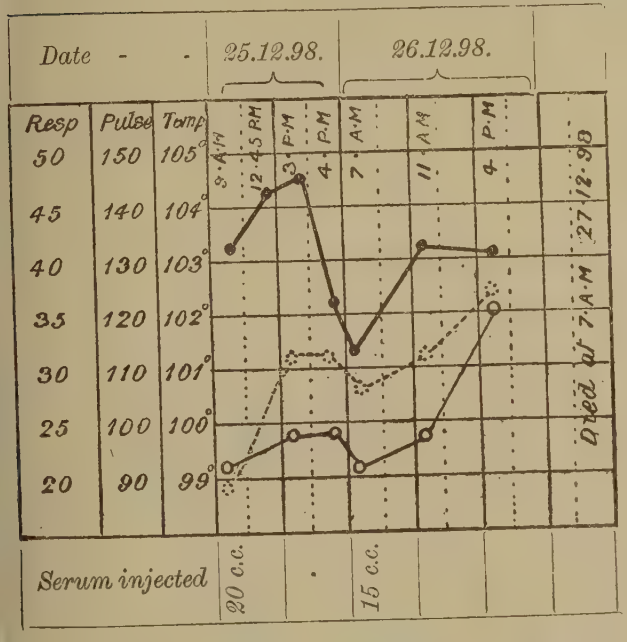
TREATED WITH

ANTI-PESTEUX SERUM FROM THE PASTEUR INSTITUTE (HORSE No. 21.)
AT BANGALORE SOUTH CAMP PLAGUE HOSPITAL.

By Lieut. S. R. DOUGLAS, I.M.S., on Special Duty with the INDIAN PLAGUE COMMISSION.

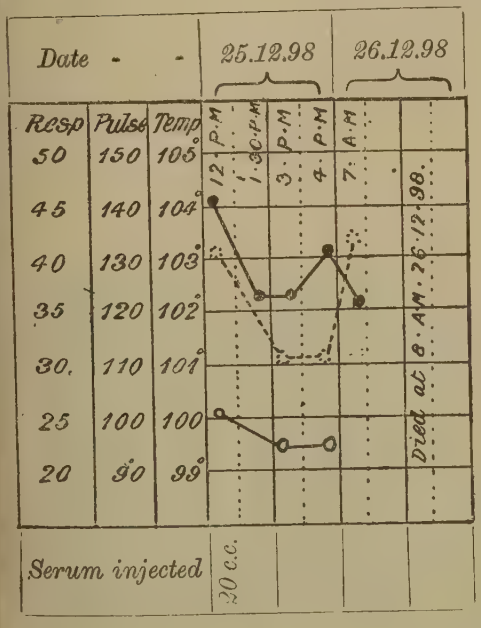
Note.—In the following charts the temperature is shown by a thick black line, the pulse by a thin black line, and the respiration by a dotted line.

CASE No. I. (Admitted on 3rd day of the disease.)



Esoph Beg. Age 45. Male. Admitted **25.12.98.** Fever for three days. Bubo in left groin. No wound of foot or leg on left side. Temperature 103·4°. Pulse 92. Good volume and tension. Respiration 20. Lungs clear. Heart normal. Spleen not felt. 20 *c.c. of serum inoculated* in left flank at 11.30 a.m. 12.45 p.m.—Temp. 104·2°. Patient volunteered the statement that pain in bubo was less. 3 p.m.—Temp. 104·6°. Pulse 96. Respiration 32. No change noticed. 4 p.m.—Temp. 102·2°. Pulse 96. Respiration 32. States that he feels better. **26.12.98.**—7 a.m.—Temp. 101·4°. Pulse 92. Respiration 28. 9 a.m.—15 *c.c. of serum inoculated* left flank. 11 a.m.—Temp. 103·4°. Pulse 96. Respiration 32. 4 p.m.—Temp. 103°. Pulse 120. Respiration 36. Delirious. Decidedly worse.

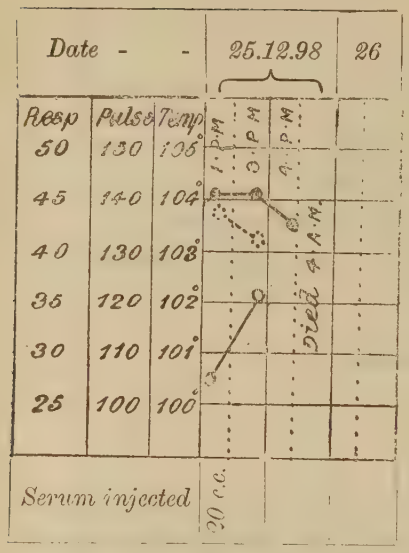
CASE No. II. (Admitted on 2nd day of the disease.)



Ala Raja (a Sais). Age 25. Male. Admitted **25.12.98.** Fever for two days, bubo one day. Bubo in right axilla and also the glands above the clavicle on the same side; a good deal of swelling round it. Temperature 104. Pulse 100. Respirations 40. Tongue brown furred. 20 *c.c. of serum inoculated* (10 c.c. into each flank) at 12.0 p.m. 1.30 p.m.—Temp. 102·4°. 3 p.m.—Temp. 102·4°. Pulse 96. Respiration 32. 4 p.m.—Temp. 103°. Pulse 96. Respiration 32. **26.12.98.**—7 a.m.—Temp. 102·2°. Pulse running; could not be counted. Respiration 44.

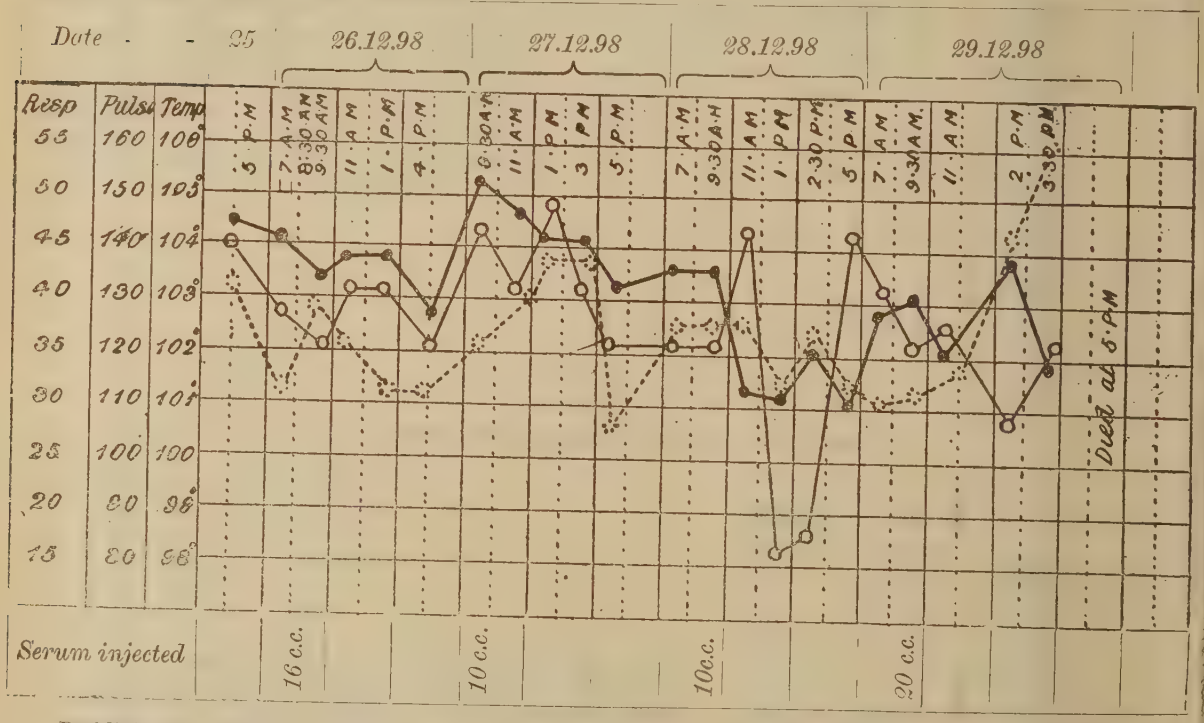
25.12.98.—One case not inoculated as he had a suppurating bubo and was convalescent.

CASE No. III. (Admitted on 3rd day of the disease.)



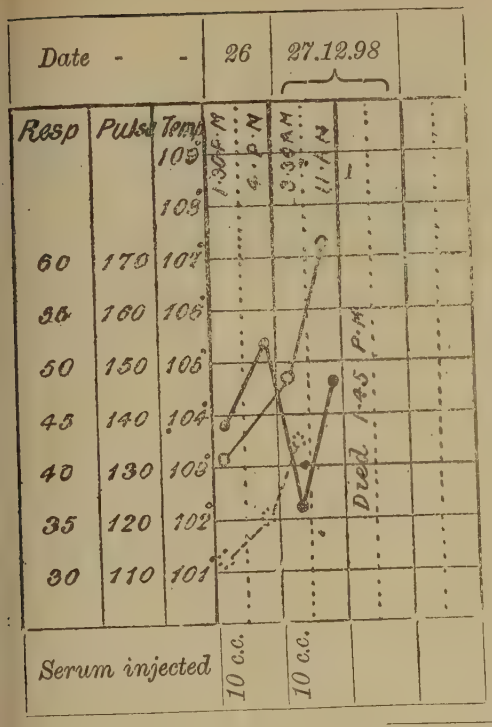
Willammah. Female. Age 25. Admitted **25.12.98.** Fever for three days, had not noticed any bubo. Temperature on admission 99°, patient shivering. Pulse very fast and weak. One ounce of arrack given, and one hour afterwards (1 p.m.) temperature was 104°. Pulse 104, weak and irregular in time and force. Respiration 44. Slightly delirious. Tongue furred; indented at edges. Spleen not felt. Bubo in right axilla. Heart and lungs normal. Left breast very prominent and hard, as if pushed forward by something. 20 *c.c. of serum inoculated* into left flank (1 p.m.). 3 p.m.—Temp. 104°. Pulse 120, distinctly stronger. Respiration 42. 4 p.m.—Temp. 103·8°. Slightly better pulse, but much muscular twitching was present, the head never being still. Patient seemed sensible and quiet.

CASE No. IV. (Admitted on 3rd day of the disease.)



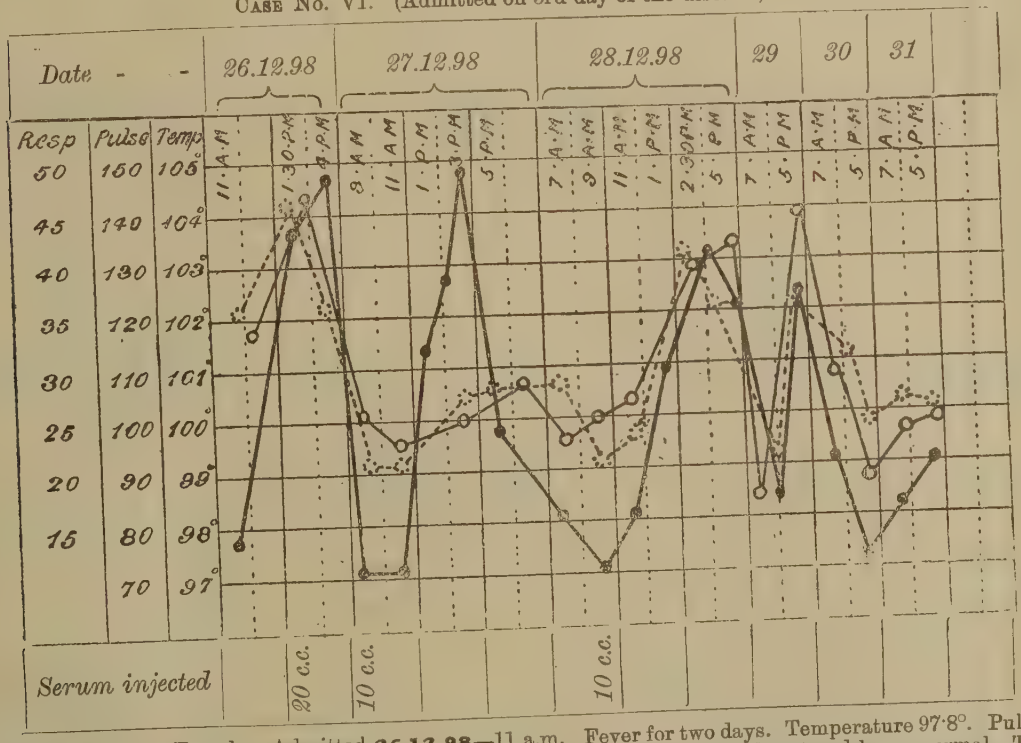
Barkli Amma. Age 20. Female. Admitted **25.12.98** (evening). Fever for two days. Temperature 104·4°. Pulse 140. Respiration 42. Bubo in right axilla. A small suppurating wound, a scratch received three days before illness on inner side of left forearm. Cover-slip preparations taken stained with fuchsin, but nothing resembling a plague bacillus seen. Numerous encapsuled diplococci present. An agar tube inoculated from wound. Cover-slip from tube two colonies, one round, opaque, yellowish, showed cocci in groups; the other smooth, thin, when magnified consisting of minute colonies, showed diplococci in chains. **26.12.98.**—7 a.m.—Temp. 104°. Pulse 128. Respiration 32. 8.30 a.m.—Heart and lungs normal. Spleen not felt. Tongue furred; indented at edges. 16 *c.c. serum inoculated* into right flank. 9.30 a.m.—Temp. 103·6°. Pulse 120. Respiration 40. 11.0 a.m.—Temp. 103·8°. Pulse 132. Respiration 36. 1.0 p.m.—Temp. 103·8°. Pulse 132. Respiration 32. 4.0 p.m.—Temp. 102·8°. Pulse 120. Respiration 32. **27.12.98.**—8.30 a.m.—Temp. 105·2°. Pulse 144. Respiration 36. Looks very much better, quiet and sensible. 10 *c.c. serum inoculated.* 11.0 a.m.—Temp. 104·8°. Pulse 132. Respiration 42. 1.0 p.m.—Temp. 104·4°. Pulse 148. Respiration 44. 3.0 p.m.—Temp. 104·2°. Pulse 132. Respiration 44. Not quite so sensible, but pulse fairly good. 5.0 p.m.—Temp. 103·4°. Pulse 122. Respiration 48. **28.12.98.**—7.0 a.m.—Temp. 103·6°. Pulse 120. Respiration 36. 9.30 a.m.—Temp. 103·6°. Pulse 120, not so strong. Respiration 36. Slightly delirious. 10 *c.c. inoculated* 11.0 a.m.—Temp. 101·2°. Pulse 144. Respiration 36. 1.0 p.m.—Temp. 101·2°. Pulse 92. Respiration 32 2.30 p.m.—Temp. 102°. Pulse 96. Respiration 36. Slightly delirious. 5.0 p.m.—Temp. 101°. Pulse 142. Respiration 32. **29.12.98.**—7.0 a.m.—Temp. 102·8°. Pulse 132. Respiration 30. 9.30 a.m.—Temp. 103°. Pulse 120. Respiration 32. Answered sensibly when spoken to, but restless and talks much to herself. 20 *c.c. inoculated.* 11.0 a.m.—Temp. 102°. Pulse 124. Respiration 36. 2.0 p.m.—Temp. 103·8°. Pulse 108. Respiration 46. 3.30 p.m.—Temp. 102·6. Pulse 120. Respiration 56. Bronchial breathing at left apex. General condition bad. No sputum obtainable. Pulse very weak.

CASE No. V. (Admitted on 2nd day of the disease.)



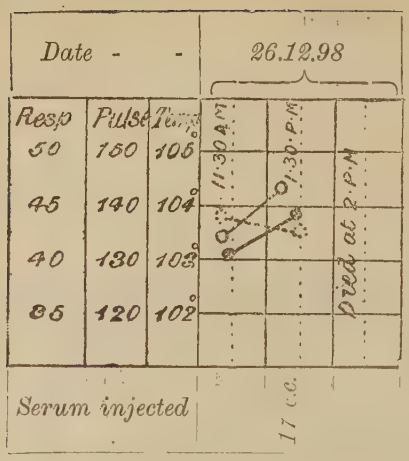
Poopathi. Age 8. Female. Admitted **26.12.98.** Bubo noticed yesterday. Fever came on last night. Bubo in right groin. Temperature 103·8°. Pulse 130. Respiration 32. 1.30 p.m.—No wound discovered on right leg or buttock. 10 *c.c. of serum inoculated.* Spleen not felt. 4 p.m.—Temp. 105·4°. Pulse 148. Respiration 35. **27.12.98.**—8.30 a.m.—Temp. 102. Pulse 180 running very weak. Respiration 43. General condition much worse. A good deal of muscular twitching. 10 *c.c. of serum inoculated* into right flank. 11 a.m.—Temp. 104·8°. Pulse running.

CASE No. VI. (Admitted on 3rd day of the disease.)



Balamah. Age 35. Female. Admitted **26.12.98**—11 a.m. Fever for two days. Temperature 97·8°. Pulse 118. Respiration 35. 1.30 p.m.—Shivering much; talking incoherently. No bubo; heart and lungs normal. Tongue brown; furred. Teeth very dirty, and covered with brown sordes. Temperature 103·6°. Cover-slip of blood taken. Blood preparation stained with eosin and methylene blue. No bacilli of any sort seen, but some malarial parasites present. Pulse, so much muscular tremor that it could not be counted (rigor). Respiration 46. Spleen not felt. 20 *c.c. of serum inoculated* into left flank. 4 p.m.—Temp. 104·6°, Pulse 144. Respiration 36. **27.12.98**—8 a.m.—Temp. 97°. Pulse 100. Respiration 20. Patient looks much better. Tongue still very foul. 10 *c.c. inoculated.* 11 am.—Temp. 97°. Pulse 96. Respiration 20. 1 p.m.—Temp. 101·4°. Pulse could not be taken on account of muscular tremor (rigor). 3 p.m.—Temp. 104·6°. Pulse 100. Respiration 27. Pulse very good considering temperature, and remarkably slow. 5 p.m.—Temp. 99·8°. Pulse 108. Respiration 28. **28.12.98.**—7 a.m.—Temp. 98°. Pulse 96. Respiration 28. 9 a.m.—Temp. 97°. Pulse 100. Respiration 30. 10 *c.c. inoculated.* 11 a.m.—Temp. 98°. Pulse 104. Respiration 24. 1 p.m.—Temp. 101; shivering much. Pulse and respiration could not be taken. 2.30 p.m.—Temp. 103·2°. Pulse 128. Respiration 40. Still shivering but skin moist. 5 p.m.—Temp. 102. Pulse 132. Respiration 36. **29.12.98.**—7 a.m.—Temp. 98·4°. Pulse 84. Respiration 22. 9.30 a.m.—Quite comfortable; looks like a severe case of malaria. No serum inoculated, but Quinine, gr., X. ordered at once; and to be repeated when sweating stage was reached. 5 p.m.—Temp. 103·2°. Pulse 140°. Respiration 35. **30.12.98.**—7 a.m.—Temp. 99°. Pulse 108. Respiration 36. 5 p.m.—Temp. 97·2°. Pulse 88. Respiration 24. **31.12.98.**—7 a.m.—Temp. 98·2°. Pulse 98. Respiration 26. 5 p.m.—Temp. 99° Pulse 100. Respiration 24. 9 a.m.—Temp. 97. Pulse 72. Respiration 20. Convalescent.

26.12.98. One not inoculated as she had had a large abscess on the middle of the right thigh for 12 days. This abscess had burst, laying bare the muscle, and a large slough was protruding. Over the head of the left fibula was another abscess. Patient very ill with high temperature. Very foul brown tongue. (?) Plague in carbuncular form.

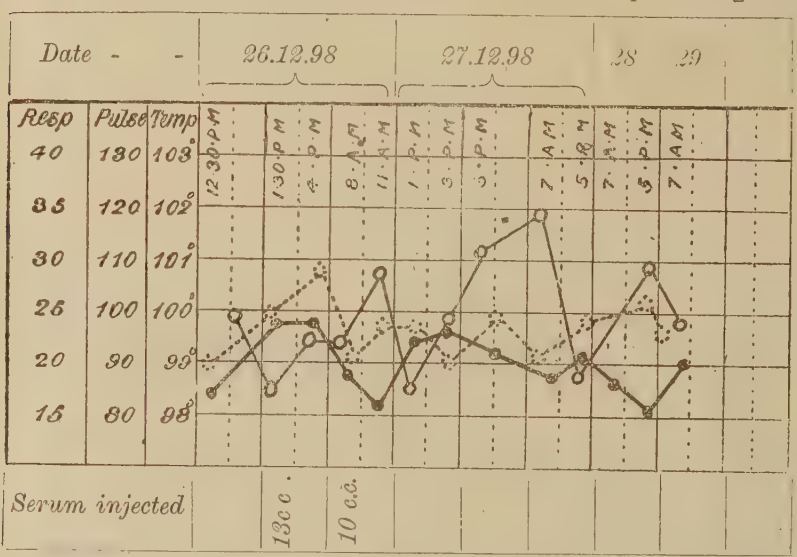


CASE No. VII. (Admitted on 2nd day of the disease.)

Almail. Age 30. Female. Admitted **26.12.98**—11.30 a.m. Fever only one day. Bubo in right groin. No wound discovered. Temperature 103°. Pulse 132. Respiration 44. 1.30 p.m.—General condition very bad, 5½ months pregnant. Temp. 103·8°. Pulse 144, weak and irregular. Respiration, gasping, 44. Patient very restless. Heart and lungs normal as far as could be ascertained. 17 *c.c. of serum injected* into left flank. 2 p.m.—Died (half hour after injection). Practically moribund on admission.

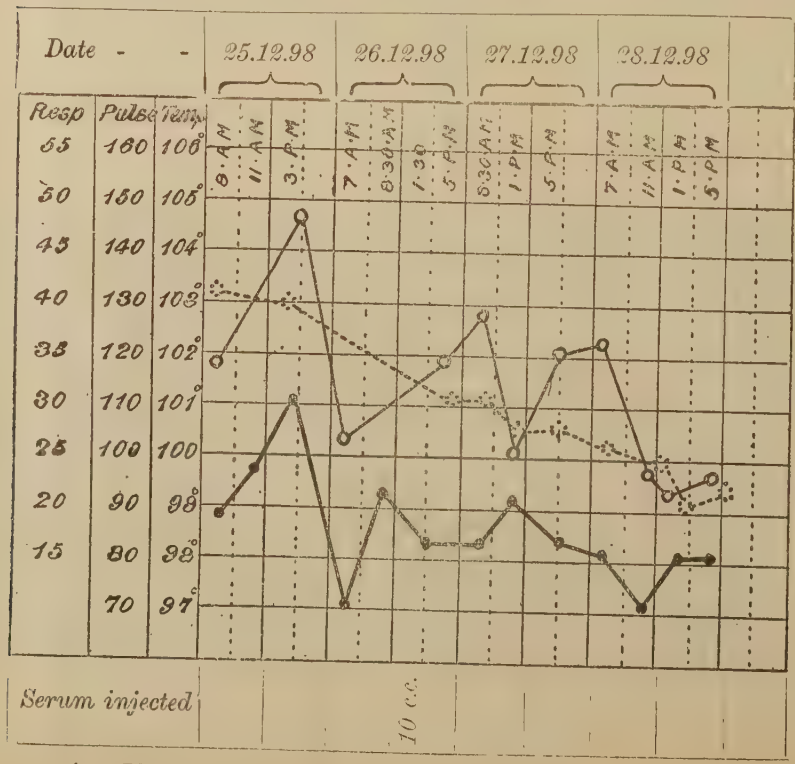
CASE No. VIII. (Admitted on 6th day of the disease.)

Lutchmee. Age 13. Female. Admitted **26.12.98**. 12.30 p.m. Five days ago fever. Four days ago bubo noticed. Temp. 98·4°. Pulse 100. Respiration 20. 1.30 p.m.—Tongue slightly furred. Lungs and heart normal. Bubo in right inguinal and also right femoral glands. No wound of right leg or foot discovered. General condition good. Spleen not enlarged. Temp. 99·8°. Pulse 84. Respiration 24. 13 *c.c. of serum inoculated* into right flank. 4 p.m.—Temp. 99·8°. Pulse 98. Respiration 28. General condition very good. **27.12.98.**—8 a.m.—Temp. 98·8°. Pulse 96. Respiration 22. Looks quite well. 10 *c.c. inoculated.* 11 a.m.—Temp. 98·2°. Pulse 108. Respiration 24. 1 p.m.—Temp. 99·4°. Pulse 84. Respiration 23. 3 p.m.—Temp. 99·6°. Pulse 96. Respiration 20. 5 p.m.—Temp. 99·2°. Pulse 112. Respiration 25. **28.12.98.**—7 a.m.—Temp. 98·8°. Pulse 120. Respiration 20. Seems quite well. Not injected. 2.30 p.m.—Bubo still present but scarcely tender on pressure. 3 p.m.—Temp. 99°. Pulse 88. Respiration 24. **29.12.98**—7 a.m.—Temp. 98·6°. Pulse 88. Respiration 22. Convalescent. 5 p.m.—Temp. 98°. Pulse 110. Respiration 26. **30.12.98.**—7 a.m.—Temp. 99°. Pulse 98. Respiration 24. Bubo still present. Removed to convalescent ward. **1.1.99.**—Bubo suppurating, opened.

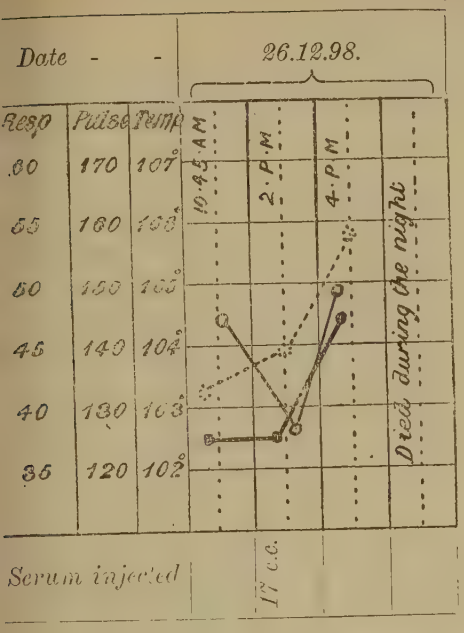


CASE No. IX. (Admitted on 3rd day of the disease.)

Beersi. Age 12. Female. Admitted **25.12.98**: early morning. Two days fever. Temperature 98·8°. Pulse 118. Respiration 42. Râles and rhonchi heard over the whole of the right lung, a few over the left. No bubo found. 11 a.m.—Temp. 99·6°. 3 p.m.—Temp. 101°. Pulse 148. Respiration 40. Tongue covered with a thin layer of white fur, rather flabby. **26.12.98.**—7 a.m.—Temp. 97°. Pulse 102. Respiration 36. 8.30 a.m.—No cough or sputum. General condition good. Temp. 99·4°. Pulse fair volume and tension. Tongue covered with white fur, indented at the edges ("typical plague tongue"). 1.30 p.m.—10 *c.c. serum inoculated* into left flank. 4 p.m.—Temp. 98·4°. Pulse 120, good. Respiration 32. **27.12.98**—8.30 a.m.—Temp. 98·4. Pulse 128. Respiration 32. 1 p.m.—Temp. 99·2. Pulse 100. Respirations 28. No serum given. 5 p.m.—Temp. 98·4°. Pulse 120. Respiration 28. **28.12.98.**—7 a.m.—Temp. 98°. Pulse 122. Respiration 26. No inoculation. 11 a.m.—Temp. 97°. Pulse 96. Respiration 24. 1 p.m.—Temp. 98°. Pulse 92. Respiration 22. 2.30 p.m.—Practically convalescent. A few squeaks heard in the lungs. 5 p.m.—Temp. 98. Pulse 96. Respiration 24. **29.12.98**—7 a.m.—Temp. 98·4°. Pulse 84. Respiration 22. Convalescent.

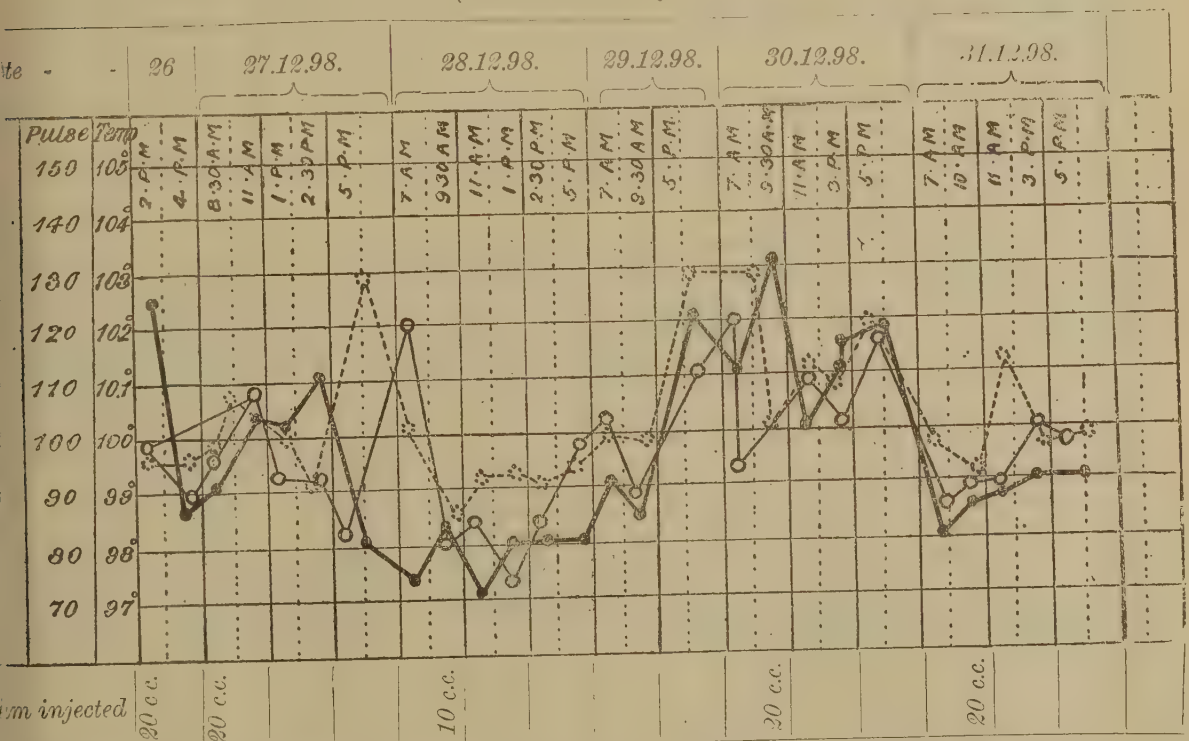


CASE No. X. (Admitted on 3rd day of the disease.)



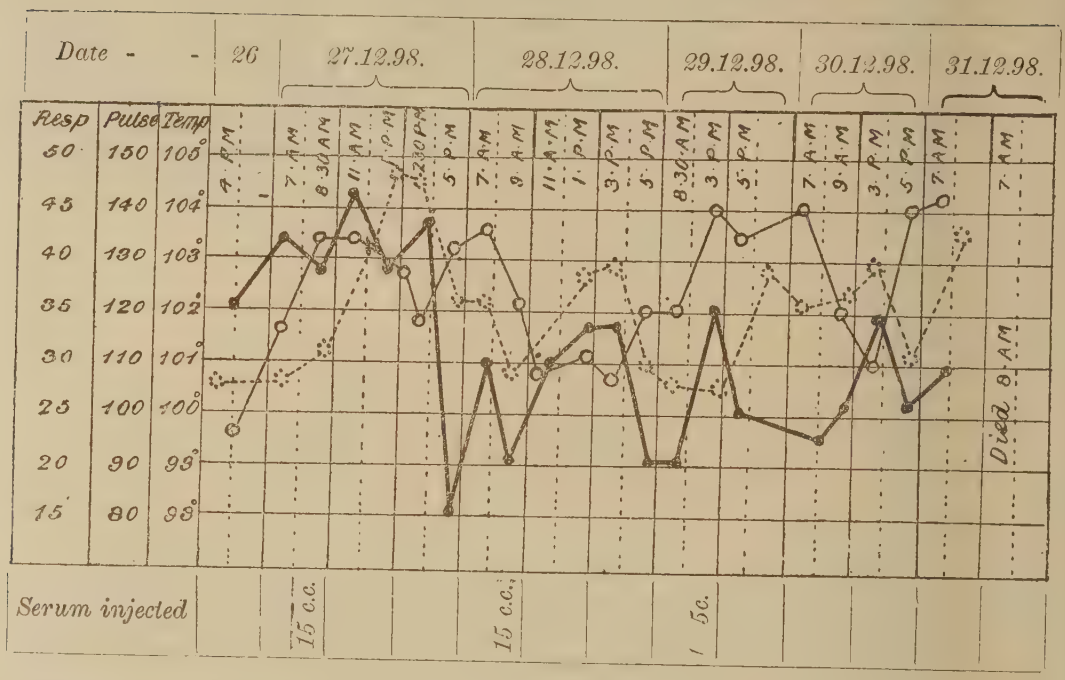
Gnana Moothoo. Aged 40. Male. Admitted **26.12.98** 10.45. a.m. Fever and bubo for two days. Temp. 102·6°. Respirations 42. Pulse 146. 2 p.m.—General condition bad, Much muscular tremor. A very large submaxillary bubo filling up the whole space between the jaw and the clavicle on the left side; the œdema spreading far down on to the chest. Temp. 102·6°. Pulse 124. Respiration 44. Great difficulty in swallowing and unable to open the mouth more than about half an inch, so that the condition of the inside of the mouth could not be observed. Spleen not enlarged. Heart and lungs normal. 17 *c.c. of serum inoculated* into left flank. 4 p.m.—Temp. 104·6°. Respiration 54. Pulse 148.

CASE No. XI. (Admitted on 4th day of the disease.)



Sewasagayan. Age 48. Male. Admitted **26.12.98** : morning. Fever for three days. Temp. 102·6°. Pulse 100. Respiration 24. 2 p.m.—General condition fair. Tongue furred, brown, indented at edges. Eyes bloodshot, tears running over the face in large quantities. Heart and lungs normal. Bubo in left inguinal region, no wound found. 20 *c.c. of serum inoculated* in left flank. 4 p.m.—Temp. 98·8°. Pulse 88. Respiratior 24. **27.12.98.** —8.30 a.m.—Glands enlarged on both sides of the neck to a slight degree, more especially on the right side. Temp. 99°. Pulse 96. Respiration 24. 20 *c.c. serum inoculated.* 11 a.m.—Temp. 100·4°. Pulse 108. Respiration 28. 1 p.m.—Temp. 100·2°. Pulse 92. Respiration 25. 2.30 p.m.—Temp. 101°. Pulse 92. Respiration 20. General condition the same. No worse. 5 p.m.—Temp. 98°. Pulse 84. Respiration 40. **28.12.98.**—7 a.m.—Temp. 97·6°. Pulse 120. Respiration 25. 9.30 a.m.—Temp. 98·4°. Pulse 80. Respiration 16. 10 *c.c. inoculated.* Patient feels quite well. 11 a.m.—Temp. 97°. Pulse 84. Respiration 22. 1 p.m.—Temp. 98. Pulse 72. Respiration 23. 2.30 p.m.—Temp. 98°. Pulse 84. Respiration 20. 5 p.m.—Temp. 98°. Pulse 96. Respiration 22. **29.12.98.**—7 a.m.—Temp. 99°. Pulse 102. Respiration 25. 9.30 a.m.—Temp. 98·4°. Pulse 88. Respiration 24. Bubo small, very slightly tender. Not inoculated. 5 p.m.—Temp. 102°. Pulse 110. Respiration 40. **30.12.98.**—7 a.m.—Temp. 101°. Pulse 120. Respiration 40. 9.30 a.m.—Temp. 103°. Pulse 92. Respiration 24. 20 *c.c. inoculated.* 11 a.m.—Temp. 100°. Pulse 108. Respiration 28. 3 p.m.—Temp. 101·4°. Pulse 100. Respiration 28 5 p.m.—Temp. 101·8°. Pulse 118. Respiration 34. **31.12.98.**—7 a.m.—Temp. 98°. Pulse 84. Respiration 24. 10 a.m.—Temp. 98·4°. Pulse 88. Respiration 20. 20 *c.c. inoculated.* 11 a.m.—Temp. 98·6°. Pulse 88. Respiration 32. 3 p.m.—Temp. 99°. Pulse 100. Respiration 23. Patient says he feels quite well, bubo the same size, very little surrounding œdema. 5 p.m.—Temp. 99°. Pulse 88. Respiration 24. **1.1.99.**—7 a.m.—Temp. 98°. Pulse 88. Respiration 18. Convalescent.

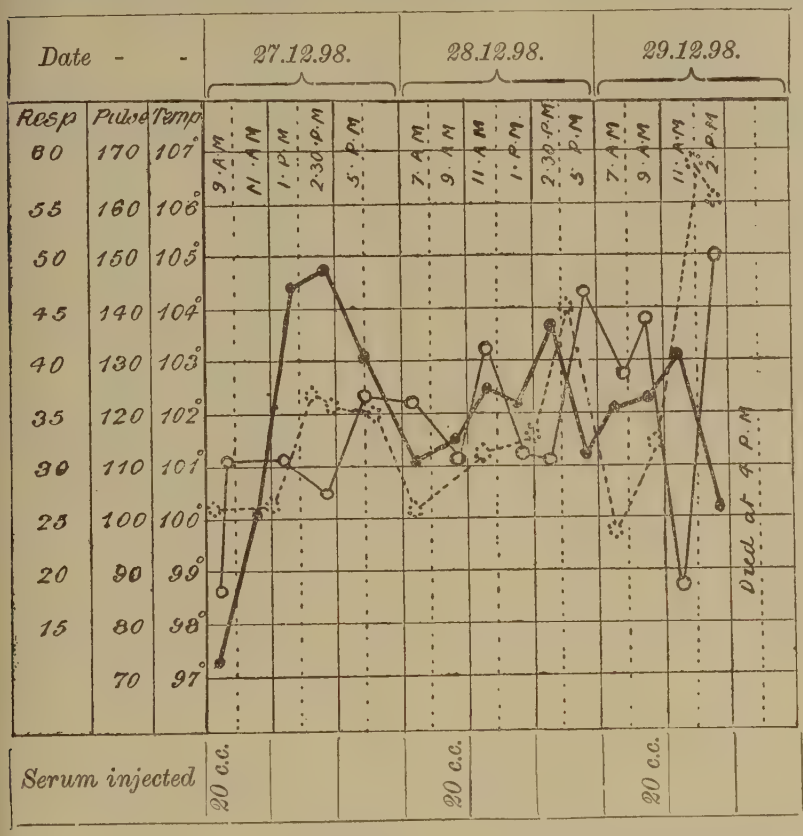
CASE No. XII. (Admitted on 3rd day of the disease.)



Goondappah. Aged 12 (looks much younger). Male. Admitted 4 p.m., **26.12.98.** Two days ago fever, one day ago bubo. On admission, temperature 102°. Pulse 98. Respiration 28. **27.12.98.**—7 a.m.—Temp. 103·4°. Pulse 118. Respiration 28. 8.30 a.m.—Temp. 102·8°. Pulse 132. Respiration 32. Tongue furred. Spleen not felt. Heart and lungs normal. Slight cough. No sputum. Bubo in right axilla. Not delirious. General condition, fair. 15 *c.c. inoculated.* Wound on right foot suppurating; seven days old. 11 a.m.—Temp. 104·2°. Pulse 132. Respiration 42. 1 p.m.—Temp. 102·8°. Pulse 128. Respiration 48. 2.30 p.m.—Temp. 103·8°. Pulse 116. Respiration 48. 5 p.m.—Temp. 98°. Pulse 132. Respiration 36. **28.12.98.**—7 a.m.—Temp. 101°. Pulse 136. Respiration 36. General condition, good; rather better than yesterday. 9 a.m.—Temp. 99°. Pulse 120. Respiration 28. 15 *c.c. of serum inoculated* into left flank. 11 a.m.—Temp. 101°. Pulse 108. Respiration 27. 1 p.m.—Temp. 101·4°. Pulse 110. Respiration 38. 3 p.m.—Temp. 101·4°. Pulse 108. Respiration 40. General condition very good. Lungs clear. 5 p.m.—Temp. 99°. Pulse 120. Respiration 30. Bubo smaller, not so painful. **29.12.98.**—8.30 a.m.—Temp. 99°. Pulse 120. Respiration 28. Tongue clean. General condition very good, so not inoculated. 3 p.m.—Temp. 102°. Pulse 140. Respiration 28. Skin moist, tongue clean. 15 *c.c. inoculated* on account of rise of temperature. 5 p.m.—Temp. 100°. Pulse 136. Respiration 40. **30.12.98.**—7 a.m.—Temp. 99·6°. Pulse 140. Respiration 36. 9 a.m.—Temp. 100·2°. Pulse 120. Respiration 36. Peevish sleeps continually. Tongue clean. 3 p.m.—Temp. 102°. Pulse 110. Respiration 40. A small bleb* has formed over the site of bubo surrounded by a purplish spot, much œdema round the bubo. 5 p.m.—Temp. 99·6°. Pulse 140. Respiration 25. **31.12.98.**—7 a.m.—Temp. 101°. Pulse 144. Respiration 48. The glands on both sides of the neck have enlarged during the night.

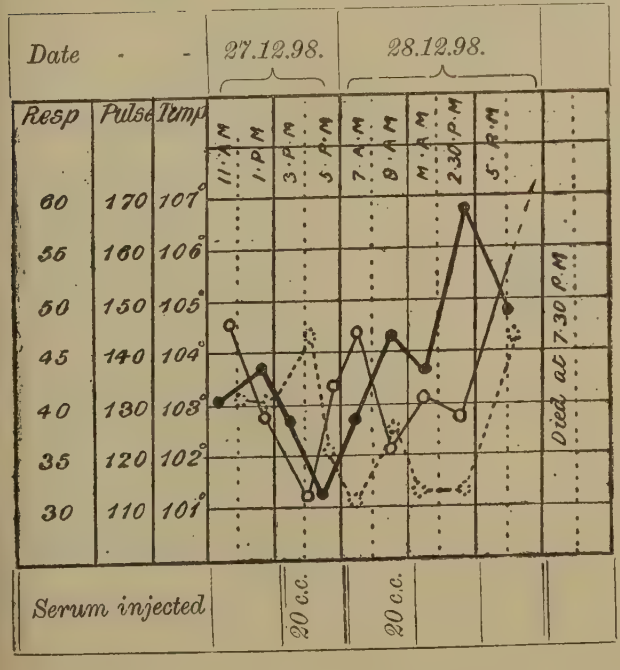
* A cover-slip preparation was taken from this bleb, which contained clear serum, and showed numerous bacilli, more or less shapeless masses. An agar tube was also inoculated.

CASE No. XIII. (Admitted on 5th day of the disease.)



Tukajee Rao. Age 25. Male. Admitted **27.12.98.**—9 a.m. Fever and bubo for four days. Patient staggering like a drunken man. Eyes suffused, unsteady voice, very dull. Pupils contracted. Temperature 98·4°. Pulse 96. Respiration 32. Bubo, one in right groin and another in right axilla; no wound found. Heart normal. Lungs, râles and rhonchi (principally rhonchi) and sibilus in left lung. Spleen could not be felt. 20 *c.c. inoculated.* 11 a.m.—Temp. 101°. Pulse 120. Respiration 32. 1 p.m.—Temp. 105·4°. Pulse 120. Respiration 44. 2.30 p.m —Temp. 105·6°. Pulse 116. Respiration 40. 5 p.m.—Temp. 104°. Pulse 134. Respiration 40. **28.12.98.**—7 a.m.—Temp. 102·2°. Pulse 132. Respiration 30. 9 a.m.—Patient very restless and delirious. Tongue dry and brown. Pupils contracted. Temp. 102·6°. Pulse 120. Respiration could not be taken. 20 *c.c. inoculated.* 11 a.m.—Temp. 103·6.°Pulse 144. Respiration 36. 1 p.m.—Temp. 103·2°. Pulse 126. Respiration 38. 2.30 p.m.—Temp. 104·4°. Pulse 120; irregular in time. Respiration not taken on account of excessive talking, very restless; had to be tied in bed. 5 p.m. — Temp. 102·4°. Pulse 156. Respiration 50. **29.12.98.**—7a.m.—Temp. 103°. Pulse 136. Respiration 28. Very delirious all night, did not sleep. 9 a.m.—Temp. 103·2°. Pulse 148. Respiration 36. 20 *c.c. inoculated.* Talking incessantly, pulse very weak. Lungs, squeaks heard. 11 a.m.—Temp. 104°. Pulse 96. Respiration 76. 2 p.m.—Temp. 101°. Pulse 160. Respiration 60. Bronchial breathing at both apices, especially the left, base of lungs clear, moribund. A cover glass preparation taken of sputum half hour before death stained with eosine and methylene blue, large quantities of diplococci and other micro-organisms seen and two small groups of polar staining bacilli. A cover glass preparation of the blood taken at the same time showed no bacilli or cocci. 4 p.m.—Had a long convulsion and died.

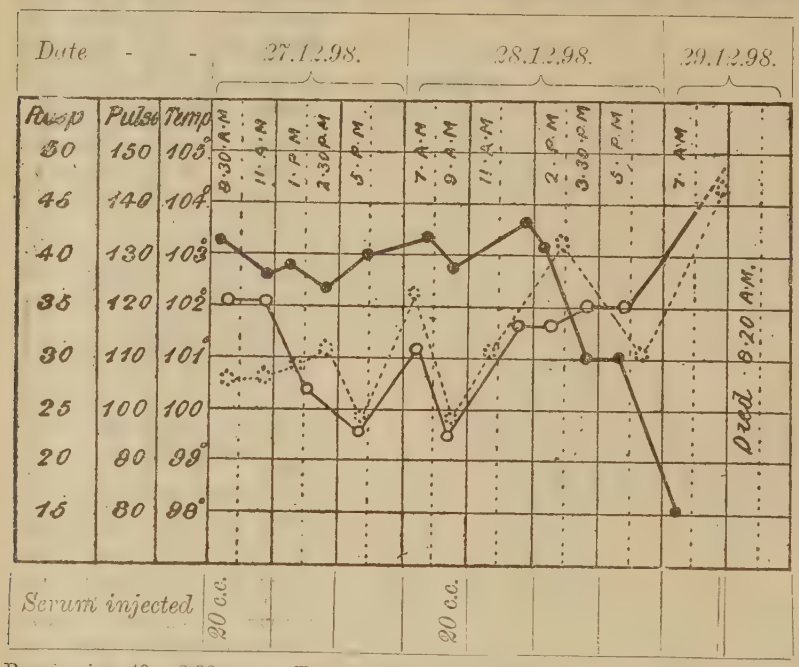
CASE No. XIV. (Admitted on 3rd day of the disease.)



Ramiah. Aged 15. Admitted **27.12.98.**—11 a.m. Fever and bubo two days. Temperature 103°. Pulse 144. Respiration 40. 1 p.m.—Temp. 103·4°. Pulse 128. Respiration 38. 3 p.m.—General condition moderate, dull, stupid. Bubo in right femoral glands. No wound detected. Heart and lungs normal. Eyes suffused. Spleen not felt. Temp. 102·8°. Respiration 48. Pulse 112. 20 *c.c. of serum inoculated* into right flank. 5 p.m.—Temp. 101·6°. Pulse 132. Respiration 35. **28.12.98.**—7 a.m. Temp. 102·8°. Pulse 144. Respiration 32. 9 a.m.—Temp. 104·6 . Pulse 120, very irregular in time and force. Respiration 36. Delirious. Tongue dry. 20 *c.c. inoculated.* 11 a.m.—Temp. 103·8°. Pulse 132. Respiration 32. 1 p.m.—Temp., &c. not able to be taken on account of violent delirium. 2.30 p.m. — Temp. 106·6°. Patient sponged, before which he was dull, apathetic, not answering when spoken to, but afterwards talked rationally, answering questions. Respiration 32. Pulse 128. 5 p.m.—Temp. 104·8°. Pulse running could not be counted. Respiration 148.

Case No. XV. (Admitted on ? day of the disease.)

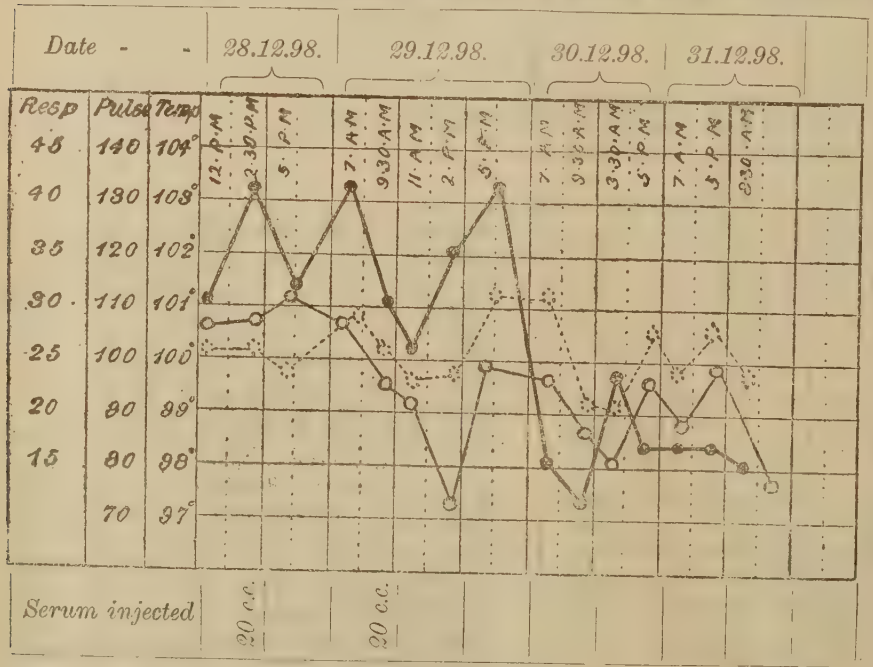
Mooniamah. Aged 55. Female. Admitted **27.12.98.**—7.30 p.m. History, none obtainable. So violently delirious that no temperature or other observations could be obtained. **28.12.98.**—8.30 a.m.—General condition bad. Very delirious still, but not violent, talking much, occasional slight cough. Tongue could not be seen. Eyes very suffused. Temp. 103.2°. Pulse 120. Respiration 28. 20 *c.c. inoculated.* Speen not felt. No bubo could be found. 11 a.m.—Temp. 102·6°. Pulse 120. Fair volume and tension. Respiration 28. 1 p.m.—Temp. 102·8°. Pulse 120. Respiration 30. 2.30 p.m. — Temp. 102·4°. Pulse 104. Respiration 32. Heart and lungs normal. 5 p.m.—Temp. 103°. Pulse 98. Respiration 24. **29.12.98.**—7 a.m.—Temp. 103·4°. Pulse 112. Respiration 36. 9 a.m.—Temp. 102·8°. Pulse 96. Respiration 24. Patient quiet but dull, no delirium, but will not put out her tongue when told to, which, however, does not look nearly so dry and cracked. No bubo found. 20 *c.c. of serum inoculated.* 11 a.m.—Temp. 103·6°. Pulse 116. Respiration 32. 2 p.m. —Temp. 103·2°. Pulse 116.



Respiration 42. 3.30 p.m.—Temp. 101°. Pulse 120. Respiration 40. General condition fair. 5 p.m.—Temp. 101°. Pulse 120. Respiration 30. **30.12.98.**—7 a.m.—Temp. 98°. Pulse running could not be counted. Respiration 48.

Case XVI. (Admitted on 2nd day of the disease.)

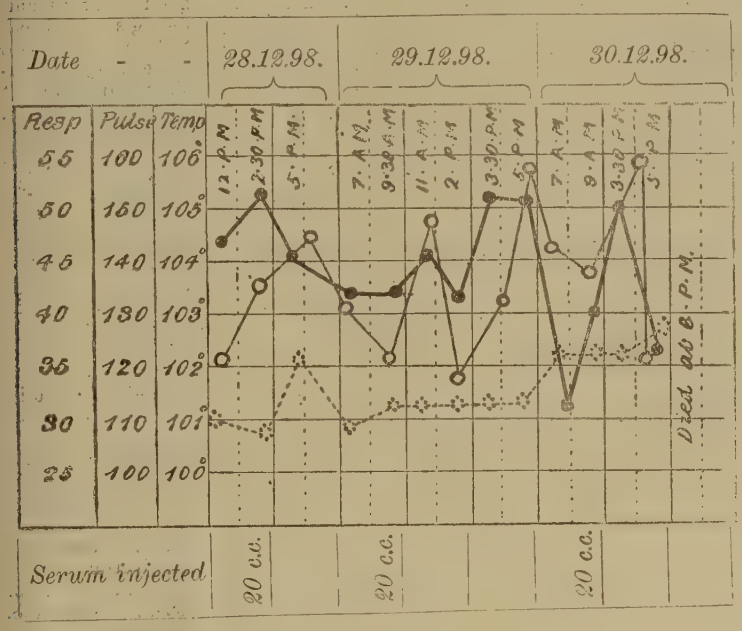
Nagamah. Age 50. Female. Admitted **28.12.98.**—12 p.m. Fever for day. Temperature 101°. Pulse 108 Respiration 26. 2.30 p.m.—General condition fair. Heart and lungs normal. Quite sensible. Spleen not felt. Tongue red at the tip; slight fur. Bubo in the left axilla not noticed before admission. Temp. 103·4°. Pulse 108°. Respiration 26. 20 *c.c. inoculated* into left flank. 5 p.m.—Temp. 101·4°. Pulse 112. Respiration 24. **29.12.98.**—7a.m.—Temp. 103·2°. Pulse 108. Respiration 28. 9.30 a.m.—Temp. 101°. Pulse 96. Respiration 26. General condition good. Bubo not painful. 20 *c.c. inoculated.* 11 a.m. — Temp. 100·2°. Pulse 92. Respiration 23.2 p.m.—Temp. 102.° Pulse 72. Respiration 24. Pulse good: patient seems in a very good condition. 5 p.m.—Temp. 103·4°. Pulse 100. Respiration 32. **30.12.98.**—7 a m.—Temp. 98°. Pulse 96. Respiration 32. 9.30 a.m.—Temp. 97·4°. Pulse 86. Respiration 20. Feels quite well. 3.30 p.m. —Temp. 99·8°. Pulse 80. Respiration 20. 5 p.m.—Temp. 98·4°. Pulse 96. Respiration 28. **31.12.98.** —7 a.m. — Temp. 98·4°. Pulse 86. Respiration 24.



5 p.m.—Temp. 98·4°. Pulse 98. Respiration 28. **1.1.99.**—8.30 p.m.—Temp. 98°. Pulse 76. Respiration 24. Bubo present but not painful. Convalescent.

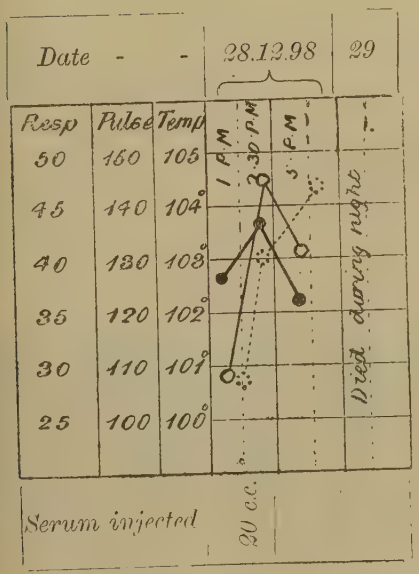
CASE XVII. (Admitted on 2nd day of the disease.)

Gnanama. Age 28. Female. Admitted **28.12.98.** 12 p.m.—Taken ill yesterday; bubo noticed last night. Temperature 104·2°. Pulse 120. Respiration 30. 2.30 p.m.—General condition fair. Heart and lungs clear. Temp. 105·2°. Pulse 132. Respiration 28. Distinct bubo in right groin. Pain complained of in left inguinal region. Small glands felt. Spleen could not be felt. Tongue furred, dry. 20 *c.c. inoculated* into left flank. 5 p.m. —Temp. 104°. Pulse 144. Respiration 36. **29.12.99.**—7 a.m.—Temp. 103·4°. Pulse 132. Respiration 28. 9.30 a.m. —Temp. 103·4°. Pulse 120. Respiration 32. No enlargement of glands or pain felt in the left groin; right bubo unchanged. General condition fair. 20 *c.c. inoculated.* 11 a.m.—Temp. 104°. Pulse 148. Respiration 32. 2 p.m.—Temp. 103·4°. Pulse 118. Respiration 32. 3.30 p.m.—Temp. 105·2°. Pulse 132; weak, and very hard to count. Respiration 32. 5 p.m.—Temp. 105°. Pulse 156. Respiration 32. **30.12.98.**—7a.m.—Temp. 101·2°. Pulse 134. Respiration 36. 9 a.m.—Temp. 103°. Pulse 128. Respiration 36. Dull, slightly delirious. Pulse very small. 20 *c.c. inoculated.* 3.30 p.m. —Temp. 105°. Pulse 156. Respiration 36. 5 p.m.—Temp. 102·4°. Pulse 110. Respiration 38. **31.12.98.**— Violently delirious all night. 6 a.m. —Died.

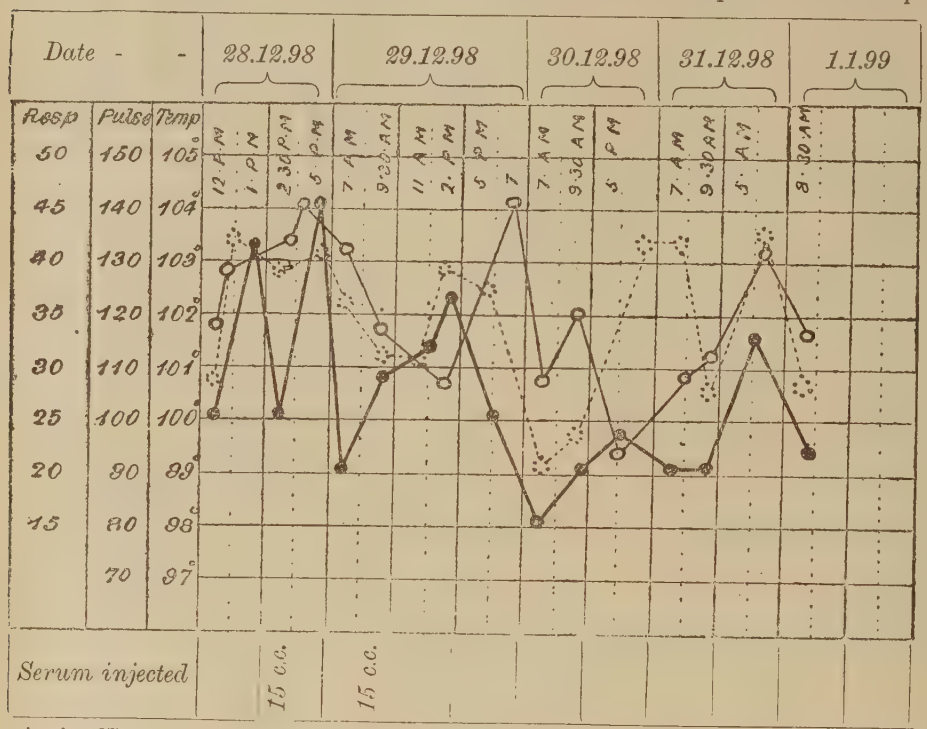


CASE No. XVIII. (Admitted on 7th day of the disease.)

Muriamah. Age 60. Female. Admitted **28.12.98.** Fever for six days. Temperature 102·8°. Pulse 108. Respiration 28. 2.30 p.m.— General condition very bad. Sensible but very weak. Tongue brown and dry, stuffed the roof of the mouth. Temp. 103·6°. Pulse 144. Respiration 40. Shallow. Pulse very small but regular. Bubo in left groin. No wound could be found. Heart normal, but sounds very weak. Lungs congested at the bases. 20 *c.c. of serum inoculated* into left flank. Practically moribund. 5 p.m.—Temp. 102·2°. Pulse 130. Respiration 48.

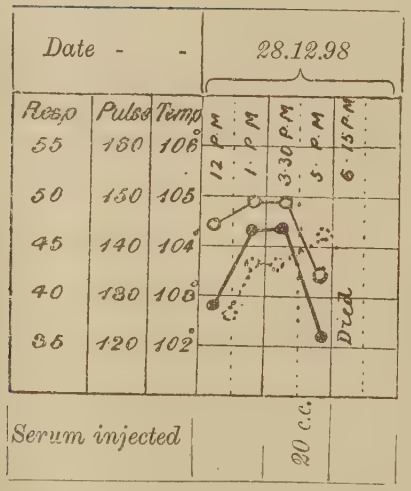


Case No. XIX. (Admitted on 6th day of the disease.)



Coopoo Sawmy. Age 11. Male. Admitted **28.12.98.**—12 p.m. Fever for five days. Temperature 100°. Pulse 118. Respiration 28. 1 p.m.—Temp. 103·4°. Pulse 128. Respiration 42. 2.30 p.m.—General condition good. Heart and lungs normal. Spleen could not be felt. Quite sensible. Bubo in left femoral glands. No wound could be found. Temp. 100°. Pulse 134. Respiration 40. 15 *c.c. inoculated* into left flank. 5 p.m.—Temp. 104°. Pulse 140. Respiration 42. **29.12.98.**—7 a.m.—Temp. 99°. Pulse 132. Respiration 36. 9.30 a.m.—Temp. 100·8° Pulse 118. Respiration 32. 15 *c.c. inoculated.* General condition good. Bubo unchanged. 11 a.m.—Temp. 101·6°. Pulse 112. Respiration 32. 2 p.m.—Temp. 102·4°. Pulse 108. Respiration 40. Quite sensible, feels fairly well. 5 p.m.—Temp. 100°. Pulse 144. Respiration 37. **30.12.98.**—7 a.m.—Temp. 98. Pulse 108. Respiration 22. 9.30 a.m.—Temp. 99°. Pulse 120. Respiration 24. Feels quite well. Not inoculated. Bubo still present. 5 p.m.—Temp. 99·8°. Pulse 94. Respiration 44. **31.12.98.**—7 a.m.—Temp. 99°. Pulse 108. Respiration 44. 9.30 a.m.—Temp. 99°. Pulse 112. Respiration 28. 5 p.m.—Temp. 101·8°. Pulse 133. Respiration 44. **1.1.99.**—8.30 a.m.—Temp. 99·2°. Pulse 116. Respiration 28.

Case No. XX. (Admitted on 6th day of the disease.)

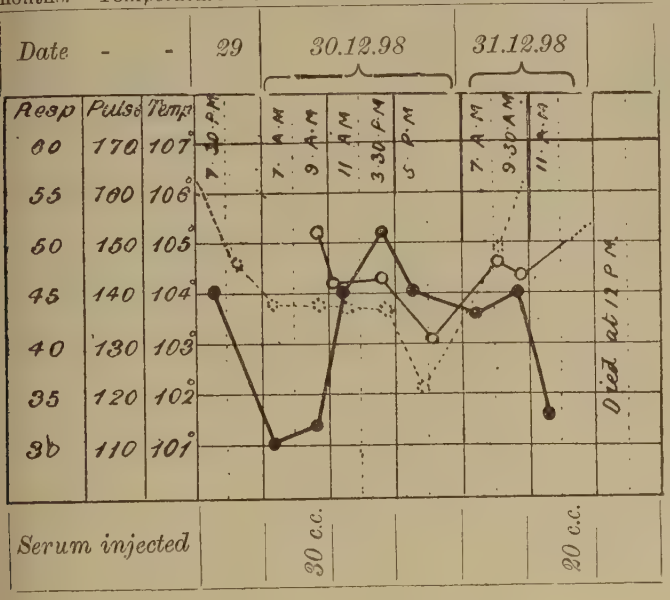


Hanmantha. Age 35. Male. Admitted **28.12.98.**—12 p.m. Fever for four or five days. Bubo for three days. Temperature 102·8°. Pulse 144. Respiration 38. 1 p.m.—Temp. 104·2°. Pulse 148. Respiration 44. 3.30 p.m.—General condition bad. Very dull and apathetic, but answering rationally when spoken to. Quiet, lying still. Tongue very furred, white. Bubo in right femoral region. No wound found. Spleen could not be felt. Temp. 104·2°. Pulse 148. Respiration 44. 20 *c.c. of serum inoculated* into left flank. 5 p.m.—Temp. 102·2°. Pulse 132. Respiration 46.

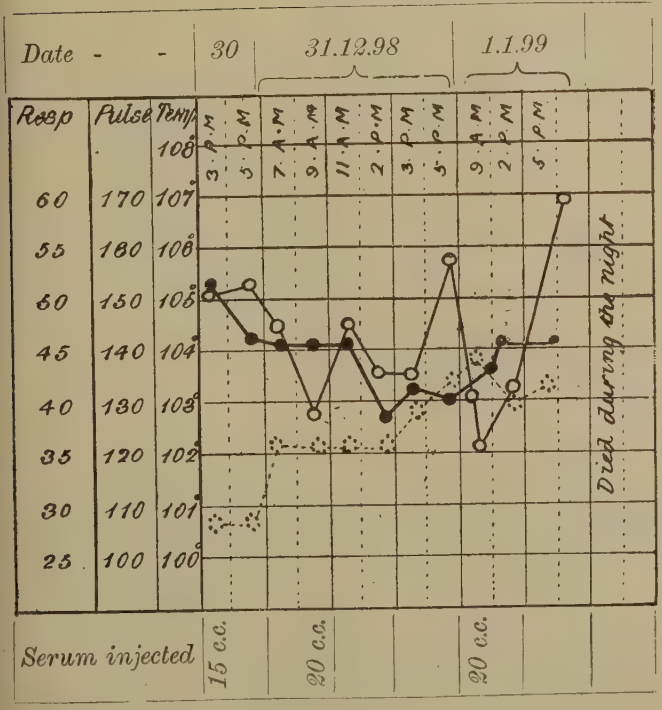
28.12.98.—One patient was admitted at the same time as cases Nos. XVI. to XX., but died almost immediately afterwards, and was not seen by me.

CASE No. XXI. (Admitted on 8th day of the disease.)

Nursamah. Age 25. Female. Admitted **29.12.98.**—7.30 p.m. Fever for eight days, and also pain in the left popliteal space. Six days ago swelling noticed in left axilla. Four days ago aborted. Fœtus about four months. Temperature 104°. Pulse uncountable. Respiration 48. **30.12.98.**—7 a.m.—Temp. 101. Pulse uncountable. Respiration 44. 9 a.m.—General condition bad. Tongue very clean considering. Pulse very rapid but fair volume. Buboes in left axilla and left popliteal space. No wound found. No offensive discharge from the vagina or pain on pressing the lower part of the belly. Temperature 101·6°. Pulse 152. Respiration 44. 30 *c.c. of serum inoculated.* 11 a.m.—Temp. 104·4°. Pulse 140. Respiration 44. 3.30 p.m.—Temp. 105·2°. Pulse 144. Respiration 44. Left lung dull on percussion. Bronchial breathing heard over dull area. A preparation stained with fuchsin showed numerous bipolar stained bacilli, also pneumococci and cocci. An agar tube was inoculated. Sputum in small quantities very like ordinary pneumonic sputum, but contained more blood colouring matter. 5 p.m.—Temp. 104°. Pulse 130. Respiration 35. **31.12.98.**—7 a.m.—Temp. 103·6°. Pulse 148. Respiration 48. 9.30 a.m.—Temp. 104°. Pulse 44. Respiration 56. No sputum brought up. 20 *c.c. inoculated* 11 a.m.—Temp. 101·6°. Pulse running.



CASE No. XXII. (Admitted on 2nd day of the disease.)

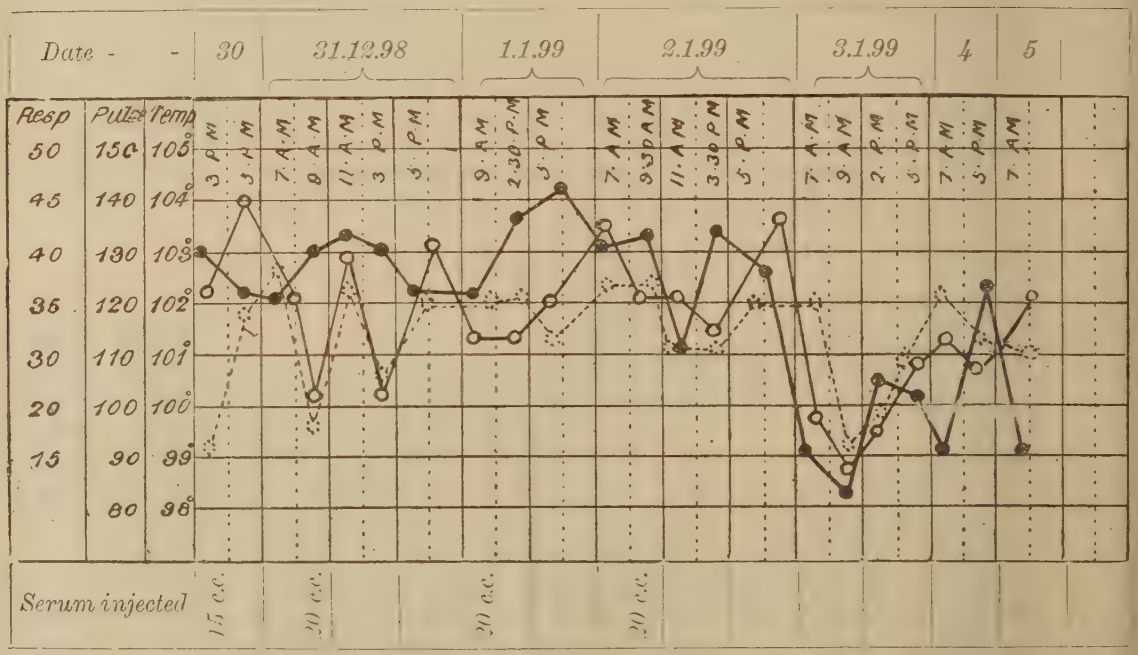


Ummanee. Age 11. Female. Admitted **30.12.98.**—2 p.m. Fever came on yesterday. Bubo appeared during the night. Temperature 105·4°. Pulse 152. Respiration 28. 3 p.m.—Very dull and apathetic. Heart and lungs clear. Tongue could not be seen. Bubo in left axilla. No wound. 15 *c.c. inoculated.* 5 p.m.—Temp. 104·2°. Pulse 154. Respiration 28. **31.12.98.**—7 a.m.—Temp. 104. Pulse 144. Respiration 36. 9 a.m.—Temp. 104°. Pulse 128. Respiration 36. 20 *c.c. inoculated.* 11 a.m.—Temp. 104°. Pulse 144. Respiration 36. 2 p.m.—Temp. 102·8°. Pulse 136. Respiration 36. 3 p.m.—Temp. 103·2°. Pulse 132. Respiration 40. 5 p.m.—Temp. 103°. Pulse 156. Respiration 42. **1.1.99.**—9 a.m.—Temp. 103·6°. Pulse 120. Respiration 44. 20 *c.c. inoculated.* 2 p.m.—Temp. 104. Pulse 132. Respiration 40. 5 p.m.—Temp. 104. Pulse 170. Respiration 42.

CASE No. XXIII. (Admitted on 5th day of the disease.)

Janakiammah. Age 13. Female. Admitted **30.12.98.**—2 p.m. Four days ago fever and bubo. 3 p.m.—General condition bad. Very restless and delirious. Bubo beneath left sterno mastoid. Tongue furred, white and dry. Tonsil could not be seen. Heart and lungs clear. Temp. 103·8°. Pulse 128. Respiration 36. 20 *c.c. inoculated* after much struggling. 5 p.m.—Temp. 104°. Pulse 144. Respiration 35. **31.12.98.**—7 a.m.—Temp. 101°. Pulse 104. Respiration 32. 9 a.m.—Temp. 101·4°. Pulse 136.—Respiration 36. General condition very bad. 20 *c.c. inoculated.* Very delirious; quieter. Pulse very weak. 11 a.m.—Temp. 102·4°. Pulse 100. Respiration 32. 2 p.m.—Temp. 104·4°. Pulse 132. Respiration 44. 3 p.m.—Very delirious, trying to get out of bed. Temp. 104·8°. Pulse 120. Respiration 36. 30 grs. of Potassi. Bromid. ordered. 5 p.m.—Temp. 105°. Pulse 148. Respiration 46.

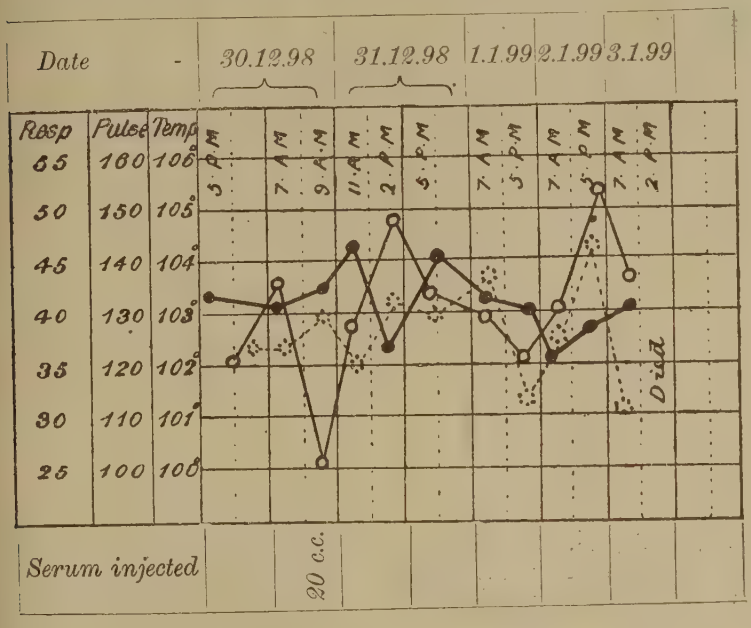
CASE No. XXIV. (Admitted on 2nd day of the disease.)



Munanee Amal. Aged 9. Fever. Sister of Case No. XXIII. Admitted **30.12.98.**—2 p.m. Fever for one day. 3 p.m.—Dull, apathetic. No bubo. Heart and lungs clear. Tongue, white fur. Temp. 103°. Pulse 122. Respiration 20. 15 *c.c. inoculated.* 5 p.m.—Temp. 102·4°. Pulse 140. Respiration 35 **31.12.98.**—7 a.m.—Temp. 102°. Pulse 120. Respiration 38. 9 a.m.—Temp. 103°. Pulse 104. Respiration 24. Bubo on right side of neck beneath the upper part of sterno mastoid. Tongue fairly clean. Still dull. 20 *c.c. inoculated.* 11 a.m.—Temp. 103·2°. Pulse 120. Respiration 36. 3 p.m.—Temp. 103°. Pulse 104. Respiration 28. Sensible. but dull. 5 p.m.—Temp. 102·4°. Pulse 132. Respiration 36. **1.1.99.**—9 a.m.—Temp. 102°. Pulse 116. Respiration 36. 20 *c.c. inoculated.* Seems better. Bubo in right axilla. 2.30 p.m.—Temp. 103·6°. Pulse 116. Respiration 36. Much brighter. 5 p.m.—Temp 104·2°. Pulse 120. Respiration 32. **2.1.99.**—7 a.m. Temp. 103°. Pulse 132. Respiration 36. 9.30 a.m.—Temp. 103·2°. Pulse 120. Respiration 36. Better. 20 *c.c. inoculated.* 11 a.m. Temp. 101°. Pulse 120. Respiration 30. 3.30 p.m.—Temp. 103·4°. Pulse 116. Respiration 30. Condition about the same. no worse. 5 p.m.—Temp. 102·8°. Pulse 136. Respiration 36. **3.1.99.**—7 a.m.—Temp. 99°. Pulse 98. Respiration 36. 9 a.m.—Temp. 98·4°. Pulse 88. Respiration 20. Not inoculated. 2 p.m.—Temp. 100·8°. Pulse 96. Respiration 24. 5 p.m.—Temp. 100·4°. Pulse 108. Respiration 20. **4.1.99.**—7 a.m.—Temp. 99. Pulse 112. Respiration 36. 5 p.m.—Temp. 102·4°. Pulse 108. Respiration 32. **5.1.99.**—7 a.m.—Temp. 99·2°. Pulse 120. Respiration 30.

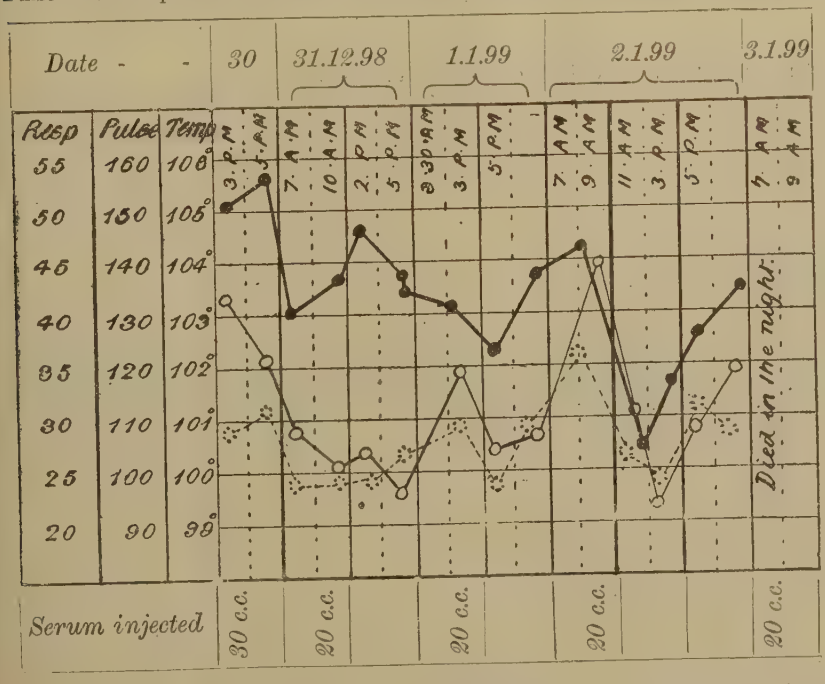
30.12.98.—Two other cases were admitted about this time, one a small child with a suppurating bubo, the other a man with a normal temperature, who complained of pains all over, which had been present for 10 days.

CASE No. XXV. (Admitted on 6th day of the disease.)



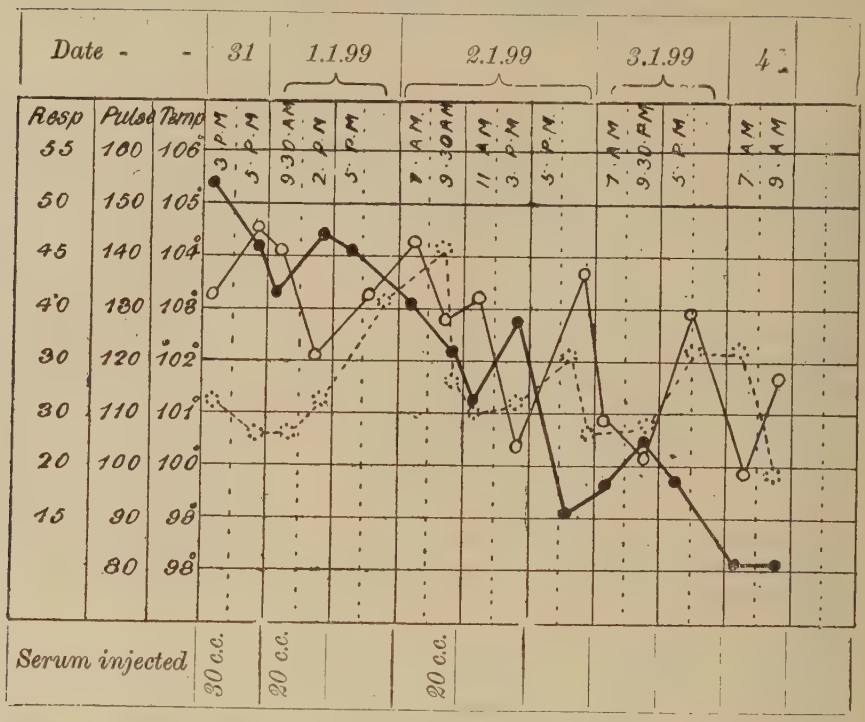
Santhiamal. Age 25. Female. Admitted **30.12.98.**—12 p.m. Illness for five days. 3 p.m.—Cold in the head. Heart normal. Râles, rhonchi, and whistling over both lungs. Most marked at left base. No sputum. 5 p.m.—Temperature 103·2°. Pulse 120 Respiration 36. **31.12.98.**—7 a.m.—Temp. 103°. Pulse 122. Respiration 36. 9 a.m.—Temp. 103·4°. Pulse 100. Respiration 40. 20 *c.c. inoculated.* 11 a.m.—Temp. 104·2°. Pulse 128. Respiration 34. 2 p.m.—Temp. 102·6°. Pulse 148. Respirations 42. 5 p.m.—Temp. 104. Pulse 136. Respiration 40. **1.1.99.**—7 a.m.—Temp. 103·2°. Pulse 130. Respiration 44. 5 p.m.—Temp. 103°. Pulse 120. Respiration 32. This case does not seem at all like a plague case, there being no marked symptoms of depression, not inoculated in consequence. **2.1.99.**—7 a.m.—Temp. 102°. Pulse 130. Respiration 36. 5 p.m.—Temp. 102·8°. Pulse 156. Respiration 46. Does not look very serious, pulse good, although fast; vomited a round worm. **3.1.99.**—7 a.m.—Temp. 103°. Pulse 132. Respiration 30. Still coughing, râles and rhonchi in lungs. Not so well.

CASE No. XXVI. (Admitted on 2nd day of the disease.)



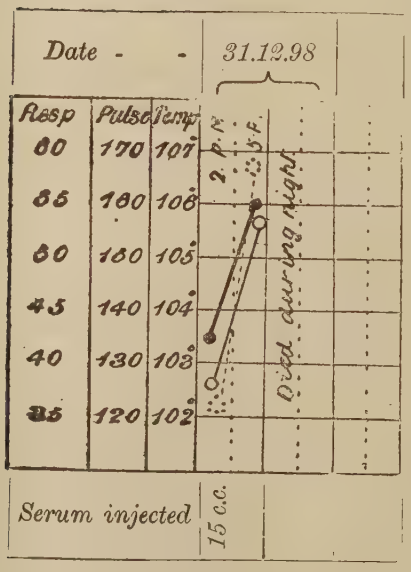
Chumappu. Age 27. Male. Admitted **30.12.98.**—1 p.m. Yesterday evening fever and bubo in left groin. 3 p.m.—Dull, quiet. Heart and lungs clear. Bubo in left groin. Tongue furred, white. Temperature 105. Pulse 132. Respiration 28. 30 *c.c. inoculated.* 5 p.m.—Temp. 105·6°. Pulse 120. Respiration 32. **31.12.98.**—7 a.m.—Temp. 103°. Pulse 108. Respiration 24. 10 a.m.—Temp. 103·8°. Pulse 100. Respiration 24. General condition improved. 20 *c.c. inoculated.* 11 a.m.—Temp. 103·8°. Pulse 94. Respiration 28. 2 p.m.—Temp. 104·8°. Pulse 104. Respiration 24. 3 p.m.—Temp. 103·6°. Pulse 96. Respiration 26. 5 p.m.—Temp. 103·2°. Pulse 120. Respiration 30. **1.1.99.**—8.30 a.m.—Temp. 102·6°. Pulse 104. Respiration 24. 20 *c.c. inoculated.* 3 p.m.—Temp. 103·8°. Pulse 108. Respiration 28. Very restless, has to be tied into bed. 5 p.m.—Temp. 104·2°. Pulse 140. Respiration 36. **2.1.99.**—7 a.m.—Temp. 100·2°. Pulse 110. Respiration 26. 9 a.m.—Temp. 101·6°. Pulse 92. Respiration 24. Much quieter, very weak, rapidly losing flesh. 20 *c.c. inoculated.* 11 a.m.—Temp. 102·6°. Pulse 108. Respiration 30. 3 p.m.—Temp 103·4°. Pulse 120. Respiration 28. Delirious, but not violent. **3.1.99.**—9 a.m.—Very bad, pulse not perceptible at the wrist. About 160. Respiration sighing, slow. Temp. 102°. 20 *c.c. inoculated.* 2 p.m.—Still living, but moribund.

CASE No. XXVIII. (Admitted on 4th day of the disease.)



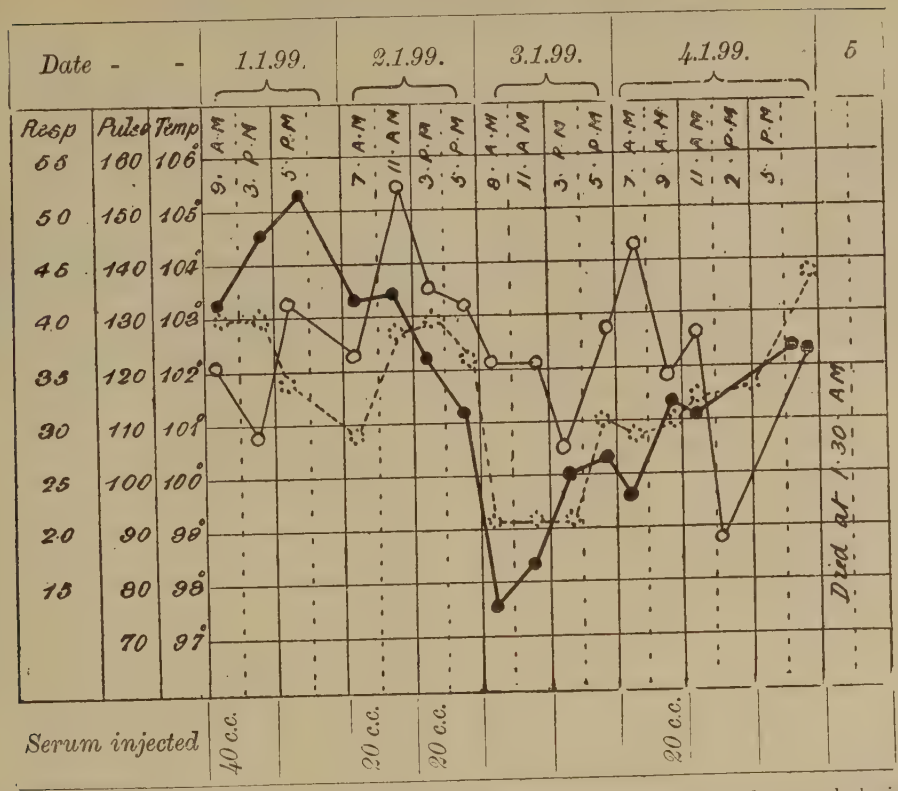
Ramabai. Age 25. Female. Admitted **31.12.98.**—1 p.m. Fever for three or four days. Bubo for two days. 3.0 p.m.—Tongue furred, white. General condition fair, but rather restless. Heart and lungs clear. Bubo in right axilla. Temp. 105·3°. Pulse 132. Respiration 32. 30 *c.c. inoculated.* 5.0 p.m.—Temp. 104·2. Pulse 144. Respiration 28. **1.1.99.**—9.30 a.m.—Temp. 103·2. Pulse 140. Respiration 28. Complains of much pain in bubo. 20 *c.c inoculated.* 2.0 p.m.—Temp. 104·4°. Pulse 120. Respiration 32. General condition very fair. 5.0 p.m.—Temp. 104°. Pulse 134. Respiration 42. Slightly delirious. **2.1.99.**—7.0 a.m.—Temp. 103°. Pulse 144. Respiration 48. 9.30 a.m.—Temp. 102·2°. Pulse 128. Respiration 34. General condition good; complains of much pain in the bubo. 20 *c.c. inoculated* (under protest). 11.0 a m.—Temp. 101·4°. Pulse 132. Respiration 32. 3.0 p.m.—Temp. 102·8°. Pulse 104. Respiration 32. Seems better. 5.0 p.m.—Temp. 99°. Pulse 136. Respiration 36. **3.1.99.**—7.0 a.m.—Temp. 99·8°. Pulse 108. Respiration 28. 9.30 a.m.—Temp. 100·4°. Pulse 104. Respiration 28. Not inoculated; much better; bubo still painful. 2.0 p.m.—Sleeping quietly. 5.0 p.m.—Temp. 99·8°. Pulse 130. Respiration 36. **4.1.99.**—7.0 a.m.—Temp. 98°. Pulse 98. Respiration 36. 9.0 a.m.—Temp. 98°. Pulse 116. Respiration 24.

CASE No. XXIX. (Admitted on ? day of the disease.)



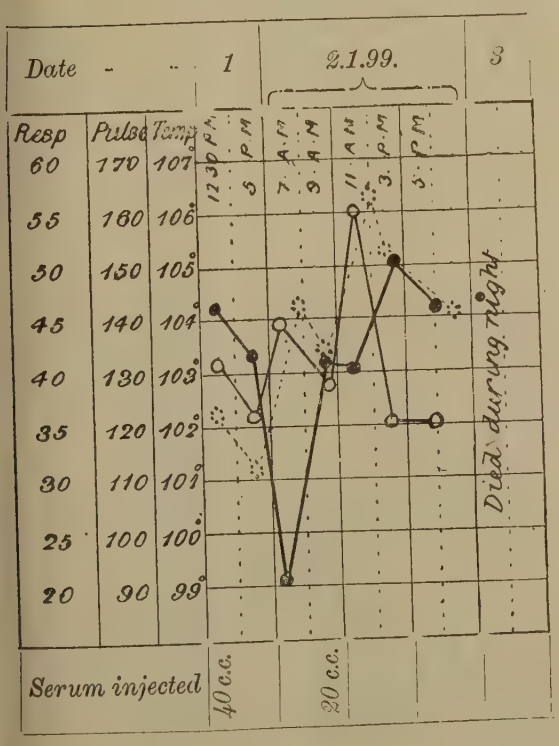
Kavariamah. Aged 2. Female. Admitted **31.12.98.**—10.30 a.m. History.—None. Temp. 103·2°. Pulse 124. Respiration 36. General condition bad, constantly shaking her head, and grinding teeth. 2.0 p.m.—15 *c.c. inoculated* into buttock. 5.0 p.m.—Temp. 106°. Pulse 156. Respiration 58.

CASE No. XXX. (Admitted on 5th day of the disease.)



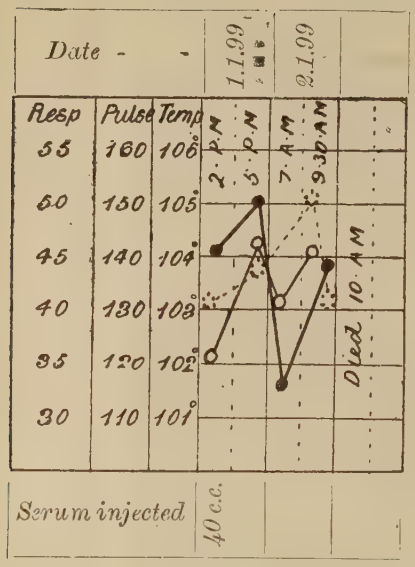
Kupram Rajoo. Aged. Male. Admitted **1.1.99.**—9 a.m. Fever for four days. Two days ago bubo in right groin. One day ago bubo on left side of the neck. General condition, very bad indeed. Pulse very weak and irregular, 120. Temperature 103·4°. Respiration 40. Never still, tendons at wrist continually twitching. Low muttering delirium. Might die at any time. 40 *c.c. injected.* 3 p.m.—Temp. 104·4°. Pulse 108. Respiration 40. Pulse stronger. 5 p.m.—Temp. 105·2°. Pulse 132. Respiration 34. **2.1.99.**—7 a.m.—Temp. 103·4. Pulse 122. Respiration 28. 9.30 a.m.—Rather better. 20 *c.c. inoculated.* 11 a.m.—Temp. 103·6°. Pulse 156. Respiration 38. 3 p.m.—Temp. 102·2°. Pulse 136. Respiration 40. Condition much the same; not sensible, but quiet. 20 *c.c. inoculated.* 5 p.m.—Temp. 101·2°. Pulse 132. Respiration 36. **3.1.99.**—8 a.m.—Temp. 97·8°. Pulse 120. Respiration 20. Very much better; pulse much stronger; patient exceedingly feeble. Not inoculated. 11 a.m.—Temp. 98·4°. Pulse 120. Respiration 20. 3 p.m.—Temp. 100°. Pulse 104. Respiration 20. Very weak, but sensible; pulse better. 5 p.m.—Temp. 100·2°. Pulse 128. Respiration 30. **4.1.99.**—7 a.m.—Temp. 99·6°. Pulse 144. Respiration 28. 9 a.m.—Temp. 101·6°. Pulse 108. Respiration 30. Not quite so well. 20 *c.c. inoculated.* 11 a.m.—Temp. 101°. Pulse 128. Respiration 32. 2 p.m.—Temp. 101·8. Pulse 88. Respiration 34. 5 p.m.—Temp. 102·4. Pulse 124. Respiration 44.

CASE No. XXXI. (Admitted on 6th day of the disease.)



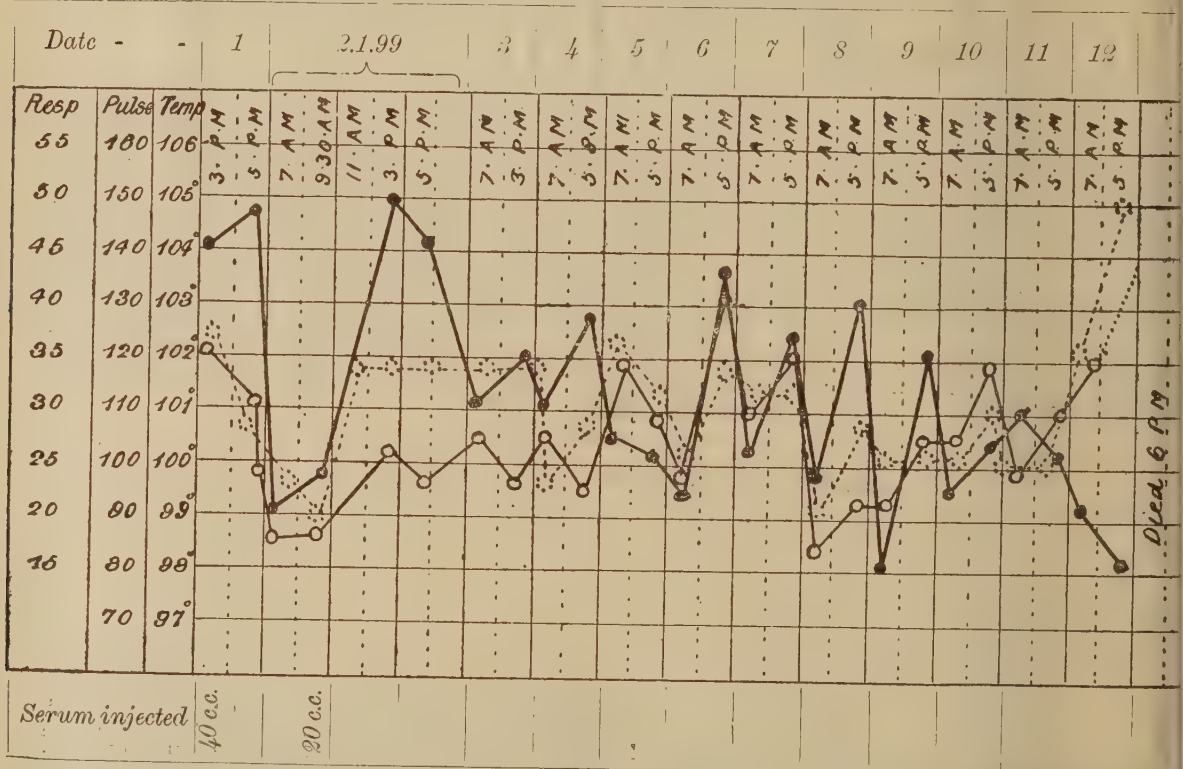
Mooniswamy. Age 18. Male. Admitted **1.1.99.**—12.30 p.m. Fever for three days. Cough for four days. Temperature 104·2°. Pulse 132. Respiration 36. 3 p.m.—General condition not good. Small wound on right forearm. Bubo in right epitrochlear glands. Lungs: Left lung diminished resonance, increased vocal vibrations, and bronchial breathing. Right lung practically normal; a few râles heard at base. 40 *c.c. inoculated.* 5 p.m.—Temp. 103·6°. Pulse 120. Respiration 30. **2.1.99.**—7 a.m.—Temp. 99°. Pulse 140. Respiration 46. 9 a.m.—Temp. 103·2°. Pulse 128. Respiration 42. Right lung involved in disease, marked bronchial breathing and coarse râles. No sputum obtained. 20 *c.c. inoculated.* 11 a.m.—Temp. 103°. Pulse 160. Respiration 56. 3 p.m.—Temp. 105·2°. Pulse 120. Respiration 52. Very bad. 5 p.m.—Temp. 104·2°. Pulse 120. Respiration 45.

CASE No. XXXII. (Admitted on 6th day of the disease.)



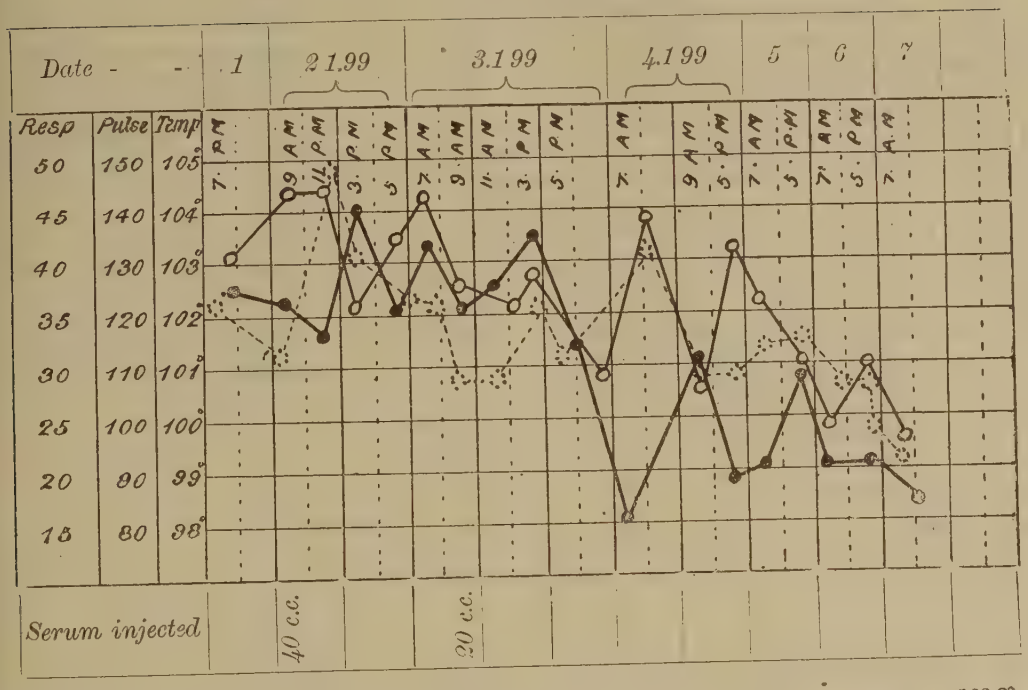
Thinakah. Age 35. Female. Admitted **1.1.99.** Fever for 5 days, and pain in left groin for the same time. 2 p.m.—General condition fair. Heart and lungs normal. 40 *c.c. inoculated.* Very small bubo in left femoral region. Temperature 104°. Pulse 120. Respiration 40. 5 p.m.—Temp. 105°. Pulse 144. Respiration 44. **2.1.99.**—7 a.m.—Temp. 101·6°. Pulse 130. Respiration 50. 9.30 a.m.—Temp. 103·8°. Pulse 140, not perceptible at the wrist. Respiration 40. Moribund.

CASE No. XXXIII. (Admitted on 1st day of the disease.)



Perrinhavahma. Age 55. Mother of XXIII. and XXIV. Admitted 3 p.m.—**1.1.99.** Has been nursing her children in hospital. This afternoon vomited twice, no rigor. Complains of headache. Tongue furred. No bubo or pain in site of any set of glands. Temperature 104°. Pulse 120. Respiration 36. 40 *c.c. inoculated.* 5 p.m.—Temp. 104·6°. Pulse 110. Respiration 28. **2.1.99.**—7 a.m.—Temp. 99°. Pulse 88. Respiration 24. 9.30 a.m.—Temp. 99·8°. Pulse 88. Respiration 20. 20 *c.c. inoculated.* 11 a.m.—Temp. 102·6°. Pulse 96. Respiration 36. 3 p.m.—Temp. 105°. Pulse 104. Respiration 36. No bubo. 5 p.m.—Temp. 104·2°. Pulse 98. Respiration 36. **3.1.99.**—7 a.m.—Temp. 101·2°. Pulse 108. Respiration 36. 5 p.m.—Temp. 102. Pulse 98. Respiration 36. **4.1.99.**—7 a.m.—Temp. 101°. Pulse 108. Respiration 24. 5 p.m.—Temp. 102·8°. Pulse 98. Respiration 28. **5.1.99.**—7 a.m.—Temp. 100·8°. Pulse 120. Respiration 36. Feels quite well. 5 p.m.—Temp. 100°. Pulse 108. Respiration 32. **6.1.99.**—7 a.m.—Temp. 99·4°. Pulse 96. Respiration 26. 5 p.m.—Temp. 103·8°. Pulse 132. Respiration 35. **7.1.99.**—7 a.m.—Temp. 100·2°. Pulse 108. Respiration 30. 5 p.m.—Temp. 102·6°. Pulse 120. Respiration 32. **8.1.99.**—7 a.m.—Temp. 99·8°. Pulse 84. Respiration 22. 5 p.m.—Temp. 103°. Pulse 96. Respiration 28. **9.1.99.**—7 a.m.—Temp. 98°. Pulse 96. Respiration 25. 5 p.m.—Temp. 102·2°. Pulse 108. Respiration 26. **10.1.99.**—7 a.m.—Temp. 99·6°. Pulse 108. Respiration 26. 5 p.m.—Temp. 100·2°. Pulse 120. Respiration 30. **11.1.99.**—7 a.m.—Temp. 101°. Pulse 96. Respiration 25. 5 p.m.—Temp. 100·2°. Pulse 110. Respiration 25. **12.1.99.**—7 a.m.—Temp. 99·2°. Pulse 120. Respiration 30. From the 5th to this date, although patient had had some fever every night, she always stated that she felt quite well. Quinine had been administered, but in no way affected the temperature. When seen at 9 a.m., patient was breathing rapidly and laboriously, râles and rhonchi were heard over both lungs; in the afternoon patient's condition was much worse, she was, however, quite conscious, dulness was not complete on percussing the chest. 5 p.m.—Temp. 98·2. Pulse running. Respiration 50. No sputum could be obtained.

CASE No. XXXIV. (Admitted on 4th day of the disease.)

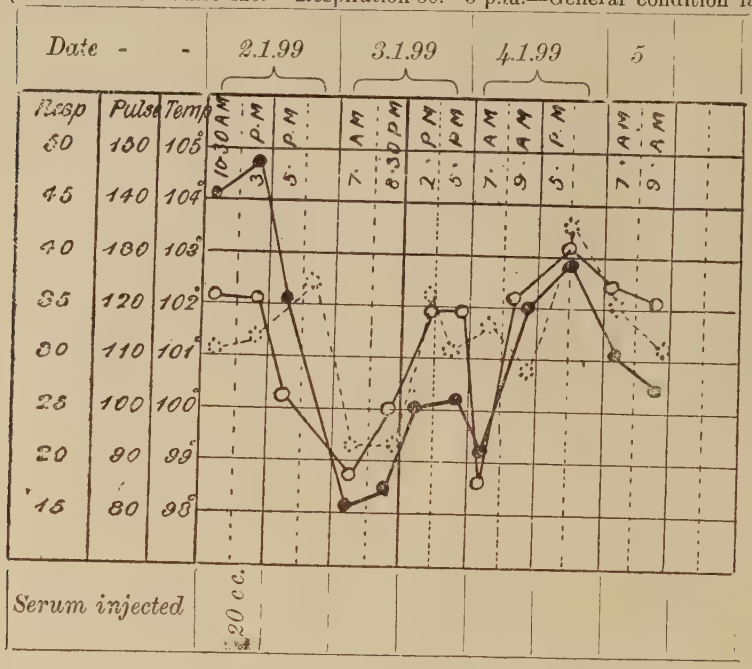


Runga. Age 14. Male. Admitted **1.1.99.** evening. Ill for three or four days. Temperature 102·8°. Pulse 130. Respiration 36. **2.1.99.**—7 a.m.—Temp. 102·4°. Pulse 144. Respiration 32. General condition fair. Heart and lungs normal. No bubo found. 40 *c.c. inoculated.* 11 a.m.—Temp. 101·6°. Pulse 144. Respiration 48. 3 p.m.—Temp. 104°. Pulse 120. Respiration 40. Complains of a pain in the belly. ? inoculation. 5 p.m. —Temp. 102°. Pulse 136. Respiration 36. **3.1.99.**—7 a.m.—Temp. 103·4°. Pulse 144. Respiration 36. 9 a.m. —Temp. 102°. Pulse 120. Respiration 28. 20 *c.c. inoculated.* No bubo. Slight cough. Sputum mucopus. 11 a.m.—Temp. 102·6°. Pulse 120. Respiration 28. 2.30 p.m.—Temp. 103·4°. Pulse 128. Respiration 36. General condition good. 5 p.m.—Temp. 101·2°. Pulse 108. Respiration 32. **4.1.99.**—7 a.m.—Temp. 98°. Pulse 136? Respiration 42. 9 a.m.—Temp. 101·2°. Pulse 116. Respiration 28. 5 p.m.—Temp. 98·8°. Pulse 132. Respiration 28. 5.1.99.—7 a.m.—Temp. 99°. Pulse 122. Respiration 32 5 p.m.—Temp. 100·8°. Pulse 108. Respiration 32. **6.1.99.**—7 a.m.—Temp. 99°. Pulse 93. Respiration 26. 5 p.m.—Temp. 99°. Pulse 110. Respiration 28. **7.1.99.**—7 a.m.—Temp. 98·4°. Pulse 96. Respiration 24.

1.1.99.—Two other cases admitted. One had had an abortion four days ago, and now had a slight temperature. The other was a very old woman, who had no bubo, and nothing to indicate plague in any way. Died.

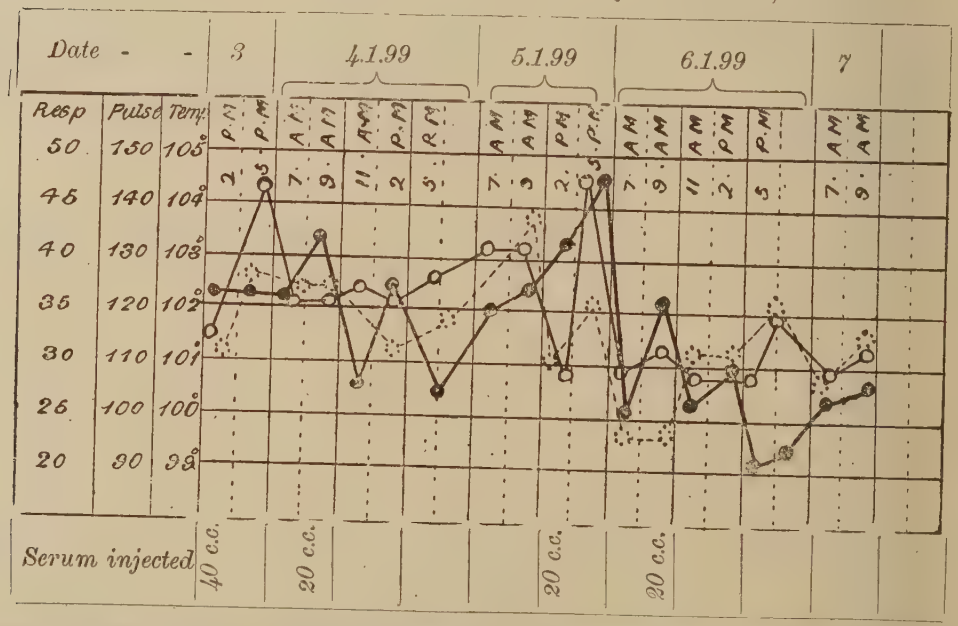
CASE No. XXXV. (Admitted on 3rd day of the disease.)

Ramalingam. Age 19. Admitted **2.1.99.**—10.30 a.m. Fever for two days. Bubo for one day. Temperature 104°. Pulse 120. Respiration 30. 3 p.m.—General condition fair. Tongue furred. Bubo in right groin. No wound found. Temperature 104·8°. Pulse 120. Respiration 32. 20 *c.c. inoculat* 5 p.m.—Temp. 102°. Pulse 1 Respiration 36. **3.1.99.**—7 a.m. Temp. 98°. Pulse 88. Respiration 8.30 a.m.—Temp. 98·4°. Pulse 1 Respiration 22. Much better. Bu in left axilla. 2 p.m.—Temp. 10 Pulse 120. Respiration 36. 5 p. —Temp. 100°. Pulse 120. Respir tion 30. **4.1.99.**—7 a.m.—Tem 99·2°. Pulse 88. Respiration 3 9 a.m.—Temp. 102°. Pulse 12 Respiration 28. Buboes very painf 5 p.m.—Temp. 103°. Pulse 1 Respiration 42. **5.1.99.**—7 a.m. Temp. 101°. Pulse 124. Respirati 35. 9 a.m.—Temp. 100·8°. Pulse 12 Respiration 30. **6.1.99.**—Remove to another ward to have the bubo opened.



2.1.99.—One case admitted with suppurating bubo in the morning and one in the afternoon.

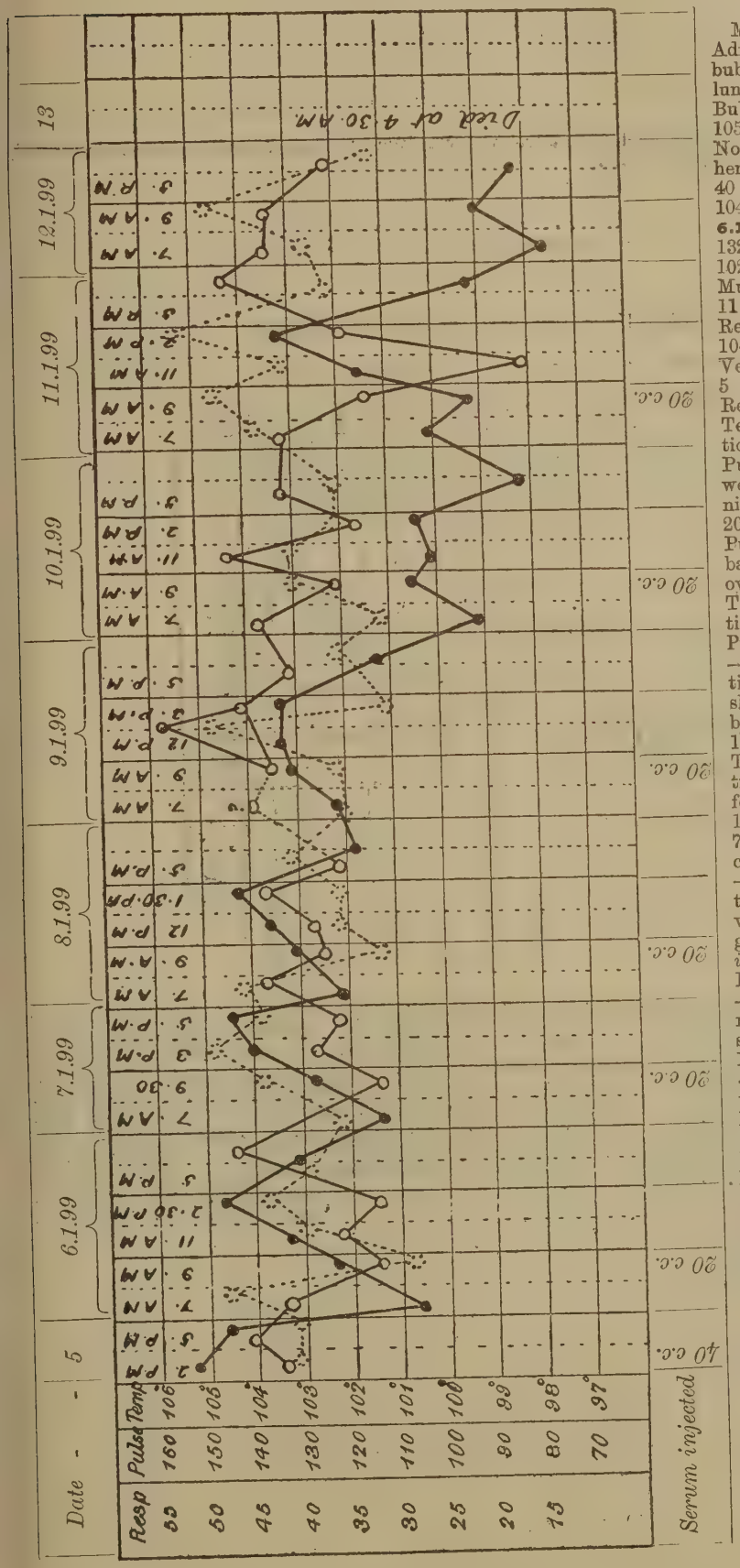
CASE No. XXXVI. (Admitted on 7th day of the disease.)



Mersamah. Age 25. Female. Admitted **3.1.99.**—2 p.m. Fever and bubo for six days. General condition fair; somewhat dull, and slightly delirious. Heart normal. Lungs: a few squeaks heard in both. Bubo in left groin. Temperature 102·2°. Pulse 114. Respiration 32. 40 *c.c. inoculated.* 5 p.m.—Temp. 102·2°. Pulse 144. Respiration 36. **4.1.99.**—7 a.m.—Temp. 102°. Pulse 120. Respiration 36. 9 a.m.—Temp. 103·4°. Pulse 120. Respiration 36. 20 *c.c. inoculated.* General condition a little worse, still slightly delirious. 11 a.m.—Temp. 100·6°. Pulse 124. Respiration 32. 2 p.m.—Temp. 102·6. Pulse 120. Respiration 40. Delirious. Bronchial breathing (modified) over the front of right lung, but resonance fair, although diminished. 5 p.m.—Temp. 100·6°. Pulse 126. Respiration 34. **5.1.99.**—7 a.m.—Temp. 102. Pulse 132. Respiration 40. 9 a.m.—Temp. 102·2°. Pulse 132. Respiration 44. 2 p.m.—Temp. 103·2°. Pulse 108. Respiration 32. 20 *c.c. serum.* 5 p.m.—Temp. 104·8. Pulse 144. Respiration 36. **6.1.99.**—7 a.m.—Temp. 100°. Pulse 108. Respiration 24. 9 a.m.—Temp. 102·2°. Pulse 112. Respiration 24. 20 *c.c. inoculated.* Lungs clear; breathing more natural. 11 a.m.—Temp. 100·2°. Pulse 108. Respirations 32. 2 p.m.—Temperature 101°. Pulse 108. Respiration 30. Complains of pain on swallowing. 5 p.m.—Temperature 99·2°. Pulse 120. Respiration 36. **7.1.99.**—7 a.m.—Temp. 100·4°. Pulse 108. Respiration 28. 9 a.m.—Temp. 100·6°. Pulse 112. Respiration 32.

5.1.99.—One case admitted suffering from a diffuse swelling of the whole thigh (left), removed to another hospital.

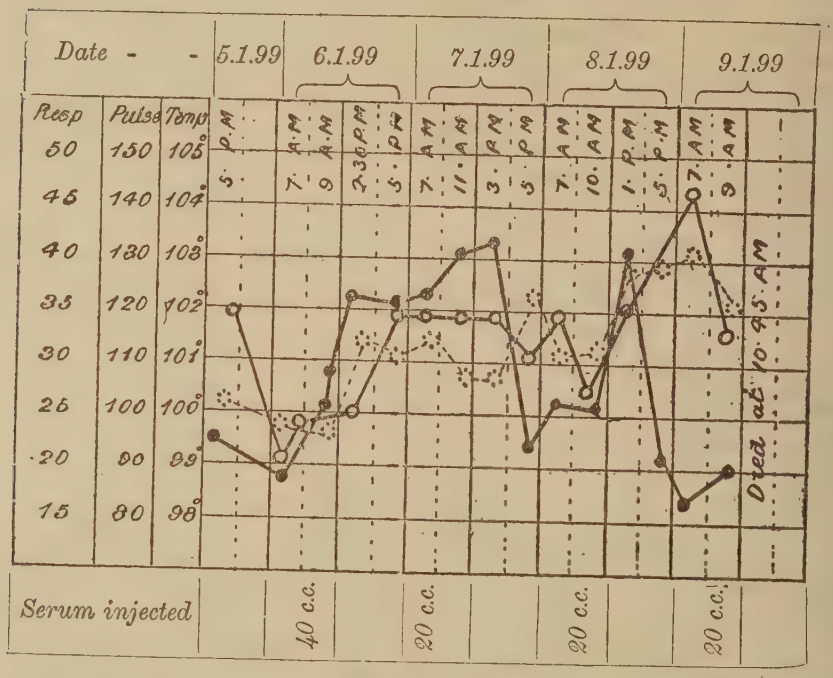
CASE No. XXXVII. (Admitted on 4th day of the disease.)



Mooniamah. Age 30. Female. Admitted **5.1.99.**—1 p.m. Fever and bubo for 3 days. 2 p.m.—Heart and lungs normal. General condition fair. Bubo in left groin. Temperature 105·2°. Pulse 132. Respiration 42. No wound, but states that she hurt her foot with a nail 3 or 4 days ago. 40 *c.c. inoculated.* 5 p.m.—Temp. 104·8°. Pulse 140. Respiration 40. **6.1.99.** 7 a.m.—Temp. 100·2°. Pulse 132. Respiration 48. 9 a.m.—Temp. 102·2°. Pulse 112. Respiration 28. Much better. 20 *c.c. inoculated.* 11 a.m.—Temp. 103·2°. Pulse 124. Respiration 40. 2.30 p.m.—Temp. 104·8°. Pulse 116. Respiration 44. Very delirious; condition worse. 5 p.m.—Temp. 103°. Pulse 144. Respiration 40. **7.1.99.**—7 a.m.—Temp. 101·2°. Pulse 122. Respiration 36. 9.30 a.m.—Temp. 102·8°. Pulse 112. Respiration 44. Pulse weak; very delirious during the night; now quiet, but very apathetic. 20 *c.c. serum.* 3 p.m.—Temp. 104°. Pulse 128. Respiration 48. Very bad. Lungs: sibilant râles heard over the front of both. 5 p.m.—Temp. 104·4°. Pulse 122. Respiration 44. **8.1.99.**—7 a.m.—Temp. 102°. Pulse 136. Respiration 45. 9 a.m.—Temp. 103°. Pulse 124. Respiration 32. 20 *c.c. serum.* Looks slightly better, but condition still bad. 12 p.m.—Temp. 103·6°. Pulse 126. Respiration 36. 1.30 p.m.—Temp. 104·2°. Pulse 136. Respiration 36. Condition very bad; pulse feeble. 5 p.m.—Temp. 101·8. Pulse 120. Respiration 42. **9.1.99.**—7 a.m.—Temp. 102°. Pulse (was uncountable). Respiration 35. 9 a.m.—Temp. 103°. Pulse 132. Respiration 36. Patient very wasted; pulse very bad. **9.1.99.**—Seems to be getting worse day by day. 20 *c.c. inoculated.* 12 p.m.—Temp. 103·2°. Pulse 156. Respiration 48. 3 p.m.—Temp. 103·2°. Pulse 140. Respiration 30. Very bad; pulse very small. 5 p.m.—Temp. 101·6°. Pulse 130. Respiration 36. **10.1.99.**—7 a.m.—Temp. 99°. Pulse 136. Respiration 30. 9 a.m.—Temp. 100·6°. Pulse 120. Respiration 40. Still exceeding weak. 20 *c.c. serum.* 11 a.m.—Temp. 100°. Pulse 144. Respiration 40. 2 p.m.—Temp. 100·2°. Pulse 116. Respiration 36. Pulse stronger; slightly better. 5 p.m.—Temp. 98·2°. Pulse 132. Respiration 36. **11.1.99.**—7 a.m.—Temp. 100°. Pulse 132. Respiration 44. 9 a.m.—Temp. 99·4°. Pulse 116. Respiration 48. Lungs: many râles. 20 *c.c. serum.* 11 a.m.—Temp. 101·6°. Pulse 80. Respiration 40. 2 p.m.—Temp. 103·2. Pulse 120. Respiration 52. 5 p.m.—Temp. 99·2°. Pulse 144. Respiration 36. **12.1.99.**—7 a.m.—Temp. 97·6°. Pulse 132. Respiration 38. 9 a.m.—Temp. 99°. Pulse 132. Respiration 48. 5 p.m.—Temp. 98·2°. Pulse 120. Respiration 32.

5.1.99.—One case admitted with normal temperature, who was found in a street seeming insensible, he had, however, drunk an unknown quantity of alcohol.

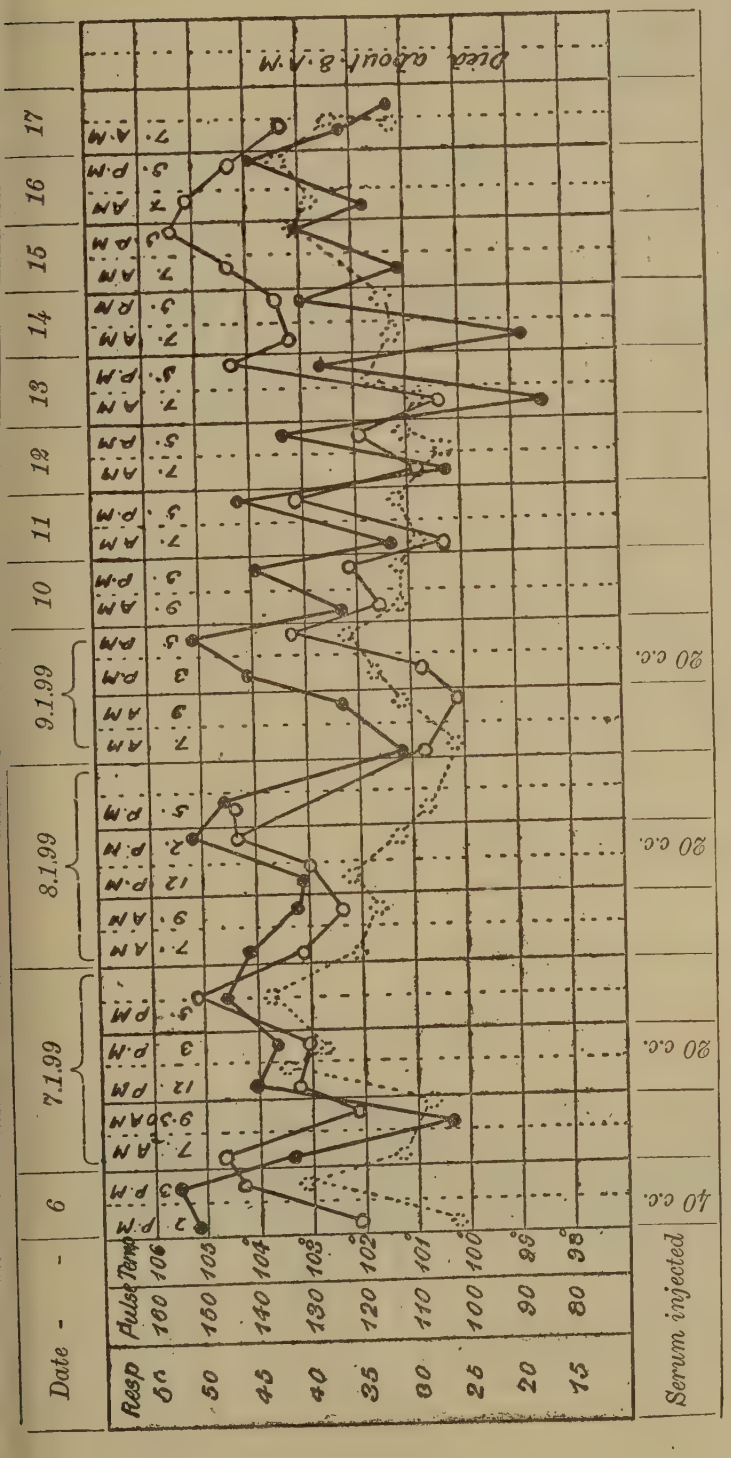
Case No. XXXVIII. (Admitted on 1st day of the disease.)



Anthony. Age 60. Male. Admitted **5.1.99.**—4 p.m. Taken ill yesterday (day of admission). Pain in the groin came on last night (5 and 6). Temp. 99·4°. Pulse 120. Respiration 26. **6.1.99.**—7 a.m.—Temp. 98·8°. Pulse 90. Respiration 24. 9 a.m.—Temp. 100·8°. Pulse 96. Respiration 24. A small painful bubo in left groin. Heart and lungs clear. Tongue very foul fur. General condition good. 40 *c.c. inoculated.* 2.20 p.m. —Temp. 102·6°. Pulse 100. Respiration 32. Some headache. 5 p.m.—Temp. 102·2°. Pulse 120. Respiration 30. **7.1.99.**—7 a.m.—Temp. 102·2°. Pulse 120. Respiration 32. 9 a.m.—20 *c.c. inoculated.* 11 a.m.—Temp. 103°. Pulse 120. Respiration 28. 3 p.m.—Temp. 103·2°. Pulse 120. Respiration 28. General condition fair. 5 p.m.—Temp. 99·4°. Pulse 110. Respiration 36. **8.1.99.**—7 a.m.—Temp. 100·4°. Pulse 120. Respiration 30. 10 a.m.—Temp. 100·2°. Pulse 104. Respiration 34. 1 p.m.—Temp. 103·2°. Pulse 120. Respiration 38. 20 *c.c. serum.* 5 p.m.—Temp. 99·2°. Pulse 128. Respiration 40. **9.1.99.**—7 a.m.—Temp. 98·4°. Pulse 144. Respiration 42. 9 a.m.—Condition very bad, extremely weak, pulse very small. Temp. 99°. Pulse 116. Respiration 36. 20 *c.c. serum.*

6.1.99.—One case of tetanus admitted, but transferred to another hospital.

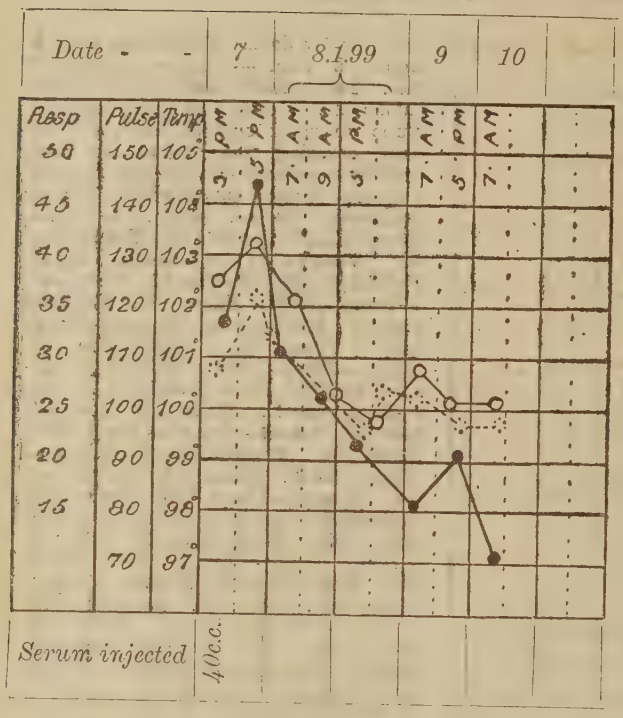
Case No. XXXIX. (Admitted on 6th day of the disease.)



Soomana. Aged 15. Female. Admitted **6.1.99.**—1 p.m. Pain in the neck for five days. Fever came on last night. 2 p.m. — General condition fair. Temp. 105°. Pulse 120. Respiration 26. Heart and lungs normal. Bubo on right side of neck, on the skin covering the bubo are four vesicles somewhat like 4th day vaccinia vesicles. Tongue furred white. Fauces reddened. 40 *c.c. serum.* 5 p.m.—Temp. 105·2°. Pulse 144. Respiration 40. **7.1.99.**—7 a.m.—Temp. 103.4°. Pulse 148. Respiration 32. 9.30. a.m. —Temp. 100·6°. Pulse 120. Respiration 28. Much better. 12 p.m.—Temp. 104°. Pulse 132. Respiration 42. 3 p.m.—Temp. 103·8°. Pulse 120. Respiration 38. 20 *c.c. inoculated.* 5 p.m. —Temp. 104·4°. Pulse 150. Respiration 44. **8.1.99.**—7 a.m.—Temp. 104°. Pulse 130. Respiration 35. 9 a.m.—Temp. 103·2°. Pulse 124. Respiration 34. 12 p.m.—Temp. 103°. Pulse 130. Respiration 36. 2 p.m.—Temp. 105·2°. Pulse 144. Respiration 32. 20 *c.c. serum.* 5 p.m.—Temp. 104·6°. Pulse 144. Respiration 28. **9.1.99.**—7 a.m.—Temp 101°. Pulse 108. Respiration 26. 9 a.m. — Temp. 102·2°. Pulse 100. Respiration 32. General condition better. Bubo extends from the middle line of the neck and fills up the whole of the submaxillary region. 3 p.m.—Temp. 104°. Pulse 108. Respiration 34. 20 *c.c. serum.* 5 p.m.—Temp. 105·2°. Pulse 132. Respiration 36. **10.1.99.**—9 a.m.—Temp. 102·2°. Pulse 116. Respiration 32. Bubo softening and showing signs of suppuration. Removed to another ward to have it opened. Bubo opened freely but no pus let out. 5 p.m.—Temp. 104°. Pulse 122. Respiration 32. **11.1.99.**—7 a.m. —Temp. 101·4°. Pulse 104. Respiration 28. 5 p.m.—Temp. 104·2°. Pulse 132. Respiration 32. **12.1.99.**—7 a.m. Temp. 100·2°. Pulse 108. Respiration 26. 5 p.m.—Temp. 103·4°. Pulse 120. Respiration 32. **13.1.99.**—7 a.m.—Temp. 98·4°. Pulse 104. Respiration 28. 5 p.m.—Temp. 102·8°. Pulse 144. Respiration 34. **14.1.99.**—7 a.m.—Temp. 98·8°. Pulse 132. Respiration 32. 5 p.m.—Temp. 103°. Pulse 134. Respiration 34. **15.1.99.**—7 a.m.—Temp. 101°. Pulse 144. Respiration 36. 5 p.m.—Temp. 103°. Pulse 156. Respiration 40. **16.1.99.**—7 a.m.—Temp. 101·8°. Pulse 152. Respiration 38. 5 p.m.—Temp. 104°.—Pulse 144. Respiration 42. **17.1.99.**—7 a.m.—Temp. 101·6°. Pulse 132. Respiration 36. On **16.1.99.** patient suddenly became much worse the breathing becoming laboured, signs of pneumonia rapidly developed, and she died on the morning of **17.1.99.**

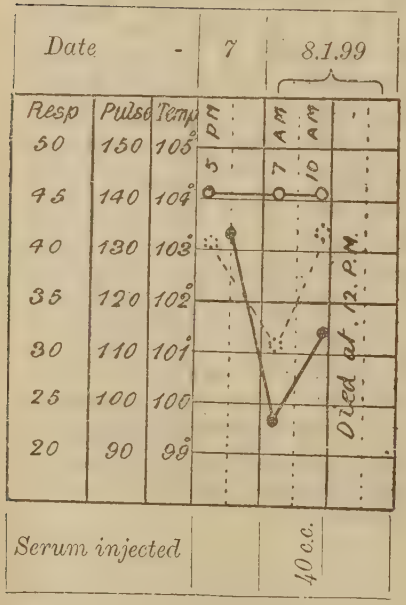
7.1.99.—One case admitted, ? acute rheumatism.

CASE No. XL. (Admitted on 4th day of the disease.)



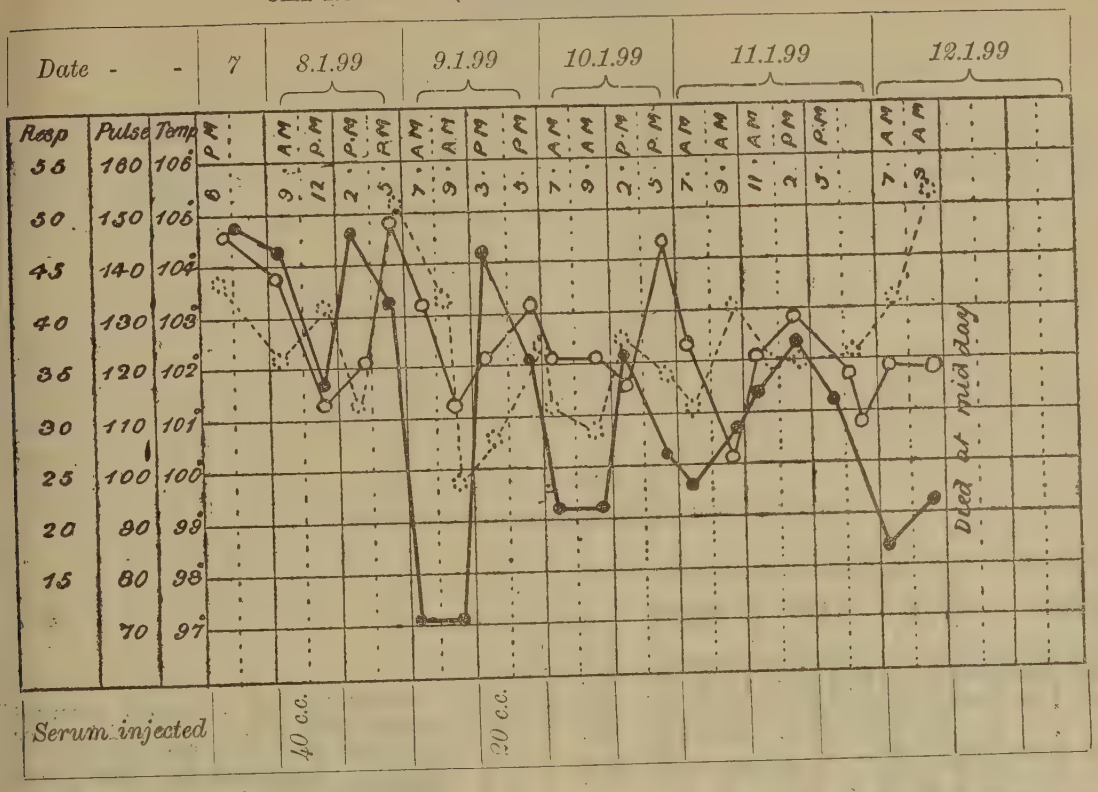
Venketamah. Age 20. Female. Admitted **7.1.99.**—11.30 a.m. Three days fever, two days bubo. 3.0 p.m.—General condition fair. Heart and lungs clear. Bubo in left axilla. Temperature 101·6°. Pulse 124. Respiration 28. 40 *c.c. inoculated.* 5.0 p.m.—Temp. 104·4°. Pulse 132. Respiration 36. **8.1.99.**—7.0 a.m.—Temp. 101°. Pulse 120. Respiration 32. 9.0 a.m.—Temp. 100·2°. Pulse 100. Respiration 28. Very much better. 5.0 p.m.—Temp. 99·2°. Pulse 98. Respiration 24. **9.1.99.**—7.0 a.m.—Temp. 98°. Pulse 108. Respiration 26. Feels quite well. 5.0 p.m.—Temp. 99·2°. Pulse 98. Respiration 24. **10.1.99.**—7.0 a.m.—Temp. 97°. Pulse 98. Respiration 24.

CASE No. XLI. (Admitted on 4th day of the disease.)



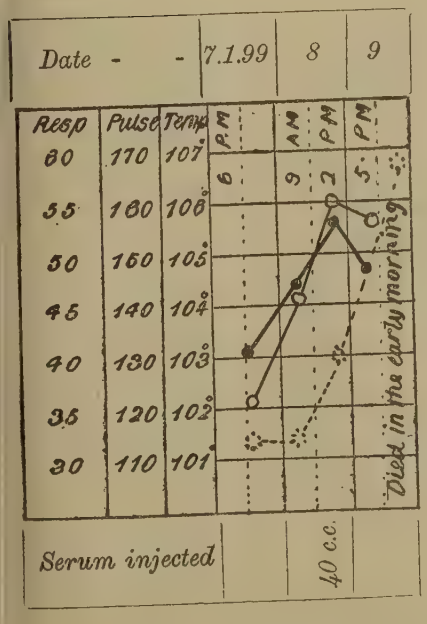
Annamaly. Age 50. Male. Admitted **7.1.99.**—5.0 p.m. Fever for three or four days. Temp. 103·4°. Pulse 140. Respiration 40. **8.1.99.**—7.0 a.m.—Temp. 99·8°. Pulse 140. Respiration 30. 10.0 a.m.—Very delirious. General condition very bad, moribund. Bubo in . Pulse very bad, 140. Respiration laboured, 44. Resonance over both lungs poor. Prolonged expiration heard over both lungs. Temp. 101·4°. 40 *c.c. inoculated.*

CASE No. XLII. (Admitted on 5th day of the disease.)



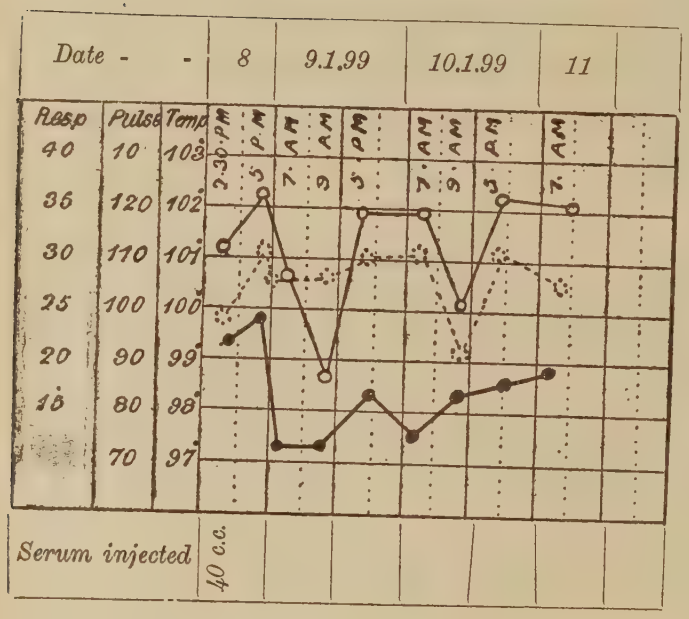
Pragasum. Age 35. Female. Admitted **7.1.99.**—6 p.m. Fever and bubo, four days. Temperature 104·8°. Pulse 148. Respiration 44. **8.1.99.**—9 a.m.—General condition indifferent. Heart and lungs normal. Bubo in right groin. Pregnant six months. Temp. 104·2°. Pulse 138. Respiration 36. 40 *c.c. serum.* 12 p.m.—Temp. 101·6°. Pulse 112. Respiration 42. 2 p.m.—Temp. 104·6°. Pulse 120. Respiration 32. 5 p.m.—Temp. 103·4°. Pulse 146. Respiration 50. **9.1.99.**—7 a.m.—Temp. 97°. Pulse 132. Respiration 42. 9 a.m.—Temp. 97. Pulse 112. Respiration 24. 3 p.m.—Temp. 104·2°. Pulse 120. Respiration 28. 20 *c.c. serum*, 5 p.m. Temp. 102°. Pulse 132. Respiration 36. **10.1.99.**—7 a.m.—Temp. 99·2°. Pulse 120. Respiration 30. 9 a.m.—Temp. 99·2°. Pulse 120. Respiration 28. Aborted last night. Complains of much pain in the bubo. 2 p.m.—Temp. 102·2°. Pulse 116. Respiration 36. 5 p.m.—Temp. 100·4°. Pulse 144. Respiration 34. **11.1.99.**—7 a.m.—Temp. 99·8°. Pulse 122. Respiration 30. 9 a.m.—Temp. 100·6°. Pulse 100. Respiration 40. 11 a.m.—Temp. 101·2°. Pulse 120. Respiration 34. 2 p.m.—Temp. 102·4°. Pulse 128. Respiration 36. Condition not so good. 5 p.m.—Temp. 100·2°. Pulse 108. Respiration 36. **12.1.99.**—7 a.m.—Temp. 98·4°. Pulse 120. Respiration 42. 9 a.m.—Temp. 99·4°. Pulse 120. Respiration 52. Condition bad, breathing very rapid, laboured; nothing discovered in lungs.

CASE No. XLIII. (Admitted on 2nd day of the disease.)



Arokin. Age 20. Female. Admitted **7.1.99.**—6 p.m. Taken ill yesterday. No bubo noticed. Temperature 103°. Pulse 120. Respiration 32. **8.1.99.**—9 a.m.—General condition indifferent. Heart and lungs normal. Tongue furred white, papillae very prominent. No spleen could be felt. Temp. 104·6°. Pulse 140. Respiration 32. 2.30 p.m.—Temp. 105·6°. Pulse 160. Respiration 40. General condition worse. Pulse small. Tenderness and slight enlargement of right cervical glands. 40 *c.c. of serum.* 5 p.m.—Temp. 104·8°. Pulse 156. Respiration 58.

CASE No. XLIV. (Admitted on 3rd day of the disease.)



Sodambah. Age . Female. Admitted **8.1.99.**—10.30 a.m. Fever and bubo for three days. 2.30 p.m.—Temperature 99·4°. Pulse 112. Respiration 24. General condition good. Bubo in right axilla. Heart and lungs normal. 40 *c.c. of serum.* 5 p.m. —Temp. 99·8°. Pulse 122. Respiration 32. **9.1.99.**—7 a.m.—Temp. 97·4°. Pulse 108. Respiration 28. 9 a.m.—Temp. 97·4°. Pulse 88. Respiration 28. Feels quite well. 5 p.m.—Temp. 98·4°. Pulse 120. Respiration 30. **10.1.99.**—7 a.m.—Temp. 97·6°. Pulse 120. Respiration 30. 9 a.m.—Temp. 98·4°. Pulse 100. Respiration 20. 5 p.m. —Temp. 98·6°. Pulse 124. Respiration 32. **11.1.99.**—7 a.m.—Temp. 98·8. Pulse 120. Respiration 28.

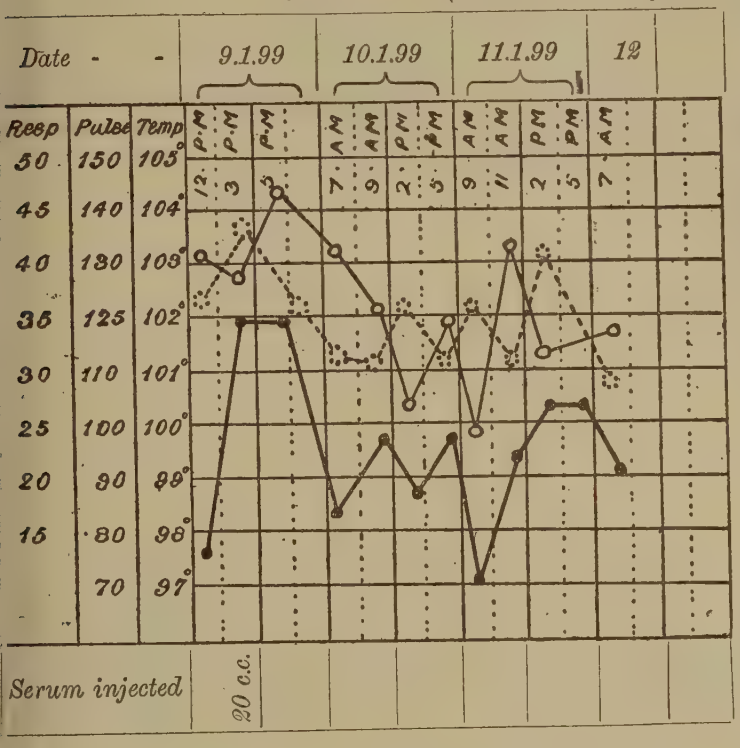
CASES OF PLAGUE

TREATED WITH

SERUM ANTI-PESTEUX FROM PASTEUR INSTITUTE. (HORSE No. 31.)

9.1.99.—One child, about three, admitted whose temperature fell to normal soon after admission, and who was, therefore, not inoculated.
One woman admitted who died almost directly after admission, and was not seen by me.

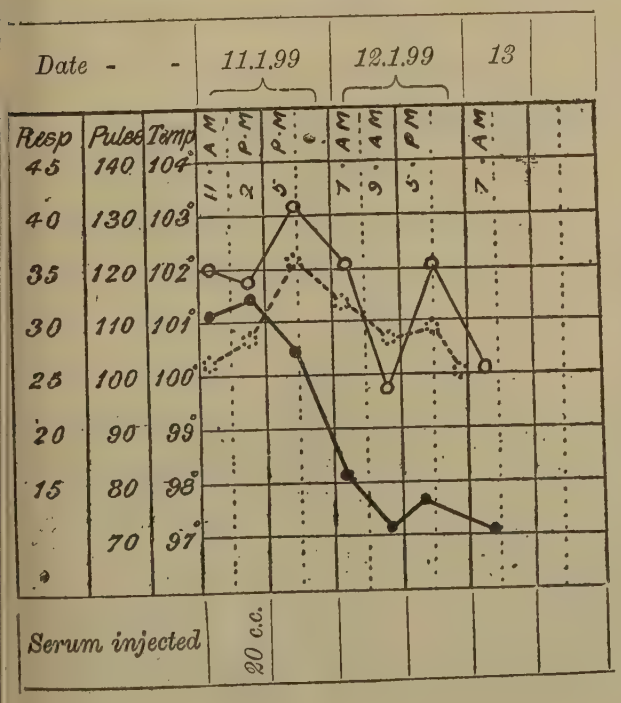
CASE No. I. A. (Admitted on 3rd day of the disease.)



Venkoba. Age 3. Male. Admitted **9.1.99.**—12.0 p.m. Fever for two days. Temp. 97·8°. Pulse 132. Respiration 36. 3.0 p.m.—General condition indifferent. Dull and apathetic. Bubo in right axilla. Temp. 102°. Pulse 128. Respiration 44. Heart and lungs clear (double cataract). 20 *c.c. serum inoculated.* 5.0 p.m.—Temp. 102°. Pulse 144. Respiration 36. **10.1.99.**—7.0 a.m.—Temp. 98·4°. Pulse 132. Respiration 32. 9.0 a.m.—Temp. 99·6°. Pulse 120. Respiration 30. Sleepy, but much better. 2.0 p.m.—Temp. 98·8°. Pulse 104. Respiration 36. Condition much improved. 5.0 p.m. Temp. 99·6°. Pulse 120. Respiration 30. **11.1.99.**—9.0 a.m.—Temp. 97.° Pulse 98. Respiration 36. 11.0 a.m.—Temp. 99·2°. Pulse 132. Respiration 30. 2.0 p.m.—Temp. 100·4°. Pulse 112. Respiration 40. Condition good. 5.0 p.m.—Temp. 100·4°. **12.1.99.**—7.0 a.m.—Temp. 99°. Pulse 116. Respiration 28. Bubo suppurated.

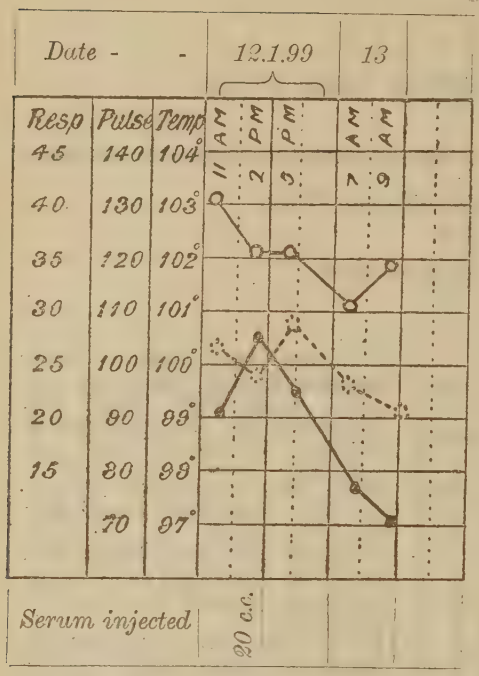
11.1.99.—Two cases admitted, but died before they were seen.

CASE No. II. A. (Admitted on 5th day of the disease.)



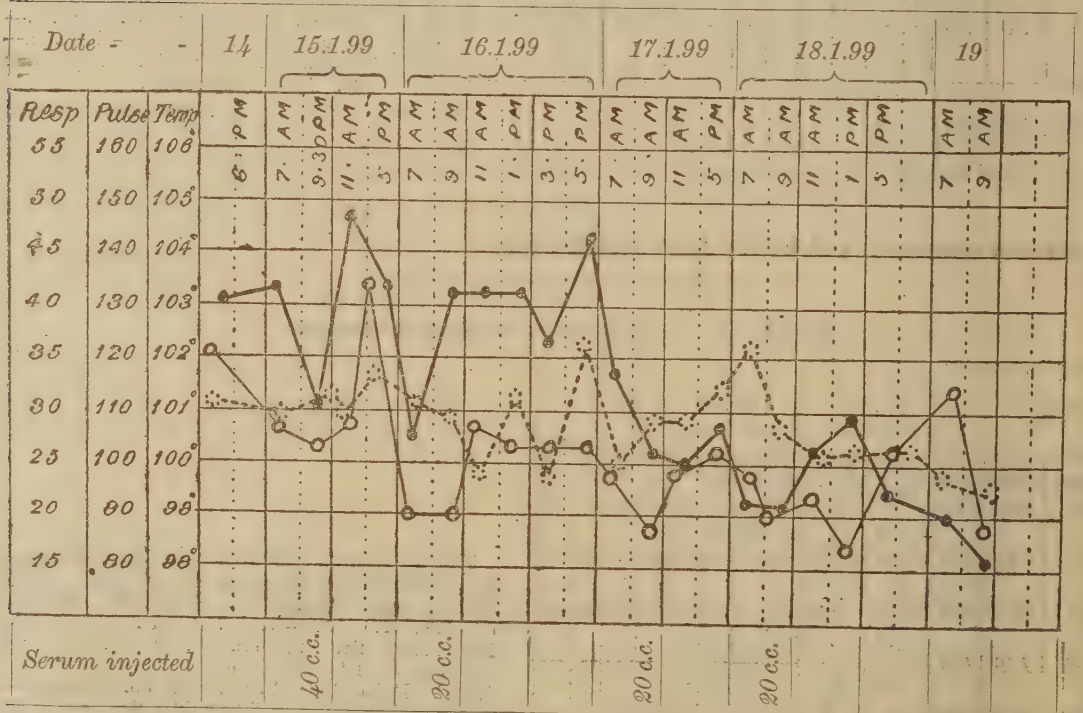
Thaiaro. Age 13. Female. Admitted **11.1.99.**—11.0 a.m.—Pain in the neck for six days. Fever for four days. Temp. 101°. Pulse 120. Respiration 26. 2.0 p.m.—Temp. 101·4°. Pulse 116. Respiration 28. 20 *c.c. serum.* Very small swelling on left side of neck. 5.0 p.m.—Temp. 100·4°. Pulse 132. Respiration 36. **12.1.99.**—7.0 a.m.—Temp. 98°. Pulse 120. Respiration 32. 9.0 a.m.—Temp. 97·2°. Pulse 96. Respiration 28. Practically well. 5.0 p.m.—Temp. 97·8°. Pulse 120. Respiration 30. **13.1.99.**—7.0 a.m.—Temp. 97°. Pulse 100. Respiration 26.

CASE No. III. A. (Admitted on 4th day of the disease.)



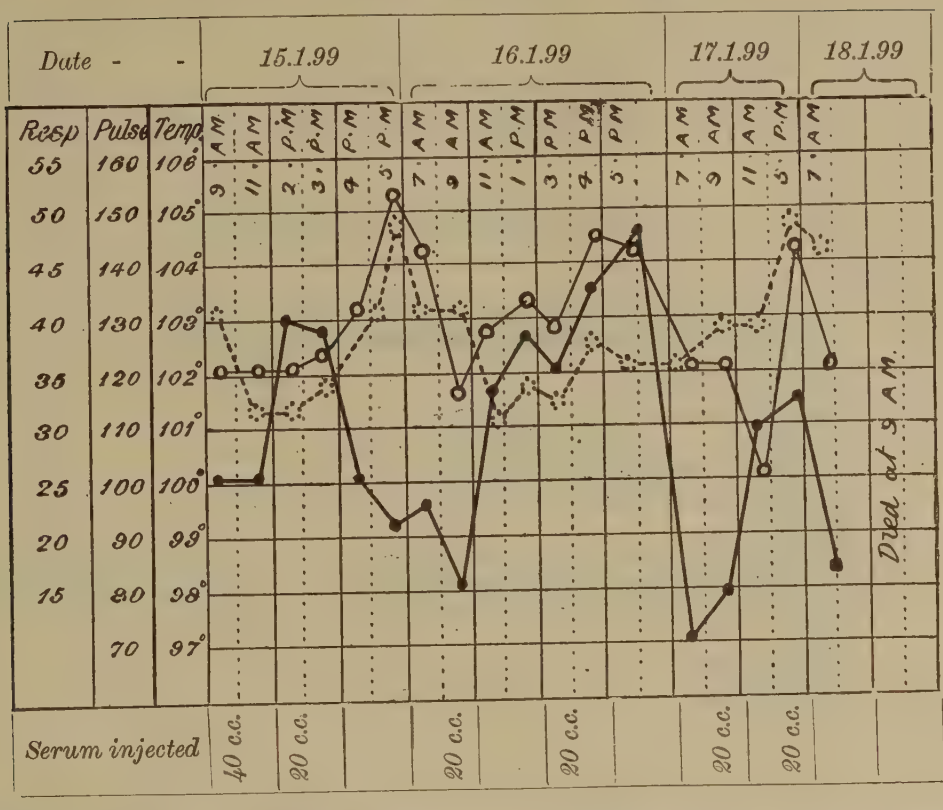
Mary. Age 2½. Female. Admitted **12.1.99.**—11 a.m. Bubo and fever three days. Temperature 99°. Pulse 130. Respiration 26. 2 p.m.—General condition good. Bubo in left groin. Heart and lungs normal. Temperature 100·6°. Pulse 120. Respiration 24. 20 *c.c. serum.* 5 p.m.—Temp. 99·6°. Pulse 120. Respiration 28. **13.1.99.**—7 a.m.—Temp. 97·8°. Pulse 110. Respiration 24. 9 a.m.—Temp. 97°. Pulse 120. Respiration 20. Practically well.

CASE No. IV. A. (Admitted on 4th day of the disease.)



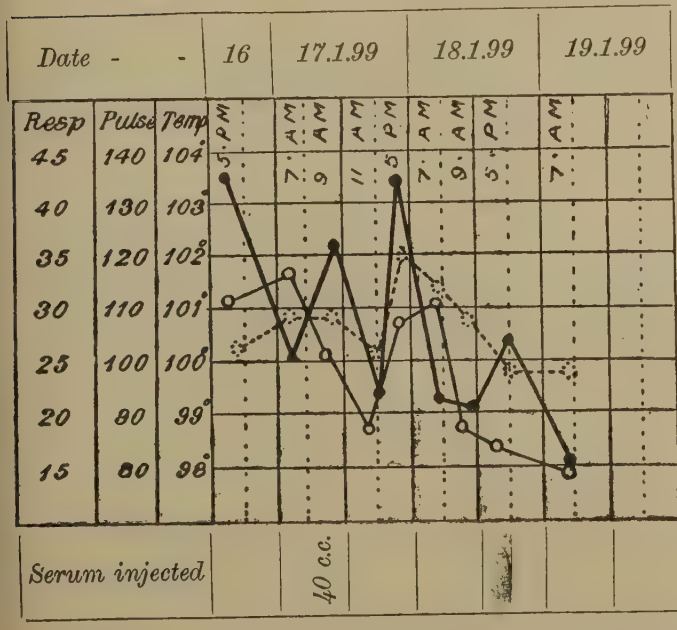
Pir. Khan. Age 35. Male. Admitted **14.1.99.**—6 p.m.—Bubo and fever for three days. Temperature 103°. Pulse 120. Respiration 30. **15.1.99.**—7 a.m.—Temp. 103·4°. Pulse 108. Respiration 28. 9.30 a.m.—Temp. 101°. Pulse 104. Respiration 32. General condition fair. Bubo in left groin. Heart and lungs natural. 40 *c.c. serum.* 11 a.m.—Temp. 104·8°. Pulse 108. Respiration 30. 5 p.m.—Temp. 103·4°. Pulse 136. Respiration 34. **16.1.99.**—7 a.m.—Temp. 100·4°. Pulse 90. Respiration 28. 9 a.m.—Temp. 103·2°. Pulse 90. Respiration 30. Pulse good. Sensible. 20 *c.c. serum.* 11 a.m.—Temp. 103·2°. Pulse 108. Respiration 24. 1 p.m.—Temp. 103·2°. Pulse 104. Respiration 32. Complains of pain in left epitrochlear region. 3 p.m.—Temp. 102·4°. Pulse 104. Respiration 24. 5 p.m.—Temp. 104·4°. Pulse 104. Respiration 36. **17.1.99.**—7 a.m.—Temp. 101·8°. Pulse 98. Respiration 24. 9 a.m.—Temp. 100·4°. Pulse 88. Respiration 28. 20 *c.c. serum.* Slightly delirious. 11 a.m.—Temp. 100. Pulse 100. Respiration 28. 5 p.m.—Temp. 100·8°. Pulse 104. Respiration 32. **18.1.99.**—7 a.m.—Temp. 99·4. Pulse 98. Respiration 36. 9 a.m.—Temp. 99·2°. Pulse 92. Respiration 28. Talkative; rather incoherent. 20 *c.c. serum.* 11 a.m.—Temp. 100·4°. Pulse 96. Respiration 26. 1 p.m.—Temp. 101°. Pulse 84. Respiration 26. 5 p.m.—Temp. 99·6°. Pulse 104. Respiration 26. **19.1.99.**—7 a.m.—Temp. 99°. Pulse 116. Respiration 24. 9. a.m.—Temp. 98·4°. Pulse 84. Respiration 22. Much better. Tongue cleaning. Bubo painful. Epitrochlear tenderness disappeared. Bubo in left groin looked as if would suppurate.

CASE No. V. A. (Admitted on 4th day of the disease.)



Muragasum. Age 21. Male. Admitted **15.1.99.**—9 a.m. Ill for three or four days. General condition very bad Delirious, very restless, constantly picking at the bed-clothes. Eyes very injected. Temperature 100°. Pulse 120. Respiration 40. Bubo on left side of neck. 40 *c.c. serum.* 11 a.m.—Temp. 100°. Pulse 120. Respiration 32. 2 p.m.—Temp. 103°. Pulse 120. Respiration 32. 20 *c.c. serum* intravenous. 3 p.m.—Temp. 102·8°. Pulse 124. Respiration 34. 4 p.m.—Temp. 100°. Pulse 132. Respiration 40. 5 p.m.—Temp. 99·4°. Pulse 156. Respiration 48. **16.1.99.**—7 a.m.—Temp. 99·8°. Pulse 144. Respiration 40. 9 a.m.—Temp. 98°. Pulse 108. Respiration 40. General condition unchanged, still the same delirium, never still, always talking. 20 *c.c. serum* sub-cute. 11 a.m.—Temp. 101·8°. Pulse 128. Respiration 30. 1 p.m.—Temp. 102·6°. Pulse 132. Respiration 34. 3 p.m.—Temp. 102°. Pulse 128. Respiration 32. 20 *c.c. serum* intravenous. 4 p.m.—Temp. 103·4°. Pulse 144. Respiration 38. 5 p.m.—Temp. 104·4°. Pulse 144 Respiration 36. **17.1.99.**—7 a.m.—Temp. 97°. Pulse 120. Respiration 36. 9 a.m.—Temp. 98°. Pulse 120. Respiration 40. General condition, no better. 20 *c.c. serum.* 11 a.m.—Temp. 101°. Pulse 100. Respiration 40. 2 p.m.—Quieter evidently from exhaustion. Muttering. 20 *c.c.* sub-cute. Cultivation from blood made. No plague bacilli. 5 p.m.—Temp. 101·4°. Pulse 144. Respiration 48. **18.1.99.**—7 a.m.—Temp. 98·4°. Pulse 120. Respiration 46.

CASE No. VI. A. (Admitted on 5th day of the disease.)



Mooniappab. Age 50. Male. Admitted **16.1.99.**—5 p.m. Ill four days. Bubo four days. 5 p.m.—Temperature 103·6°. Pulse 110. Respiration 26. **17.1.99.**—7 a.m.—Temp. 100°. Pulse 118. Respiration 28. 9 a.m.—Temp. 102·2°. Pulse 100. Respiration 28. General condition good. Heart and lungs normal. Bubo in left groin. 40 *c.c. serum.* 11 a.m.—Temp. 99·4°. Pulse 88. Respiration 24. 5 p.m.—Temp. 103·4°. Pulse 108. Respiration 36. **18.1.99.**—7 a.m.—Temp. 99·4°. Pulse 110. Respiration 30. 9 a.m.—Temp. 99°. Pulse 88. Respiration 28. Much better. 5 p.m.—Temp. 100·4°. Pulse 84. Respiration 24. **19.1.99.**—7 a.m.—Temp. 98°. Pulse 80. Respiration 24.

ABSTRACT OF STATISTICS AT BANGALORE.

Cases admitted into South Camp Hospital before December 25th, 686.
Deaths, 403.
Percentage of deaths, 58·7.

Total cases admitted from 25.12.98 to 12.1.99, 73.
Deaths, 35.
Percentage of deaths, 47·9.

Total bubonic cases during same period, 39.
Deaths, 20.
Percentage, 51·3.

Total non-bubonic cases during same period, 34.
Deaths, 15.
Percentage of deaths, 44·1.

Total cases treated with Roux serum from 25.12.98 to 16.1.99, 49.
Deaths, 31.
Percentage, 63·26.

Total cases admitted into North Camp from September 15th to November 19th, 365.
Deaths, 275.
Percentage of deaths, 75·13.

Total cases from December 25th to January 12th, 54 cases.
Total deaths, 29.
Percentage of deaths, 53·7.

Total bubonic cases, 14.
Deaths among bubonic cases, 10.
Percentage, 71·42.

S. R. Douglas, Lt., I.M.S.

APPENDIX No. LXXX.

NOTES

OF

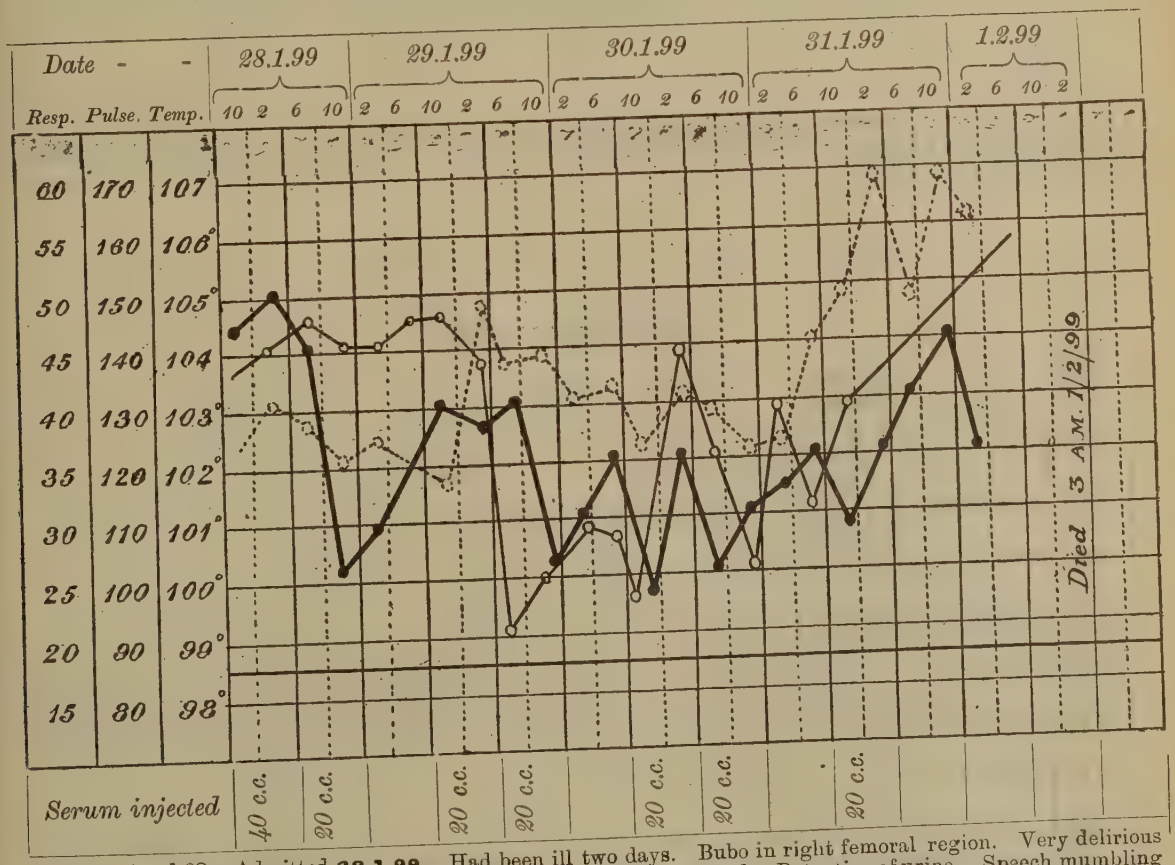
CASES TREATED WITH ROUX' SERUM AT MODI KHANA HOSPITAL, BOMBAY, WITH AN EQUAL NUMBER OF CONTROLS,

BY

LIEUT. S. R. DOUGLAS, I.M.S.
On Special Duty with the Indian Plague Commission.

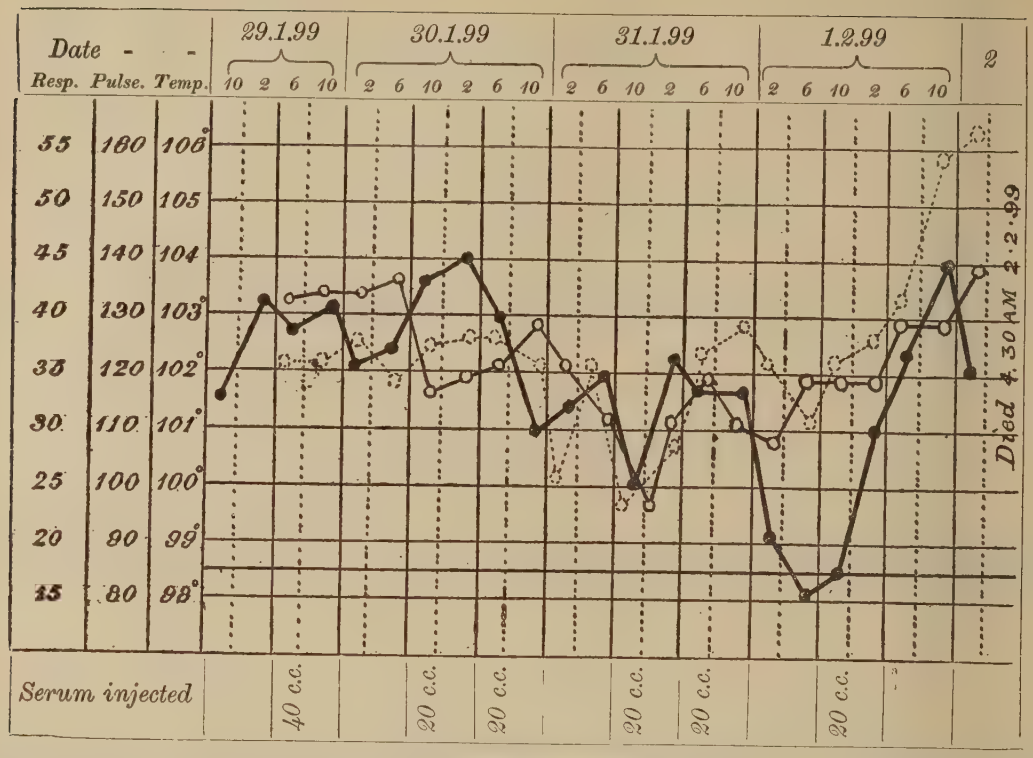
NOTE.—The numbers in brackets are the numbers of the cases in the Register at the Modi Khana Hospital. Observations in cases treated with Roux' serum are made every 4 hours, at 2, 6, and 10 a.m., and at 2, 6, and 10 p.m. The thick black line indicates the temperature, the thin black line the pulse, and the dotted line the respiration.

CASE No. I. (No. 1654.)



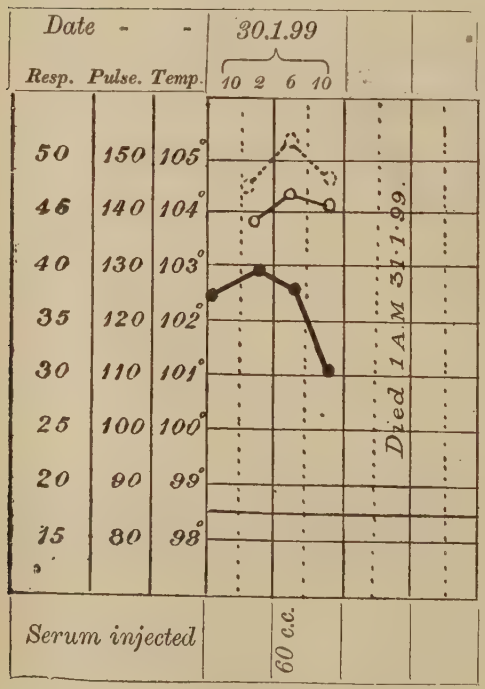
Male. Aged 28. Admitted **28.1.99.** Had been ill two days. Bubo in right femoral region. Very delirious Much bruised about the face. Heart and lungs, no abnormalities found. Retention of urine. Speech mumbling indistinct. **29.1.99.**—Slightly better apparently, still delirious. Bubo has more infiltration round it. **30.1.99** —Has developed a corneal ulcer. **31.1.99.**—Quiet, seemingly exhausted. Bubo softer and has extended upwards.

CASE No. II. (No. 1669.)



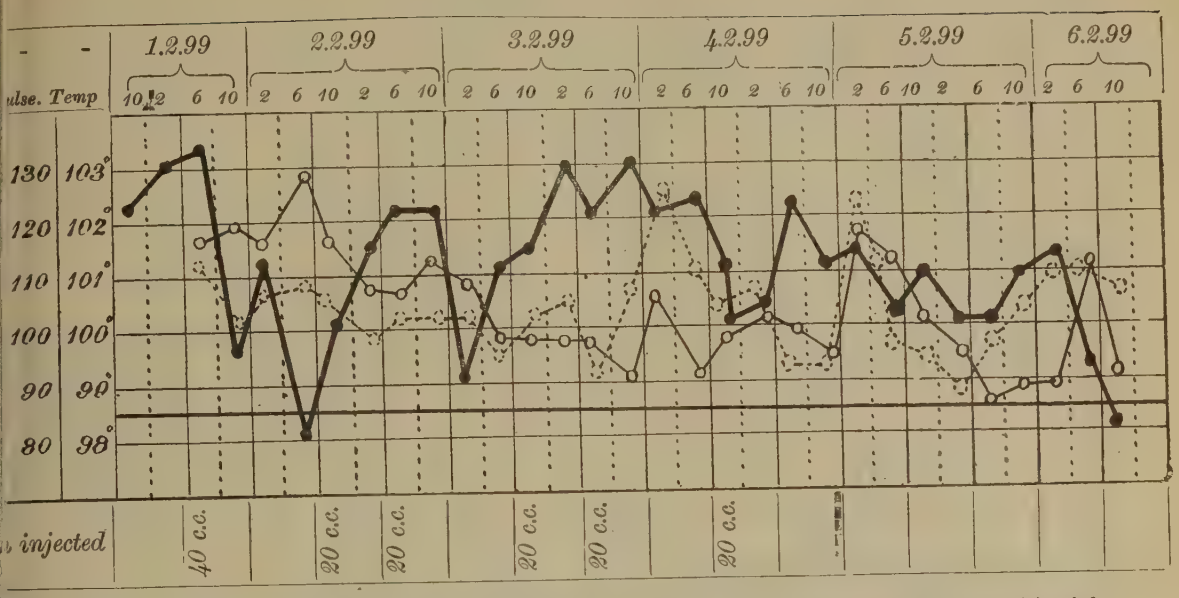
Male. Aged 35. Ill three days before. Bubo in left groin. Patient is conscious. Heart and lungs, nothing abnormal found. **30.1.99.**—Rather delirious. Speech very indistinct. **31.1.99.**—Slept well, not so delirious. **1.2.99.**—Worse, very delirious.

CASE No. III. (No. 1680.)



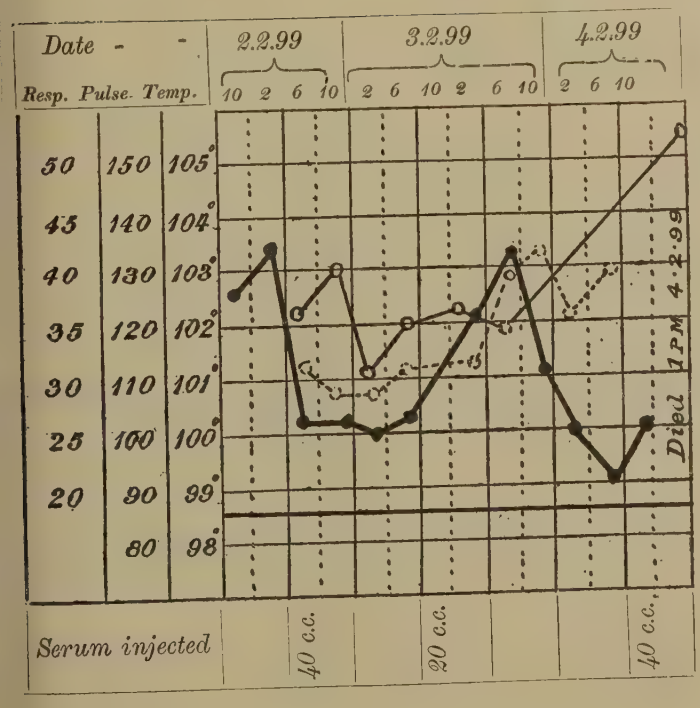
Male. Age 28. Admitted **30.1.99.** Ill for two days. Bubo in left groin. Unconscious. Quiet, but rather restless. Blood drawn off. (Culture made; many colonies of bacilli.)

CASE No. IV. (No. 1700.)



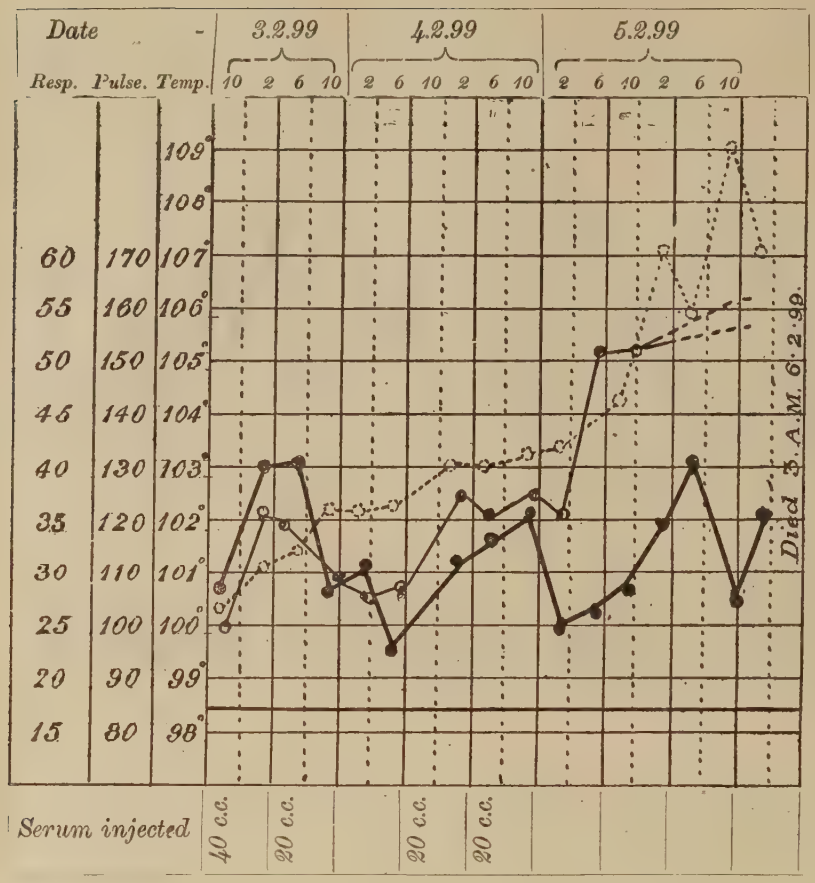
Male. Age 30. Admitted **1.2.99.** Ill for two days. Bubo in left groin. Has cough; râles in rhonchi in right ng. **2.2.99.**—Slept well. Pulse good. **3.2.99.**—Gland less painful. Pulse fair. **4.2.99.**—Eyes rather ected. Pulse good. **5.2.99.**—No serum given. Patient improving. Very weak. **6.2.99.**—Bubo incised. pus, but some necrosed gland removed. **17.3.99.**—Patient discharged.

CASE No. V. (No. 1712.)



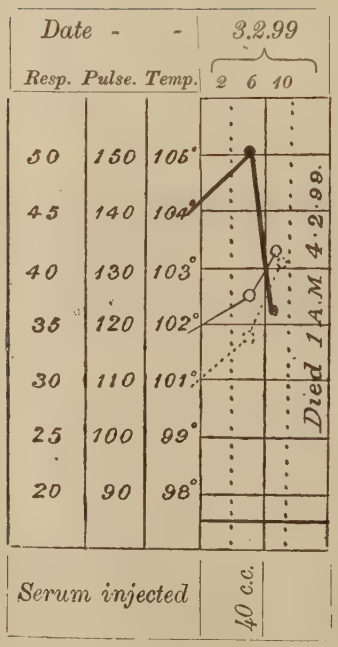
Male. Age 40. Admitted **2.2.99.** Ill for two days. Bubo in left inguinal region. Speech indistinct. Prolonged expiration and rhonchi over both lungs. **3.2.99.**—No sleep. Gurgling respiration. Eyes injected. **4.2.99.**—Limbs cold. Pulse running.

CASE No. VI. (No. 1722.)



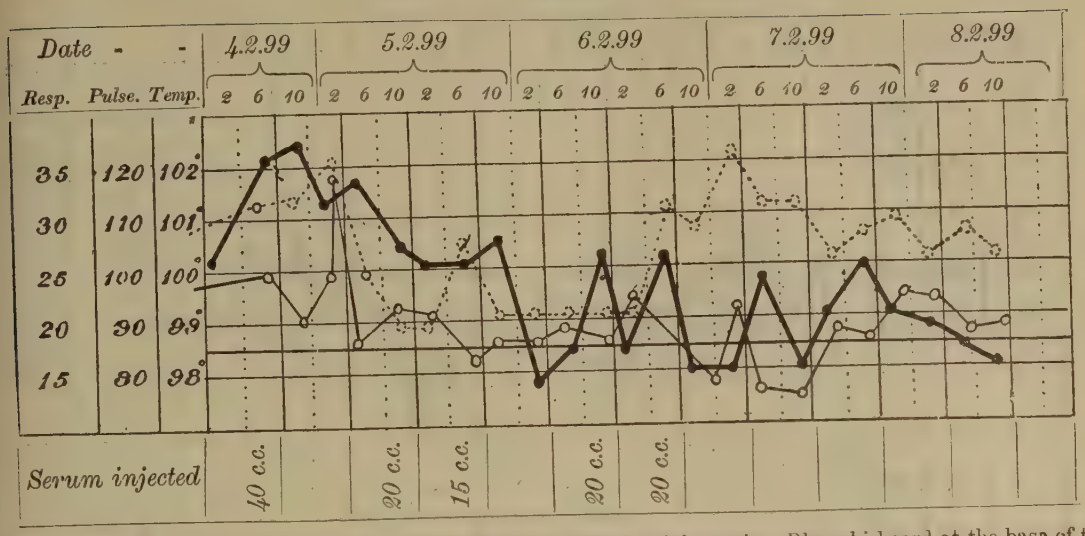
Male. Age 30. Admitted **3.2.99.** Ill for three days. Bubo in right submaxillary region. Very large. Eyes injected. Has difficulty in swallowing. **4.2.99.**—Muttering delirium. No sleep. Swelling round bubo has increased. **5.2.99.** — Respiration very rapid. Pulse running.

CASE No. VII. (No. 1730.)



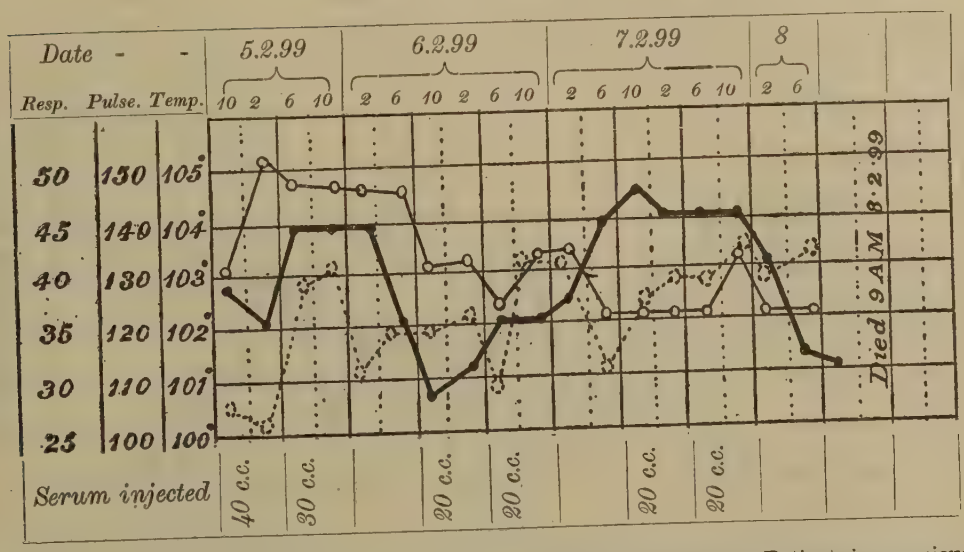
Male. Age 30. Admitted **3.2.99.** Ill for four days. Bubo in left femoral region. Lungs not affected. Speech very indistinct. Limbs cold.

CASE No. VIII. (No. 1743.)



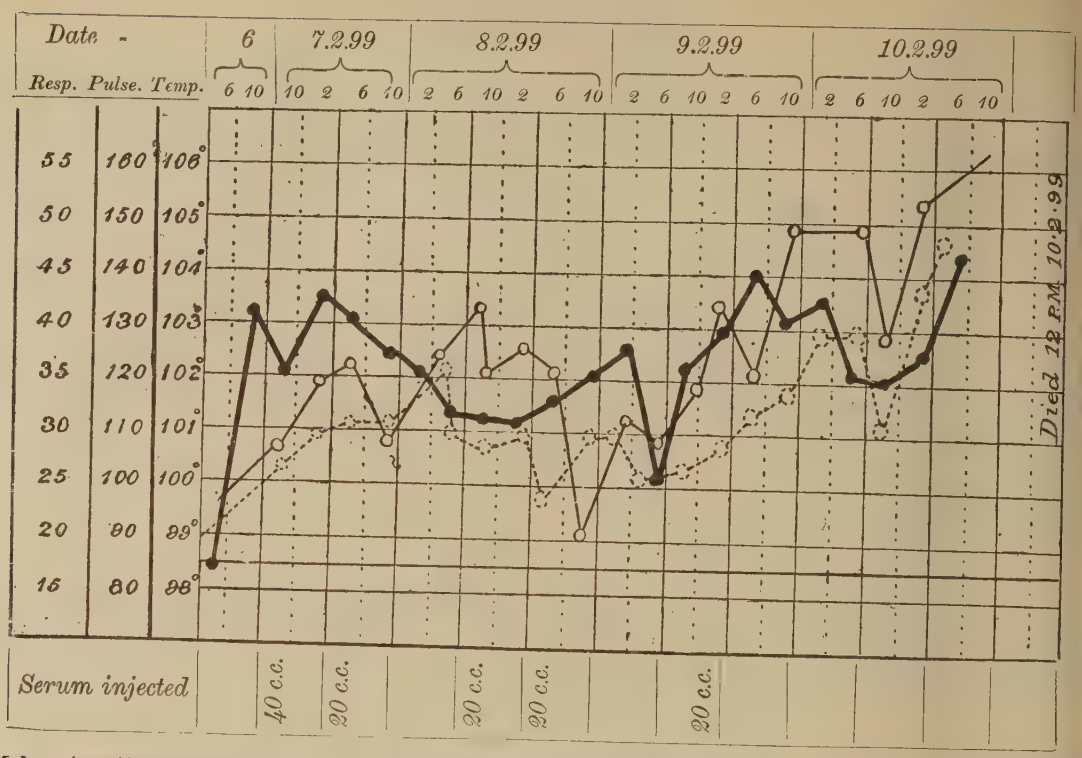
Male. Aged 25. Admitted **4.2.99.** Ill for four days. Bubo in right groin. Rhonchi heard at the base of the right lung. Speech indistinct. **5.2.99.**—Condition unchanged. Slept well. 6.2.99.—Gland still hard, slight improvement. **7.2.99.**—Serum discontinued. improvement continues. 10.2.99.—Bubo incised, some pus and broken down gland escaping. **15.3.99.**—Discharged.

CASE No. IX. (No. 1747.)



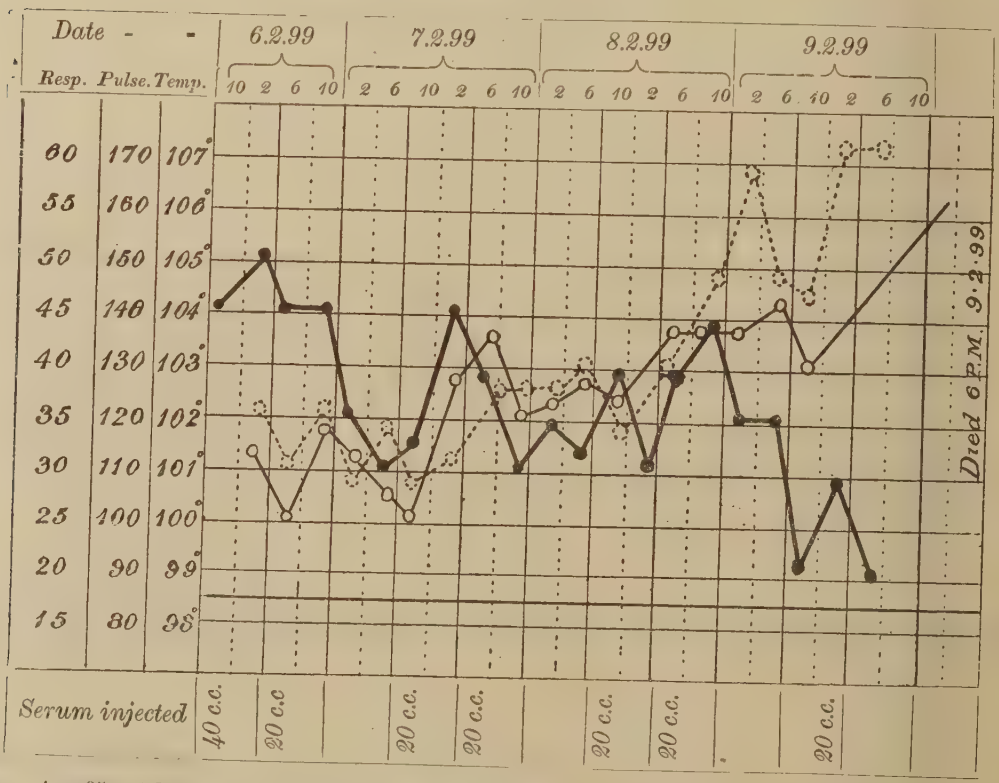
Male. Aged 24. Admitted **5.2.99.** Ill for four days. Bubo in right axilla. Patient is conscious. Heart and lungs clear. **7.2.99.**—Quiet all night. Eyes very injected. **8.2.99**—Respiration shallow. Extremities cold.

CASE No. X. (No. 1763.)



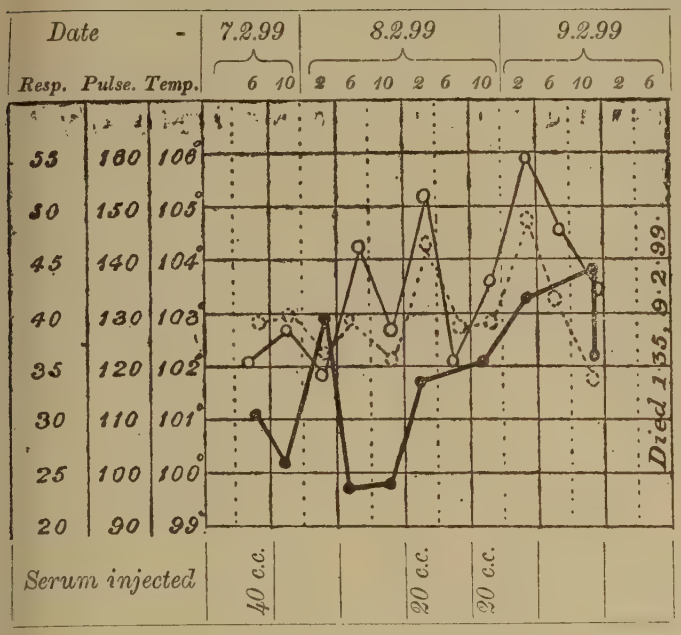
Male. Age 40. Admitted **6.2.99.** Ill for three days. Bubo in left axilla. **7.2.99.**—Râles and rhonchi heard over both lungs. Is very delirious. **8.2.99.**—Very delirious. Pulse very bad. **9.2.99.**—Condition unchanged. **10.2.99.**—Speech very indistinct. Pulse running. Extremities cold.

CASE No. XI. (No. 1766.)



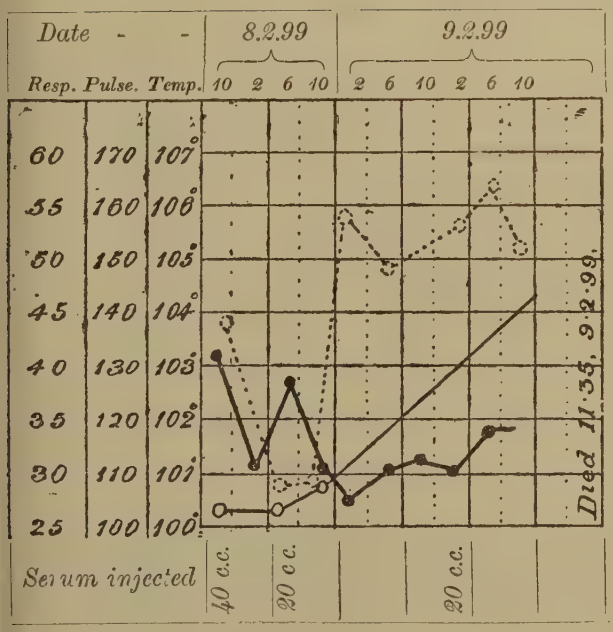
Male. Age 25. Admitted **6.2.99.** Ill for two days. Bubo in right femoral region. Conscious. Heart and lungs unaffected. **7.2.99.**—Slept well. Has retention of urine. **8.2.99.**—Has another gland in left groin, retention of urine continues. **9.2.99.**—Violently delirious. Respiration very rapid. Pulse running.

CASE No. XII. (No. 1783.)



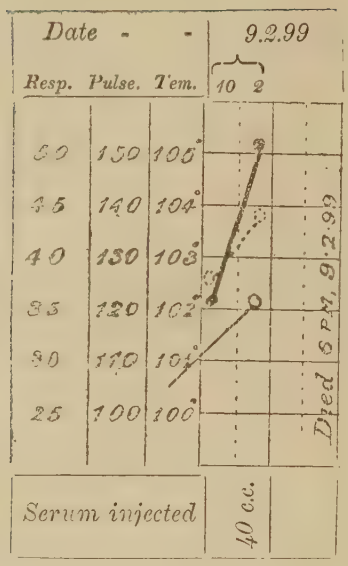
Male. Age 24. Admitted **7.2.99.** Ill for two days. Bubo in left axilla. Conscious. Sibilant rhonchi over both lungs. **8.2.99.**—Quiet, but general condition worse. Pulse rapid. **9.2.99.**—Very delirious.

CASE No. XIII. (No. 1788.)



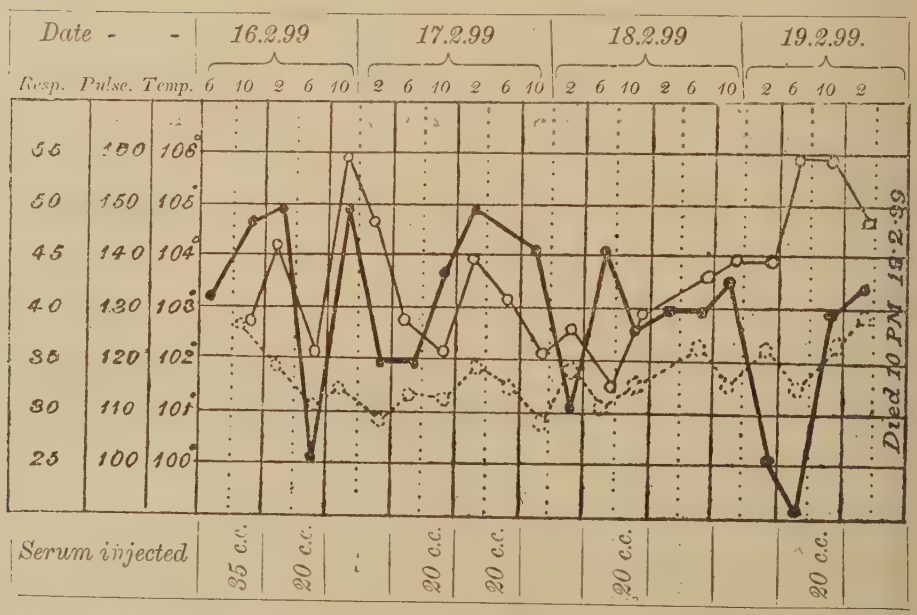
Male. Age 30. Admitted **8.2.99.** Ill for two days. Bubo in right groin. Lungs not affected. **9.2.99.**—Breathing has become very rapid. Pulse running.

CASE No. XIV. (No. 1805.)



Male. Aged 25. Admitted **9.2.99.** Ill for three days. Bubo in right groin. Bronchial breathing heard over the whole of the left lung.

CASE No. XV. (No. 1864.)

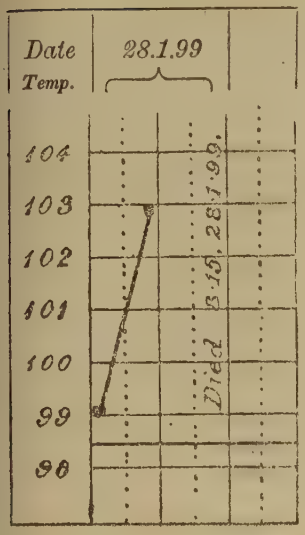


Male. Aged 25. Admitted **15.299.** Ill for two days. Bubo in right femoral region. Patient conscious. Lungs, prolonged expiration at both bases, with rhonchi at the end of expiration. **16.2.99.**—Rather restless. Pulse weak. **17.2.99.**—Condition unchanged, rather worse if anything. **18.2.99.**—Becoming weaker, pulse rapid, very soft. **19.2.99.**—Delirious during night. Now exhausted.

CONTROLS.

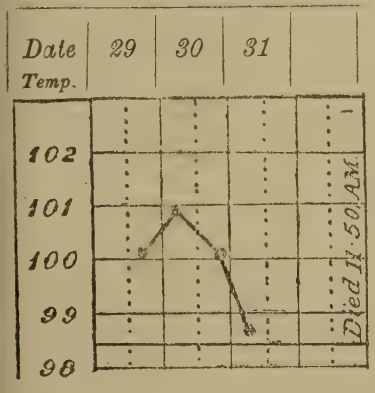
NOTE.—The charts of control cases show the temperature only, observed in the morning and evening each day.

CASE No. I. c. (No. 1652.)



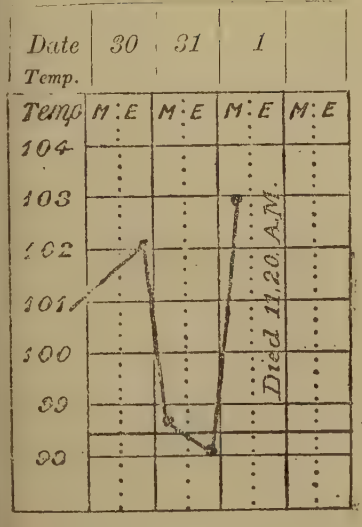
Male. Age 20. Admitted **28.1.99.** Ill two days. Bubo in right femoral region.

CASE No. II. c. (No. 1673.)



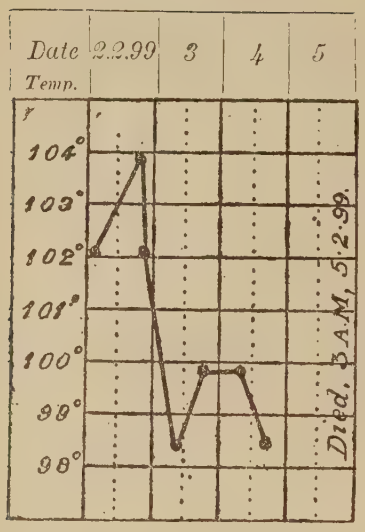
Male. Age 22. Admitted **29.1.99.** Ill three days. Bubo in right axilla.

CASE No. III. c. (No. 1685.)



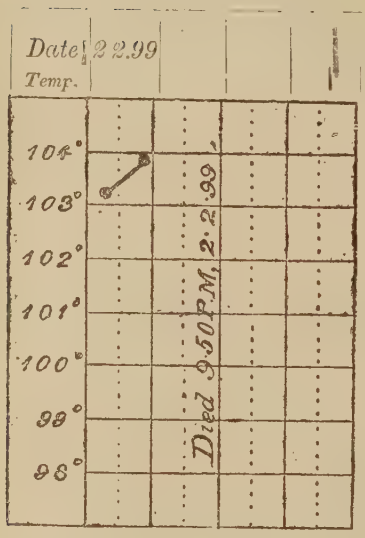
Male. Age 30. Admitted **30.1.99.** Ill two days. Bubo in left popliteal region.

CASE No. IV. c. (No. 1707)



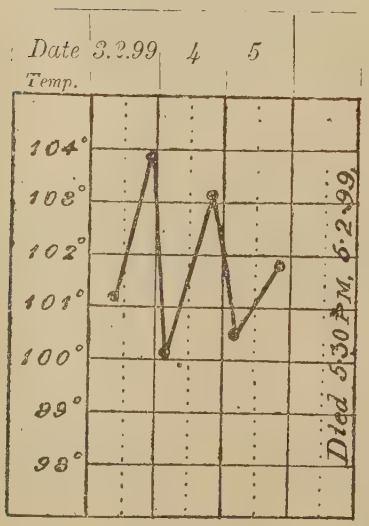
Male. Aged 40. Admission **1.2.99.** Ill six days? Bubo in right groin.

CASE No. V. c. (No. 1714.)



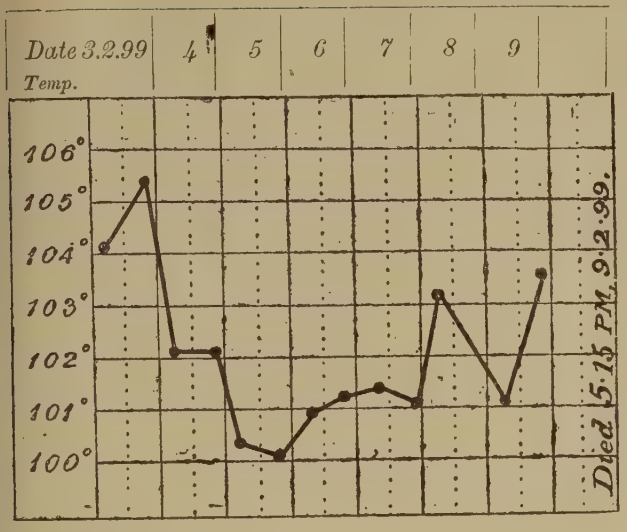
Male. Aged 35. Admitted **2.2.99.** Ill three days. Bubo right femoral region.

CASE No. VI. c. (No. 1725.)



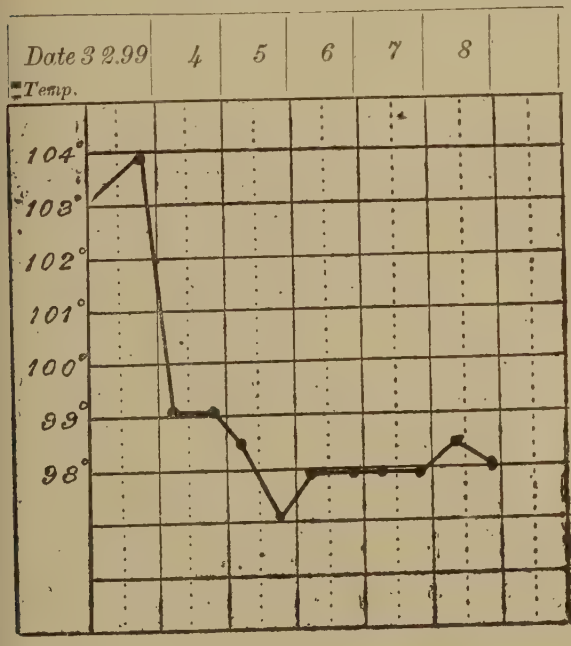
Male. Aged 50. Admitted **3.2.99.** Ill three days. Bubo in right femoral region.

CASE No. VII. c. (No. 1729.)



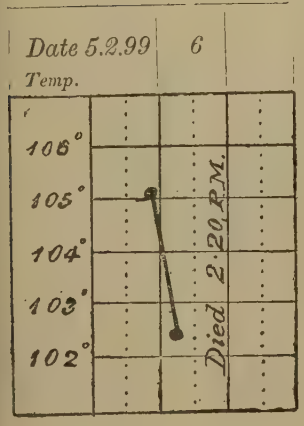
Male. Age 20. Admitted **3 2.99.** Ill for four days. Bubo left groin.

CASE No. VIII. c. (No. 1731.)



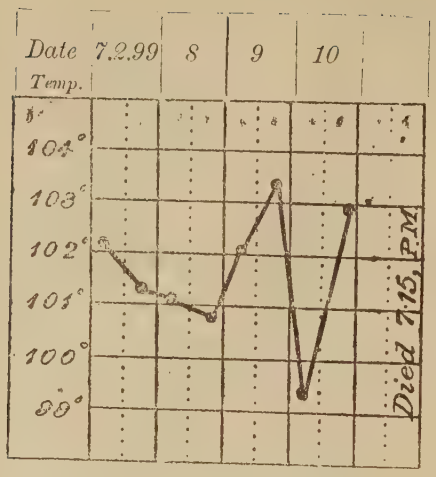
Male. Age 19. Admitted **3.2 99.** Ill for seven days. Bubo in right groin. Discharged cured **13.2.99.** Bubo did not suppurate.

CASE No. IX. c. (No. 1753.)



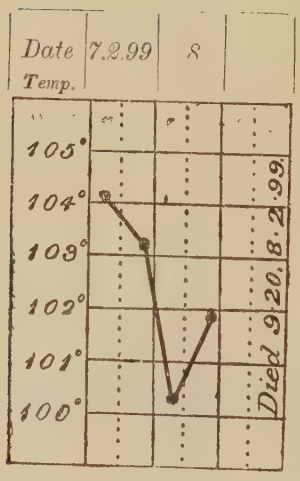
Male. Age 35. Admitted **5.2.99.** Ill for three days. Bubo in right groin.

CASE No. X. c. (No. 1774.)



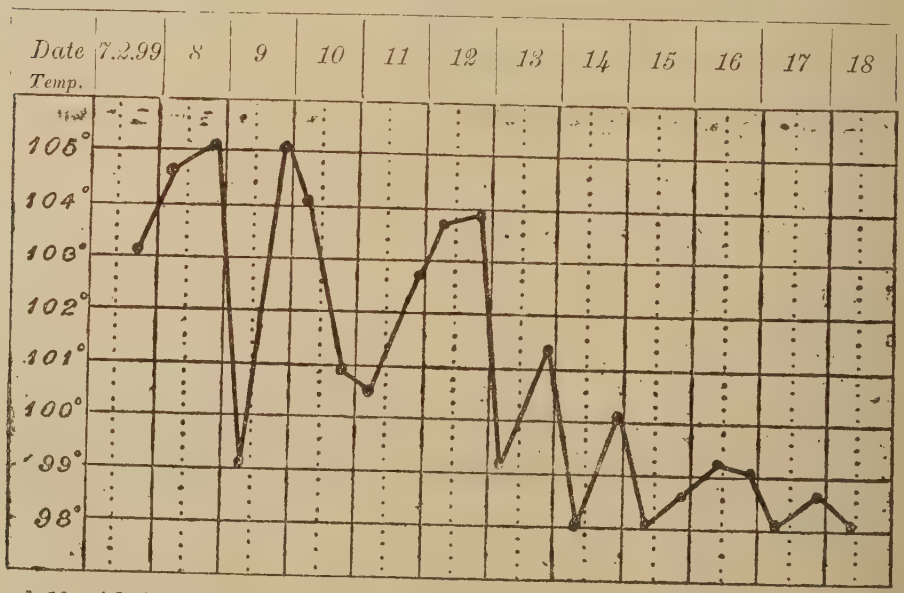
Male. Aged 18. Admitted **7.2.99.** Ill for two days. Bubo in right groin.

CASE No. XI. c. (No. 1777.)



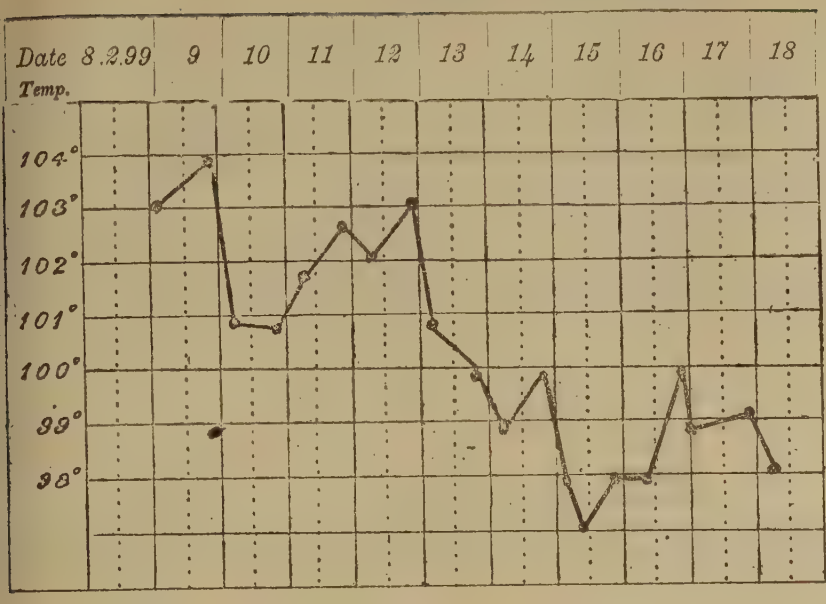
Male. Aged 25. Admitted **7.2.99.** Ill for two days. Bubo in right femoral region.

CASE No. XII. c. (No. 1782.)



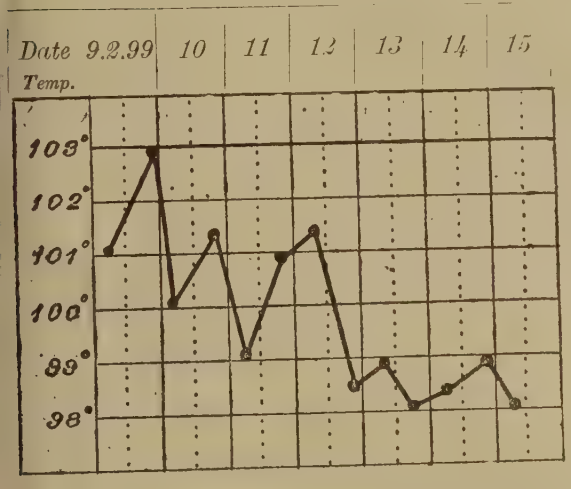
Male. Aged 10. Admitted **7.2.99.** Ill for two days. Bubo left side of neck. Bubo opened **4.3.99.** Developed many (5 or 6) pyæmic abscesses, from which were grown bacilli resembling coli. Discharged well, **23.3.99.**

CASE No. XIII. c. (No. 1901.)



Male. Age 25. Admitted **8.2.99.** Ill three days. Bubo in right femoral region. Bubo opened **16.2.99.** Discharged **23.3.99.**

CASE No. XIV. c. (No. 1807.)



Male. Age 50. Admitted **9.2.99.** Ill for three days. Bubo in left axilla. Bubo opened **24.2.99.** Discharged **17.3.99.**

CASE No. XV. c. (No. ——.)

Notes lost.

Patient died.

Results :—

15 cases treated with serum.
2 recoveries.

15 cases treated without serum.
4 recoveries.

Up to February 7th, 1899, horse 31 serum was used, after that date Horse No. 21.

S. R. DOUGLAS, Lt., I.M.S.

APPENDIX No. LXXXI.

RECORD OF
PLAGUE PATIENTS AT THE MODI KHANA HOSPITAL, BOMBAY,
TREATED WITH M. ROUX' SERUM,
TOGETHER WITH RECORD OF CONTROL CASES,

by

H. J. WALTON, M.B., F.R.C.S., LIEUT. I.M.S.,
On Special Duty with the Indian Plague Commission.

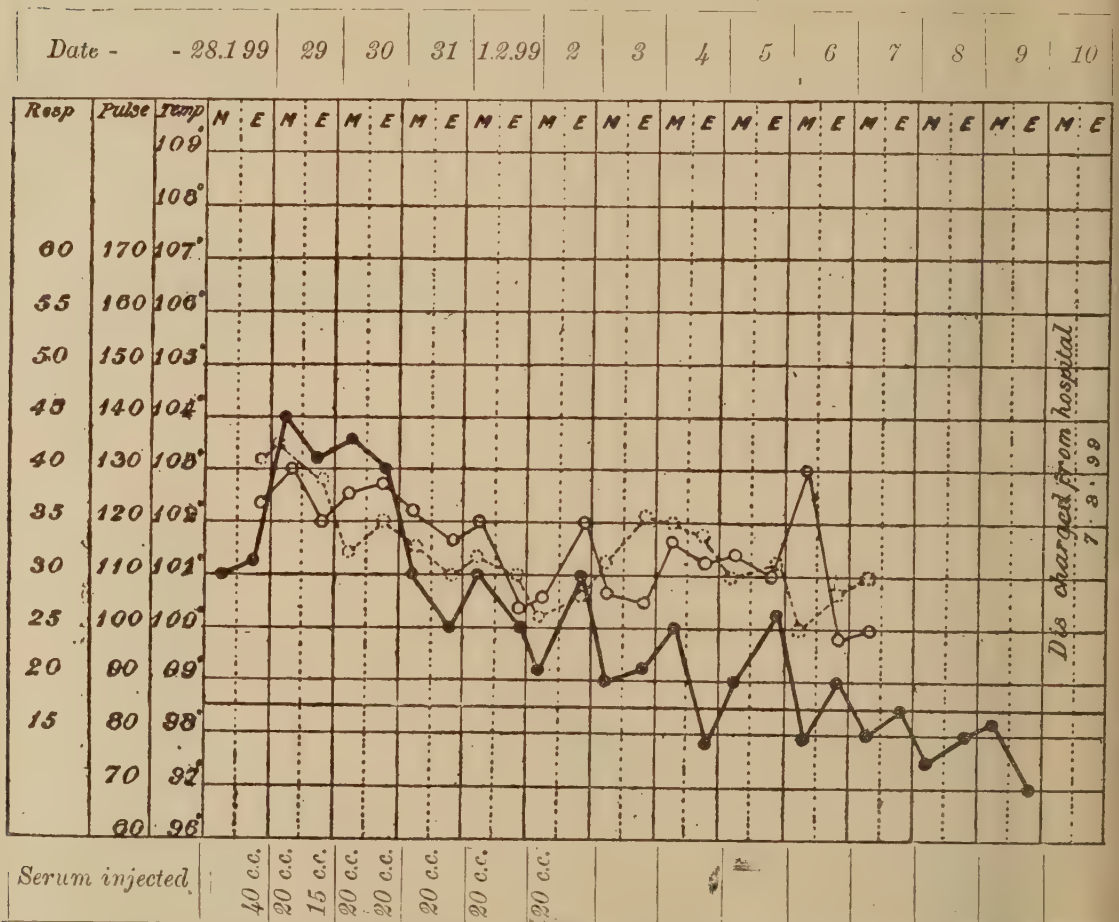
I.

CASES TREATED WITH ROUX' SERUM.

Total number	13
Died	10
Recovered	3
Percentage mortality	76·9

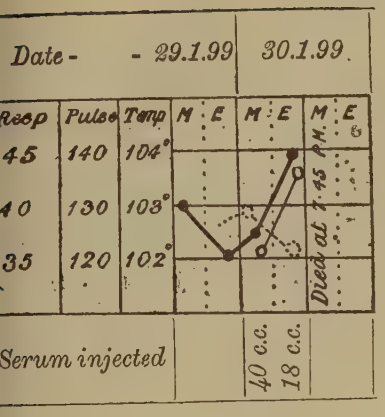
NOTE.—The numbers in brackets at the beginning of each case are those of the Register at the Modi Khana Hospital. The temperature is shown by a thick black line, the pulse by a thin black line, and the respiration by a dotted line.

CASE No. I. (No. 1656 (Control No. 1664).)



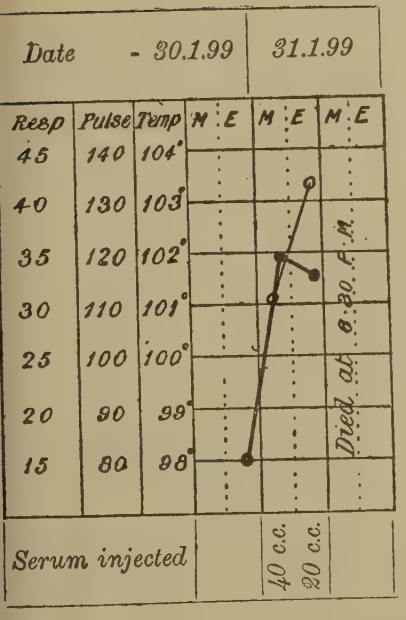
Dhondoo Trimak. Male. Age 23. Brahman. **28.1.99.**—Admitted to hospital with fever and gland in the left axilla. Well developed man. Has been ill for six days. Small tender bubo in left axilla. Is delirious and throwing himself about. Has retention of urine, necessitating catheterisation. 4.30 p.m.—Pulse 128; very poor volume. Temperature 102°. Respiration 32. Tongue thickly coated. Spleen not felt. Liver dulness normal. Heart and lungs normal. A blood film was examined microscopically and blood planted on agar; both gave negative results. 40 *c.c. of serum* in flanks. **29.1.99.**—10 a.m.—Was very delirious all night; quiet and conscious this morning. Pulse 130; better volume. Respiration 42. Temperature 104°. 20 *c.c. of serum*, No. 31. 6 p.m.—Quite quiet and sensible. Heart and lungs again examined and found normal. 15 *c.c. of serum*. **30.199.**—10 a.m.—Quite sensible. Bubo larger. Pulse, fair volume. 20 *c.c. of serum*. 6 p.m.—Condition unaltered. 20 *c.c. of serum*. **31.1.99.**—Quiet and sensible. Volume of pulse steadily improving. Says he feels much better. Tongue cleaning. 20 *c.c. of serum*. **1.2.99.**—Doing very well; talks well. Complains of pain about the bubo. 20 *c.c. of serum*. **2.2.99.**—Slept badly. Retention of urine continues, but passed a small quantity after application of turpentine stupes. Is very weak. 20 *c.c. of serum*. **3.2.99.**—Doing well. Passes urine naturally. Patient gradually improved, and was discharged from hospital on March 7th, 1899.

CASE No. II. (No. 1670 (Control No. 1667).)



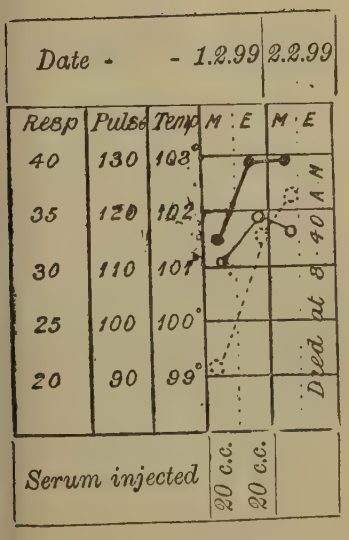
Pandu Mahadu. Male. Age 28. Hindu. Mill hand. **30.1.99.**—10 a.m.—Ill for three days. Small, painful gland in right axilla. Tongue furred at sides, bare in centre. Heart and lungs normal. Quite conscious and talks rationally. Pulse very weak. 40 *c.c. serum.* No. 31. 6 p.m.—Much weaker. 18 *c.c. of serum.*

CASE No. III. (No. 1686 (Control No. 1690).)



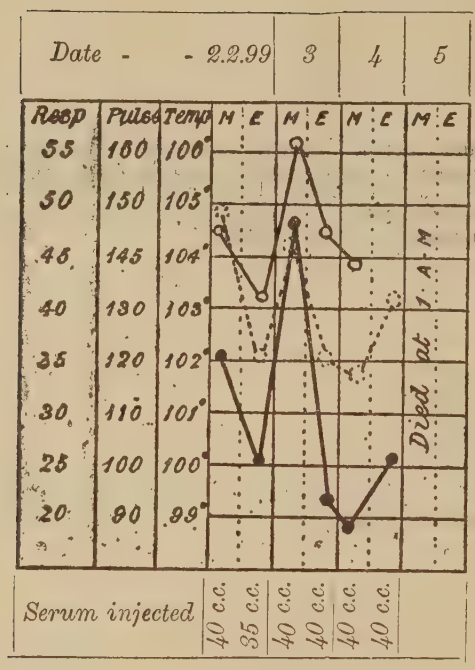
Yanu Paya. Male. Age 40. Hindu. Hamal. **30.1.99.**—Ill for two days. Fever and gland in right femoral region. Is quite conscious and answers questions. Tongue coated. Pulse very compressible and of poor volume. **31.1.99.**—10 a.m.—Small bubo, not very tender. Much œdema around. Heart normal. Lung, râles and rhonchi at both bases behind. Spleen not felt. Liver dulness normal. Conjuctivæ injected. 40 *c.c. of serum,* No. 31. 5 p.m.—Pulse very weak. Patient is semi-conscious. Answers when roused. Alternately drowsy and restless, throwing himself about and picking at the bed clothes. 20 *c.c. of serum* in subcutaneous vein.

CASE No. IV. (No. 1697 (Control No. 1675).)



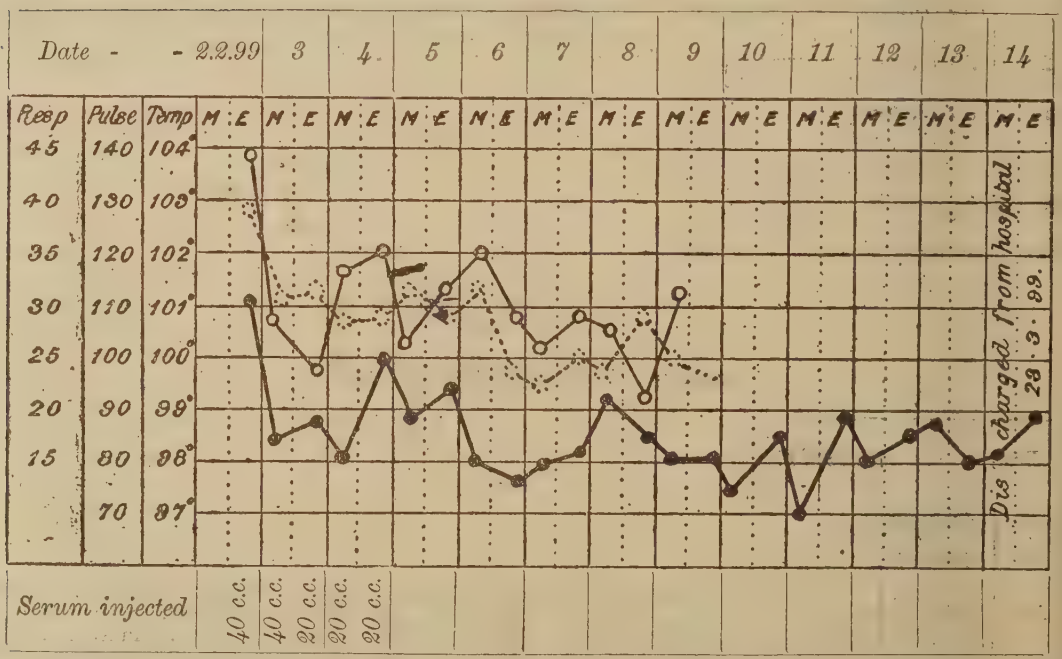
Tatwardas Ramnath. Male. Age 45. Hindu. Mendicant. **1.2.99.**—10 a.m.—Ill for three days. Moderate bubo in left groin. Hard and painful. Eyes injected. Tongue furred all over. Pulse fairly good volume. Heart normal. Lungs. Moist sounds at both bases behind. A few scattered rhonchi, both sides front. Spleen and liver normal. 20 *c.c. of serum,* No. 31. 6 p.m.—General condition fairly good. 20 *c.c. of serum.* **2.2.99.**—Slept fairly well. Pulse became very bad at 6 a.m.

CASE No. V. (No. 1708 (Control No. 1709).)



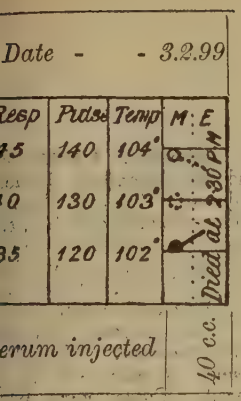
Ghulam Imamdin. Male. Age 18 (?). Muhammadan. Labourer **2.2.99.**—10 a.m.—Admitted with fever and gland in right axil Extremities cold. Shivering. Semi-conscious, but very drows Tongue coated, eyes injected. Too ill to admit of detail examination. 40 *c.c. of serum*, No. 31. 6 p.m.—Pulse sho marked dicrotism. Very drowsy. Can scarcely be roused, a then does not understand what is said to him. 35 *c.c. of seru* **3.2.99.**—10 a.m.—Condition very bad. Noisy delirium, a twitching in limbs and facial muscles. 40 *c.c. of serum.* 6 p.m —Condition unchanged. 40 *c.c. of serum.* **4.2.99.**—10 a.m. Pulse extremely weak. Lips covered with sordes. Extremiti cold. 40 *c.c. of serum.* 5.30 p.m.—Patient appears to moribund. Profuse sweat on forehead. Picks feebly at the be clothes. 40 *c.c. of serum.*

CASE No. VI. (No. 1716 (Control No. 1699).)



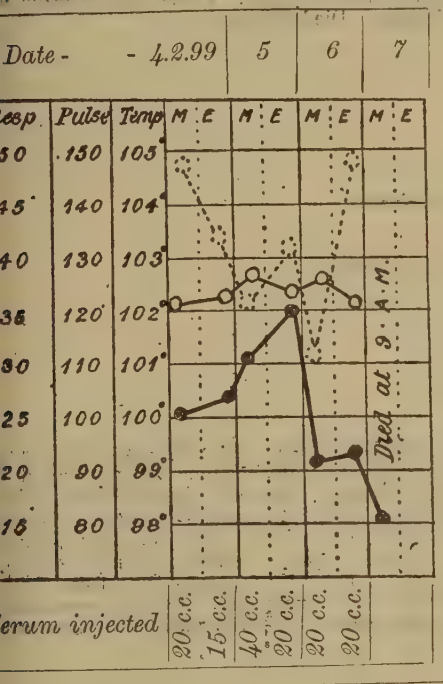
Daji Bobaji. Male. Age 30. Hindu. Labourer. **2.2.99.**—Said to have been ill for eight days. Quite conscious. Eyes injected. Speech slightly affected. Bubo with ulcerated surface in right groin. Râles, rhonchi and bronchial breathing scattered over both lungs. Pulse soft and compressible. 40 *c.c. of serum*, No. 31. **3.2.99.**—10 a.m.—Had a very bad night. Is quite conscious, but very weak this morning. Occasional paroxysms of coughing. Lungs full of râles. Was collapsed for a short time early this morning. 40 *c.c. of serum.* 6 p.m. —General condition bad. Pulse very poor volume. 20 *c.c. of serum.* **4.2.99.**—10 a.m.—Very weak. Cough rather better. Says he feels better than yesterday. 20 *c.c. of serum.* 5.30 p.m.—20 *c.c. of serum.* **5.2.99.**— Distinctly better. Pulse still very weak. **8.2.99.**—Doing well.

CASE No. VII. (No. 1720 (Control No. 1721).)



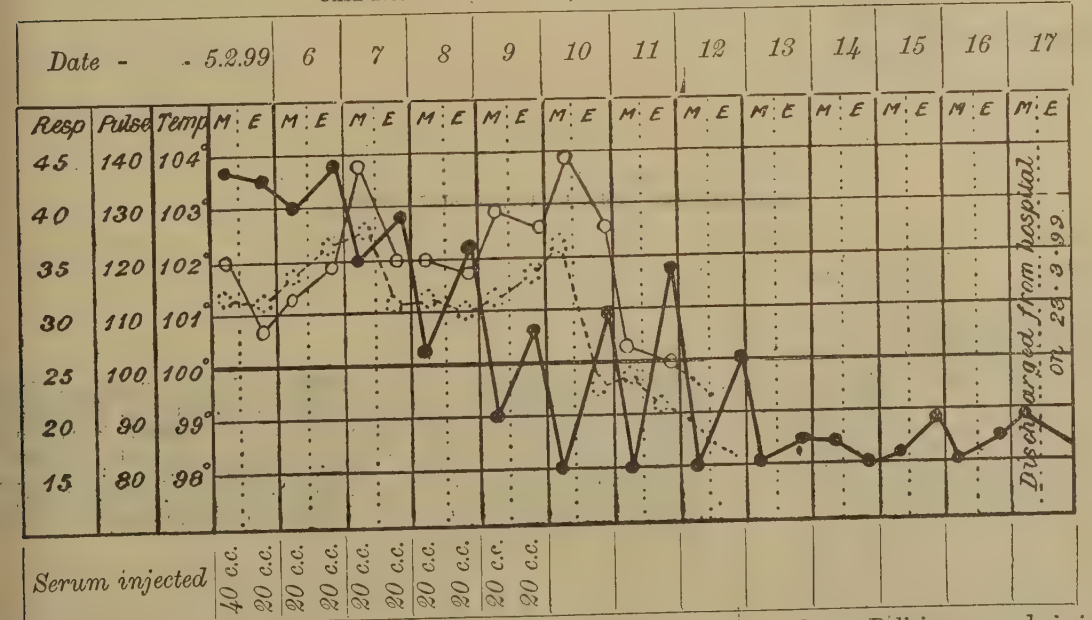
Luxman Rama. Male. Age 35. Hindu. Labourer. **3.2.99.**—About four days ago had a swelling in the right groin, which appeared suddenly, and has been acutely tender since its first appearance. There is a moderate sized bubo in the right groin. Skin has been cauterised over it. Pulse thready. Is conscious, but apparently moribund. Breath and extremities cold. Chest normal. 9.30 a.m.—40 *c.c. of serum.*

CASE No. VIII. (No. 1738 (Control No. 1740).)



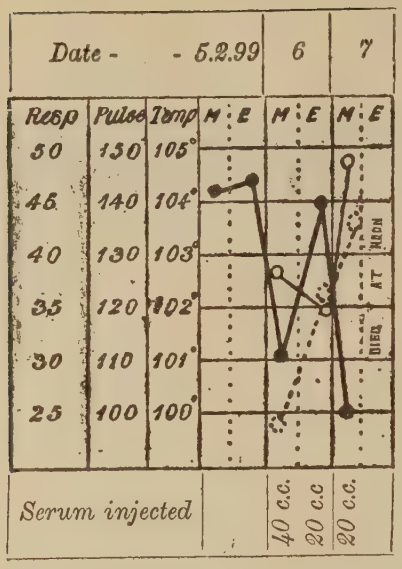
Dhondu Sadu. Male. Age 22. Hindu. Labourer. **4.2.99.**—Admitted with fever and bubo in right femoral region. Has been ill for two days. Eyes not suffused. Is quite conscious. Tongue covered with white fur. Pulse good. Second pulmonary sound, accentuated. Lungs: Patchy, moist sounds, mostly of left side. Much cough. Bubo size of a walnut. Very tender. Spleen not felt. Liver dulness, normal. 10 a.m.—20 *c.c. of serum*, No. 31. 5.30 p.m.—15 *c.c. of serum.* **5.2.99.**—Cough very troublesome. Pulse fair volume. General condition pretty good. 10 a.m.—40 *c.c. of serum.* 10 p.m.—20 *c.c. of serum.* **6.2.99.**—10 a.m.—Patient collapsed with cold extremities, and very feeble pulse. Rallied somewhat after injection of ether and strychnine. Became very bad again at 11 a.m. Much frothy sputum. Chest full of râles. Pulse imperceptible. 20 *c.c. of serum.* 6 p.m.—Moribund. 20 *c.c. of serum.*

CASE No. IX. (No. 1746 (Control No. 1739).)



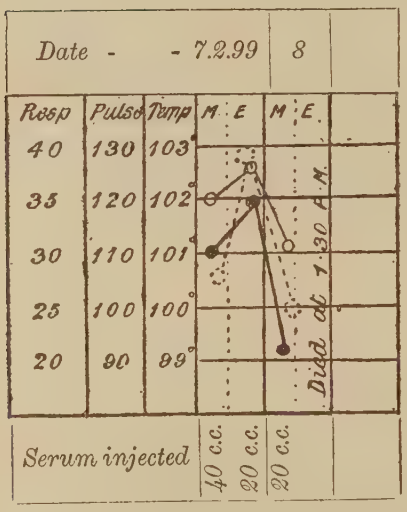
Rama Narayan. Male. Age 29. Hindu. Coolie. **5.2.99.**—Ill for three days. Delirious on admission, emperature 103·8°. Several contusions about face. Pulse, fair volume. Tongue dry, thick yellow fur. reath very fœtid. Eyes not suffused. No enlargement of liver or spleen. Bubo. A string of hard, rather nder glands in left femoral region. Heart and lungs, normal. 10 a.m.—40 *c.c. of serum*, No. 31. 10 p.m.— 0 *c.c. of serum.* **6.2.99.**—Slept fairly well. Conscious this morning. Pulse 120, weak. Has some cough. 0 a.m.—20 *c.c. of serum.* 6 p.m.—20 *c.c. of serum.* **7.2.99.**—Delirious at intervals. Pulse fairly good. 0 a.m.—20 *c.c. of serum.* 6 p.m.—Pulse not so good. 20 *c.c. of serum*, No. 21. **8.2.99.**—Quiet during the ight, but no sleep. Speech very thick. Cough and expectoration. Râles both sides of chest. 10 a.m.— 0 *c.c. of serum*, No. 21. 6 p.m.—Semi-conscious. 20 *c.c. of serum.* **9.2.99.**—Condition better. 10 a.m.— 0 *c.c. of serum*, 6 p.m.—20 *c.c. of serum.* **23.3.99.**—Convalescence slow.

CASE No. X. (No. 1762 (Control No. 1765).)



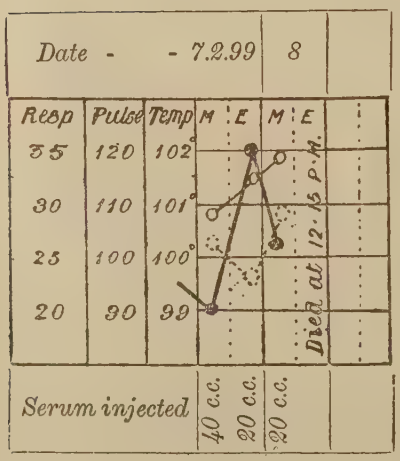
Dhondoo Bhanji. Male. Age 30. Hindu. Labourer. **5.2.99.**—Ill for two days. Admitted with fever and gland in right axilla. Patient is quite conscious. Tongue coated. Eyes injected. Speech slightly affected. **6.2.99.**—Patient is conscious. Speech markedly affected, the ends of words being much prolonged. Tongue tip protruded to left of mid line. No ocular or other paralysis. When talking, from time to time, there is a sort of spasm of the orbicularis oris, which stops his speech. Pulse, poor volume. Bubo in right axilla about size of walnut, very tender. Spleen not felt. Liver normal. Lungs: patches of pneumonia, both bases behind. Heart normal. When left alone, patient mutters incoherently to himself. 10 a.m.—40 *c.c. of serum*, No. 31. 6 p.m.—Condition unaltered. Pulse, very poor volume. 20 *c.c. of serum*. **7.2.99.**—Quiet all through the night. Delirious this morning. Condition very bad. Pulse, imperceptible at wrist. 10 a.m.—20 *c.c. of serum*. Died at noon.

CASE No. XI. (No. 1775 (Control No. 1776).)



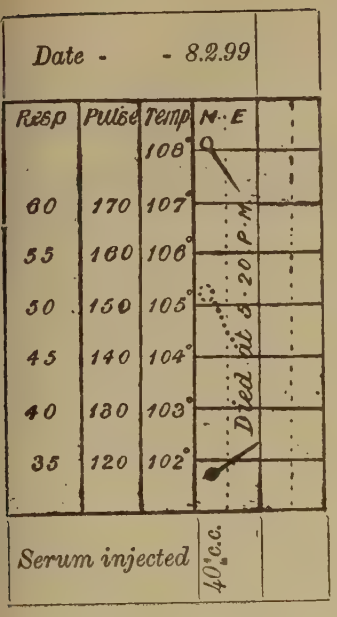
Narayan Ooma. Male. Age 14. Hindu. Peon. **7.2.99.**—Ill for six days. Fever and hard painful gland in left groin. Tongue coated in patches with dry yellow fur. Eyes not suffused. Speech slightly thick. Quite conscious. Heart, spleen, and liver, normal. Lungs. Patches of moist sounds posteriorly in both lungs. Pulse of poor volume. Bubo, size of a walnut. 11 a.m.—40 *c.c. of serum*, No. 31. 6 p.m.—Very weak. 20 *c.c. of serum*, No. 21. **8.2.99.**—No sleep. Very delirious all night. This morning is exhausted. Pulse imperceptible at wrist. Running in brachial artery. Extremities cold. 10 a.m.—20 *c.c. of serum*, No. 21.

CASE No. XII. (No. 1773 (Control No. 1752).)



Kalloo Parbutti. Male. Age 25. Hindu. Labourer. **7.2.99.**—Ill for two days. Admitted with fever and gland in right axilla. Dorsum of tongue covered with dry brown fur. Eyes injected. Speech not affected. Is quite conscious. Pulse poor volume, but regular. Heart, lungs, spleen, and liver normal. Much œdema around the bubo. 11 a.m. —40 *c.c. of serum*, No. 31. 6 p.m.—Pulse very poor. 20 *c.c. of serum*, No. 21. **8.2.99.**—Noisy delirium. Pulse almost imperceptible. 10 a.m. —20 *c.c. of serum*, No. 21.

CASE No. XIII. (No. 1786 (Control No. 1787).



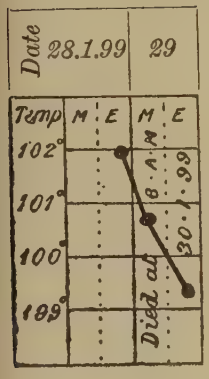
Sakharam Krishna. Male. Age 12. Hindu. Labourer. **8.2.99.**—Four days ill. Bubo, left axilla. Tongue coated. Speech not affected. Eyes somewhat injected. Heart normal. Lungs. Harsh breathing over whole of left chest. Liver and spleen, normal. Pulse, running about 180. 11 a.m. —40 *c.c. of serum*, No. 21. 5 p.m.—Had a short convulsive fit.

II.

CONTROL CASES, to those TREATED WITH ROUX' SERUM.

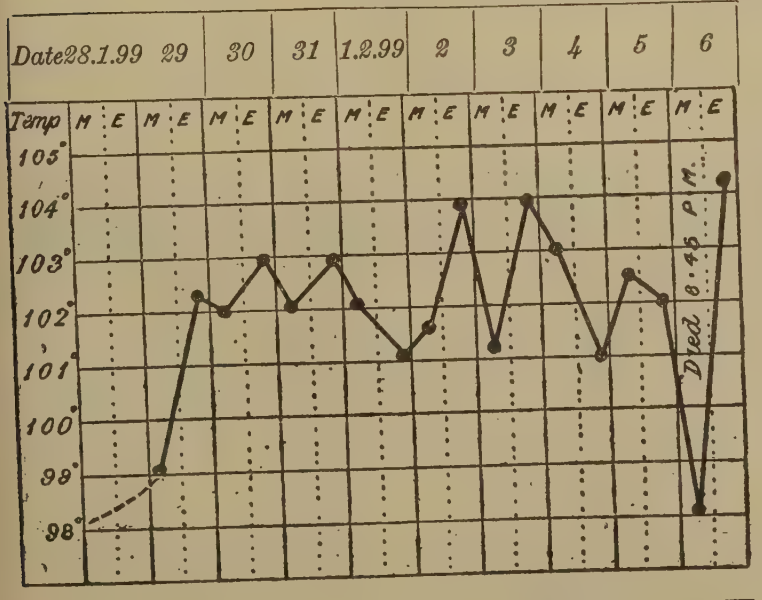
Total number - - -	13
Died - - - -	13
Percentage mortality -	100

CASE No. I. c. (No. 1664 (Control to No. 1656).)



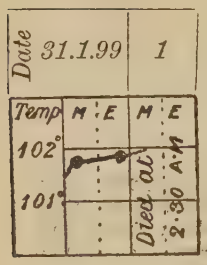
Anandroo Gennoo. Male. Age 20. Hindu. Labourer. **28.1.99.**—Ill for two days. Right femoral bubo. Tongue coated. Pulse very feeble. **29.1.99.**—Slept well. Pulse very feeble and rapid.

CASE No. II. c. (No. 1667 (Control to No. 1670).)



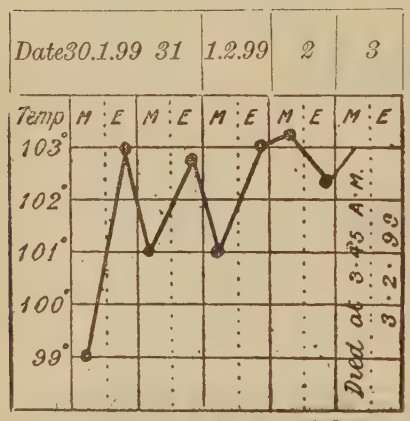
Luxman Yessoo. Male. Age 18. Hindu. Carpenter. **28.1.99.**—Ill for two days with fever, and painless hard swelling on the right side of the neck; says that the swelling is of long duration. **30.1.99.**—The swelling in the neck is becoming painful. Tongue coated. Eyes red. **1.2.99.**—Bubo about the same, still hard. **2.2.99.**—Small glands have appeared in both femoral regions. Patient very restless. **3.2.99.**—Had a fairly good night. Condition fairly good. **4.2.99.**—Is delirious this morning. Pulse of good volume. Cervical bubo incised, very little pus. **6.2.99.** —No sleep. Eyes much injected. Pulse fairly good in the morning.

CASE No. III. c. (No. 1690 (Control to No. 1686).)



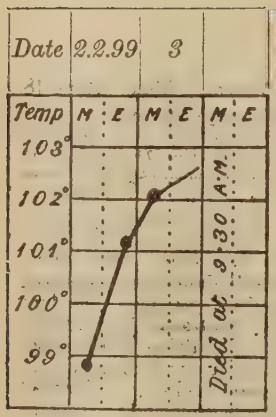
Dhondu Narayan. Male. Age 18. Hindu. Labourer. **31.1.99.**—Admitted to hospital with fever and gland in the right axilla. Semi-conscious. Speech affected. Tongue coated. Pulse very weak.

CASE No. IV. c. (No. 1675 (Control to No. 1697).)



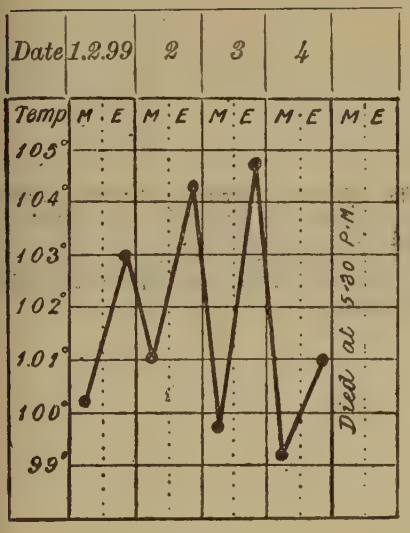
Madhoo Rama. Male. Age 25. Hindu. Polisher. **30.1.99.**—Ill for four days. Speech slightly affected. Small gland in right groin. Not tender. **31.1.99.**—Gland more prominent. **1.2.99.**—Delirious last night. Extremities rather cold. Tongue furred. Very noisy. **2.2.99.**—Slept well. Is quiet this morning. Pulse weak. Respiration hurried.

CASE No. V. c. (No. 1709 (Control to No. 1708).)



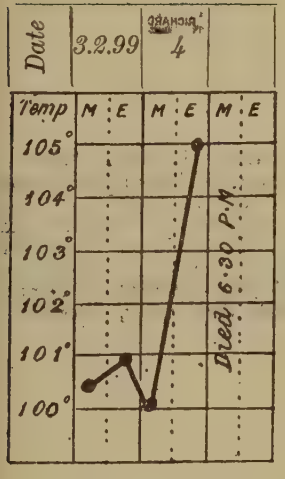
Barbosa. Male. Age 30. Christian. Servant. **2.2.99.**—Ill for six days. Admitted with fever and gland in left femoral region. Skin over the bubo is ulcerated. Tongue coated, eyes suffused. Bowels constipated. Speech somewhat affected. Pulse rapid and soft. **3.2.99.**—Pulse very feeble. Extremities cold. Very restless. Cannot swallow.

CASE No. VI. c. (No. 1699 (Control to No. 1716).)



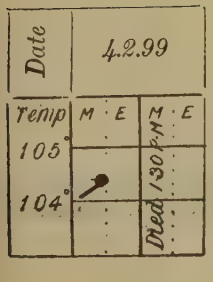
Takaram Babaji. Male. Age 40. Hindu. Carpenter. **1.2.99.**—Ill for two days. Has a cough, which he says is of long standing. Fever dates from two days ago. Tongue slightly coated. Has a chronic glandular enlargement in the right groin. **2.2.99.**—The glands are more painful. Pulse fair. **3.2.99.**—Restless during the night. Gland decidedly more painful and bigger. Pulse fair. **4.2.99.**—Cough troublesome. Delirious during the night. Pulse still fairly good.

CASE No. VII. c. (No. 1721 (Control to No. 1720).)



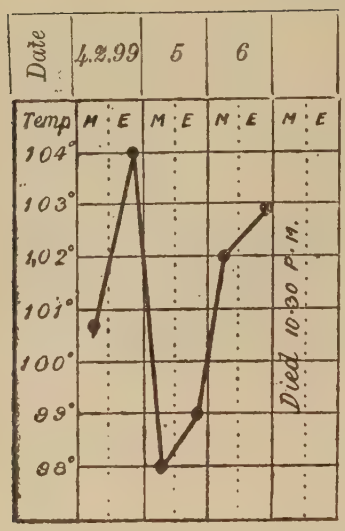
Bhaja Visram. Male. Age 15. Hindu. Labourer. **3.2.99.**—Ill for eight days. A swelling, in the right groin, appeared eight days ago, without fever. Was not painful at first. Four days ago, had fever, with slight shivering. Is now restless and delirious. Eyes injected. Tongue coated. Pulse very soft. Extremities cold. **4.2.99.**—Had no sleep. Pulse fairly strong. Tongue very coated. Appears to be more conscious.

CASE No. VIII. c. (No. 1740 (Control to No. 1738).)



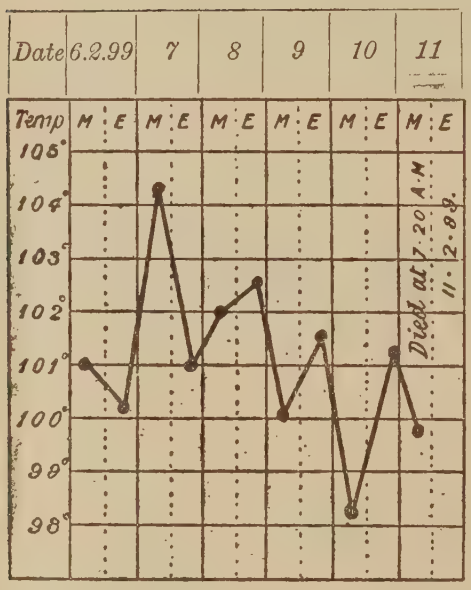
Ganoo Rawji. Male. Age 40. Hindu. Labourer. **4.2.99.**—Ill for six days. Admitted with fever and bubo in right axilla. Tongue coated. Eyes injected. Pulse feeble. Respiration hurried. Speech not affected. Slight hæmoptysis. Ecchymosis over chest and abdomen.

CASE No. IX. c. (No. 1739 (Control to No. 1746).)



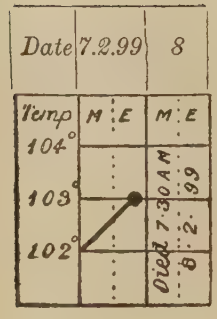
Dhakoo Bhikoo. Male. Age 25. Hindu. Labourer. **4.2.99.**—Ill for three days. Admitted with fever and bubo in left axilla. Is conscious. Tongue coated. Speech not affected. Eyes somewhat injected. Says that his fever began with rigors, and that he noticed the gland on the same day. Pulse rapid, but of fair volume. **6.2.99.**—Was noisy and delirious all night. No sleep. Pulse rapid and of poor volume.

CASE No. X. c. (No. 1765 (Control to No. 1762).)



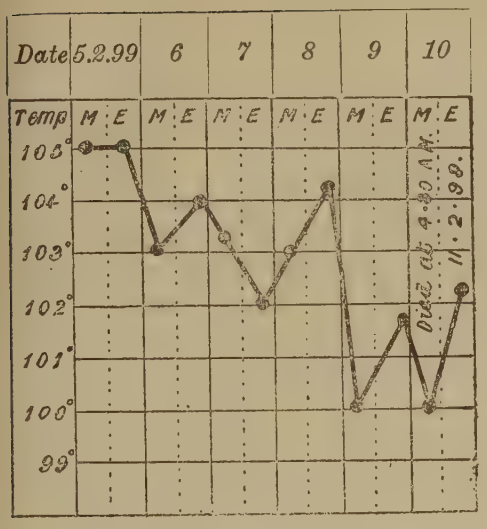
Takhoram Bhagu. Male. Age 9. Hindu. Labourer. **6.2.99.**—Ill for three days. Admitted for fever and bubo in right groin. There are two small glands, which are very tender. Speech somewhat thick. Pulse, soft and compressible. **7.2.99.**—Had no sleep. Delirious this morning. Very thirsty. Pulse very feeble. **8.2.99.**—Very restless and noisy, but slept up to 7.0 a.m. Pulse feeble. **9.2.99.**—More quiet to-day. **10.2.99.**—Was noisy and restless until midnight. Pulse very weak, extremities rather cold.

CASE No. XI. c. (No. 1776 (Control to No. 1775).)



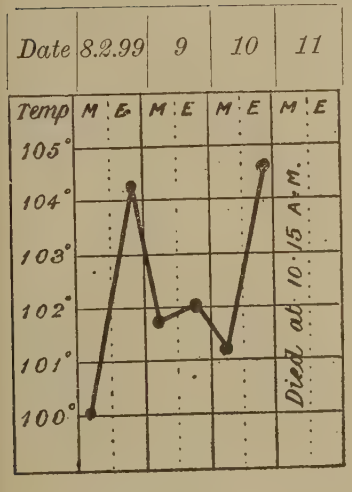
Dhanu Joti. Male. Age 40. Hindu. Labourer. **7.2.99.**—Ill for two days. Fever and gland in right groin. Tongue coated. Eyes injected. Some cough. Pulse soft and rapid. Bubo consists of three small painful glands.

CASE No. XII. c. (No. 1752 (Control to No. 1773).)



Ram Chandra Essoo. Male. Age 24. Hindu. Labourer. **5.2.99.**—Has been ill for three days. Fever commenced with shivering. Speech markedly affected. Is conscious. Spleen normal. **6.2.99.**—Tongue furred. Condition fairly good. **7.2.99.**—Complains of pain in the right groin, where there is a small bubo. Tongue coated. Pulse rapid and soft. **8.2.99.**—Noisy at times. Pulse weak. **9.2.99.**—Pulse bad. **10.2.99.**—Slept a little, but was restless during the past night. Pulse rapid and weak.

CASE No. XIII. c. (No. 1787 (Control to No. 1786).)



Korida Kristoo. Male. Age 18. Christian. Clerk. **8.2.99.**—Ill for two days. Fever and bubo in left axilla, which appeared last night, and is very tender. Tongue furred. Lungs clear. There was another plague case in the same house two days ago. Condition good. **9.2.99.**—Says the bubo is less tender. **10.2.99.**—Diffused swelling around the bubo. Tongue much coated. Pulse weak and rapid.

APPENDIX No. LXXXII.

(*See* Question No. 26,508.)

Letter No. 260A of 1898.

From W. M. HAFFKINE, Esq., C.I.E., to the SECRETARY TO THE GOVERNMENT of INDIA, Home Department, Simla.

Plague Research Laboratory,
SIR, Mazagaon, Bombay, June 7, 1898.

I HAVE the honour to forward a note by Surgeon Lieut.-Col. Warden, on the work done by him in this laboratory in connexion with the preparation of peptone from goat's meat, by digesting it in acidulated water.

As the result of this work, a preparation now entirely manufactured in the laboratory has been substituted for commercial peptone.

The following is the formula of that preparation:—

1,000 grammes of goat's meat mixed with 2,000 grammes of water and 150 c.c. of hydrochloric acid, and heated for 5½ hours in an autoclave under 2½ kilos of pressure.

The filtrate is mixed with 45 grammes of caustic soda and 75 grammes of common salt, heated further under two kilos of pressure for half an hour, filtered, and the volume of the filtrate brought, by addition of water, up to 3,000 grammes.

For the preparation of the medium in which the plague prophylactic is cultivated, one part of the above solution is mixed with nine parts of neutralised goat's meat extract, obtained by boiling fresh goat's meat in 18 volumes of water. The surface of the liquid is then covered with a thin layer of minute droplets of ghee (clarified butter).

The plague prophylactic is the result of a prolonged fermentation produced in the above medium by the plague bacillus, *plus* the bodies of the bacilli themselves killed by maintaining them for an hour at a temperature of 65° C.

2. Apart from the substitution of peptonised goat's meat for commercial peptone, no modification is, for the present, introduced in the preparation of the plague prophylactic, as adopted after the tests in the Byculla House of Correction and the Umarkhadi Common Jail, in Mora, in Damaon, Bombay, Lonaula, Kirkee, Undhera, and in the Bombay Khoja community.

The observations collected in the above epidemics have demonstrated that the plague prophylactic possesses the two properties which were aimed at in our programme, viz., the power of reducing the number of attacks amongst the inoculated, when compared with uninoculated, and the power of reducing the number of deaths in inoculated attacked, when compared with the attacked uninoculated. The observations have shown that the two effects combined amount to a final reduction of between 80 and 90 per cent. of death in inoculated, when compared with the uninoculated exposed to the same epidemic.

These properties of the plague prophylactic may now be considered as an established fact which is not likely to be shaken by any further tests or experiments; and no sudden alteration in the nature of the drug is desirable. This is because it is impossible to say for certain whether the above results were obtained just in the way I imagined they would, and in answer to the theoretical conjectures which guided me. Future alterations and improvements must be made gradually, and in conformity with those conjectures, which will be confirmed or refuted only by a critical scrutiny of results.

In the course of my observations on the inoculated against cholera I formed the idea that the invasion of an individual by morbid microbes is very effectively controlled by a treatment with preparations made of the bodies of microbes, but that the issue of the disease when invasion has not been prevented is not influenced by such a preparation. On the other hand, the fact that almost in all infectious diseases, studied up to now, the morbid organism remains present in the patient for a long time after recovery, without causing any inconvenience, goes to show that recovery is a phenomenon essentially different from the resistance to microbial invasion, or from the ridding of the body of such microbes after invasion.

The strengthening of the organism for a struggle against morbid symptoms should be sought for in a treatment with preparations made from the products of microbes, not from the bodies of the microbes themselves.

Confirmation or otherwise of these views may result from cautious attempts at improving the results of the plague inoculation.

For the present the plague bacilli introduced into the fermentation medium are permitted to grow there for an average of four weeks; after this they are killed, but left in liquid. The mixture combines at once a large amount of bodies of microbes, corresponding to the vaccines used in cholera, with the products secreted into the liquid, which are not present in the cholera inoculation. In the interest of the last ingredient it is desirable that the fermentation should be maintained as long as possible, much longer than the period given above, and extended, if possible, for months; in the interest of the microbial preparation, on the contrary, the cultivation should be as young and vigorous, and as unaltered by age, as possible.

The method at present used was selected as presenting a great simplicity of execution; and the period of four weeks was taken as being an average not so long as to cause a complete deterioration of the microbes, and yet long enough for the accumulation of a certain amount of fermented products.

The first step for improving this preparation should consist in separating the above two operations, viz., in cultivating fresh microbial vegetation separately, in a way similar to that used in cholera, and in maintaining, apart from this process, a fermentation in liquid media for periods longer than it is possible at present. The mixture of the two materials should be made artificially, and the result of the modification carefully studied in epidemics. The addition of fresh crops of microbes should reduce further the number of cases, while the use of cultures of older age should increase the resistance to the symptoms of the disease.

The problem of reducing the discomfort caused by the preventive inoculation, may possibly be solved by modifying the vaccine. It appears that, of the two elements constituting the present plague prophylactic, the liquid part, filtered or decanted off the solid residue, causes the rise of temperature observed after inoculation; while the solid part suspended in the liquid is responsible for the pain developing in the seat of inoculation. Under the effect of chemical and physical agencies the liquid loses its power of producing fever. On the other hand, it is possible that the solid residue may be modified mechanically or by dissolving it in chemicals, so as to cease to cause the local reaction. Whether such modifications are compatible with the maintaining of the protective power of the prophylactic can be solved only by direct observation, and by cautious and gradual attempts.

Lastly, an important problem in connexion with the manufacture of the prophylactic consists in fixing the pathogenic properties of the microbe so as to ensure uniform and accurately measurable effect. This has not been done up to now, chiefly on account of the difficulties of procuring animals, and in the interest of simplicity in the operations; but as this prophylactic does not contain living microbes, and is a fixed chemical drug, it was sought to compensate the alterations in the fermenting organism by modifying the doses of the drug. The result obtained with the weakest material up to now used amounted to the suppression of 77·9 per cent. of deaths in inoculated compared with the uninoculated, as was observed in the outbreak in the native followers of the Artillery at Kirkee.

Although, therefore, the plague prophylactic admits, and, it is to be hoped, will for a long time continue to admit, of many useful modifications, it may be considered that the question at issue has been, for practical purposes, solved by the experiments and observations made up to now; and I have no hesitation in recommending as wide an encouragement of the adoption of the method as possible, with the understanding that the treatment is optional, and offered as a voluntary substitute for the separation of the sick and healthy, and their removal from the place of habitation.

I have, &c.
Sd. W. M. HAFFKINE.

APPENDIX No. LXXXIII

STATEMENT HANDED IN BY M. HAFFKINE

REGARDING THE

ANTI-PLAGUE PROPHYLACTIC.

(This Statement is published at Mr. Haffkine's desire.)

On the 7th June 1898 I addressed to the Government of India a letter, No. 260A, in which the following three problems were put down in connection with the further development of the inoculation against the plague :—

(1) The first referred to the separation of the manufacturing of the bodies of the plague bacilli and of the metabolic substances, the mixture of which constitutes the plague prophylactic ;

(2) the second to the mitigation of the local effect caused by the inoculation ; and the third to

(3) the fixing of the properties of the plague bacilli so as to permit of a more accurate standardizing of the prophylactic.

The above problems, as well as several others, were early recognised as urgent in the Plague Research Laboratory. Unfortunately, the rapid spread of the epidemic, and the necessity of meeting the demands for prophylactic while the epidemic lasted, entailed such an amount of current work that, up to the present time, I was unable to sufficiently attend to their solution, though many attempts in the above direction have been already made in my Laboratory. I have requested now the Government to kindly give me a senior Assistant, to whom I could delegate the administration of the current Laboratory work, and thus be set free to attend to further inquiries. I have fair reasons to hope that the above request will be granted, and the problems mentioned above will be persistently and carefully pursued.

Among the problems enumerated I have not mentioned the one announced to the public by the Medical Officers at Hyderabad, the reason being that that problem of keeping the material free from common microbes is one that has been always the object of much care in the Laboratory. The Commission, however, knows that to attain that object in a complete form is not always feasible, and that such a state of affairs is not fraught with danger. Thus, for instance, there is no possibility of having a vaccine lymph for small-pox free from foreign microbes. The lymph taken direct from a vesicle of a calf almost without exception contains already extraneous microbes, that can be seen under the microscope and cultivated in nutritive media. When the lympth is squeezed out of the vesicle, it comes further in contact with the surface of the calf's skin, and with all the microbes adherent to it. These microbes cannot be killed in vaccine lymph, which is itself a living matter, as by doing so, the lymph itself is killed. The only thing that can be, and is done, is to prevent the foreign microbes from multiplying in the material, and this is to a great extent obtained, for instance, by the addition of glycerine to the lymph. The result, of course, is perfectly satisfactory. Every one has seen frequently in this country, in villages, batches of 200—300 children at a time, paraded in the streets with their vaccinated arms outstretched before the Inspectors of Vaccination, without a single case of complication due to those common germs being observed.

The plague prophylactic admits of being prepared with a far greater freedom from those common microbes than is the case with the vaccine lymph. To attain, however, this object fully, better conditions of work are required than those that we could command up to late. We had to work in the Laboratory with a staff of subordinates that were not prepared for bacteriological work, and with materials and appliances that we had to buy urgently at Bhendi Bazar, and at Abdul Rahman Street in Bombay. The Government Medical Stores did their utmost to procure for us supplies. At one time, however, they were able to get for us only bottles for which no corks of suitable size could be got; and afterwards, when the corks became plentiful, there were no more bottles in Bombay to suit fittingly the corks. Into a large number of such bottles common microbes penetrated after the material had been corked and sealed, owing to their deficient corking and sealing. In other instances, among the bottles prepared and sterilized for being filled up with the prophylactic there were such as had escaped complete sterilization. For effecting such, it is necessary that the temperature in the steam sterilizer should be raised 10° to 15° C. above the boiling point. Whenever, owing to the defect in the registering apparatus, or to other reasons, the bottles are subject to a lower temperature, complete sterilization is not effected. This, however, is no source of serious risk. Every one knows, for instance, that a surgeon preparing for an operation on a patient is perfectly safe if he keeps his instruments for a quarter of an hour in boiling water. No surgeon ever sterilizes his instruments at a higher temperature. The reason of this is that the temperature of boiling water kills all *pathogenic* microbes, capable of causing diseases ; and though the instruments, when taken out of the water and put into cultivation media, will always show a number of living germs sticking to them and which have escaped the effect of boiling, these germs are harmless, as are harmless all those innumerable microbes that we eat and drink and breathe in our daily life, or as is harmless an apple, or an onion, or a mango, or any other living vegetable or fruit that we eat uncooked.

Whenever, therefore, steam is formed in the sterilizer, and the bottles remain there, be it for a quarter of an hour only, they become safe as to the risk of having in them microbes causing disease ; but if the temperature is not raised high enough *above* that point, there will remain common microbes in those bottles and they may be found subsequently in the prophylactic put into those bottles. Such microbes will be capable of growing when transferred into cultivation media ; as an apple is capable of growing when put into the soil. In a number of cases these microbes will be killed by the ½ per cent. of carbolic acid contained in the prophylactic. There are other microbes that will not be killed by that antiseptic, but their growth and multiplication will be arrested, as glycerine arrests the growth of microbes in vaccine lymph, which is all that is required in the case. But some microbes resist even that effect, and it is the latter that it is desirable to avoid. This can be done sucessfully and very easily in ordinary quiet times, and even in times of urgent and hasty work when the Laboratory gets fully officered, and all the officers get to know their work. During the last epidemics in Hubli, Dharwad, Gadag, Bangalore, those were not the conditions that obtained, and we had to consider whether we were justified in doing our work and supplying urgently those places with prophylactic under the then existing conditions or not. We decided that we were, and I have no doubt every

one will share our view when it is considered how much the facts justified our calculations. This can be easily done from any concrete instance.

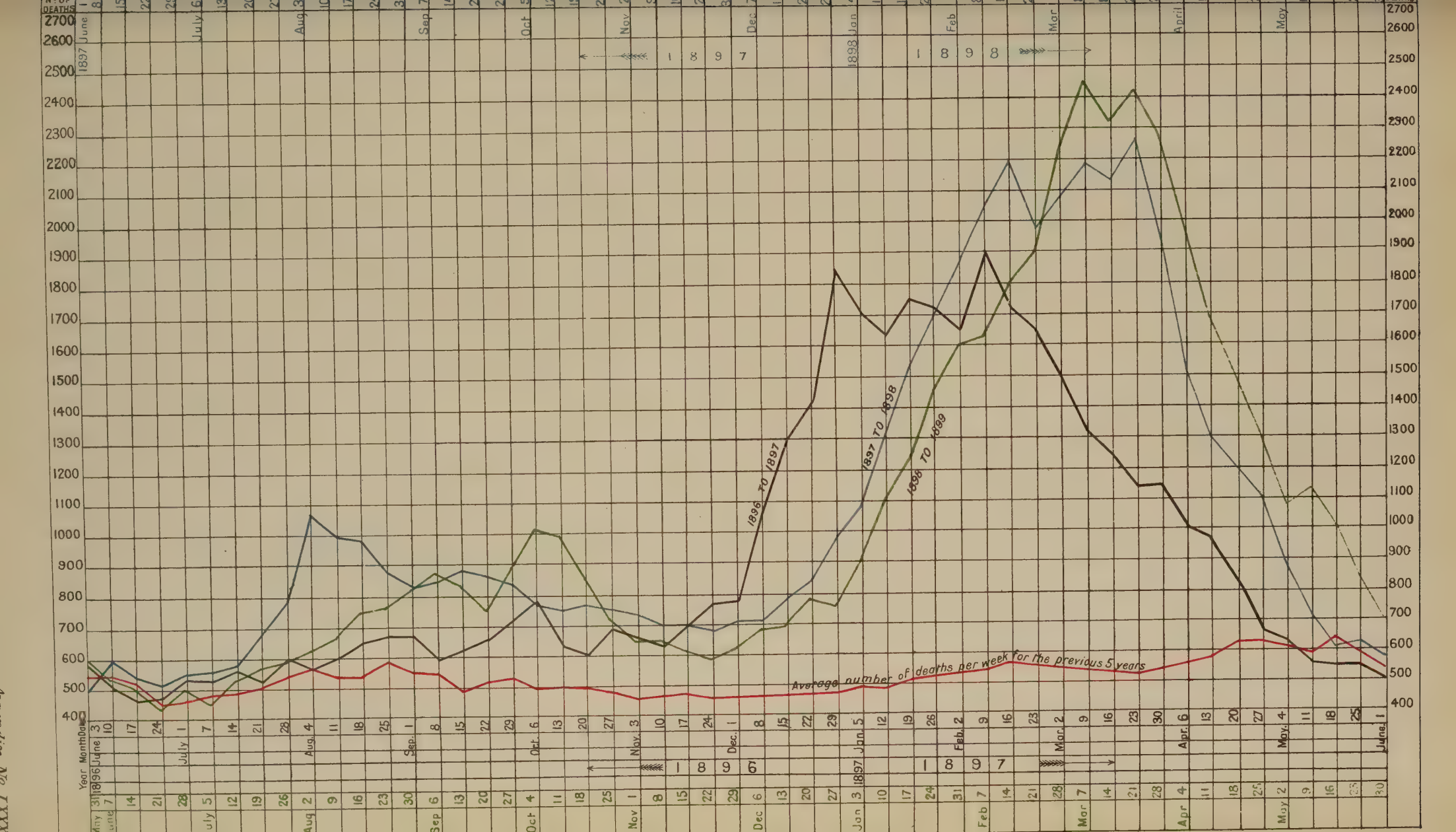
The Commission have it on evidence from the medical officers who were in charge of the plague operations at Hubli, during the last epidemic there, that they performed 78,000 injections with the plague prophylactic in that town alone, and that in these operations they observed 12 cases where the injection caused complication in the shape of abscesses. All these 12 abscesses subsequently healed up, and no complications of any other nature have ever occurred —in this respect they were perfectly emphatic. The facts collected in Hubli show that the total number of deaths that took place there during the epidemic was 2,755; but that if no inoculation had been done, and the death-rate in the whole population had been equal to that observed in the uninoculated, 24,899 would have died. The inference is that the inoculation with the plague prophylactic saved 22,144 lives. In adopting the plan of work which the best of our knowledge suggested to us as correct and safe, the officers of the Plague Research Laboratory admitted the risk of causing *one* abscess that subsequently healed up, for each 1,835 lives that they have saved.

I beg, however, the Commission to believe me that we are doing our very utmost to avoid even the possibility of that *one* abscess.

In conclusion, permit me to express my regret, if it so happened that the Hyderabad Medical Officers, who startled the public by their statements and deterred a very large number of unknowing people from taking advantage of inoculation, had the requisite knowledge for estimating the above considerations, but represented the case to the public in an incomplete form. On the other hand, I have no reproach to address to them if they acted when being themselves under a misapprehension.

(Signed) W. M. Haffkine.

Bombay, March 22nd, 1899.

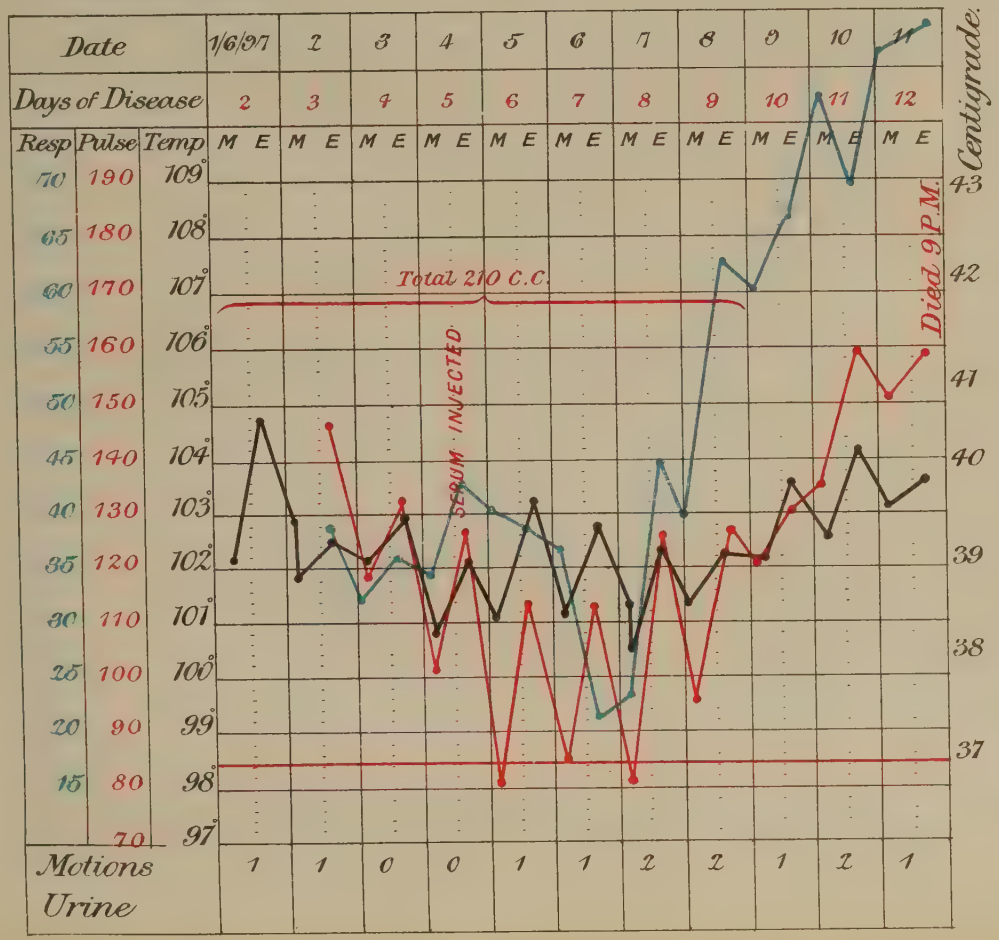

1896 TO 1897
1897 TO 1898
1898 TO 1899
Average number of deaths per week for the previous 5 years

CLINICAL CHARTS OF CASES TREATED WITH ROUX' SERUM.

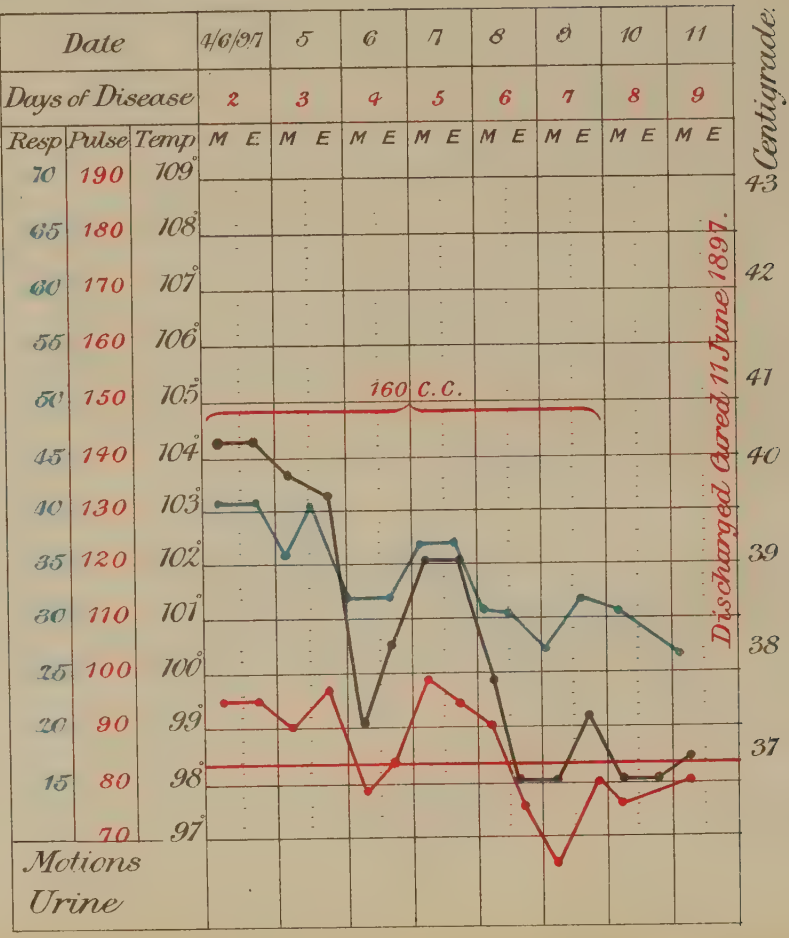
IN THE CUTCH STATE.

BY CAPT. H. D. MASON, R.A.M.C.

1. SIVJI HIRJI. Male. Aged 8.
(Brahmapuri Hospital.)



2. GODAVERIBAI. Female. Aged 30.
(Brahmapuri Hospital.)

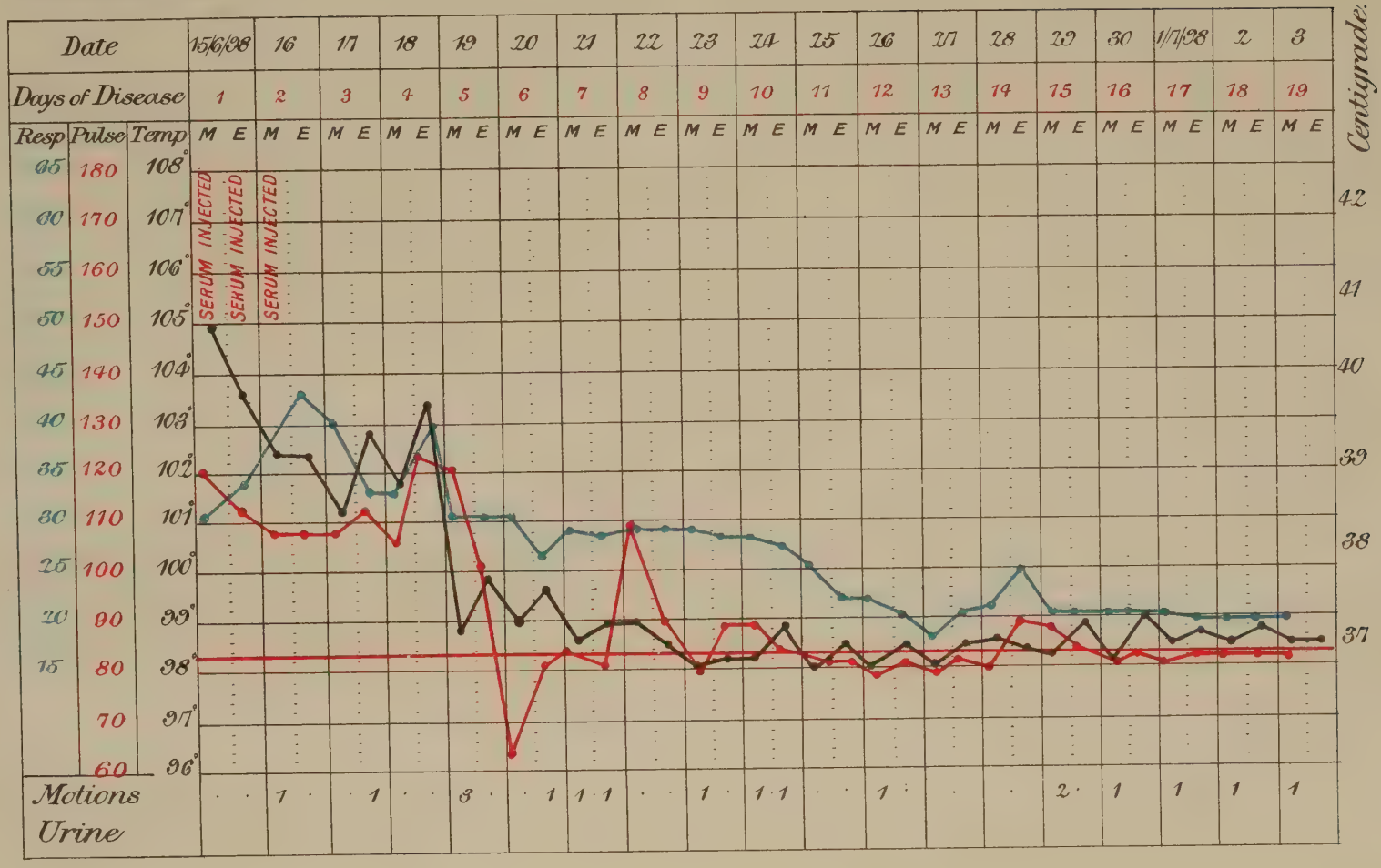


CLINICAL CHARTS OF CASES TREATED WITH ROUX' SERUM.

IN THE CUTCH STATE.

BY CAPT. H. D. MASON, R.A.M.C.

3. MUHAMMAD. Male. Aged 10.
(Muhammadan Plague Hospital.)



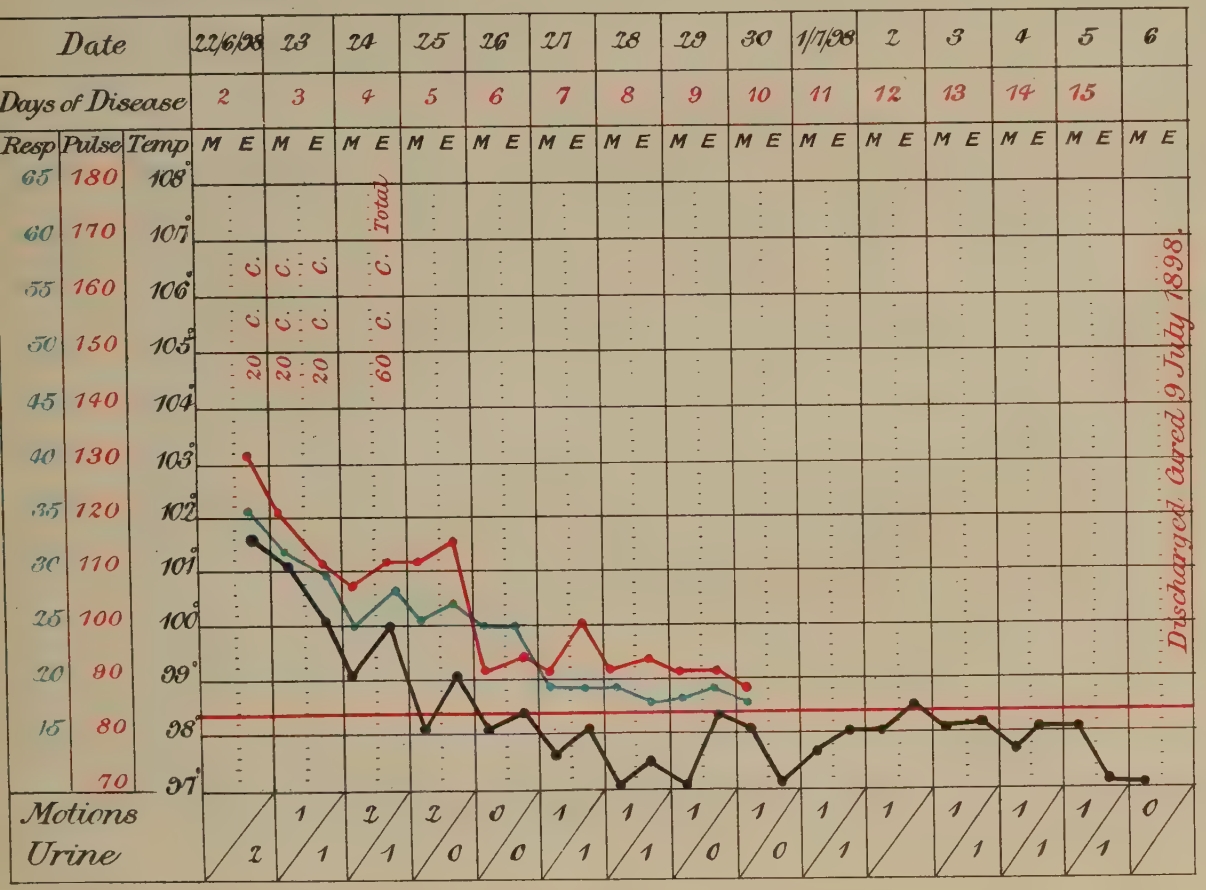
APPENDIX No. LXXXV. (See Question No. 27,049.)

CLINICAL CHARTS OF CASES TREATED WITH ROUX' SERUM.

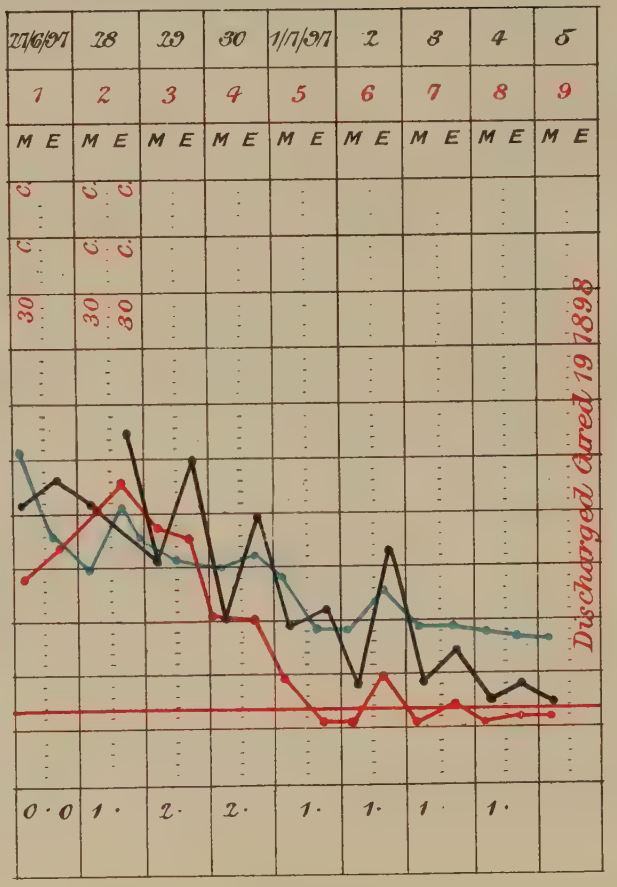
IN THE CUTCH STATE.

BY CAPT. H. D. MASON, R.A.M.C.

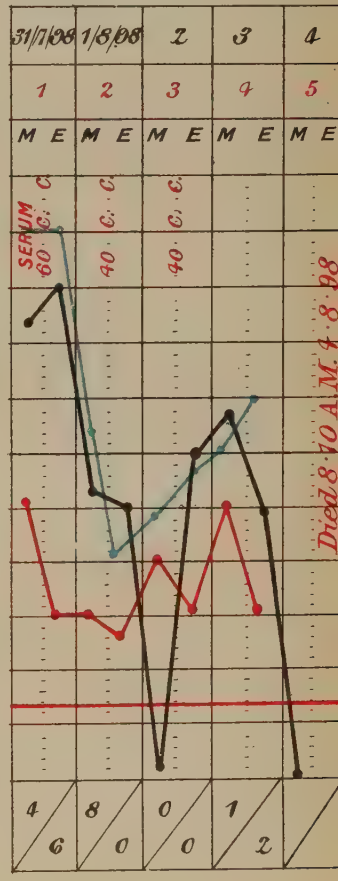
4. CONBAI OOMUAGI. Female. Aged 50.
(Brahmapuri Hospital.)


Date
22/6/98 23 24 25 26 27 28 29 30 1/7/98 2 3 4 5 6
Days of Disease
2 3 4 5 6 7 8 9 10 11 12 13 14 15
Resp Pulse Temp
M E
Discharged Cured 9 July 1898.
Motions
Urine

5. KHANAMISA. Male. Aged 11.
(Muhammadan Plague Hospital.)



6. HASSAN SULEMAN. Male. Ag
(Muhammadan Plague Hospital.)



APPENDIX No. LXXXVI.

The following abstract of the evidence, which he was prepared to give before the Indian Plague Commission, was submitted by Brigade-Surgeon Lieutenant-Colonel D. D. Cunningham, I.M.S., M.B., F.R.S., C.I.E., and as the witness was unable, through ill-health, to appear for examination, it is published as an Appendix to the Proceedings of the Commission.

I can say little regarding the clinical phenomena presented by the individuals who were supposed to be suffering from plague in Calcutta in the latter part of the year 1896. The number of suspected cases was very limited and the only ones which I saw were some men of the Shropshire Regiment, who were suffering from chronic inguinal and femoral buboes similar to those which were on record as having been of periodic occurrence in the Station Hospital for many years previous, and a case in the Howrah Hospital, which was suspected because the patient had recently returned from Bombay, but which, by observers better qualified to form an opinion regarding it than I was, was regarded as presenting a history and clinical features indicative of venereal origin.

I had, however, ample opportunity of examining the question from a bacteriological point of view, the materials at my disposal being of the following nature:—

I. Type-specimens of cultivations of plague bacilli from Bombay, supplied by Messrs. Haffkine and Hankin.

II. Type-specimens of cultivations derived from the blood of cases of disease in Calcutta and the neighbourhood and supposed by some observers to demonstrate the presence of the plague bacillus.

III. A corresponding series of microscopic preparations of blood and the growths in cultivations.

IV. Cultivations and preparations of blood personally obtained from suspected cases of disease in Calcutta.

V. Specimens of dead and moribund rats from Calcutta and cultivations and microscopic preparations derived from their organs and blood.

The materials, which were supplied by the Health Officer of Calcutta as specimens of growths of plague bacilli from the blood of cases of disease in Calcutta, consisted of cultivations of various distinct species of schizomycete organisms, but none of them presented the distinctive features, either macroscopic or microscopic, of growths of the plague bacillus, and the majority were unequivocally results of aërial contamination, as was clearly demonstrated by the results of a series of experiments in which specimens of the blood of various species of animals were exposed to the influence of contact with the air of the laboratory, in which I was working, for periods of longer or shorter duration. The following example will serve to illustrate the nature of the procedure adopted and the results obtained in these experiments:—

Specimens of blood were drawn directly from the jugular vein of a healthy fowl into two sterilised test-tubes plugged with cotton wool. One of the tubes was kept continuously plugged during a period of three weeks' duration without showing any signs of the presence of any schizomycete contamination; the coagulum contained in it never underwent any contraction, save the small amount incident on evaporation, the formed elements remained well preserved throughout, and cultivations and microscopic preparations, derived from it at the close of the period, failed to yield any evidence of the presence of any schizomycete organisms. The blood in the other test-tube was, shortly after it had been obtained, decanted into half-a-dozen sterilised watch-glasses, which remained exposed to the air for about twenty minutes and were then enclosed within a sterilised moist chamber. Decomposition rapidly set in in all the specimens and, within the course of a few days, the latter yielded abundant growths of various schizomycetes—two species being identical with and subsequently yielding pure tube-cultures of two of the so-called plague bacilli supplied by the Health Officer. The other specimen of blood was eventually exposed in watch-glasses in similar fashion and yielded like results.

The microscopic preparations of blood and of corresponding cultivations obtained from the Health Officer in no case showed any satisfactory evidence of the presence of any true plague bacilli. Certain of the preparations of blood did contain schizomycetes, but these had the form of elongated rods, and in one instance were common large aërial putrefactive bacilli. No fixative had been employed in the preparation of the specimens of blood, and consequently fragments of chromatin abounded in most of them, and may have been mistaken for schizomycete organisms. Some of the preparations, which were labelled as having been derived from the blood and tissues of a rat, which had died in consequence of inoculation of a cultivation of blood derived from a suspected case of plague, certainly did contain numerous schizomycetes, but, as these did not present the characters of plague bacilli, and as the material injected had consisted of 1 c.c. of decomposing broth, or an amount equivalent to about 300 c.c. for an ordinary human adult, neither the fatality attending the procedure nor the presence of schizomycete organisms in the blood and tissues of its victim can be regarded as of any special significance.

The microscopic preparations and cultivations of blood which I myself made were derived from the suspected cases of plague already alluded to as having been personally inspected, and in not a single instance afforded any evidence of the presence of schizomycete organisms of any kind; the specimens of blood were of perfectly normal character, and the cultivations remained, without a single exception, absolutely sterile.

There can be no question that there was a considerable amount of mortality among common house-rats for some time in certain parts of Calcutta during the latter part of 1896, and this was, not unnaturally, regarded by those who affirmed the existence of plague in the city as a strong confirmation of the correctness of their view. Very much stress cannot, however, be laid on the phenomenon of the occurrence of destructive epidemics among rodents unless the precise nature of the disease have been unequivocally demonstrated. Anyone who has had any practical experience of gardening in Calcutta must have had ample opportunity of observing the extraordinary fluctuations in the number of common palm-squirrels and Indian mole-rats — *nesocia bengalensis* — which periodically recur and which can apparently be only accounted for as the result of fatal epidemics on a very large scale. During the earlier half of the decade, 1880–89, the numbers of both these rodents was very great, and latterly so excessive as to be the cause of much damage. An abrupt and excessive decrease followed, and for some years both species were very rare; this was succeeded by a gradual increase, and by the year 1896—the last complete year which I spent in Calcutta—the numbers of both species had once more become troublesomely high. I had an experience of the actual occurrence of a similar epidemic in the forest clothing the slopes of

Mainom—a mountain in Sikkim—in the autumn of 1889, when, in the course of one day's march between the Yangong and Ralong Gompas, I was able to collect a whole series of fine fresh specimens of either *mus fulvescens* or *mus jerdoni*, which were lying dead and scattered along the track. The facts that, according to Babu R. B. Sanyal, the Superintendent of the Zoo in Alipur, a considerable mortality among house-rats is not by any means an unusual phenomenon in the early part of the cold weather in Calcutta, and the abrupt disappearance of the epidemics of field-voles which occasionally occur in this country and other parts of Europe, and which can only, apparently, be accounted for in like fashion, afford additional evidence that the mere occurrence of destructive epidemics among the rodents of a locality does not in itself afford any satisfactory evidence of the presence of plague or of conditions adapted to give rise to the disease there. The phenomenon was, however, sufficiently suspicious to demand special inquiry, and a careful examination was accordingly made of dead and dying rats obtained from the part of the town in which the epidemic prevailed. The results were not of a nature to encourage a belief that the animals were affected by plague. There were no evidences of any special glandular affection beyond the presence of a very large number of coccidia in the liver, and numerous cultivations of the blood and of materials derived from the lymphatic glands and other organs, although in many instances yielding growths of various kinds, failed to give any characteristic of the plague bacillus.

From the above data it is evident that, in so far as my experience went, there was no evidence of the existence of any disease presenting the bacteriological phenomena of plague in Calcutta in 1896.

April 19th, 1899. D. D. CUNNINGHAM.

APPENDIX No. LXXXVII.

(*See* Question No. 7269.)

Additional Note to Dr. Clemow's Evidence.

The following are the additional details of the second tance, given in my evidence, of a second attack of gue occurring in the same person:—

The patient was a Hindu, female, whose first attack plague occurred in October 1897, when she was ated in the Arthur Road Hospital. With some ficulty I succeeded in tracing her case-sheet, and nd that she was admitted to the hospital on tober 16th, 1897. She is described as aged 10 (on case-sheet in the Parel Hospital her age is given 12). There was a bubo in the left groin, involving h the femoral and inguinal regions; the swelling s deep-seated, and there was much infiltration round On two occasions the temperature rose to 105°. ere is no note in the case-sheet as to whether the bo suppurated or was incised. The temperature rve was an irregular one, varying between 98° and °. She was transferred to the Parel Hospital on vember 16th. Her case-sheet there contains no ther details. She was discharged on December 13th.

The second attack occurred in April 1898. The details of this attack have already been given in my evidence. It is noteworthy that in each attack the bubo was in the left groin. The facts of the case, given in my evidence before the Plague Commission (*see* Question No. 7261) may be repeated here, for facility of reference, and are as follows:—

Case II.—Register No. 1060.—Hindu female, æt. 12, was admitted to the Parel Hospital on April 12th, 1898, with symptoms of plague. It was ascertained that she had had a previous attack of plague five and a half months earlier; she had been treated in the Arthur Road Hospital, Bombay, and had been transferred as a convalescent to Parel in December; her first discharge from the Parel Hospital was dated 13th December 1897. The second attack in April was a severe one; there was a left inguinal bubo, from which plague bacilli were isolated. She died on 14th April, 47 hours after admission, and 55 hours after the onset of symptoms.

F. G. Clemow.

APPENDIX No. LXXXVIII.

CORRESPONDENCE WITH W. M. HAFFKINE, ESQ., AND THE GOVERNMENT OF INDIA, REGARDING THE CORRECTNESS OF FIGURES CONCERNING INOCULATION IN THE BOMBAY JAILS, SUPPLIED TO THE INDIAN PLAGUE COMMISSION.

I.

On August 14th, 1899, Mr. Haffkine wrote to the President of the Indian Plague Commission, calling attention to correspondence published in the "Times," in which it was stated that "the impression left on the minds of many" was that Mr. Haffkine's statistics regarding prophylactic inoculation "are somewhat inaccurate." He continued: "In case, therefore, you, "or any member of the Commission, entertain any "doubt, or have any difficulty as to my reports on "inoculation, I should like, as I requested several "members of the Commission in India, that an "explanation be asked from me before your Report is "made public." Upon this, the following correspondence took place:—

II.

Letter from the Secretary to the Indian Plague Commission to W. M. Haffkine, Esq., C.I.E., dated India Office, 31.8.99.

SIR,

IN reply to your letter of the 14th August, 1899, to the President, I am directed to say that if you are anxious to make any further explanations regarding the statistics given in your own reports and evidence regarding inoculation, the Commission will be glad to receive them verbally from you on September 12.

2. I am at the same time to say that certain points of difference between your own evidence and reports and the evidence of other witnesses has attracted the attention of the Commissioners. The enclosed memoranda (A. and B.) of the official figures contain particulars of these differences, and the Commissioners will be glad to receive in writing anything you may desire to convey to them before September 7th.

3. The Commissioners will be glad to learn, when you reply to this letter, whether you wish to appear before them on September 12th or not. In case you do, I am to request that you will be present at the room occupied by the Commissioners in the India Office at 2.30 p.m.

MEMORANDUM A.

STATISTICS regarding PLAGUE in the BYCULLA HOUSE OF CORRECTION, BOMBAY, in JANUARY, 1897, comparing M. HAFFKINE'S FIGURES with OFFICIAL FIGURES.*

	Non-inoculated.				Inoculated.				Names and Official Numbers of Inoculated Prisoners who contracted Plague.
	Mr. Haffkine's figures.		Official figures.		Mr. Haffkine's figures.		Official figures.		
	Cases.	Fatal Cases.	Cases.	Fatal Cases.	Cases.	Fatal Cases.	Cases.	Fatal Cases.	
To January 30th, 1897, inclusive.	15	8	14	7	3	3	0	0	*a.* 672.—Keshav Umishankar *b.* 832.—Rama Lakshman. *c.* 1,222.—Govind Pandu. *d.* 1,356.—Abdul Karim Golam Hussain. *e.* 1,336.—Ganput Babajee. *f.* 988.—Mahomed Hossein. *g.* 1,366.—Shamrao Sittaram.
Inoculations performed on January 30th, 1897.	Mr. Haffkine's figures show 183 prisoners not inoculated and 154 inoculated. The Official figures show 185 prisoners not inoculated and 153 inoculated.								
January 31st, 1897 -	2	1	2	1	1	0	0	0	
February 1st „ - -	1	1	1	1	0	0	4 (*a.* *b.* *c.* *d.*)	3 (*a.* *c.* *d.*)	
„ 2nd „ -	1	1	1	1	0	0	1 *e.*	0	
„ 3rd „ - -	0	0	0	0	0	0	0	0	
„ 4th „ -	1	1	0	0	0	0	1 *f.*	1 *f.*	
„ 5th „ - -	2	1	2	1	0	0	0	0	
„ 6th „ -	5	1	4	1	1	0	0	0	
„ 7th „ - -	0	0	1	0	0	0	1 *g.*	0	
Total January 31st to February 7th -	12	6	11	5	2	0	7	4	

* *See* App. No. LX. in this volume.

MEMORANDUM B.

HER MAJESTY'S COMMON PRISON, UMARKHADI, BOMBAY.

The inoculations were done on the morning of January 1st, 1898.

According to Mr. Haffkine's figures, at the time of inoculation, the total population of the jail was 402, of whom 201 were inoculated; the official figures† show a population of 399, of whom 199 were inoculated.

According to Mr. Haffkine's figures, among those who were in jail on January 1, 1898, 10 of the non-inoculated got plague, of whom six died, while three of the inoculated got plague, none of the cases being fatal.

According to the official figures, it appears that 20 prisoners in all got plague. Of these four non-inoculated prisoners and one inoculated prisoner should be left out of consideration as they were not in jail on January 1, 1898, while three prisoners were attacked before the inoculations were done. Of the remaining 12 prisoners who got plague nine were not inoculated, and five of them died, while three were inoculated, among whom none died.

† *See* App. No. LIX. in this volume.

III.

From W. M. Haffkine, Esq., C.I.E., to The Secretary, Indian Plague Commission, Whitehall, London, dated London, 9th September 1899.

Sir,

I have the honour to acknowledge with thanks the receipt of your letter, dated India Office, 31st August 1899, which reached me in Paris. I am glad that my letter to the President, of the 14th August, elicited the information, of which I was not aware, that there existed any discrepancy between mine and "the official" figures referring to the plague in the Byculla and Umarkhadi Jails in Bombay, though my being separated from my office records prevents me from substantiating my explanations as fully as I should desire.

The following is the analysis of the discrepancies pointed out in the appendices to your letter, so far as the materials at my disposal here permit me of analysing them.

Byculla House of Correction.

(*a.*) The number of attacks and deaths of plague to January 30th, 1897, inclusive, is uninoculated: my figures—15 and 8; "official figures"—14 and 7.

My figures represent the number of the following prisoners, whose names and particulars were supplied to me by the authorities of the House of Correction:—

Biva Bagu, prisoner, No. 977, admitted to hospital for plague on 25.1.97, died on 27.1.97.
Rama Gobin, prisoner, No. 1093, admitted to hospital for plague on 26.1.97, died on 30.1.97.
Ganu Baloo, prisoner, No. 1242, admitted to hospital for plague on 27.1.97, died on 31.1.97.
Ramgulab Rampratab, prisoner, No. 1094, admitted to hospital for plague on 27.1.97.
Narain Dondu, prisoner, No. 1359, admitted to hospital for plague on 27.1.97, died on 30.1.97.
Adamji Alibai, prisoner, No. 1349, admitted to hospital for plague on 27.1.97.
Katoo Lakmidas, prisoner, No. 1235, admitted to hospital for plague on 27.1.97.
Anton Salvador, prisoner, No. 1247, admitted to hospital for plague on 27.1.97.
Balkishna Madu, prisoner, No. 1272, admitted to hospital for plague on 27.1.97, died on 30.1.97.
Syed Junas Ali, prisoner, No. 1336, admitted to hospital for plague on 30.1.97.
Jiwa Virchand, prisoner, No. 1144, admitted to hospital for plague on 30.1.97.
Hussein Mahomed, prisoner, No. 1276, admitted to hospital for plague on 30.1.97.
Visram Jairam, prisoner, No. 1310, admitted to hospital for plague on 30.1.97, died on 5.2.97.
Valichand, prisoner, No. 1312, admitted to hospital for plague on 30.1.97, died on 3.2.97.
Ranji Jotie Pawa, prisoner, No. 1129, admitted to hospital for plague on 30.1.97, died on 1.2.97.

I am quite unable to explain, without an inquiry at the House of Correction, how, after having supplied me with the above details, which they knew were required by me for an official report to Government, the authorities could have given the above "official figures" of 14 and 7. The discrepancy should be the easier to clear up that it refers not to a mild or doubtful case, but to a case which ended fatally.

(*b.*) Same item, referring to inoculated: my figures 3 and 3; "official figures" 0 and 0.

My figures refer to the following three inoculated prisoners:—

Govinda Pandu, No. 1222.
Keishoo Umar Shunkar, No. 672.
Abdul Karim Gulam Hussein, No. 1356.

They were inoculated, together with the other 154 prisoners in the evening of the 30th January 1897. During the day of the 31st and part of 1st February, all had fever, but whereas in the others the fever disappeared or subsided towards the evening of the 1st, the above three had still high fever on the morning of the 2nd, and were taken to the hospital, where they were found suffering from painful glands. They were at once admitted as plague cases, and entered as such in the Jail Hospital book, under the date of admission. But Mr. King, Assistant Surgeon, on duty at the House of Correction, and myself, went to take notes of the symptoms of the patients, and found that:

Govinda Pandu, who had been inoculated in the left flank on the 30th January, had a painful bubo in the left axilla, which he felt since the evening of the 30th January, but *subsequent* to inoculation.

Keishoo Umar Shunkar, who had been inoculated in the right flank, had a painful bubo in the left groin, which he felt since the day of inoculation, *previous* to being inoculated; and

Abdul Karim Gulam Hussein, who had been inoculated also in the right flank, had a painful bubo in the left armpit, which he felt since the evening of the 30th January, *subsequent* to inoculation.

The details, of which I took at the time very careful notes, as they had a very great importance with regard to the question as to the time which the inoculation would take to exercise a protective effect, will certainly be found reproduced also in Mr. King's clinical notes which he kept at the time of the outbreak. The Jail Hospital tabulated registers have, I believe, a column for the date of admission of patient to the hospital; and the patients, in the Byculla House of Correction, during the outbreak of 1897, were admitted to the Plague Hospital shed only after the fact of their suffering from plague became clear, which, in some instances, was a day or two after the appearance of initial symptoms. During this undecided period the patients were kept in an observation ward; but with regard to the above-mentioned question, viz., the length of time which the inoculation required for preventing the occurrence of an attack in an inoculated individual, the date of the appearance of those initial symptoms, and not of the admission of the patient to the Plague Hospital, was of importance, and that is why I stated that Govinda Pandu, Keishoo Umar Shunkar, and Abdul Karim Gulam Hussein were attacked on the 30th January 1897, when their glands, which subsequently developed into actual plague buboes, first appeared, and not on the day when they were admitted to the plague shed.

(*c.*) Strength of jail population on 30th January 1897: my figures, 183 uninoculated and 154 inoculated; "official figures," 185 uninoculated and 153 inoculated.

Unfortunately I have not with me here the nominal list of the inoculated in the House of Correction of 30th January 1897; but I have with me a table which I made out on the 8th February 1897, together with Mr. Hall, the Superintendent of the Jail, in his office, from the jail books, and which shows that, excluding the prisoners already taken to the Plague Hospital, the numbers of inoculated and uninoculated were:—

In the evening of the 30th January 1897, when the inoculations where done, 154 and 183; on the mornings of—

January 31, 1897, 151 and 177;
February 1, 1897, 150 and 172;
February 2, 1897, 146 and 173;
February 3, 1897, 176 and 172;
February 4, 1897, 146 and 171;
February 5, 1897, 146 and 169;
February 6, 1897, 146 and 169;
February 7, 1897, 146 and 165; and of
February 8, 1897, 146 and 161.

(*d.*) January 31st, 1897, cases and deaths in inoculated: my figures, 1 and 0; "official figures," 0 and 0.

My figures refer to the prisoner Rama Luxman, No. 832, inoculated on 30th January 1897 in right flank, who was admitted to the plague shed on 1st February 1897, but had a painful bubo in the right axilla since the previous day. On account of the reasons above mentioned under paragraph (*b*) he is entered in the Jail Hospital register under the date of the 1st February 1897, and in the laboratory books under the date of January 31st. These details will, undoubtedly, be found also in Mr. King's clinical notes, as all my rounds in the jail were made in the company of the jail authorities, and my information obtained through their own inquiry made in my presence.

(*e.*) February 1st, cases and deaths in inoculated: my figures, 0 and 0; "official figures," 4 and 3.

The explanation of the discrepancy is given in the above paragraphs (*c.*) and (*d.*).

(*f.*) February 2nd, cases and deaths in inoculated: my figures, 0 and 0; "official figures," 1 and 0.

The official figures refer to prisoner Ganpat Babajee, No. 1336, inoculated on 30th January 1897, in the evening.

He was discovered at 5 p.m. of the 2nd February, with a temperature of 102° Fahr., and was admitted to the plague shed; but next morning was free from fever. During the day of the 3rd February he complained, however, of pain in the lower jaw, but no swellings

could be found there; and the pain disappeared during the day of the 4th.

In informing me about this case, the authorities of the jail stated that Gunpat Babajee suffered from 12 hours of fever, that certainly it could not have been a case of plague, but that he had been entered into the books as admitted into the plague shed, and that it was impossible to make any corrections there.

(*g.*) February 4th, cases and deaths uninoculated: my figures, 1 and 1; "official figures," 0 and 0.

My figures refer to prisoner Mahomed Hussein, No. 988, who undoubtedly, by an error, is placed in the "official figures" for the same date among the inoculated.

Unfortunately, as mentioned above, I am unable to find among the notes I have with me here, a nominal list of the prisoners who were inoculated in the Byculla Jail on the 30th January 1897. Such a list does exist in the Laboratory Office in Bombay, and the fact of inoculation must also have been entered in the jail ticket of the inoculated prisoners. But I cannot conceive any possibility of a mistake on my part on the point in question. I have with me here a copy of my report, dated 15th February 1897, to the Secretary to the Government of India in the Home Department. In the list of plague occurrences in the jail it is stated, referring to the 7th of February:—"On the 5th day "after inoculation there were, amongst 171 non-inocu- "lated, 1 case which proved fatal: amongst 146 inocu- "lated, nil." When sending the above report to the Home Secretary, I sent a copy of it also to Mr. C. P. H. Snow, I.C.S., Municipal Commissioner of Bombay, who asked also for a report from Lieut.-Colonel Waters, I.M.S., the medical officer of the jail. The report of the latter was communicated by the Municipal Commissioner to the Bombay papers, and was reproduced by them, and I remember distinctly that it was identical with mine.

In the experimental note book which I have with me here, on page 696, the following details are given under date 5.11.97.

"(3.) Mahomed Hussein (not inoculated). Fever since yesterday, 4.11.97. Now unconscious, cannot answer questions."

"6.11.97. Still unconscious. No glands."

"7.11.97. Since yesterday evening painful and swollen gland in right femoral region."

"8.11.97. Mahomed Hussein died at 3.30 p.m."

On page 702 of the same note book, in a table made by Mr. Hall, Superintendent of the Jail, Mr. King and myself, in the office of the jail, under the date of 4.11.97, the space for inoculated shows a blank, that for the uninoculated shows one case fatal.

In another table referring to the attacks of plague in vaccinated against small-pox, and which I made out on the 8th February 1897, also with the assistance of the authorities of the jail, Mahomed Hussein, re-vaccinated on 4th March 1897, is shown also among the uninoculated; and at no moment, although the details of the outbreak were for many weeks examined and re-examined time after time, has it been mentioned that a fatal case of plague among the inoculated occurred after the first three fatal cases (those of the 30th January). It should be no difficulty whatever to find out from the jail authorities how it occurred now that the prisoner in question has been stated to be an inoculated.

(*h.*) Byculla House of Correction, 4th February; cases and deaths in inoculated: my figures, 0 and 0; "official figures," 1 and 1.

Vide above, paragraph (*g*).

(*i.*) February 6th, cases and deaths in uninoculated: my figures, 5 and 1; "official figures," 4 and 1.

My figures include prisoner Mummo Ibrahim, No. 993, who, although admitted to the plague shed on 7th February and shown in the "official figures" under that date, had fever already since the 6th.

(*j.*) Same date, cases and deaths in inoculated: my figures, 1 and 0; "official figures," 0 and 0.

My figures refer to prisoner Shamram Sittoram, No. 1,366, who, although admitted to the plague shed on 7th February, and shown in the "official figures" under that date, had fever already since the 6th.

(*k.*) The difference under the date of February 7th, is explained in the above paragraphs (*i.*) and (*j.*).

Summary of the Details referring to the Byculla Jail.

It will be seen from the above that the important differences between mine and the "official figures" consist in—

(1) Prisoner Mahomed Hussein being referred to in the "official figures" as an inoculated, which seems to me to be undoubtedly a mistake, and which can be verified by reference to the list of inoculated; and

(2) In the date under which prisoners Govinda Pandu, Keishoo Umar Shunkar, and Abdul Karim Gulam Hussein are referred to, and which in my table represents the date of the outset of the disease, while the "official figures" refer to the date of their admission to the plague shed and of entry in the Plague Hospital register.

The Umarkhadi Common Jail, Bombay.

(*a.*) Paragraph 1 of your Appendix: only 199 prisoners were inoculated, as the "official figures" rightly show. I do not remember having stated that 201 were inoculated. If I did, my mistake was probably due to the fact that the number of persons inoculated in the Umarkhadi jail on the 1st January 1898 was, indeed, 201, but two of them were Major Collie, I.M.S., the medical officer of the jail, and Mr. N. M. Wadiagaonkar; the latter, I believe, is the Assistant Surgeon on duty in the jail. But I believe it to be likely that I have not stated that 201 prisoners were inoculated; that that number was concluded, by the author of your appendix, from my stating that the total number of prisoners brought to the courtyard on the 1st January 1898 was 402, and that every second man or woman were inoculated. The author, taking the half of 402 as having been inoculated, did not consider that I have added that two of the prisoners who happened to be second in a pair, did not choose to be inoculated, which brought the number of 201 down to 199, and that of uninoculated to 203. In any case, it is certain, from the nominal list of the inoculated on that occasion, which I have here with me, that the number 199 is the correct one.

On the other hand, the number of uninoculated was by three higher than that mentioned in the "official figures" (203 instead of 200), and the total was 402, as I have stated. This is apparent from the nominal list, with all the particulars of the uninoculated prisoners present in the jail on the 1st January 1898, which list I have also with me here. The list shows in all 216 names, of whom six, viz.,—

Mahadu Kondaji	- -	Prisoner No. 7
Kassamkhan Amer Mahomed		,, No. 127
Kissan Hari	- -	,, No. 214
Hari Bahaji	- -	,, No. 257
Lenna Penna	- -	,, No. 306 and
Jiwee, wife of Peera	-	,, No. 42

were released the same morning while we were doing inoculation, five were on the sick list, suffering from various diseases, and kept under conditions different from those of the bulk of the prisoners, viz.:—

Kasmat Waman Lele, prisoner No. 205, suffering from curvature of spine since 26.11.97;

Kameedali Taigali, No. 183, down with an abscess since 17.11.97;

Lakmanji Mathabhai, No. 225, down with an abscess since 3.12.97;

Mahomed Walikhan, No. 83, suffering from chronic diarrhœa since 12.10.97; and

Koudia Siwa, No. 120, suffering from ague since 3.12.97;

and lastly, two were isolated in the plague ward, viz.:—

Moolabux Rahimbux, No. 151, attacked with plague on 31.12.97.; and

Arjuna Raji, No. 215, attacked on same date.

This leaves 203 uninoculated, who lived under the same conditions and remained exposed to the same chance of infection as the 199 inoculated.

(*b*) Paragraph 2 of your appendix, referring to the Umarkhadi Jail. In the list of 216 uninoculated prisoners who were present in the jail on the day of inoculation (including the plague patients who were still alive), the following 12 are shown to have been attacked with plague:—

Molabux Rahimbux, No. 151, attacked on 31.12.97, died on 1.1.98.

Arjuna Raju, No. 215, attacked 31.12.97, died 6.1.98.
Booda Hanma, No. 13, attacked 1.1.98, died 4.1.98.
Andrew Menuse, No. 278, attacked 2.1.98, recovered.
Ramjan Elahibux, No. 186, attacked 17.1.98, died 22.1.98.
Sadu Rama, No. 147, attacked 18.1.98, recovered.
Sheik Hussein Sheik Baban, No. 33, attacked 18.1.98, died 20.1.98.
Govind Rama, No. 65, attacked 20.1.98, recovered.
Bhori Sakaram, No. 163, attacked 20.1.98, recovered.
Karimkhan Azamkhan, No. 283, attacked 24.1.98, died 27.1.98.
Maruti Babaja, No. 35, attacked 24.1.98, died 26.1.98.
Girder Motiram, No. 104, attacked 30.1.98, died 3.11.98.

But there must have been a 13th fatal case of plague in the jail, not mentioned, among the 216 prisoners, as he was already dead on the 1st January 1898, when the inoculations were done. This results from the following: the above list of 12 patients begins with two cases of plague which occurred on the day before the inoculation, viz., on the 31st December 1897, and which were still alive at the time of inoculation. These were:—

Molabux Rahimbux, No. 151, attacked on 31.1.97, died on 1.1.98, and
Arjuna Raju, No. 215, attacked 31.12.97, died on 6.1.98.

On the 31st of December of 1897 I was in Poona, organizing with Lieut.-Colonel McConaghy, I.M.S., then Civil Surgeon there, two inoculation stations; and there must have occurred in the Umarkhadi Jail a fatal case a few days before the 31st December, as before leaving for Poona I had arranged with Dr. Leon, on plague duty in the Umarkhadi District of Bombay, that should another case occur in the jail he should wire the information to me to Poona, and I would come down to Bombay to inoculate the prisoners personally. It was on the occurrence of those two subsequent cases of the 31st of December that I received the wire from him, upon which I indeed returned to Bombay and inoculated the prisoners on the New Year's Day. Dr. Leon is in London, serving in the Paddington Infirmary, at Harrow Road, and I wired to him on the 6th instant asking for his notes on the plague cases at Umarkhadi; but, as will be seen from the attached reply, they could not be got immediately. I shall forward further details to the Commission if I get them.

The inoculations were done very early, about 6 or 7 a.m., on the 1st of January 1898, and it was because of this circumstance that I did not exclude from the list the case of Booda Hanma, No. 13, attacked in the course of that day. The "official figures," mentioning only the attacks in those who were still alive and present in the jail on the 1st January 1898, omit the case of the prisoner who died a few days before (including him possibly among the four non-inoculated prisoners attacked who were not in jail on 1st January 1898); on the other hand, I presume, they thought it safer to reckon the case which occurred on the 1st of January among those that took place before inoculation, although, as it appears from the numbers of 199 inoculated and 203 uninoculated, Booda Hanma must have been in the courtyard, in the ranks of prisoners paraded before us, while the inoculation was done.

The above completes the analysis of the discrepancies. I can only regret that those discrepancies did not come before us while in Bombay. It will be seen from the above explanations that they are of a nature easily to be substantiated by reference to the official books and by a few explanations from the officials as to details not recorded in the tabulated official books.

In case the Commission finds it desirable to have any further data as to the observations in the jails, or any others on which the evidence given by me bears, I shall hold myself in readiness to meet them, if invited, on the 12th instant.

I take this opportunity only of drawing their attention to a circumstance which may possibly render less paradoxical the table of mortality in Karachi among the inoculated and uninoculated Khoja children with regard to which I was questioned by the Commission in Bombay.

I did not know at the time that Dr. Kaka, the Health Officer of Karachi, who carried out the inoculations there, had not inoculated children in arms and under one year of age, on the ground that they do not take plague (which is, indeed, to a very great extent, the case). On referring to the vital statistics it will be seen that the normal mortality among children in India falls chiefly among those under one year; I believe it is often four, five, even six times higher than the mortality among children above one and up to five years; and when compared with the total mortality in a community that of children below one is by far the largest item. By leaving children below 1 in the group of the uninoculated, Dr. Kaka freed the inoculated from that largest item of the normal mortality. Considering further that the observations refer to a time subsequent to a very severe epidemic, which must have killed off a number of sickly individuals in the groups above one year; the absence of deaths amongst the inoculated from general causes during the six months under observation seems to me more comprehensible.

In giving the above considerations, I, of course, do not mean to take upon myself the responsibility for the Karachi observations, as I have never been in Karachi and am unacquainted with the conditions under which the observations were made. At the same time, in the inquiries made in Bombay, I could not detect the least reason for doubting the perfect good faith and veracity not only of the officers who carried out the observations but of the inoculated community itself amongst whom the observations were carried out.

Enclosure in No. III.

Paddington Infirmary,
Harrow Road, W.,
September 7th, 1899.

DEAR HAFFKINE,

I WENT to Portland Place to-day and looked for the notes of the plague cases, but I am sorry to say I could not find them. Our house is in the hands of the builders and everything is stowed away all over the place, so it is quite possible that they will turn up later on, when I will let you have them. Kind regards from,

Yours sincerely,
JOHN P. LEON.

IV.

On the 12th September 1899, the Commission informed Mr. Haffkine that they did not propose to request him to appear to put any oral statement before them, but that they were making inquiries by telegram from the Government of Bombay as to whether Mahomed Hussein, Prisoner No. 988, in the Byculla Jail, Bombay, was or was not inoculated in January 1897. The Commissioners also desired Mr. Haffkine to ascertain by telegram whether Mahomed Hussein appears in the records in his laboratory as inoculated or not. A telegram was accordingly sent by the Commission to the Government of Bombay through the Government of India, and Mr. Haffkine also telegraphed to his laboratory in Bombay asking that the required information might be sent by telegram through the India Office.

V.

The following telegram was received from the Government of India through the India Office on the the 20th September, 1899:—

"Your telegram, 12th September, Prisoner 988, Mahomed Hussein, Byculla Jail, was not inoculated."

INDICES TO THE EVIDENCE,

ALSO

GLOSSARY, MAPS, AND SUMMARY

OF THE

REPORT AND APPENDICES.

VOL. IV.

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GLOSSARY

OF

VERNACULAR TERMS

USED IN THE

EVIDENCE AND APPENDICES.

ABKARI - - - The department entrusted with the administration of the law relating to the manufacture and sale of intoxicating drugs and liquor.

ACHARJ - - A sect of Brahmans who assist in the performance of the obsequies of the dead.

ABRO - - - The name of a caste.

ADHA KUMBH - An important religious festival held on particular occasions at Hardwar, in the North-Western Provinces, and attended by very large numbers of Hindus.

AGARIA - - The name of a caste.

AHIR - - - The name of a caste the members of which are principally cow-herds and agriculturists.

ALARI - - The name of a caste of metal moulders.

AMALDAR - - A man in authority, a manager, an agent; in Mysore, the chief native officer of a taluka (*q.v.*).

AMAWAS - - The conjunction of the sun and the moon; a Hindu religious festival held on the day of the new moon.

AMIL - - - The name of a caste.

ANAWLA - - The name of a caste.

ANNA - - - A coin of the Government of India currency, the sixteenth part of a rupee (*q.v.*).

ARJA - - The name of a caste.

ATIT - - - The name of a caste of Hindu ascetics and holy men.

AWALKARKUN - The chief karkun or clerk in a Mamlatdar's (*q.v.*) office.

AYAH - - - A lady's maid or female attendant on children.

BABU - - - A writer or clerk.

BAID (BAIDYA) - A medical practitioner who follows the Hindu system of medicine.

BAIRAGI - - The name of a class of Hindu ascetics who live on alms.

BAJANIA - - The name of a caste.

BAJRA (BAJRI) - A cereal grain or millet, the *penicillaria typhoidea*.

BAKALI - - The name of a caste.

BAKKAL - - A trader or shopkeeper.

BALUCHI - - A native of Baluchistan.

BAN - - - A kind of grass (*Saccharum munja*) of which the fibre is used for making ropes.

BANDAR - - Dock, jetty, wharf, landing place.

BANIA - - - *See* Banniah.

BANJARA - - The name of a caste of which the members for the most part live on the profits of the carriage of goods from place to place on bullocks belonging to them.

BANNIAH - - The name of a caste of which the members are for the most part traders in grain and provisions, money-lenders, and small shopkeepers; the word is often used to signify Hindu shopkeepers or money-lenders without special reference to caste.

BARA - - - Great, important, chief, as "Bara Daktar Sahib," "the Chief Medical Officer."

BARIA - - The name of a caste.

BARODIA - - The name of a caste.

BAROT - - The name of a caste.

BATSHIRERJWAR - A fever said to be found in Bengal, which "is brought on by swelling or subsequent enlargement of the glands . . The seasons of the fever are generally the winter and rains." (Vol. I., p. 521, s. 11.)

BAZAR - - Market place, quarter in which the shops of traders are situated.

BEEDEE. - - The name of a caste.

BETEL NUT - - The fruit of the *Areca catechu*, which, prepared with lime and other ingredients, is wrapped up in the leaf of the *piper betel* and chewed.

BHAGAR - - The name of a caste.

BHANDARI - - The name of a caste in Bombay of which the members are for the most part toddy drawers; a steward.

BHANG - - - An intoxicating preparation made for drinking from the leaves of the Indian hemp (*cannabis sativa*).

BHANGI - - A sweeper; the name of a caste of which the members are employed as sweepers and scavengers.

BHANSALI - - The name of a caste.

BHARBHUJA - - A grain parcher; the name of a caste of grain parchers.

BHATELI - - The name of a caste.

BHATIA - - The name of a caste of which the members are largely engaged as traders, shopkeepers, and clerks.

BHAVSAR, or BHAVSAT The name of a caste.

BHIKSHUK - - A beggar, or mendicant, who lives on alms.

BHIL - - One of the aboriginal hill tribes in the north of the Deccan.

BHISTI - - - A water carrier.

BHOI, BHOIJI - The name of a caste of which the members are employed chiefly as fishermen and bearers or porters.

BHUTIA - - - A native of Bhutan.

BHYA - - In Southern India signifies a man from Northern or Central India outside Bombay.

BIGAREE (BEGARI) - An unskilled labourer, originally a forced labourer.

BORAH - - A Musalman community of traders and bankers.

BRAHMAN - - One of the sacerdotal caste of the Hindus, who now engage not only in priestly duties but also to a large extent in professional and business employments.

BRAHMO SAMAJ - The new theistic church in India, which, while not accepting as its basis of belief any book written by man, respects the Vedas, recognises only one Supreme God, and discourages the observance of ceremonies, idolatry, and caste restrictions.

BUDMASH (BADMASH) - A man of bad character, a disreputable person, a criminal.

BUND - - A masonry or earth dam or bank generally used for the confinement of water in tanks and other suitable places.

BURUD - - The name of a caste of basket makers.

BUSTEE (BASTI) - A group of thatch and mud houses, usually with a tank in the centre, constituting a native village; a group of thatch and mud houses situated in the midst of the better class of houses in Calcutta.

CHABUTRA - - A raised platform, usually of masonry.

CHADDAR - - A sheet or cloth; the cloth worn by women as an outer wrap or cloak, covering the head and reaching to the ankles.

CHAMAR - - The name of a low caste of which the members work in hides and leather and are engaged in menial service.

CHAMADIA - - *See* Chamar.

CHANDAN - - Sandal-wood.

CHANDIO - - The name of a caste.

CHANNA - - Cakes of cowdung, used as fuel.

CHAPPAR - - A hut or shelter of thatch or grass.

CHAPPATTI - - A thin cake of flour and water without leaven, baked or toasted over an open fire.

CHAPRASI - - A messenger or orderly of a Court or officer, who wears on a belt a "chapras," or plate, as a mark of his office.

CHARI - - The stalks of bajra (*q.v.*) or jawar (*q.v.*) used as fodder or as thatching material.

CHARPOY - - A native bed, usually having strings stretched inside a wooden frame.

CHATRI - - *See* Khatri.

CHATTI - - An earthen pot generally used to hold water.

CHAUDRI - - A headman; the head or representative of a caste, profession, or trade, in a town, or of a gang of coolies; in Bombay, the assistant to the Patel (*q.v.*) in a village.

CHAUHAN - - The name of a clan of Rajputs.

CHAUK - - A square or courtyard in a town or house.

CHAUKI - - A police or customs station, a guard's post.

CHAUKIDAR - - A watchman, a village servant employed to perform duties of watch and ward and other miscellaneous duties, a policeman.

CHAVDI - - The public meeting place or hall of a village.

CHAWL - - A house let in tenements, a large building divided into small rooms let separately.

CHIMBA - - The name of a caste of which the members are generally washermen.

CHINA - - A variety of millet (*panicum miliaceum*).

CHIPPI, CHHIPA - The name of a caste of which the members for the most part are engaged in the business of stamping patterns on cloth.

CHIT - - A memorandum, note, or letter.

CHOKRA - - A small boy, a youth.

CHOLAM - - A grain of the millet kind (*sorghum vulgare*).

CHUA - - - A sort of grain, amaranth.

CHUNAM - - Lime-cement, used for making cement floors, &c.

CHUPPAR - - *See* Chappar.

COIR - - Cocoanut fibre, used for the manufacture of mats, ropes, &c.

COLLECTOR - - The chief civil officer of a District or administrative territorial unit.

COLLECTORATE - A district or subdivision of a Province, under the charge of a Collector or Deputy Commissioner.

CONJEE - - Water strained after boiling it in rice or sago, &c.

COOLIE - - An unskilled labourer.

CRORE - - Ten millions, a hundred lakhs (*q.v.*).

CUTCHA - - *See* Kachcha.

CUTCHEE, CUTCHI - A native of the Cutch State.

DABGAR - - The name of a caste of which the members generally are workers in leather.

DADU - - The local name at Hardwar for a cold wind that blows down the Ganges valley at night.

DAHGAR - - The name of a caste.

DAHIWALA - - A seller of curdled milk or curds.

DAK BUNGALOW - A rest house maintained by Government for the use of travellers.

DAKHAUTI - - A Hindu festival.

DAL - - The pea of *mung* (*phaseolus mungo*), *urad* (*dolichos pilosus*), *arhar* (*cajanus flavus*), and some other pulses; dal water is water in which dal has been boiled.

DANDI - - A litter, a kind of palankin.

DARBAR - - The Government of a Native State.

DAROGHA SAFAI - An inspector or overseer of the conservancy establishment of a town.

DARZI - - A tailor.

DASEHRA (DASAHRA) An important Hindu festival, celebrated in the month of Asvaj (September—October), at which the victory of Rama over Ravana is commemorated.

DAXINI - - *See* Deccani.

DECCANI - - A native of the Deccan, or table land between the Eastern and Western Ghats (*q.v.*).

DEGCHI - - A pot or caldron.

DEPUTY COMMISSIONER. An officer in non-regulation Provinces corresponding to the Collector (*q.v.*) in regulation Provinces.

DESH - - - In Bombay, the open country as opposed to the mountainous country; a desh village therefore is a village on the flat open Deccan as opposed to the Konkan, or hilly country.

DHAI - - - A wet nurse, an accoucheuse.

DHAN - - - Rice in the husk.

DHANGAR - - An owner of flocks and herds, the name of a caste of which the members generally are shepherds.

DHARAMSALA - - A building set apart by Hindus as a free rest house for travellers and pilgrims.

DHARALA - - Literally "the arm-bearing class," applied in Gujarat to Rajputs, watchmen, and others who bear arms.

DHEBRA - - The name of a caste of which the members generally are sellers of dried fish.

DHEBARWAD - The quarter of a town occupied by Dhebras.

DHERS - - The name of a low caste in Gujarat which corresponds to the Mahar (*q.v.*) caste in the Mahratta country. The members of the caste are tanners and do menial service generally.

DHOBI - - A washerman.

DHOBI GHAT - A place in a stream, tank, or other place where water is available, used by dhobis for the washing of clothes.

DHODIA - - The name of a forest tribe of low-caste Hindus.

DHOTEE (DHOTI) - A loin cloth generally worn by Hindus, carried round the waist and between the legs and fastened behind.

DIWALI - - A festival in honour of Lakhshmi, the Goddess of Fortune, celebrated by the Hindus in mid October; during this festival houses are generally illuminated and gambling prevails.

DIWAN - - A minister, a chief officer of a Native State.

DEHLA - - A courtyard with houses round it.

DELI - - - *See* Dehla.

DISTRICT - - *See* Collectorate.

DOM, DOME - The name of a low caste of Hindus, who have no objection to touching corpses and are employed to assist at post-mortem examinations and in connexion with funerals as corpse bearers, &c.

DOOBAR - - The name of a caste.

DOOLY (DULI) - A native litter, an ambulance.

DUBASH - - A stevedore.

DUBLA - - The name of a Gujarat forest tribe.

DURZI - - *See* Darzi.

DURWAN - - A doorkeeper.

FAKIR (*fem.* FAKIRIN). A mendicant who leads a "holy life."

GACHERIA - - The name of a caste.

GADI - - The name of a caste.

GAIKWAR - - The ruler of the Native State of Baroda.

GAIKWARI - - Belonging to, or a native of, the Native State of Baroda.

GALA - - - The name of a caste.

GALI - - A narrow passage between two houses, a drain.

GANCHI - - *See* Ghanchi.

GANESH - - The Hindu God of Wisdom, usually represented with an elephant's head.

GANJA - - An intoxicating preparation made for smoking from the hemp plant (*cannabis sativa*).

GAOLI - - A cowkeeper; the name of a caste of which the members generally are cowherds.

GAON - - A village.

GARHWALI - - A native of Garhwal, a district of the North-Western Provinces situated in the Himalayas.

GARI - - - A wheeled conveyance, a carriage.

GARODA - - The name of a caste.

GAULI, GAWLI - *See* Gaoli.

GHANCHI - - The name of a caste of oil pressers.

GHANCHIWAD - The quarter of a town occupied by Ghanchis.

GHARA - - An earthern water pot.

GHARRIWALA - The driver of a conveyance for hire. *See* Gari.

GHAT - - - A pass or steps over mountains or down to or across rivers. Burning ghats are places generally on the banks of rivers at which the dead are burnt. Dhobi ghats are places used by washermen for the washing of clothes. The Ghats (the Sahyadri Mountains or Western Ghats) are the most important range of mountains in the peninsula of India proper, this wall of "stairs" or passes to the inland plateau running along the West coast from the Tapti River to Cape Comorin parallel to the sea at a distance of from 40 to 50 miles from it, with an average elevation of about 1,800 feet, though single peaks rise to double that height.

GHEE (GHI) - Clarified butter.

GHORAWALA - - A groom, a sais.

GIRANA - - A section of the Brahman caste.

GIRASIA, GIRASIA MAULE SALAM. The name of a class of people in Gujarat who have land grants and certain special rights and privileges, granted in commutation of a form of blackmail formerly levied by them.

GOANI - - A native of Goa.

GODOWN - - A warehouse or storeroom.

GOHAL - - The name of a clan of Rajputs in the Cutch State.

GOLA ROG - - Literally "the disease with a lump or bubo," the name by which the endemic plague of Kumaun is locally known.

GOLA - - The name of a caste of which the members are generally rice-pounders.

GOLAWAD - - The quarter of a town occupied by Golas.

GOONDLEE - - The name of a caste of religious musicians.

GOSAIN - - - A Hindu religious mendicant or ascetic.

GOSHA - - Retirement, privacy; the seclusion of women.

GOWLI - - *See* Gaoli.

GRAM - - - Pulse, the product of the *Cicer arietinum*.

GRANTH - - The Scriptures or Sacred Book of the Sikhs.

GRANTHI - - A reader or reciter of the Granth, a Sikh priest.

GUJAR - - The name of a caste in Upper India, of which the members are chiefly engaged in agriculture and the keeping of cattle; in the Mahratta country the name is used to signify a member of a caste of Gujarat money-lenders and traders.

GUNDI (GANDI - A druggist.

GUNNY BAGS - Rough bags made from jute for the storage and transport of grain and merchandise.

GUR - - Unclarified sugar, the juice of the sugar cane boiled into cakes.

GUSAI - - *See* Gosain.

HAJJAM - - A barber.

HAKIM - - - A native medical practitioner, as a rule a follower of the Yunani system of medicine.

HALA - - The name of a clan of Rajputs in the Cutch State.

HALALKHORES - A term used in Bombay for scavengers.

HALDI - - Turmeric.

HALWAI - - A maker and seller of sweetmeats.

HAMAL - - A porter.

HAVILDAR - - A sergeant in the Army or Police, a subordinate officer in a position of trust.

HEADMAN - - A person of influence in his village caste or community who represents his fellows. *See* Lambardar, Patel.

HOLI - - An important Hindu festival, held at the approach of the vernal equinox.

HOM - - - Sacrifice or oblation.

HOOKAH - - - An Indian tobacco pipe, in which the smoke is passed through water.

HUKUM (HUKM) - A command or order.

ID - - - A Muhammadan religious festival.

IDGAH - - A platform, sometimes screened by a wall, used for the assembly of Muhammadans for devotion at the Id festival, and occasionally at other times.

ILAKA - - - A tract of country, an area of jurisdiction; in Bombay signifies one of the three Presidencies.

JADEJA - - The name of a clan of Rajputs in the Cutch State, the members of the clan being of the same caste as the Rao (*q.v.*) or ruler of the State.

JAGIR - - - A grant of land or revenue made by Government, an estate.

JAIN - - The name of a sect which is an offshoot of Hinduism, of which the followers lay particular stress upon the sanctity of animal life of all kinds.

JAMAAT - - - An assembly, a community.

JAMADAR - - A superior officer in the Army or Police.

JANJAM - - The priestly division of the caste of the Lingayats (*q.v.*).

JAT (*fem.* JATNI, JATTIWEE). The name of the most important caste of agriculturists in the Panjab and Northern India.

JAUHARI - - A jeweller.

JAWAR - - The Indian millet (*sorghum vulgare*).

JEMADAR - - *See* Jamadar.

JHALA - - The name of a clan of Rajputs in the Cutch State.

JHIL - - A swamp or shallow piece of water.

JHINGORA - - A kind of grain, a variety of panicum.

JHINWAR (*fem.* JHINWARI). The name of a caste of which the members are principally fishermen and carriers.

JHOPDI (JHOMPRI) - A grass or thatch hut or shed.

JINGAR - - The name of a caste of which the members generally are saddle makers.

JOLAHA - - *See* Julaha.

JOWARI - - - *See* Jawar.

JULAHA - - The name of a caste of which the members are for the most part weavers.

KABIRAJ - - *See* Kaviraj.

KACHCHA - - The opposite of pukka (*q.v.*); rough, temporary, uncooked, unbaked: thus, "a kachcha bandobast" means "a temporary arrangement," "kachcha bricks" are "sun-dried bricks," and "kachcha houses" are "houses built of earth and sun-dried bricks."

KACHCHI - - A native of the Cutch State.

KACHCHIA - - The name of a caste of which the members generally are glass workers.

KAHAR - - - The name of a caste of Hindus who act as water carriers, dooly bearers, &c.

KAHTRAI - - The name of a caste. *See* Khatri.

KAKA - - - The name of a caste.

KALAL - - The name of a caste of which the members distil and sell liquor.

KALI - - - The Hindu Goddess of Death.

KAMAGAR - - The name of a caste.

KAMIN - - A menial servant, a person who performs customary services for a village community.

KANDOI - - The name of a caste.

KANEDA - - A small swelling or bubo.

KANKAR - - A coarse limestone, found chiefly in nodules in the soil of many parts of India, and used for road making, &c.

KANSARA - - The name of a caste of which the members generally are brass workers.

KANUNGO - - A subordinate supervising officer of the revenue establishment employed for the maintenance of village agricultural records.

KARANJKIR - - The name of a caste of saddlers.

KARBI - - The stalks of jawar (*q.v.*) or bajra (*q.v.*) used as thatching material as well as for fodder.

KARI (*pl.* KARIAN) - A rafter.

KARKUN - - A clerk.

KARNAM - - A village accountant.

KASAR - - *See* Kansara.

KATNI - - Lime exported from Katni.

KAVIRAJ - - A Bengali native medical practitioner who follows the Hindu system of medicine.

KAYASTH - - The name of a caste of Hindus who are usually clerks and accountants.

KHALASI - - A native sailor.

KHALIFA - - The name of a caste.

KHALPA - - The name of a caste.

KHAN - - In Bombay, an inn.

KHARWA - - The name of a caste of fishermen and sailors.

KHATI - - The name of a caste.

KHATIK - - The name of a low caste of which the members support themselves principally by keeping pigs and poultry.

KHATRI - - The name of a caste of which the members are money-lenders, bankers, and traders; they also own land, though they do not, except very rarely, cultivate it themselves.

KHETRI - - *See* Khatri.

KHOJA - - An influential and well-to-do section of Muhammadans, who are merchants and traders; the majority follow the spiritual leadership of H.H. the Aga Khan of Bombay. In Karachi there are two sects, of which one, the Panjabhai sect, recognises the authority of the Aga Khan, and the other, the Pirai sect, does not recognise his authority.

KHUD (KHAD) - A hillside, a steep place, a declivity.

KHUDABADI - - The name of a caste.

KOLI - - The name of a low caste of which the members are generally fishermen or cultivators.

KOMATI - - The name of one of the principal cultivating castes in Madras.

KONI - - A kind of grain, a variety of panicum.

KONKANI - - A native of the Konkan, the districts south of Bombay, between the Ghats (*q.v.*) and the sea.

KOONBI - - *See* Kunbi.

KOS - - A measure of distance, generally about two miles.

KOTWAL - - A superior officer of police, an officer in charge of a police station; in the Central Provinces, a village official.

KULKARNI - - A village accountant.

KUMBH - - A great religious festival held every twelfth year at Hardwar in the North-Western Provinces.

KUMBHAR - - *See* Kumhar.

KUMHAR (*fem.* KUMHARIN). A potter; the name of a caste of which the members are generally potters.

KUNALI - - A drain, a pipe.

KUNBI - - The name of a cultivating caste widely distributed over Central India and other parts of India.

KURESHI - - A member of a class of Muhammadans who claim connexion with the tribe of Kuresh, of which the father of the Prophet Muhammad was chief.

KUTCHA - - *See* Kachcha.

LAC - - *See* Lakh.

LAKH - - One hundred thousand.

LAKHERI - - The name of a caste of which the members work in lac, and make and sell bangles.

LALA - - An honorific title among Hindus; the name of a caste.

LALARI - - *See* Lilari.

LAMBARDAR - The headman of a village, who is responsible for the collection of the Government revenue, and for assistance in the maintenance of order and the execution of Government orders.

LASCAR - - Originally, a camp follower; an employé, *e.g.*, in the Arsenal Establishment or in the Fire Brigade; a sailor.

LATHI - - A stick or club.

LEEPO - - To plaster with earth and cow-dung, the floors of most native houses being every week renewed in this way.

LEPO, LEPOED, LEPOING. *See* Leepo.

LILARI - - The name of a caste of which the members are cloth dyers.

LINGAET, LINGAIT, LINGAYAT. The name of a sect of Shivaite Hindus which wears the "linga" as a badge.

LODHA - - The name of a cultivating caste in Northern and Central India.

LOHANA - - The name of a caste of Hindus of Sind, who are traders, agriculturists, and Government servants.

LOHAR - - A blacksmith; the name of a caste of which the members are blacksmiths.

LONARI - - The name of a caste of charcoal burners and sellers.

LUVANA - - *See* Lohana.

MACHCHI (MACHI) - The name of a caste of which the members are generally fishermen and boatmen.

MADHUA - - *See* Mandua.

MADRASSEE - - A native of the Madras Presidency.

MAGH SANKRANT - The beginning of the Hindu month of Magh (January—February).

MAGH MELA - An important Hindu festival held in certain years near Allahabad in the month of Magh (January—February).

MAHAJAN - - A Hindu merchant or banker.

MAHAL - - A subdivision of a jurisdiction, area, or town; a ward or quarter of a town.

MAHALKARI - An officer, assistant to the Mamlatdar (*q.v.*), in charge of a mahal or subdivision of a taluka (*q.v.*).

MAHAMARI - - Literally "the great disease," a name applied to the plague endemic in Kumaun and Garhwal.

MAHANT (MAHUNT) - The head of a body of Hindu ascetics or of a Hindu religious establishment or mat (*q.v.*).

MAHAR - - The name of a low caste of villagers in the Deccan, who work as menial servants, scavengers, &c.

MAHARWAD, MAHARWARA. The quarter of a town or village occupied by Mahars.

MAHRATTA, MAHRATTI. A race or people divided roughly into two important classes, the Brahmans and the low castes, formerly the rulers of a large part of India, and now inhabiting the Konkan, Khandesh, the Berars, and the Deccan.

MAIDAN - - A plain, a piece of open ground.

MAKRANI - - A native of Makran, in Baluchistan, a class of Muhammadans.

MALA - - - A garden.

MALHI - - The name of a caste.

MALI - - - A gardener; the name of a caste of which the members are chiefly gardeners and agriculturists.

MAMLATDAR - A native officer with revenue and magisterial powers in charge, as assistant to the Collector, of a taluka or subdivision of a Collectorate.

MANDUA, MANDWA - A kind of grain (*Eleucine coracana*).

MANDWAH - - A light bamboo hut or shed with grass roof.

MANG - - - The name of a low caste of which the members are often weavers, scavengers, and menial servants.

MANGELA - - The name of a caste.

MANJHAND - - The name of a caste.

MARWARI - - A native of Marwar in Central India; a name often used to signify a banker, money-lender, or grain-dealer.

MASAN - - The name of a caste.

MASJID - - A mosque, a Muhammadan place of worship.

MASSAL, MASSALCHI A domestic servant.

MAT - - - A Hindu shrine, an establishment or monastery for Gosains or other Hindu ascetics.

MATADAR - - In the Baroda State, hereditary village officers who act as sub-Patels or assistants to the village headmen.

MEGHWAL, MEGHWAR The name of a low caste who work as weavers, coolies, and sweepers.

MEHROBIR - - The name of a caste.

MEHTAR, MEHTER - A sweeper or scavenger; the name of a caste of which the members are sweepers.

MEKRANI - - *See* Makrani.

MELA - - A Hindu religious fair or gathering.

MEMON - - The name of a caste of Muhammadans who engage extensively in trade.

MEMRIS - - The name of a caste.

MENGWAR - - *See* Meghwar.

MOCHI - - A shoemaker, the name of a caste of which the members are principally shoemakers and workers in leather.

MODH - - The name of a section of the Banniah caste.

MODI - - A shopkeeper, grocer, or grain-seller.

MOGA - - An opening, an outlet for a drain.

MOHALLA - - *See* Muhalla.

MOHANA - - The name of a caste.

MOHARRIR - - *See* Muharrir.

MOHURRUM (MUHARAM). The first month of the Muhammadan year, in which the Sunni sect of the Muhammadans celebrate a religious observance in commemoration of the death of Hussain, the son of Ali, the son-in-law of the Prophet Muhammad.

MOMIN - - The name of a caste.

MORI - - An opening, an outlet for a drain.

MUCCADUM - - The superintendent of a gang of workmen, a headman of coolies.

MUFASSIL (MOFASIL) The part of a Presidency or Province in India outside the Presidency, or capital, town.

MUGHAL - - A descendant of a native of Tartary, a class of Muhammadans.

MUHALLA - A ward or quarter of a town.

MUHARRIR - - A clerk or vernacular writer.

MUKHTYARKAR - In Sind, a revenue officer.

MULLA - - A Muhammadan priest.

MULO - - The name of a caste.

NAGAR BRAHMAN - The name of a section of the Brahman caste in the Bombay Presidency.

NAHNI - - A washing place, a lavatory.

NAI - - A barber.

NAIB-TAHSILDAR - Native officers who act as assistants or deputies to Tahsildars (*q.v.*).

NAIK - - - A subordinate officer, a corporal in the Army or Police.

NAIKINI - - A dancing girl.

NAKA - - - A guard, post, or picquet, at the junction of cross roads, fords, or other places where the movements of travellers can be checked.

NAKADAR - - A guard at a naka (*q.v.*).

NAND - - A large earthen vessel or pan.

NARWEKAR - - The name of a caste.

NASARPURI - The name of a caste.

NATRA - - The name of a caste of gypsies who live by begging and peddling things for household use such as iron tongs, spoons, &c.

NAWAB - - - An honorific title among Muhammadans; the ruler of a Muhammadan Native State.

NELA TUTIYA - Copper sulphate.

NIZAM - - The ruler of the Native State of Hyderabad, in the Deccan.

NULLAH - - A watercourse or stream.

NUMRIO - - The name of a caste.

OOPLIES - - Cakes of dried cowdung made for use as fuel.

OSWAL - - The name of a section of the Banniah caste.

OUTCASTE - - A member of a caste which does not abstain from acts considered impure—such, for instance, as dealing with night soil or handling dead bodies—and with whom the members of castes which do abstain from the acts considered impure do not associate in eating, drinking, or social intercourse.

PADAMSALI - The name of a caste.

PADHAN - - The headman of a village in Kumaun and Garhwal.

PADIAR - - The name of a caste.

PAGEE (PAJI) - An outcaste (*q.v.*).

PAGRI - - A headcloth or turban.

PAHARAWALA - A watchman, a guard.

PALKI - - A litter, a palankin.

PALWEGAR - - The name of a caste of silk workers.

PAN - - The leaf of the *piper betel*, chewed with the areca nut (supari) and other ingredients.

PANCH - - - A headman, an arbitrator, a man of influence in a village or community.

PANCHAYAT - A committee of panches (*q.v.*), by which local affairs are discussed and settled.

PANCHOLI - - The name of a caste.

PANDA - - The name of a section of the Brahman caste who have to do with death ceremonies, &c.

PANDAL - - A large tent or temporary shelter.

PANJABI - - A native of the Province of the Panjab.

PARAWALLA - - *See* Paharawala.

PARBHU - - The name of a section of the Brahman caste.

PARCHERRY - - In Madras, the outskirts of a town where the lower castes generally live.

PARDAH - - A curtain or screen, privacy; the seclusion of women.

PARDA-NASHIN - A woman who observes the practice of seclusion.

PARDESI - - Literally "a foreigner," a word used in Bombay to signify a person who comes from Upper India or from a place outside Bombay.

PARGANA - - A subdivision of a district or jurisdiction.

PARIAH - - An outcaste (*q.v.*).

PARSEE - - - A prosperous community of traders and professional men, descendants of the ancient Persians, who have retained the religion of Zoroaster, and have been settled chiefly in Bombay and towns on the Bombay coast for about 1800 years.

PARWARI - - The name of a caste.

PATEL - - - The headman of a village.

PATH - - The reading or reciting of the Hindu or Sikh religious books.

PATHAN - - - A native of the districts on the North-West of the Panjab and the country immediately beyond.

PATTAWALA - A chaprasi (*q.v.*).

PATTI - - - A division of the proprietary holding or of the proprietary body in a village.

PATTIDAR - - Literally "a sharer," a village landowner descended from one of the founders of the village; the name of a caste.

PATWARI - - A village accountant.

PEON - - A messenger or orderly, a chaprasi (*q.v.*).

PET, PETA, PETH, PETTAH. A market or bazar, a ward or quarter of a town.

PHAURA - - A hoe or mattock.

PHOOS - - Dried grass or stalks of corn.

PHUTKIA ROG - A name by which the plague endemic in Kumaun and Garhwal is known there.

PICE - - A coin of the Government of India currency, equal to three pies, or the 64th part of a rupee (*q.v.*).

PIE - - A coin of the Government of India currency, the 192nd part of a rupee (*q.v.*).

PIR - - - A venerable personage, a Muhammadan saint.

POKARNA - - The name of a section of the Brahman caste.

POLE - - The houses situated on each side of a narrow lane which has an opening at one end only.

POOS - - - A month of the Hindu calendar, December—January.

PORWAL - - The name of a section of the Banniah caste.

PUCCA, PUCKA - *See* Pukka.

PUJARI - - A priest in attendance at a Hindu temple or shrine.

PUGRI - - *See* Pagri.

PUKKA - - - Mature, firm, substantial, lasting; with reference to houses, a "pukka house" is one made of brick or stone and mortar in contradistinction to one made of mud or thatch, and in reference to floors, a "pukka floor" is a paved floor in contradistinction to a floor of earth, or of earth and cowdung.

PULA - - Long grass, used for thatching, making huts, &c.

PUNCHAYAT - - *See* Panchayat.

PUNDIT - - A learned man, a common title for Brahmans, the name of a section of the Brahman caste.

PUNKHA - - - A fan.

PURDAH - - *See* Pardah.

QASAI - - - A butcher; the name of a caste of which the members are butchers.

RAHBARI - - The name of a caste of which the members are generally owners of camels used for transport, &c.

RAJBHARA - - The name of a caste.

RAJA - - An honorific title; the ruler of a Native State.

RAJPUT - - - The name of the highest lay caste of the Hindus, the members of which claim descent from the ancient Hindu Kings of India.

RAMDASI - - A Sikh convert from the Chamar caste.

RAO - - - An honorific title among Hindus; the ruler of a Native State.

RAWAL - - The name of a caste.

RAWALIA - - The name of a caste.

RAWAT - - The name of a caste.

RAZAI - - - A quilt or coverlet usually stuffed with raw cotton.

REGUR (REGAR) - Black cotton soil.

RESAI - - *See* Razai.

RUPEE - - - The standard coin of the silver currency of the Government of India, equal to $\frac{1}{15}$ of a pound sterling.

RUS KAPOOR - A mixture of perchloride of mercury and calomel.

RYOT - - - A cultivator or agriculturist.

SADABART - - A Hindu perpetual endowment for a religious or charitable purpose, such as the daily distribution of food or alms to the poor or to travellers.

SADHU - - A Hindu mendicant who begs in the name of religion.

SADR - - The chief town in a district at which the headquarters of the civil administration are fixed.

SAGAR - - - The name of a caste.

SAHIB - - A gentleman, generally used to signify an Englishman.

SAHTO - - - The name of a caste.

SAHUKAR - - A Hindu banker, money-lender, or trader.

SAIS - - - A groom.

SALAT - - The name of a caste.

SALI - - - The name of a caste.

SANGAR - - The name of a caste of blanket sellers.

SANJAR - - The term used in Kumaun and Garhwal for an infectious fever, probably typhus.

SANKRANT - - The sun's entering into a new sign.

SANSI - - - The name of a low caste, of which the members lead a wandering life and subsist by begging and thieving.

SARAVAT - - The name of a caste.

SARI - - - A shawl or outer covering worn by women, a chaddar (*q.v.*).

SARKARI - - Official, governmental.

SARKARKUN - - A superior officer of the Salt or Customs Departments stationed at one of the smaller ports in the Bombay Presidency.

SARSWAT, SARSUD - The name of a section of the Brahman caste.

SATA - - - The name of a caste.

SAUJRANI - - The name of a caste.

SAWAR - - - A mounted man; a mounted policeman; a cavalry soldier.

SAYAD - - A class of Muhammadans who claim descent from the Prophet Muhammad through his son-in-law and successor Ali and his daughter Fatma.

SEER - - The name of the standard weight in India, about two pounds avoirdupois.

SEPOY - - A soldier, a constable or guard employed in the Police, Customs, or other Department.

SERAI - - - A rest house for travellers.

SETHI (SETH) - A Hindu merchant or banker.

SHAHTIR - - - A rafter.

SHASTRAS - - The sacred books or scriptures of the Hindus.

SHEIKH - - A title taken by Muhammadans who are descended from the Prophet Muhammad, and given to those who become converts to Muhammadanism.

SHENNI - - The name of a caste of fish-eating Brahmans.

SHIAH - - - The name of the sect of Muhammadans which does not acknowledge the authority of the traditions attached to the Quran, or of Abubakr, Umar, and Usman, the immediate successors of the Prophet Muhammad, and regards Ali as the second Khalif.

SHIDI - - The name of a caste.

SHIKARI - - A hunter, a sportsman.

SHIMPI - - - The name of a tailor caste.

SHRAFF - - A money changer, banker, or money-lender.

SHRAWAK - - - A lay votary of a Buddha or Jaina.

SIKH - - The name of the principal class of the people of the Panjab, who are distinguished by their religion as followers of the teachings of the Guru Nanak.

SIRKHI - - Reed grass, principally used for mat making.

SIR-SUBA - - A high revenue officer in charge of a division of a Native State.

SONAR - - *See* Sunar.

SONI - - - *See* Sunar.

SOWAR - - *See* Sawar.

SUB-PATEL - - *See* Matadar.

SUBA - - A division or jurisdiction in a Native State, the officer in charge of a Suba.

SUDR - - - *See* Sadr.

SUMMA - - The name of a caste.

SUNAR - - A goldsmith; the name of a caste of which the members are generally goldsmiths.

SUNNI - - The name of the orthodox sect of the Muhammadans which recognises the authority of Abubakr, Umar, Usman, and Ali, the four immediate successors of the Prophet Muhammad, and accepts the authority of certain traditions attached to the Quran.

SURTI - - - A native of Surat in Bombay, a caste of people from Surat who go out to Bombay and other places for service as grooms, cooks, butlers, &c.

SUTAR - - The name of a caste.

SYCE - - - *See* Sais.

TAHSIL - - An administrative subdivision of a district or Collectorate (*q.v.*).

TAHSILDAR - - A native officer with revenue and magisterial powers in charge, as assistant to the Collector, of a Tahsil.

TAI - - - The name of a caste of which the members are generally weavers.

TAIWAD - - - The quarter of a town occupied by Tais.

TAKIA - - A Muhammadan shrine, the abode of a Muhammadan fakir.

TANAI - - - The name of a caste.

TALATI - - A village accountant.

TALAVIA - - The name of a caste.

TALUKA - - An administrative subdivision of a District or Collectorate (*q.v.*).

TALUKDAR - - In the Hyderabad State, an officer corresponding to the Collector (*q.v.*) in British India, in charge of a jurisdiction corresponding to a Collectorate.

TAMBOLI - - The keeper of a shop for the sale of tobacco, pan (*q. v.*), and betel nut.

TAPODAN - - The name of a caste.

TARKHAN - - A carpenter; the name of a caste of which the members are carpenters.

TATTI HUTS - - Huts of reed and grass.

TELI - - An oil presser and seller; the name of a caste of which the members are generally oilmen.

THAKUR - - A title of respect among Hindus; the name of a caste.

THANA - - A police station.

THANADAR - - A superior police officer in charge of a police station.

THATHARI - - A brassworker; the name of a caste of which the members are generally brassworkers.

THOKDAR - - A leader or man of influence in a village or caste.

TICCA - - A term used to signify vaccination or inoculation.

TICCA GHARRI (THEKA GARI). A wheeled conveyance licensed for hire, a cab.

TICCA WALLAH - The driver of a conveyance for hire, a cabman.

TILAT - - A Chief of the Rajput clan of the Jadejas in the Cutch State.

Tola	A measure of weight, $\frac{1}{80}$ of a seer (*q.v.*), equal to about 180 grains troy.
Tomtom	A native drum.
Topeewallah (Topiwala).	A hatter; a person who wears a hat, a European.
Toti	The name of a caste.
Turia	The name of a caste of which the members are generally weavers.
Vaghri	*See* Waghri.
Vahivatdar	In the Baroda State, an officer in charge of a local jurisdiction.
Vaid, Vaidia, Vaidu.	*See* Baid.
Vakil	An agent, and especially a legal agent, a pleader.
Valund	The name of a caste.
Vanho	The name of a caste.
Vanja	The name of a caste.
Veragi	*See* Bairagi.
Waddar	The name of a wandering caste of stone cutters and rat catchers.
Wadho	The name of a caste.
Wadi	A garden, a house or group of houses in a garden.
Waghila	The name of a clan of Rajputs in the Cutch State.
Waghri	The name of a low caste of which the members earn their living by snaring and selling game, by basket weaving, and by acting as shikaris (*q.v.*) and labourers.
Wahivatdar	*See* Vahivatdar.
Walki	The name of a caste.
Wanan	The name of a caste.
Wani	*See* Banniah.
Wari	*See* Wadi.
Yogi	A Hindu ascetic or mendicant.
Zaildar	An influential landowner appointed for a zail, or subdivision of a Collectorate, to assist in the suppression of crime and in the execution of the orders of Government.
Zanana, Zenana	The quarter of a house in which the women live in seclusion.
Zamindar	A landowner, an agriculturist.
Zulm	Tyranny, oppression.

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Heman Shah; case of, I. 491, 26,880–86.
Hodges, Grace; case of, I. 490, 26,875–9.
Marcellies; case of, I. 494, 6845–58, 26,915.
Medical Board, Report on suspected plague in 1896, I. 487–96.
Medical Board Report, disagreement with 1029–31.
Medical Board, Constitution of, 26,818.
Merchandise from Bombay suggested as source of infection in Giga's case, 7101–16.
Opinion that cases which occurred in 1896 not plague, 7028–9.
Presidency Hospital, Calcutta, two suspicious cases in October 1896, I. 491.
Rats: Heavy mortality of rats in Armenian Street, I. 493.
Rats: Notes by Dr. Cunningham regarding mortality of rats, and examination of rats which died during supposed plague in 1896, no evidence of plague among them being obtained, III. 655–6.
Rats: Mortality among rats which occurred during plague in connexion with Giga's case, rats suffering from glandular sickness, and bacillus like plague being recovered from them, 26,898–907.
S.M.N.; case of, 26,887–90.
Shropshire Regiment, cases in, different from ordinary glandular fever, 26,964–85.
Shropshire Regiment; cases in, I. 494–5.
Shropshire Regiment, cases in, bacillus morphologically like bacillus pestis isolated but inoculated into animals without effect, 26,843–67.
Shropshire Regiment, cases in, not plague, 26,843.
Tin Cauri Pal; case of, I. 491, 26,868–74.
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CAMPS:
Arrangements in Camps:
Ankleshwar camp, poor maintained in, by the municipal council, 14,228.

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Bandra camps, each family had a separate hut, and castes were kept together, 8236–43.
Baroda, camp organisation at, 14,619–20.
Baroda, two kinds of camp at; authorised and unauthorised camps, 14,615–6.
Camps under the trees a great comfort, 10,239.
Castes kept together in camp as far as possible 8214.
Combination of segregation, health and detention camps a more economic and better working measure than a series of separate camps, 18,393.
Cutch, village camps in, 13,445.
Hardwar union, camp arrangements in, 9327–43.
Malegaon, arrangements for camp at, 18,074.
No plague-stricken person admitted in health camp, 12,658.
N.W.P.: arrangements and rules for camps, 9334–41.
Sholapur camps, streets of shops in, 20,259.
Thatched huts erected by Government as a plague hospital in the private camp at Jawalapur, 9341.
Two chappars separated from the camp at Hardwar for observation of suspicious cases, 9336.
Umreth, arrangements for camps in, 14,028–36.

Attitude of the People towards Camps:

Baroda, people in the health camps at, reluctant to go back to town, 14,687, 15,168.
Better surroundings and the absolute safety from infection are highly appreciated by the people, 26,555.
Bombay: Fifteen hundred people pay rent in one camp at Bombay, 8281.
Bombay people, health camps popular with; some prefer to stop and pay rent, 1345, 8273.
Bombay: People object to leave in Bombay, 8275.
Broach: People behaved most admirably in camp, 15,503.
Camp life liked in summer, 15,509.
Jawalapur: Permission to attend the Id and Holi festivals gave no trouble at all, 9343.

Camps provided for the People:

Bangalore: Two hospital camps and health camps at, to accommodate 400 persons, 2550.
Bangalore City: details of accommodation provided for, I. 131.
Bombay: Accommodation of camps in, 1081.
Bombay: Camps provided for evicted people from each ward. 19.
Government huts built with regard to sanitation should be ready, 22,289.
Government huts should be ready to receive the whole population, 22,289.
Hubli: For 2,600 erected at, 1611, 1652.
Karachi: Accommodation for 1,500 persons in segregation camp at, 11,633.
Karachi: Conditions at, favourable for large segregation camps, 11,831.
Poona: Camps erected for infected regiments on Gibbet Hill, 21,251.
Sholapur: Large camps laid out at, at Government expense, 20,231.
Surat: camps provided by Government, 24,419.

Disinfection in Camps:

Ankleshwar: Clothes and effects disinfected on going to camp, 14,224.
Bandra: Clothes not disinfected, 18,600–1.
Bangalore: On removal of persons at segregation camps, disinfection by pouring a solution over them, and afterwards bathing them in water, 3300.
Baroda: Evacuation, accompanied by disinfection of persons and belongings, 14,625.
Calcutta: Property of persons taken to segregation, and isolation camps disinfected on arrival, 6536.
Chipagiri: All persons in segregation camp disinfected before returning to their houses, 4415.
Clothes, disinfection of, should always precede admission to, 24,196.
Dharwar: Contacts disinfected on arrival in camps, 1911.

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Effects of people disinfected before return from camp, 11,314.
Evacuation must be combined with the disinfection of the infected area and persons, 10,363.
Evacuation must be combined with disinfection of clothes, 24,193, 24,196.
Evicted people on arrival, disinfection of, 12,657.
Hardwar: Coolies engaged in disinfecting work kept in separate camp with wives and children, 9342.
Hyderabad State: Clothing and bedding boiled on entering and on leaving camp, 5,466.
Karachi: Disinfection of persons effected before removal to camp with good results, 11,143–8, 18,451.
Karachi: Exposure to sun the only method of disinfection used in the Nassarpuri camp, 11,490.
Khandrauni: New clothes given to people in camp before they returned to their houses, 9661.
N.W.P.: People, except at Hardwar, disinfected on going to camp, 9263–5, 9294–7.
Panjab: Before leaving evacuation camp every person given a phenyle bath, 10,049, 10,235.
Panjab: Clothes not disinfected, 18,600–1.
Panjab: Clothes of inmates exposed to sun for ten days, 10,048.
Panjab: Effects of people disinfected after they went to camp, 10,471.
Panjab: People disinfected before removal to camp; good effects observed at Shikohpur and other places, 9993–5, 10,145–6.
Poona: Comparison between cases in which clothes disinfected and a case in which clothes not disinfected to establish the value of disinfection, 21,251–7.
Poona: Contacts sent to segregation camps disinfected, 19,936, 21,272–4.
Proper disinfection and organisation of segregation and other camps preclude the possibility of a camp epidemic, 18,391.
Sholapur camp: Disinfection of all new arrivals, &c., 20,235.

Occupation of People in Camps:

All ordinary handicrafts carried on in the camps at Sholapur, 23,922.
Ankleshwar: People allowed to go into town on business without bad results, 14,225–8, 14,408–9.
Baroda: People allowed to go to their shops for business, 14,632.
Bombay: People allowed to go about their work from segregation camp if back at night, 1354.
Difficult problem to find occupation for the people in camp, 9335.
Hubli: People allowed to go about their daily business, 1652.
Hyderabad: People in evacuation camp allowed to go to town for their business under supervision, 18,119.
Hyderabad villages: Difficulties of evacuation with weavers, 5456.
Jawalapur: All beds and tables used in the plague hospitals made locally in the camps, 9335.
Karachi: People in segregation camp allowed to go to their occupations in town, 11,320–5, 11,433, 11,678, 12,752–5.
Kotri: People allowed to go to the town on business, but not to their houses, 3760–9.
Malegaon: Weavers with their looms taken into the health camp at, 18,075, 13,777.
Nasik: No bad results from allowing people to attend to their occupations in town during the day, 13,809.
People may safely visit the town by day on business if they sleep in camp, 24,197–8.
Restraining the people entirely from going into town during the day for business would be too extreme a measure, 14,405.
Weavers and other handicraftsmen in Savadi camp allowed to go to their work in town, 18,986.

Passes for Camp Inmates:

Bombay: Contacts allowed out from segregation camps on passes, 1356.

CAMPS—*cont.*

Only one pass system in vogue at Hardwar camp, 9337.
Sukkur: People allowed to visit the town only with passes, 3731.
System of permanent and temporary passes in Government camp at Baroda, 14,619.

Provision of Accommodation, Food, &c. by the People:
Broach: About 1,000 people provided camps for themselves, 15,484, 15,502.
Care taken to see that huts are built with regard to sanitation, 19,515.
Cutch Mandvi: People provided their own camps, 13,377.
Entails no cost in small villages; people build their huts themselves of straw, 24,564.
Hardwar: People provided their own huts, difficulties in inducing them to make a camp on a sanitary plan, 8802.
Hardwar: Poor not relieved by charity received rations from Government; but daily distribution of grain to a fixed amount to all who came to ask for it from the Sadabart reduced the camp charges paid by Government, 9335.
Hyderabad State: Villages supply their own camps at suitable places, of which position largely settled by water supply; large camps inadvisable, 5459.
Jawalapur: People provided their own huts, but crowded them too much together, 9178–9.
Palanpur: 8,000 people built huts for themselves, 13,084.
Wardha: People allowed to put up private camps, 6121.
Wise plan to allow the people to put up their own huts. It costs Government nothing, and satisfies the people, 25,823.

See Voluntary Evacuation.

Return of People from Camps to evacuated Houses:
Broach: Evacuated houses sealed up, 15,519.
Dharwar: Measures adopted for the re-admission of the people permitted only after double inoculation, 23,746–7.
If people prevented from returning to houses, plague stops, 8–9.
Impossible to keep people from returning to their houses, 23,171.
Nasik: Impossible to keep the people out of their houses at night, temperature going down to 40°, 14,734.
No cordon around the camps in Palanpur, but roll-call, and people effectually prevented from return to houses, 13,087, 13,164.
People deliberately run risk of return from camps to infected houses in order to see that their property is safe, 22,637–9.
Police patrols and guards necessary to keep people from trying to return to the evacuated houses, but unsuccessful, 13,802, 13,807, 14,229–30.
Prevention of return from camp to village necessary, 20,158, 22,085, 22,604.
Satara: Evacuated houses locked and sealed, and could only be opened at the presence of the authorities, 22,417.
Strict exclusion from evacuated villages depends greatly on the village officers, 22,605.

Supervision in Camps:
Cutch Mandvi: An officer visited the camps each day and brought back cases, 13,423.
Hardwar: A Tahsildar assisted by a patwári appointed superintendent over each camp at, 9334.
Jamalpur: Roll-call of camp occupants made and checked night and morning, 9169.
Karachi: Concealment of cases led to regular inspection of the camps twice a week, 11,886.
Khandesh: Roll-calls made in the camps, 24,557–9.
Khandesh villages: Roll-call proved sufficient without cordon or police in the, 24,562.
Mandvi: From 33,000 to 35,000 persons placed in camp at, and kept under police supervision, 13,423.
No roll-call, but admission and discharge books kept in Malir camp, 12,498.

CAMPS—*cont.*

Palanpur: The village patels made daily roll-calls in the evacuation camps in, 13,165.
Rohri: Eight police to guard 840 people in segregation camp at, 12,061.
Roll-call in Nassarpuri camp, Karachi, 12,772.
Schoolmasters, village officials, and paid enumerators employed in the Kaira district to take the roll-calls, 14,064.
Staff corps officer charged with the supervision of Malegaon health camp took a roll-call twice a day, 13,777.
Surat: Segregation camp at, placed under supervision of a sergeant-major of gunners, 24,417–9.
Umreth: Census, roll-call, and hut inspection segregation camp at, 14,034–6.
Umreth: Golas in segregation camp guarded by police in, 14,033.

Spread of Plague in Camps:
Ankleshwar: Table showing admissions in the segregation camp, and the number of days after admission when the cases occurred, II. 239.
Bangalore: Plague cases among Sepoys, 4889.
Bangalore: Segregation of contacts at; admissions and deaths in camp, 2621, 2941, 25,028.
Baroda: Nine cases in camp among 11,000 people, 14,624, 14,672–5.
Bellary: No plague cases in, 4403.
Bombay: Details of segregation camps at, III. 117, 118.
Bombay: Very little plague among contact in camp, 1200, 1248, 1355.
Bombay: Very little plague in segregation camps, comparatively, 1355.
Camp, Malir, no case incurred from infection caught in the, 12,438.
Camps, in voluntary camps, unlike houses where four or five people get plague in one house, more than one case not observed in huts, 11,892.
Cases do not communicate infection as much in camps as in towns, 14,659-61.
Cases in camp among people who had been out less than 10 days or had slept away, 17,295.
Contact cases do not spread in camp, 23,349–50.
Cutch Mandvi: Borahs go into camp at, and suffer little, 475.
Cutch Mandvi: Cases numerous among contacts in first epidemic but no record kept, 13,380-1.
Cutch Mandvi: Number of cases in camps in second epidemic, 13,437-40.
Cutch Mandvi: Table of cases in the segregation camps at, 13,542.
Dharwar: Contact or segregation camp at, details of numbers admitted, 1902.
Dharwar district: Mortality in segregation camps very small, 1653.
Dharwar: Details for villages exhibiting effects of evacuation, 23,167.
Disease continues in camp because people are housed in insanitary huts, 22,291–6.
Dropping cases in camp only among people who slept in evacuated houses, 10,191–3, 13,807–13.
Duration of plague after evacuation not dependent on rainy or dry season, 22,704.
Hardwar: Four cases only in contact and segregation camps, 8797.
Hinganghat: Statistics of segregation and other camps, I. 23.
Hyderabad Sind: Eighteen cases in contact and eight or nine cases in other camps, 18,125.
Jawalapur: Among 13,000 people taken in camp at, only 69 cases during the whole period, 8796–7.
Jawalapur: Huts too close together, and overpopulated: otherwise there would have been less plague, 9178.
Jawalapur: Table of average weekly population of contact camps at, and weekly number of plague attacks, II. 45, 9.
Karachi: A case in camp traced to visit to town, 12,776.
Karachi: A few cases where huts too close together, 11,263.

CAMPS—*cont.*
Karachi: Kiamari: Only three plague cases in Government health camp at, in 1897, 11,969.
Karachi: Plague in health camp as bad as in the most infected parts of a big town, 21,752.
Karachi: Plague statistics from the Nassarpuri camp in 1897, II. 155.
Karachi: Statement of total attacks and mortality in voluntary camps, from May 1st to August 14th, 1898, 11,893.
Karachi: Table of average population in contact camps at, with the number of plague cases and deaths, 11,628, 11,671-2, II. 148, 155.
Karachi: Table of plague mortality in the unevacuated portions of the town, from April 1st to November 4th, 1898, II. 154b.
Khari, near Bandra: No cases among about 1000 people in camp, 18,936.
Kotri: Cases in camp due to communication with the town, 12,645.
Kotri: Only one plague case in contact camp at, 12,570.
Kotri: Statement of cases in the various camps at, 12,643.
Molagavalli: One thousand five hundred people go out into the fields at; cases continue, 5000-4.
Mysore: Statement of cases in contact camps, III. 276.
No fear of plague becoming epidemic in camps, 18,391.
Palanpur: Details for villages showing effects of evacuation, I. 199 (13,159).
Panjab: Details of cases occurring more than 20 days after evacuation in several villages, II. 81-2 (9981), II. 105-6 (10,384), II. 116-7.
Panjab: Information concerning persons attacked with plague more than 10 days after evacuation of the villages, (10,603) II. 115-6.
Panjab, Mahlgahla: Statistics relating to history of plague, II. 103-4.
Panjab: The majority of cases occurring 20 days after evacuation ascribed to the people having returned to the village surreptitiously, 10,515-28.
Panjab: Want of cordons around the villages cause of plague in the camps, 10,758.
People in camp infected by re-visiting their houses, 9, 11,890, 12,230, 12,795, 13,456, 13,729, 13,807, 18,075, 22,601, 24,155.
Plague dies out in camp in a week, 20,451.
Poona: Particulars of cases in camp among the regiments, 21,252.
Poona: Statement of cases in segregation camp, 21,539.
Poona: Statement of cases in segregation camps since February 14th, 1899, 21,832-5, III. 317.
Porbandar: Statement of cases in contact camps, 13,722.
Porbandar: Table showing daily attacks and deaths among the Kharwas from date of evacuation, II. 221.
Porbandar: Table showing number of cases in evacuation camp, II. 253a.
Proper organisation of segregation camp and proper methods of disinfection exclude the possibility of a camp epidemic, 18,391.
Sholapur camp kept entirely free from plague, 20,234.
Sholapur: Table of cases among the evacuated population, 23,940.
Sholapur: Table showing progress of plague week by week in the city and health camp, III. 109.
Sirohi: Cases in camp at, 9836, 9865, 9877.
Surat: Camp cases occurred in those who visited the town, 15,776.
Surat: Majura Health Camp, average population, 3,754; only 15 cases in camp, 15,775.
Surat: Table showing mortality in health camps at, II. 304.
Timmencherla, no cases in camp at, 4129.
Wardha: Statistics of segregation and other camps, I. 224.

CAMPS, OBSERVATION:
Dharwar: Observation at, for people from infected localities, 1927.

CAMPS, OBSERVATION—*cont.*
Dharwar: Observation camps closed November 1898 at, as useless, owing to spread of infection in district, 1930.
Hardwar: Pilgrims from infected areas conducted to the special camp at Hardwar, and medically inspected morning and night, 8751.
Hubli: 10 days' detention for all arrivals in, 2429.

CAPSULE:
Bacillus of plague not found with a capsule, 5791-8714.

CARBOLIC ACID:
Clothes, Disinfection of:
Clothing disinfected at Ankleshwar by placing it in carbolic acid lotion for half an hour or simply boiling it in water, 14,255-7.
Efficacy:
Action of the phenols and their allies on the bubonic microbe, II. 5.
Bacillus cannot live in Haffkine's fluid because of the carbolic acid, 8411.
Bacillus of plague killed by two per cent. carbolic acid in three minutes, 27,401.
Bacillus of plague killed by ½ per cent. of carbolic acid, 105.
Experiments with:
Experiments on the effect of carbolic acid in destroying micro-organisms on a wall gave no satisfactory results, 26,351.
Results of tests with carbolic acid, II. 5.
Inefficacy:
Plague bacillus not sensitive to the action of carbolic acid, II. 5, a.
Perchloride of mercury at Calcutta, carbolic acid given up in favour of, 6528.
Phenyle more active than carbolic acid on the microbe of plague, II. 5.
Person, Disinfection of:
Carbolic acid in solution strong enough to kill bacillus would hurt people so much that they could not stand it, 4305.
Persons disinfected in carbolic bath at Ahmedabad, 13,896.
Plague patients receive carbolic bath on leaving hospital to disinfect them, 557
Poona: Persons on entering camp bathed in carbolic at first, but, at a later period, in phenyle, 21,272-4.
Where used:
Common Jail, Bombay, used in, as disinfectant, 20,344.
Daman: Disinfection by crude carbolic acid diluted with water, in strength of 1 in 40, 13,629, 16,409.
Palanpur: Carbolic acid resorted to as disinfectant in, 13,091-4.

CARBONIC ACID GAS:
Experiments on the growth of the plague microbe in air with admixture of carbonic acid, 18,893.
Experiments to recover the plague bacillus from earth by cultivation in carbonic acid gas, 26,144, 26,233-4.
The carbonic acid method the only successful way to isolate bacillus of plague from earth, 26,179.

CARBUNCLES:
Carbuncle, which in one case was point of entry of infection, contained multitudes of bacilli of plague, 6910.
Carbuncles observed, 7302, 12,875.
Details of 10 cases of plague with carbuncles, 12,360-74.
Lymphatic glands enlarged in about half the cases of primary carbuncles, 12,487.
Microbes resembling plague bacilli discovered by microscopical examination of the serum of a plague carbuncle, 12,377.

CASE MORTALITY:
Ahmedabad: 66·6 per cent. of the imported cases proved fatal, 13,907.
Ankleshwar: Totals of cases and deaths in the town and district, 14,214.

CASE MORTALITY—*cont.*

Baroda Cantonment: Cases and deaths, 15,424–6.
Baroda City: Total cases and deaths, 14,637.
Baroda State: Number of cases in villages with results, II. 517, App. R.
Bhiwandi: Attacks and deaths, 14,482.
Igatpuri: Table of cases and deaths, III. 90.
Ilkal: Table of cases and deaths, III. 90.
Jawalapur: Number of cases with results, 9539–40.
Kotri: Per-centage of recovery cases, 39·8, 12,638, 12,649.
Palanpur: Total attacks and deaths, 13,159.
Panjab: Cases and deaths, 10,330.
Panjab: Jullundur district: Plague mortality, 62·73 per cent., 10,205.
Porbandar: Total of cases with results, 13,692.
Mortality greatest in septicæmic, next in pneumonic, and least in bubonic plague, 12,391.
Remarks of the Bombay Plague Research Committee on case mortality, I. 382.
Satara: Mortality during first epidemic, 66 per cent., 22,259.

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CASTE AS AFFECTING LIABILITY TO PLAGUE:

Ankleshwar: Table showing proportion of attacks in the principal sub-classes among the Hindus, II. 246*b*.
Ankleshwar: Table showing number of cases classified according to the population of each caste, II. 246*a*.
Bangalore: Table of number of seizures and deaths by races and castes in the Civil and Military Station, I. 109, 25,015.
Bangalore Hospital: Relative mortality according to castes, 2913, 2923.
Baroda State: Plague cases according to sex and caste in, 14,989, 15,219.
Bombay Hospital: Table of mortality among castes and races, I. 391.
Bombay, Parel Hospital: Incidence of cases by sex, age, and religion, 7221.
Calcutta: Incidence of plague by age, race, and sex at, I. 474.
Calcutta: Table of attacks and deaths according to the classes of the population, I. 474.
Caste does not influence mortality, 10,210.
Deaths among different castes, I. 306.
Disease spreads in circles of castes, 16,006.
Immunity of particular castes depends on their habits as to fresh air and cleanliness, 22,274.
Incidence of plague on various castes, I. 378.
Karachi: Statistics of plague cases in the hospital, with reference to caste, 12,358.
Karachi: Table of number of cases and deaths, according to castes, in the municipal limits, II. 157.
Karachi: Table showing, by castes, attacks and deaths of plague during the last six week of the epidemic, II. 157.
Mysore City: Castes and plague in, 25,015.
No difference in the mortality of people from habits, caste, or age, 10,210.
No difference in the predisposition to plague of certain classes, 16,315.
Observation on immunity in different castes during the present epidemic, 22,264–7.
Observations on plague incidence in different castes at Bombay, 17,681–5.
Porbandar: Statement showing plague cases and deaths among the various communities from May to November 1898, II. 222.
Spread of plague at Dharwar and Hubli was not marked by an inter-connexion of castes, 1715–7.
Statistics of plague cases in the Sassoon Hospital bearing on caste, sex, and position of buboes, 12,897.
Surat: Castes and plague at, 2046–8.
Surat: Number of plague cases according to castes, 15,632–3.
Surat: Table of mortality from plague among the different castes, II. 295.
Umreth Town: Table of total attacks and deaths by caste, sex, and age, II. 233.
Very dirty classes, such as Dhers and Mangs, not so liable to plague, 5478.

See Acharj.
,, Banniahs and Traders.
,, Bhatias.

CASTE AS AFFECTING LIABILITY TO PLAGUE—*cont.*

See Bhils.
,, Borahs.
,, Chamars.
,, Dhers.
,, Dhobis.
,, Domes.
,, Fakirs.
,, Ganchi.
,, Golas.
,, Infected places, *particulars regarding*.
,, Inoculation with Haffkine's plague prophylactic.
,, Jains.
,, Khoja Community.
,, Mahars.
,, Mangs.
,, Memons.

CAT:

Affected by Plague:

Ahmednagar: Cats seen suffering from plague, 24,310.
Ahmednagar: Overrun with cats having open buboes in their necks, 18,990.
Bandra: Cats had plague, 19,095.
Baroda: Glandular swellings below the jaw noted in a cat, 14,641.
Bombay: Cats suffered from plague, 1187–8, 20,423.
Cutch Mandvi: Cats died of plague, 13,565.
Kaira: Cats died of plague, 14,079.
Karachi: During the first epidemic cats were found with buboes and killed in large numbers, 11,311, 11,839.
Umreth: Suspicious deaths among cats, 14,079.

Discovery of Bacillus in:

Found in cats with typical plague symptoms, 20,424.
Involution forms of plague bacilli seen after death at the seat of inoculation in a cat and guinea pig, 26,227.

Immunity from Plague:

Cats inoculated with large doses die of plague, 26,316.
No evidence that cats are affected by plague, I. 378.

Infection by:

Masur: Instance in which plague was probably caught from a cat, 14,484.

Reaction of Plague Virus in:

Cat inoculated with plague virus showed more local reaction than any other animal, 26,316.

Theory that Plague not recently imported to India:

Plague cases said to follow on a cat suffering from bubo in Bombay in 1893, 18,276.

CATTLE:

Diplo-Bacteria found in:

Diplo-bacteria similar to those found in plague rats discovered in the blood of cattle during the epidemic among them at Calcutta in 1897, 26,942.

Disease among:

Foot-and-mouth disease among cattle just before plague outbreak in Bombay, 901.
Great mortality among cattle in Bombay in middle of 1896. 27,016.
Mesenteric glands were perfectly black in animals dying of the Calcutta epidemic in 1897, congested and enormously enlarged, 26,944.
Outbreak of a disease among cattle, the symptoms of which were different from rinderpest, in January 1897, in Calcutta, 26,941.

Housing of:

All villagers inclined to keep cattle and manure close to their houses, 7922.
Baroda: Cattle and men live all under same roof, 15,193.
Cattle in one enclosure with living rooms at Pali, 6423.

CATTLE—*cont.*

Cows kept in filthy state in Kumaun dwelling-houses, 6163.
Cutch Mandvi: Cattle kept by the people of, in the lower rooms of the houses, 13,353.
Dharwar district: Kept in the dwelling-houses in all the villages in the, 23,180, 23,431, 23,518.
Housing of cattle in human dwellings a confirmed habit which cannot be altered, 7922.

CENSUS.

Dharwar:

Differences between figures of census and inoculation registers, 1764-7.
Error in statistics, because inoculated people come in from the huts outside to die in their houses, 2351–9.
Lowest population of Dharwar 9,000, 1972.
Margin of error in the census returns of inoculated and uninoculated, on account of the people having left the town, 1974–2001
Weekly census in Dharwar since October 19 1898, 1763.

Gadag:

Census taken under the directions of Mr. Vincent, and checked once a week by supervisors, 23,832–6, 23,853–62.

Hubli:

Census checked every week by supervisors, who each had a ward of about 100 houses; tested for the whole of Hubli by one European officer, 3174–8.
Census figures obtained by an enumeration and subsequent deductions and additions, and approximately correct, 3123–30, 3166–7.
Census figures probably under the figures of actual population; lowest population of Hubli probably 6,000 to 8,000, 2488–92, 2518–9.
Census kept up to date by daily inquiries regarding arrivals and departures by supervisors, 1959.
Census made once for all, and verified periodically by additions and subtractions to keep it up to date, 2523–7.
Census stated whether persons were inoculated or not, and, in spite of difficulties, was fairly accurate, 3179.
Census taken in December 1897, under the directions of the Collector, no European being employed in connexion with the taking of it, 20,172–6.
Census taken weekly, but attempts made at concealment, 2520–1.
City divided into 10 sub-divisions, each division having about 10 supervisors who each looked after 90 or 100 houses, 19,796–8.
Difference between Captain Leumann's figures in his inoculation report and census figures due to the fact that the former refer to periods which do not accurately agree with those of the census, and were corrected by special inquiries, 25,143–50.
Discrepancies between figures in census and figures given by Captain Leumann in his Report on Inoculation in Hubli, 3056–65.
Each supervisor took the census for 100 houses, probably taking a week over the work, 20,181–3.
Lowest uninoculated population, about 3,000 or 4,000, 19,849–51.
Margin of error in statistics of inoculated and uninoculated, on account of people having left the town, 1987–2001.
Number of uninoculated persons probably never went below 5,000, 23,461–2.
Original census kept up to date by subtractions for deaths and departures, and additions for arrivals; result fairly accurate, 19,841–6.
Plague census taken first in October 1897, in exactly the same way as the Imperial census, and figures fairly accurate; the uninoculated population not as great as 5,000 at its minimum, 25,157–60.
Rains prevented supervisors from doing their work properly, and many, being volunteers, ran away from fear of plague, 1589.

CENSUS—*cont.*

Supervisors check the census every week, each having a ward of about 100 houses, 1623, 1665–6.
Supervisors visited houses in their wards every day, the town having 12,000 houses, and each ward having about 100 houses, 20,165.
Uninoculated persons tried to keep their names from being recorded in the census register, 19,890.
Uninoculated persons had no inducement to get registered, 3169–71.
Uninoculated population arrived at by deducting numbers of inoculated reported to be present from census returns of total population, 2490, 2523.
Ward supervisors went to every house once a week, to check the census; returns, when tested, found fairly accurate, 19,870–3, 198,91.

See Roll-call.

CHAMARS:

Chamars frequently attacked, as they are not only workers in leather, but also the servants of the village; and hence in close contact with plague patients, 10,319.

CHARACTER AND PREPARATION OF HAFFKINE'S PLAGUE PROPHYLACTIC.

See Haffkine's plague prophylactic.

CHARACTERISTICS OF BACILLUS OF PLAGUE.

See Bacillus of plague.

CHILDREN:

Clinical feature of Plague in:

Clinical description of plague case in a child, 7587.
Convulsions prominent symptom in plague-stricken children, 5075, 12,987.
Half the number of cervical buboes occur in children under 12 years of age, 12,397.
Prognosis better than among grown-up people, I. 352.
Temperature rises more quickly and is greater in children than in adults, 20,689.

Medical Treatment of:

Administration of carbolic to children causes gastritis, 11,934.

Mortality of Children:

Infinitesimal death-rate from plague among children under one year of age, 11,815.
Jullundur District: List of villages where children under two years suffered from plague, II. 103.
Karachi: Many die after epidemic at, 4310.
Karachi: Table showing infant mortality after first epidemic of plague, II. 156.
Karachi: Table showing ordinary mortality of infants from 1894–8, II. 156.
Mortality from measles and influenza in Poona heavy among children under five during January and February 1899, 25,838.
Pneunomic plague always fatal in, 12,314.
Surat: Plague mortality among children 40 per cent., 15,643.
Surat: Table of mortality among children from small-pox and measles, II. 301.

Plague and Children:

Baby at the breast of plague patient escapes infection, 7407.
Cases of children under two years of age in the voluntary camps at Karachi, 11,890.
Childhood chiefly immune from plague, 14,395.
Children at the breast little liable to the infection, 10,329.
Children under one year of age generally escape plague, 11,739, 11,815.
Mild case of plague in children, 9467–75, 10,338.
More or less immune from plague attacks or fatal terminations, 23,061.
Only one case seen among infants, 5926.
Plague rarely attacks young children, 15,446.

CHILDREN—*cont.*

Very few children under five contract the disease, 16,510.

See Age.

„ Inoculation of Haffkine's plague prophylactic: *Age and inoculation; Bars to inoculation;* and *Dosage.*

CHLORINE:

Plague bacillus killed by freshly electrolysed water with chlorine in solution, 3632.

CHLORO-NITROUS OZONE:

Used as disinfectant: statistics of results, II. 258.

CHOLERA:

Decrease of:

Ahmedabad: Cholera has disappeared from, since the introduction of the new water supply, 13,962.

Calcutta: Cholera diminished by introduction of good water into, 6998.

India: Cholera in, much diminished of late years, 8061.

Mysore: Cholera has disappeared since the new water supply was introduced, 25,129.

Disinfection:

Very little disinfection done in cholera epidemic. The only provision made is the distribution of medicines, 8015–6.

Notification of:

Promptly notified, 7818.

Reports of cholera cases more easily obtained than those of plague, 8876.

Outbreak of:

Appeared late in Calcutta in 1898, and much less than usual, 6878.

Poona: Cholera and plague at the same time during the first epidemic, 21,679.

Surat: Sudden outbreak of cholera and plague in jail, 15,789.

Severity of:

In cholera, as the outbreak proceeds, cases become milder; the reverse holds true of plague, 6938.

CIVIL MEDICAL DEPARTMENT:

Assistant surgeons are trained at Calcutta or Lahore, generally intelligent men, 7775.

Civil Medical Administration in the Bombay Presidency, system of, 17,147–56.

Doctors do two years' army medical work before going to Civil Department, 7903.

Emergencies: Methods of securing additional help, 7967.

Hospital assistants officer small hospital and dispensaries, and are mostly educated in the vernacular, and have only moderate qualifications, 7773–82.

Military duty, liability to, does not deter men entering the Indian Medical Service, 7953.

Number and duties of medical officers in the N.W. Provinces, 8928, 8971.

Officer in charge of dispensary might be utilised for checking returns of mortality, 10,292.

Organisation of medical body in India, I. 272–86.

Promotion: Medical officers if put to special scientific work would lose best chances, 8011.

Question of control over Civil Surgeons by Head of Civil Medical Department, and also by Head of Sanitary Department, 7960–6.

Staff insufficient to deal with plague; local and temporary men were employed, 7915.

See Civil Surgeons.

„ Health Officers.

„ Sanitary Department.

CIVIL SURGEONS:

Civil Surgeons greatly assist the Municipalities with their advice on sanitary measures, 17,204.

Duties of the Civil Surgeon of the Bombay Presidency and the Civil Surgeon in other parts of India, 17,140–4.

CIVIL SURGEONS—*cont.*

Duties of the Civil Surgeon of the district, 10,512.

Duties of Civil Surgeon in ordinary circumstances do not go beyond suggesting improvements in sanitation, 22,507.

Many pressing calls on Civil Surgeons, so that they cannot properly scrutinise death returns, 7811–4.

Multifarious duties of the Civil Surgeon leave him no time to direct plague measures, 7730, 7912–4.

No longer recruited from Uncovenanted Medical Service. The vacancies filled up either by Military or Civil Assistant Surgeons, 7731.

CLIMATE.

See Meteorological conditions.

CLINICAL FEATURES OF PLAGUE:

Aberration of mind a symptom of plague, 4547.

Aphasia a symptom of plague, 21,029.

Aphasia not noted among the patients in the Sassoon Hospital, Poona, 12,905.

Appetite, varied, 20,841.

Arthritis in 4 or 5 per cent. of the cases, 20,907.

Blisters observed, in a few cases all over the body, 20,873, 21,020.

Cardio-vascular symptoms: Peculiar symptoms in circulatory system a clinical feature of plague, 20,800.

Cardio-vascular symptoms: Throbbing of the carotids a clinical feature of plague, impulse at the apex more diffuse than normal, I. 380.

Central deafness, 20,793.

Cerebral symptoms grave in cases where no bubo appeared, 12,897.

Convulsions in children, 20,786.

Convulsions prominent symptom in plague-stricken children, 5075, 12,987.

Delirium, I. 380.

Delirium, character of, 20,756–8.

Diarrhœa generally just before death, 20,850. I. 380

Disturbances in the circulatory, respiratory, digestive, urinary, and reproductive systems, I. 381.

Dysentery, a clinical symptom of plague, 511.

Epistaxis seen in plague cases, but only if complicated with relapsing fever, 20,809.

Erysipelas noted especially in cases of cervical buboes, 20,723.

Expression: peculiar facial expression a clinical feature of plague, 12,398, 15,294, 20,673.

Gangrene and blebs observed in Daman, 16,750.

General description of clinical symptoms of plague in a child, 7587.

General description of clinical symptoms in the different forms of plague, III. 44, 1386, 20,404–7, I. 379.

General description of cases in Sassoon Hospital, Poona, 12,897–905.

General features of plague cases, I. 58–63.

Hæmorrhage in the bowels and stomach, 20,854.

Hiccough persistent, defying all treatment, 20,848.

Leucocytosis a clinical feature of plague, 20,803.

Liver not enlarged, 12,412.

Liver in bubonic plague somewhat large and congested, 10,421.

Locomotor system: staggering gait a characteristic early symptom of plague, 12,898, 15,294, 20,668.

Mania in two cases accompanied an attack of plague, 20,791.

Melæna, 20,852.

Meningitis, 13,046.

Nausea a clinical feature of plague, I. 380.

Nervous disturbances, 12,987.

Nervous prostration, I. 379.

Nervous symptoms most prominent among Europeans in Sassoon Hospital, 12,934.

Onset and progress differ enormously in different cases, I. 351–2.

Paralysis, I. 380.

Paralysis and hemiphlegia, 16,766.

Partial laryngeal paralysis, I. 380.

Petechiæ observed all over the body, like a rash, but not a characteristic sign of plague, 20,883.

Petechiæ not observed in pneumonic form, I. 369.

Position: peculiar flexed position of patients 7015.

Pulse feeble and dicrotic, but of varying character in different patients and different stages of an illness, 20,800.

CLINICAL FEATURES OF PLAGUE—*cont.*

Pulse similar to that of aortic regurgitation, 21,035.
Pulse small, easily compressible, and frequent, 12,398.
Renal complications: albumenuria, 10,422, 12,409, 15,300.
Renal complications: hæmaturia, 15,307.
Respiration hurried and oppressive in cases without bubonic swellings, 12,897.
Respiration hurried, I. 380.
Respiration generally hurried, voice disconnected and slurring, 12,399.
Respiration in pneumonic cases much more hurried than in other forms of plague, 12,914.
Skin: cellulo-cutaneous necrosis, 20,875.
Skin: cutaneous manifestations of plague, 20,868–73.
Skin: no local affections of the skin observed in cases of plague, 9571.
Skin: phlegmonous condition of the skin in cases when no bubo appears, I. 351.
Skin dry and harsh in plague, I. 380.
Smell: bubonic cases characterised by bad, 7015.
Smell: peculiar, 20,892–4.
Speech: aphonia very common, 12,402, 12,901, 16,527, 17,581.
Speech: eight cases of absolute aphonia; recovery, 12,400.
Speech: no aphonia noted among patients at Sassoon Hospital, Poona, 12,905.
Speech: thick, I. 379, 15,295, 20,675.
Speech: voice disconnected and slurring, 12,399.
Spleen: no abnormal symptoms in spleen, 12,411.
Spleen in bubonic plague very much enlarged and congested, 10,421.
Symptoms of the enteric form, 18,543–9.
Symptoms of typical cases at Wardha, 6291.
Table showing special symptoms in 100 consecutive fatal cases and 100 consecutive recovery cases, II. 176*b*.
Table showing special symptoms in 100 consecutive fatal cases and 100 consecutive recovery cases, II. 176*b*.
Tetanus observed in plague cases, 20,770.
Thirst marked, 20,841–3.
Tonsil: pseudo-diphtheritic membrane on, 20,813.
Urine diminished in quantity, of high specific gravity and intensely acid, I. 38*c*.
Vomiting, 8221, 12,398, I. 380.
Vomiting, bilious, 20,844.

See Bubonic plague.
,, Bullæ.
,, Carbuncles.
,, Diagnosis.
,, Eye.
,, Glands.
,, Glandular fever.
,, Inoculation: *Effect on clinical symptoms.*
,, Lesions.
,, Lungs.
,, Œdema.
,, Pneumonic plague.
,, Post-mortem appearances.
,, Pregnancy.
,, Prognosis.
,, Pulse.
,, Recurrence of plague in the same persons.
,, Relapses.
,, Septicæmic plague.
,, Skin.
,, Sputum.
,, Temperature.
,, Tongue.

CLOTHES:

Bacillus of Plague: isolation from clothes:
Failure to isolate it from clothing, 619–20, 635, 995, 8627–8, 25,347–51.
Never found in clothes worn by plague patients, 25,347.
No method for identifying the microbe in articles of clothing, 8537.
Once found in a piece of soiled matting which had been used by a plague patient, 35,393–6.
Plague cultures said to have been made from clothes, 7559.

Bacillus of Plague: vitality in clothes:
Bacillus of plague kept alive in clothes indefinitely, 10,094.

CLOTHES—*cont.*

Disinfection of Clothes:
Comparison between case of evacuation with disinfection and case of evacuation without disinfection at Poona proves value of disinfection, 21,251–7.
Disinfecting of personal clothes, very useful, 10,921, 23,187.
Effect of disinfecting clothes at Kalankot and Gharibabad, 11,144, 11,162.
Effect of disinfecting clothes in Shikohpur, Panjab, and other places, 9994, 10,145–6.
In Kankhal and Jawalapur infected effects taken to camp without disinfection, and soon ceased to be infective, 9294–7.
Natives conceal infected clothes in order to prevent their disinfection, 8780, 9224, 9256, 24,393.
Personal disinfection more disagreeable to the natives than the disinfection of clothes, 21,510
Villagers bribe disinfectors to save their kit from being put into perchloride, 21,174–5.

See Boiling.
,, Burning.
,, Camps, *Disinfection in.*
,, Carbolic Acid.
,, Jeyes' Fluid.
,, Perchloride of mercury.
,, Phenyle.
,, Port Regulations.
,, Railway measures.
,, Steam.
,, Sun.

Infectivity of Clothes:
Case of probable infection by clothes, 320.
Case of infection at Karachi through clothes that had three months before belonged to a man who died of plague, 11,961.
Case of infection through effects of a dead man, 2107.
Cases of infection at Dharwar Hospital from pneumonic sputum on clothes, 1892.
Cause of plague case at Multan traced to clothes sent from Bangalore, 10,871–99.
Clothing brings infection to Hardwar, 6435.
Clothing destroyed, and no further infection in Bangalore Hospital, 2894.
Clothing stolen from dead bodies infected a whole family at Satara, 22,024.
Clothes, one of the only factors which has proved infective, 21,717.
Danger of infection through clothes much less than through bags containing grain, in which rats would nestle, 994.
First case at Kariam traced to infection by clothes, 10,394.
First case at Kulewal traced to infection by clothes, 10,747.
Infection carried to village near Satara by clothes stolen from bodies of the dead, 22,527.
Infection carried by clothing in Hubli, 3121.
Infection carried by wearing apparel, 8885–90.
Infection carried by property from infected houses, 24,391–8.
Infection conveyed by clothes from plague corpses, 23,011.
Infection conveyed by clothes stolen from plague patients by temporary attendants, 23,957.
Infection from clothes given to Mahars, 24,084.
Infection probably due to clothes, 2107–14, 9271, 10,081, 10,082–93, 10,397–8, 10,754, 11,333, 12,712, 12,731, 12,997–8, 14,347, 15,170, 15,202, 17,994, 18,129, 18,502, 19,028, 20,066–74, 20,076, 22,572, 22,878.
Plague introduced at Hardwar in the clothes of a person from Karachi, 8911.
Plague infection may harbour for a considerable time in clothes kept in the dark; case in camp on opening box of clothes brought out seven days before, 9483.
Probable case of infection by clothes at Thana of Parsee lady, 320.
Rats, clothing, and some unknown agent in the retail grain shops are the chief causes of the spread of plague, 11,278.
Supposed case of infection from clothes at Rahon after the people returned to the town

Clothes—*cont.*

free from plague, having been 28 days in camp, 10,562.

See Acharj.
,, Dhobis.
,, Domes.

Infected Clothes concealed :

Natives hide infected clothes and this probably causes spread of plague, 8780, 9224, 9256, 24,393.

Opinion that Clothes by themselves not very infective :

Clothing cannot spread disease without human agency, 10,328.
Clothing not very infectious to hospital attendants, 237.

COLD WEATHER :

Baroda : In the cold weather plague showed a tendency to spread a little in camps, 15,111.
Baroda : People crowd their houses in the cold weather and plague is then worst, 15,240–3.
Bombay : Epidemic increases during the cold weather owing to the people sleeping more indoors ; 350,000 people sleep in the streets in the hot weather, 26,104.
Bombay : In cold weather natives sleep in their houses huddled up together and are more liable to plague, 550, I. 378.
Bombay : Virulence of the plague attributed to the excessive cold weather, 1032.
Bombay : Virulence of the plague in the third epidemic, and the prevalence of the pneumonia form, must be attributed to the excessive cold weather, 17,888.
Broach : The habit of the people to sleep outside their houses in the hot weather the reason that the plague remained dormant in May and June, 14,385.
Kolaba : Plague increased in each of the epidemics in the cold weather, dropped as the hot weather came on ; during the rains there were no cases at all, 17,650.
North winds prevailing in cold weather cause that part of the town which the wind reaches first to be first attacked by the disease, 10,844.

COLI-BACTERIUM :

Plague bacillus appears to be at antagonism with bacterium-coli, 1531.
Tests on the effect of bacillus coli on the growth of the plague bacillus proved it to have some retarding influence, 26,150.

CONCEALMENT OF PLAGUE CASES :

Causes of :

Disinfection, wholesale, and evacuation led to concealment of cases at Surat, 16,004.
Enforcement of restrictive measures led to the secreting of the dead and the running away of the people, 17,864.
Evacuation impossible in a big city ; only leads to concealment of cases and frightens people away, 7854, 8272.
Female cases, concealment of, tends to make statistics doubtful, 6576.
Ignorance, concealment of cases mostly due to, 16,137, 17,949.
Plague measures, objection to, so great that people will not report cases, 7824.
Police activity in discovering cases objected to, I. 469.
Segregation leads to concealment of cases, 2672, 3390, 3480.
Too strict measures and compulsory segregation would lead to concealment of cases, 12,822.
Undiscovered cases most dangerous source of infection, 10,994.
Worrying the people about sending their patients to hospital leads to concealment, 25,806.

Extent of :

Bandra : General disinclination in the district to give information, 18,956.
Bangalore : Concealment even among the officials and the more intelligent part of the people, 3288.

Concealment of Plague Cases—*cont.*

Bangalore : Greatest concealment among Muhammadans, 2993, 25,022.
Bangalore : Information of cases usually not given till after death, 2604, 3348.
Bangalore : Serious concealment and throwing out of dead bodies due to application of segregation and plague measures, 2593, 2604, 2671, 2696, 3223, 3282.
Bombay : Cases concealed, 487–9, 4033.
Bombay : Dead bodies spirited away, 10,954.
Bombay : Difficult to get information in Mussulman districts, 1349.
Bombay Plague Committee, great difficulty in ascertaining number of plague cases under the, 17,862.
Calcutta : Low mortality in, not attributable to concealment of cases, 6758.
Calcutta : Probable concealment makes statistics unreliable, 6555, 6576.
Cases concealed and carried from house to house, 489, 6599, 14,603, 16,229–32.
Daman : Bodies found in an advanced state of decomposition, 16,392.
Guntakal : Concealment of cases by Muhammadans, 4197.
Hubli : At beginning cases fully reported, but system broke down later, 19,799–803.
Hubli : Dead bodies found in the streets, 3079–81.
Hubli : Good many cases escaped record, 1590.
Hubli : In camps deaths escaped record, 1981.
Hubli : In one house 9 out of 13 inmates died of plague before the authorities knew the house was infected, 23,383.
Hubli : Plague mainly notified after death, 19,836.
Kankhal : Cases in Muhallah camp reported, but at Jawalapur efforts were made to conceal them, 9345.
Karachi : Concealment of deaths impossible, 11,183.
Karachi : Concealment of cases led to regular inspection of the camps twice a week, 11,886.
Karad : Plague concealed for two months, 21,060.
Khandraoni : Plague existed in the village for two months before it came to the notice of the authorities, 9697.
Panjab : Organised arrangements for concealment of cases in the villages, 9948–57.
Poona : Concealment practically impossible, staff being very strong, and working from daylight till dark, 10,913.
Rohri : People hiding with their sick in the date plantations hunted out by mounted men, 12,117.

Measures to prevent :

Belgaum : Non-removal of patients and contacts in second epidemic caused people to give accurate information of cases, 23,138.
Bombay : Under the present system of not removing patient to hospital except by the desire of his friends, concealment of cases is the exception, 25,815.
Bulsar : Concessions given to inoculated prevented the concealing of cases, 16,291.
Mysore : Under voluntary system cases mostly detected, 24,969.
Surat : Concealment punished by removal to general instead of caste hospital, I. 257.
Surat : Concealment of cases threatened with removal to Government hospital, 13,157.
Surat : Segregating neighbours of plague cases not reported, resulted in voluntary report of 230 out of 281 cases, 16,029–32.
Surat : Ward system broke up the conspiracy of silence as to plague, 16,004.

CONSTITUENTS OF HAFFKINE'S PLAGUE PROPHYLACTIC :

See Haffkine's plague prophylactic.

CONTACTS :

Difficulty in securing :

Difficult to trace contacts, 18,934.
Impossible to get hold of the contacts in a crowded chawl, 8261.
Vigilance Committee fail to remove contacts, 7610.

CONTACTS—*cont.*

Not removed:

Bombay: District medical officers have no power to remove contacts, 7610.
Bombay: Not a single case among contacts in, after thorough disinfection of room where patients had died, 25,803.
Daman: Contacts left in the house, 16,369.
See Treatment of the sick in their own houses.

Number segregated:

Average number of people to be segregated, four or five per case of plague, 310.
Bangalore: Instead of three or four contacts per family less than two found on the average, 3351.
Bombay: Only 400 persons in contact camp; if removal of contacts properly worked the number ought to have been 2,000, 10,953.
Bombay: Only one contact secured to four cases of plague, 4031.
Cutch Mandvi: Seven or eight segregated per case, 493.

Segregation of:

Contacts should be kept in a camp by themselves if possible, 23,346.

See Camps.
,, Segregation.

COPPER SULPHATE:

Result of tests with metallic salts, II. 6.

CORDONS:

Cordon arrangements and details of:

Daman and British India: intercourse between, periods in which stopped and permitted, 15,964–5.
Daman: Cordon removed in November 1898, 16,609.
Daman: Frequenting of Portuguese liquor shops not forbidden to British subjects when plague discovered in Daman, 15,967–8.
Daman: From April 1897 to October 1898, inter-communication permitted with passes, and of 2768 persons detained four developed plague, 15,965, 15,993–4, 16,609.
Daman Frontier: Description of cordon operations, 15,979.
Daman: One hundred and thirty-nine persons arrested for attempts to break the cordon, 15,989.
Daman: Plague did not spread, even before cordon put on, as villagers themselves refused admittance to suspicious persons, 15,964, 16,000.
Daman: Salt cordon increased during plague, 15,969.
Daman: Three lines of protection on British side of frontier: salt cordon, police, and villagers, 15,977.
Hubli: Chawls cut off by cordon of armed police, 1581–4.
Kaira: Strict measures to prevent communication between Kaira district and the Gaikwari territories by the Mahi river, 14,015.
Karachi: Advantageous for imposition of cordon, shut in by the desert and the sea, 11,297.
Karachi: Cordon at, 11,240, 11,297.
Karachi: Malir camp surrounded by thorn fence and guarded by police and sepoys, 12,441–45.
Karachi: Nine plague cases stopped by the cordon, 12,433–4.
Khandraoni: Cordon broken by one person, 9669.
Khandraoni: Villagers placed under a cordon, 244 soldiers for 435 people, 9648, 9669, 9693–4.
Kotri: Cordon regulations at, 12,605–19–37.
Kotri: Cordon placed three days after second outbreak, 12,603.
Kotri: No one could get away without a pass, 12,680.
Kotri: Railway quarter not included in cordon, 12,620.
Palanpur State: No cordon around the camps, 13,164.

CORDONS—*cont.*

Panjab: Cordons placed round village area to prevent communication of people of infected village with outsiders, and round village site to prevent visits to infected houses, 10,057.
Panjab: Cordons and evacuation the measures natives most dislike, 10,310.
Panjab: System of police cordons, 10,057–63.
Poona: Cordon arrangements, 21,299–300.
Rohri: Cordoned against plague from Sukkur, 12,923.
Rohri: Cordoned to protect Panjab, but infection passed through the cordon, 12,046, 12,078, 12,083.
Sholapur: Cordon arrangements, 20,229–30.

Cordons efficient:

Bilron: Cordoning of the area successful, but the village re-infected from outside, 10,741.
Cordon at Mahi river was sufficient to stop the spread of plague until it was relaxed, 14,193–201.
Cordons, effective measures for preventing the spread of disease outside, 9737.
Cordon of police has a great moral effect on the people, 10,061.
Cutch Mandvi: Cordon of 20 men sufficient for a village in Cutch, with 900 inhabitants, 13,584–9, 13,649–53.
Daman: Registration accurate because of cordon in, 160.
Karachi: Few cases escaped the cordon, 18,140.
Kotri: Only two individuals got through the cordon, 12,621, 12,633.
Panjab: Want of cordons around the villages cause of plague in the Panjab camps, 10,758.
Sirohi: Cordons round infected villages, plague did not spread out, 9737, 9901.
Umra: Cordon effective in preventing spread of plague to Surat, 2051–2.

Cordons inefficient:

Cordon of eight to the mile insufficient, 10,779.
Cordons round infected villages should in all cases be combined with inspection, 10,637.
Cutch Mandvi: Cordons no good in Cutch, only tried in two cases, 13,501–3.
Daman: Notwithstanding cordon, people escaped, 16,446.
Hubli: Cordon not efficient through slackness of police, 1584.
Hubli: Cordon of 60 native police insufficient to keep people out of evacuated chawl, 20,149.
Hubli: First cases attributed to default of the police and clandestine meetings with people belonging to the chawls when they were cordoned, 2407, 2436, 20,151.
In practice quite impossible to maintain the cordon in an efficient manner, 8782, 8922.
Mahi river cordon failed to keep out plague, 14,055.
Molagavelli: Police guard inefficient at, 4442.
N.W.P.: Position of Kankhal very favourable for cordon, but cordon ineffective, 9213, 9366.
Panjab: Cordons not strictly effective, 10,528–9.
Panjab: Effectiveness of cordon decreases in proportion to the extent of area, 10,316.
Panjab: Facility for passing the cordon by bribing the police, 10,242.
Panjab: Only eight men to the mile in cordons round village areas, and cordons not strictly effective, but moral effect good, 10,058–67, 10,242.
Poona: Not the least difficulty in evading the cordon, 21,328.
Sholapur: Cordons drawn round the Sadr Bazar and town to confine the plague a complete failure; precautionary measures frustrated by bribing the police, 20,230, 20,237–8.

CORPSE BEARERS:

Corpse bearing is not dangerous, 19,962.
Instance of plague among corpse bearers in Bombay, 19,036.

CORPSE BEARERS—*cont.*

No cases of plague from corpse bearing in Poona, 21,566–9.

People kept plague alive in camp at Surat by going into the town and acting as corpse bearers, 24,422.

See Funerals.

CORPSE INSPECTION:

Arranged under the following subheads:

Adoption at.

Agency by which carried out.

Complaints made in Places where carried out.

Efficacy of Corpse Inspection in Classification of Causes of Death.

Efficacy of Corpse Inspection if Females exempted.

Efficacy of Corpse Inspection in Identification of Plague.

Efficacy of Corpse Inspection in detecting Pneumonic Plague.

Efficiency of Corpse Inspection with reference to the Place of Inspection.

Feelings of the Natives regarding Corpses.

Feelings of the Natives regarding Corpse Inspection generally.

Feelings of the Natives regarding Corpse Inspection of Females.

Opinions regarding Corpse Inspection as a Measure.

Adoption at:

Ahmedabad: Corpses seen after death from plague, 13,945.

Ankleshwar: Carried out in 1898 at, by three medical officers sent from Bombay, whose duty it was to inspect the corpses, which should be disposed of only on death certificates or after corpse inspection, 14,264–7, 14,280–2, 14,295–6.

Bangalore Civil and Military Station: Carried out in, without objection or opposition, 2709–10, 2712, 25,101–3, 25,105.

Bangalore City and Mysore City: Carried out in, without disturbance, 25,101–3, 25,105.

Baroda City: No corpse inspection at, between first and second outbreak, 14,592–3.

Baroda State villages: Carried out in, but requires great tact, 14,665–6.

Belgaum district villages, none in, 23,019.

Bhagda, corpses seen at, 16,199–200.

Bombay: At cemeteries, no corpse inspection in end of 1898, 1122.

Bombay: At cemeteries, medical officers have orders not to examine bodies under any conditions, 25,654–5.

Bombay: Bodies examined at cemeteries in suspicious cases, till this excited opposition, when examination given up, 1119–21.

Bombay, in, no system of examination of dead in end of 1898, 1129.

Bulsar: In absence of death certificate all bodies examined, 16,169–72.

Calcutta: In many cases when suspicious case heard of, inspection of body allowed by people of the house, 6514.

Calcutta: No system of corpse inspection in, 6513, 6515.

Cutch Mandvi: Carried out at, after January 1898, except when death certificate produced, 13,435, 13,552.

Hubli: Bodies examined by Dr. Cardoz, who was occasionally, but not regularly, employed on the duty, 2473, 3136–7, 19,840.

Hubli: Bodies examined with post-mortem in doubtful cases, 3076–8, I. 400 (XVIII.), 20,007.

Hyderabad, Sind: Carried out in, without objection, 18,132–3.

Hyderabad State: Carried out in, 5928–9.

Kaira district: No regular corpse inspection in, as most plague cases died in hospital, but corpses, male and female, examined if necessary, 14,083.

Karachi: None at, before or at beginning of first outbreak, 12,987.

Karachi: Carried out between the first and second epidemics, 11,183.

Karachi: At end of 1898 modified corpse inspection carried out, being dispensed with for children under two, and in cases where death properly certificated, 11,384–90, 12,777–8, 12,790.

Karachi: All corpses seen for which death certificate not forthcoming, 11,394.

Karachi: Compulsory in the voluntary camps at, for every person who had not been under treatment, 11,937.

CORPSE INSPECTION—*cont.*

Karachi: Corpse inspection not necessarily resorted to in case of Khojas, 13,259.

Karad: Corpse inspection resorted to, 21,064.

Kotri: All corpses of children inspected at, 12,688.

Kotri: Carried out in, without objection, 18,132–3.

Kotri: Was carried out at, in first epidemic, being discontinued in May 1897, but re-instituted in September, and continued throughout second epidemic, 12,554–5, 12,681–6, 12,734–5.

Mysore: Corpses regularly inspected without disturbance, 25,101–5.

North-Western Provinces: Both Hindus and Muhammadans examined; the latter did not resent it more than the former, 9236–8.

North-Western Provinces: Carried out in, those who would not submit to it being treated as infected, 9191, 9197–9.

Palanpur: Carried out in, 13,166.

Poona City, Carried out in, 21,355–83, 21,416–50, 21,774–8.

Poona City: Corpse of every person who dies in, inspected, 21,486.

Poona City: Diagnosis made by, plus inquiries into history of cases, 21,422.

Poona City: Done at the house in Poona, not at cemeteries, 19,916–8.

Poona City: Statistics relating to, in, 21,416, 21,788–94, 21,859–64.

Poona Cantonment: Carried out before removal of body, 21,261, 21,299.

Porbandar: Of all Hindus and male Muhammadans carried out at, 13,730.

Punjab: Carried out and not objected to in Banga and other villages, 10,253.

Rohri: Carried out in, 12,026.

Satara Town: Carried out in, without objection, 22,481–6.

Satara district villages: Carried out in, 22,629, 22,747.

Sholapur: Carried out at, without opposition, males and females, Muhammadans, Hindus, and even pardah nashin women, 23,879–86.

Sirohi: Not resorted to in, 9778, 9899.

Surat: Corpses examined at the instance of the Vigilance Committee, 15,854.

Umreth: Bodies seen at, 14,024–5.

Agency by which carried out:

Ankleshwar: Done in, by Hospital Assistant Cooper, 14,265–6.

Bhagda: Corpses seen by Hospital Assistant, 16,199.

Bhiwandi: Carried out in, by hospital assistants, 22,185–8.

Bhiwandi: Memons at, object to corpse inspection of their women by hospital assistants without presence of Europeans, 22,189.

Bombay: Hindus and Mussalmans willing to have corpses examined by Europeans, but not by Native doctors of another faith, 17,968.

Bulsar: Carried out in, by hospital assistants, 16,169–171.

Calcutta: Objection to would depend on manner in which carried out; with Brahman doctors Hindus would not object to corpse inspection of males, 6606, 6682.

Cutch Mandvi: Done by Native doctors and hospital assistants, and certificates liable to suspicion, U., 474.

Hubli: Done by Assistant Surgeons and Hospital Assistants, 3077.

Kaira district: Done by Hospital Assistants, 14,083.

Kotri: Done in, by Assistant Surgeon Macmullen and by hospital assistants, 12,700, 12,735.

North-Western Provinces: To meet Native views, carried out by assistant surgeons accompanied by Native medical practitioners; if they disagreed, Commissioned Medical Officer called in, 9200.

Poona City: Arrangements for carrying out in, 21,423–442, 21,462–4.

Rohri: Done in, by hospital assistants, 12,026.

Satara: Carried out at, by Hospital Assistants, 22,482, 22,575.

Satara: If done only by European doctors, could be done only at headquarters, as no staff available elsewhere, 22,102.

Umreth: Bodies seen by Hospital Assistant, 14,024–5.

CORPSE INSPECTION—*cont.*

Complaints made in Places where carried out:

Caste, objection to inspection of a Brahman in Poona City, except by Brahman doctor, 21,373.
„ objection to examination in case of a fakir in Pooma City; body afterwards examined by Brahman doctor, 21,373, 21,825.
„ in examination sometimes caused great annoyance in Poona City, 21,549.
„ two or three complaints in Poona City that laying out of body delayed, 21,569–71.
Delay in objections have been made in Poona City on account of delay in disposing of body, 21,869–71.
„ objection to, in the North-Western Provinces on ground that it delayed disposal of body, 9200.
In Poona not objected to, except on two or three occasions, 19,920–1, 19,934.
Sex, only one objection in Poona City to examination of female corpse, 21,375.
„ one objection in Poona City regarding Muhammadan female, whose body was finally inspected, 21,825.
„ only one objection in Sholapur, not serious, female doctor being asked for, 23,884.

Efficacy of Corpse Inspection in Classification of Causes of Death:

Allows of classification only into "certainly plague," "certainly not plague" and "suspicious," 21,902-5.
In Poona City deaths classified as plague only when bubo found; character of deaths classified as suspicious, houses in which they occurred being disinfected, 21,906-13.

Efficacy of Corpse Inspection if Females exempted:

Useless unless bodies of women examined, 3810.
Would give good idea of existence of plague if most males examined, 6606, 9239.

Efficacy of Corpse Inspection in Identification of Plague:

Best means for early detection of epidemic, 9521–2.
Disease cannot always be diagnosed after death 11,366.
Every case of plague could be discovered by corpse inspection, by existence of buboes or œdema, 9620.
Found useful in Poona City for the identification of plague cases when done by European doctors, 10,979–82.
In many cases impossible to detect cases of plague by, 21,958.
In Poona City 85 per cent. of deaths due to bubonic plague, which can be detected by corpse inspection, 21,865.
In Poona City 106 cases of plague, mostly bubonic, discovered by corpse inspection among 3,035 corpses examined, 21,376–9, 21,444–5, 21,449, 21,477–84.
Instances in which the inspection of corpses actually carried out at Karachi has failed to detect presence of plague, 12,780–8.
More useful than sick visitation, only one case of plague discovered by latter in Poona City to four discovered by corpse inspection, 21,381–2.
No example met with of subsidence of bubo in bubonic plague just before or after death, 21,443, 21,866.
Suspicious cases; general look of body and retention of warmth assists detection by corpse inspection, 21,365–6.
Useful but not reliable for the detection of every case of plague, 12,779.
Useless where bubo is not pronounced, and in puerperal cases, 11,391.
Would help in determining whether there was plague or not, 6605.
Would not materially assist in finding plague cases in Bombay, 17,887.

Efficacy of Corpse Inspection in detecting Pneumonic Plague:

Pneumonic plague, after death, could not be detected by, 1135, 3808, 11,393, 14,285, 18,134, 26,744.
Pneumonic plague can, generally speaking, be detected by medical examination and inquiry, but not certainly, 21,732–8.

CORPSE INSPECTION—*cont.*

Pneumonic plague cannot be diagnosed in case of dead body with certainty without bacteriological examination, 20,017–8.
Pneumonic plague, could be discovered by corpse inspection by existence of œdema, 9618–20.
Pneumonic plague, could not be absolutely certain about, by corpse inspection, 19,922.
Pneumonic plague, difficult to diagnose by, 21,465–8.
Pneumonic plague, not much value in cases of, without post-mortem, 8812–3, 9434–5.
Pneumonic plague, not sufficient to ascertain whether a person has died of pneumonic plague without history which is inquired into, 9240–2.
Pneumonic plague, of doubtful use in, and would give uncertain results, but indications might be gained from dribbling saliva, 6686–8.
Useless for discovery of plague, as pneumonic form of the disease cannot be found out by it, 26,799.

Efficiency of Corpse Inspection with reference to the Place of Inspection:

Advantage of showing where plague has occurred, not realised if examination takes place at cemeteries, &c., 8815–6.
Better done in house where death occurred, as, if death due to plague, information as to locality of disease obtained, 19,919.
Bodies in some cases removed from houses in which they were living, 8317.
No corpse could be surreptitiously buried in Poona City in beginning of 1899, 21,570-2.

Feelings of the Natives regarding Corpses:

Against Hindu and Muhammadan religions to touch a corpse after death, 12,521.
Corpses not to be touched after the death ceremonies performed, 26,745.
Dead not regarded with same respect as living, no objection to body being touched, 3805–7.
Objection to body being seen or touched, 10,987.
People have great horror of their dead being examined after death, 13,435, 13,552.
People often quite callous as to what is done with their sick and dead, 22,749.
Unnecessary to bare body for corpse inspection, 3307.

Feelings of the Natives regarding Corpse Inspection generally:

Bangalore Civil and Military Station: Muhammadans object to their houses being entered for, 2711.
Bhiwandi: Would have been difficulty about in, for Muhammadan females, and could not have been carried out without presence of Europeans, 22,193–4.
Bombay: Anything more than summary inspection of body would be impossible, and resented on religious grounds, 1136.
Bombay: At the cemeteries not objected to in, 17,745.
Bombay: Corpses sometimes examined at cemetery, till opposition caused by it led to abandonment of such examination, 1120.
Bombay: Done in, without difficulty, but Natives would object to it in most cases, especially if done by young medical men, 17,967, 17,970.
Bombay: Would arouse much opposition, 25,619.
Bombay: Would have a very disturbing effect in, 17,887.
Calcutta: Allowed by people of houses in several cases, 6514.
Calcutta: Difficult, and would require delicate handling, 6682, 6684–5.
Calcutta: Muhammadans would not object to, for males, if doctors good class and right sect, 6683.
Calcutta: Some Natives rather pressed adoption of, as a measure, 6685.
Calcutta: Would be objected to, even for males by Hindus and Muhammadans, who would yield neither to it nor to rule requiring death certificate in lieu, 7548.
Cutch Mandvi: No objection to, at all among Muhammadans or others, 27,174–6.
Karachi: Hateful to the people, because against Hindu and Muhammadan religions to touch a corpse after death, 12,521.

Corpse Inspection—*cont.*

Karachi: Muhammadans in first outbreak got accustomed to, but before second outbreak asked for services of lady doctor, for which they paid fees, 3803.
Karachi: No great objection to, 3802.
Many classes to whose feelings corpse inspection is repugnant, 11,284.
Muhammadans have submitted to it in certain parts only because they saw the hopelessness of opposition, 26,811.
Muhammadans would be prepared to have their female corpses examined in preference to undergoing segregation, 9440–2.
Muhammadans would object specially, but corpse inspection is objectionable to all people, 17,886.
Northern India: It would be dangerous to make corpse inspection compulsory in the North-Western Provinces and the Punjab, and not worth the opposition it would give rise to, 26,744, 26,801.
North-Western Provinces, disturbances and riots at Jawalapur, due to dislike of corpse inspection and to other causes, 9192–6.
North-Western Provinces: More difficult in Jawalapur than in Kankhal and Hardwar, 9243–4.
North-Western Provinces: Natives prefer corpse inspection, even of women, to segregation, 9439.
North-Western Provinces: Not liked in, but carried out without opposition if not insisted on where death properly certified, 8808–10.
Panjab: Not objected to in villages, 10,253.
Poona: No objection to in, where Muhammadans not strong, 10,883, 10,885.
Poona: No objection to, 21,824.
Poona: Not opposed in, because the Natives were utterly disheartened, 26,744, 26,775–7.
Rajputana: Not possible among the Muhammadans in, especially in large towns, 9780.
Satara: Natives would have no objection to examination of corpses by European doctors, 22,098.
Satara: Hindus would not object to examination by Muhammadans and *vice versâ*, 22,099–100.
Satara: No difficulty about corpse inspection at, Muhammadans being a small and inconsiderable portion of the population, 22,481–6.
Satara district villages: Corpse inspection offensive to the feelings of the people in, 22,629–30.
Satara district villages: People as a rule show both male and female corpses freely, 22,748–9.
Sholapur: No opposition, 23,879–86.
Sind: Not liked anywhere in, 3802–6.
Sind: No very violent opposition to in, 11,284.
Sind: Muhammadans would probably object to examination by Hindus, 22,099–100.

Feelings of the Natives regarding Corpse Inspection of Females:

Bangalore Civil and Military Station: Female bodies examined by females, 2711, 25,104.
Belgaum district villages: Would be offensive and impossible in case of pardah women, 23,020–4,
Bhiwandi: Difficulty probable at, in the case of females, 22,193–4.
Bhiwandi: Memons at, object to females being examined by Natives in absence of Europeans, 22,189.
Bombay: Muhammadans might resent examination of females, but have not done so, 17,969.
Calcutta: Better left alone as regards females; even by female doctors, would not be liked, 6606–7.
Hyderabad Sind: Female corpses inspected by lady doctor, 18,134.
Hyderabad State: Some Muhammadans object in case of females, but female bodies not examined, 5930–1.
Karachi: Bodies of women examined, 3810.
Karachi: Muhammadan females, bodies of, examined by ordinary doctors, and later by female doctors, 3804.
Mysore: Women examined by women, 25,104.
North-Western Provinces: Carried out in case of women, 9433.
North-Western Provinces: Females examined by men, but inspection of female corpses not insisted on to the same extent as that of males, 9202–4, 9239.
North-Western Provinces, Hardwar and Jawalapur: Muhammadans objected to in case of females, but no great objection, 9265–8.
Objection anticipated, especially in case of females, 26,745.
Poona Cantonment: Done in houses in, without objection, regarding Muhammadan women or others, 21,260–2.
Poona City: No objection particularly to inspection of female corpses, 21,372.
Porbandar: Only during the height of the epidemic insisted on for female Muhammadans, 13,730.
Satara: Muhammadan females have been examined by doctors, though the examination usually done by female doctors, 22,101.
Satara district villages: Of females not offensive to feelings of people, except in pardah families, 22,748–9.
Sholapur: Examined at, without opposition, 23,882–4.
Sholapur: No objection to examination of groin of female bodies, 23,973–4.
Umreth: Female bodies seen by Hospital Assistant, 14,025.
Utterly repugnant to Muhammadans and Hindus, especially in the case of females, 26,810.

Opinions regarding Corpse Inspection as a Measure:

Bombay: By slow process corpse inspection in, at houses might be ultimately introduced, and would be advantageous, 25,620–1.
Bombay: Corpse inspection would undoubtedly give more complete information than system of inquiry in force in Bombay, but would arouse much opposition, 25,618–9.
Bombay: Not recommended in, where Muhammadans too strong, 10,983–5.
Bombay: Probably not applicable to, 19,924–5.
Bombay: Recommended in, at cemeteries, where it is probable that correct addresses of deceased would be obtained, 17,748–52.
Cardinal measure before sanitation and preventive measures, but the difficulty is how to do it, 26,738.
Death registration without corpse inspection would be valuable, 26,748.
Desirable in places where considered feasible by authorities, 9438.
Difficult to make it penal to move or dispose of corpses before examination, 26,737–8.
Most important in large towns at the beginning of an epidemic to ascertain presence of plague; unnecessary after plague has spread, 9247.
Most valuable agency there can be employed, combined with bacteriological examination of suspicious cases, 21,929.
Necessary for early detection of first cases, 9430–1.
Necessary measure, 8810, 8815.
Necessary, but should be carried out with as little inconvenience to people as possible, 21,549–50.
Next best measure to notification of diseases, 19,923.
Northern India: In large towns, would require great tact, but could be arranged at beginning of epidemic with co-operation of Native leaders, who would be anxious to trace and suppress first cases; impossible after spread of plague, 9247–51.
Not recommended, 26,743, 26,747, 26,774, 26,795–7.
Not recommended that Health Officer should have the power of corpse inspection if, under a system of death registration, he doubted the correctness of a certificate, 26,773, 26,804–9.
Option may be offered of submitting to or being treated as infected, 8814.
Possible to be carried out in most communities, 9432.
Unnecessary in Karachi, owing to efficiency of system of supervision of mortality, 11,284.
Unnecessary, as plague cannot always be diagnosed by, after death, 11,366.

CORPSE INSPECTION—*cont.*
Unnecessary if mortality carefully watched, when, if suspicion aroused, it can be resorted to, 11,285.
Very valuable, 11,284.

COTTON:
See Merchandise.

COW-DUNG:
Identification of Bacillus of Plague in:
No method for identifying the microbe in cow-dung, 8537.
See Isolation of plague bacillus.
Life of Bacillus of Plague in:
In laboratory experiments the plague bacillus lived several months in sterile cow-dung, 18,877.
In unsterilised cow-dung the microbe did not live more than six days, 18,880.
See Perchloride of Mercury: *action on the bacillus of plague.*
,, Permanganate of potash.

CULTURES OF BACILLUS OF PLAGUE:
Aeration of:
Addition of ghee to assist aeration of cultures, 27.
Characteristics:
Characters of bacillus not constant in cultures, very varying, and different from characters of bacillus obtained from the blood of animals, which are constant, 26,123.
Size varies considerably in the different growths, 26,402.
Specular appearance of colonies on agar, not an important characteristic, 8667, II. 4.
Stickiness a very variable attribute in the microbe of plague, 8669.
Tangled skeins, in bouillon and gelatine the bacilli form long, 8719.
Thready colonies produced on agar plates, 27,320.
Typical appearance of plague cultures, 3654.
Isolation of Plague Bacillus in Cultures:
Method for isolating the plague microbe in cultures, 8594.
See Isolation of bacillus of plague.
Media of Cultivation:
Agar: In cultures with salt agar involution forms will develop in 24 hours if kept in the incubator, 8717.
Albumose: Grows better in albumose than in real peptone, 17,387.
Bouillon: Characteristic growth of the microbe of bubonic plague in bouillon II. 4, 8719.
Gelatine: Characteristic growth of the microbe in, II. 4, 8719.
Peptone: Grows better in albumose than in real peptone, 17,387.
Plantain: Grows on sterilised plantain, 26,428.
Warden's broth: Bacillus of plague cultivated in Warden's broth and ghee for Haffkine's prophylactic, 27.
Post-mortem:
Method of making post-mortem cultures, 8393.
Temperature for:
Favourable cultivation temperature for bacillus of plague, 80–90 F., 99.
Plague virus grows fastest at a temperature of 37° C., 26,342.
See Bacteriological characteristics of bacillus of plague.
,, Coli Bacterium.

DAMAN:
Inoculation in, with Mr. Haffkine's Prophylactic:
Daman statistics, correctness of, 160.
Figures checked by Mr. Damaun Wala and his servants: effects of inoculation in each of three years, 25,429–41.
Increase of plague among the uninoculated at Daman, 18,701–7.
Inoculations among the Parsees, 16,787–93.
Inoculations at Daman, 25,417–26.
Inoculations done in 1897 and 1898: House inspections showed that the uninoculated got plague while the inoculated escaped, 16,361–4.
Inoculations introduced in March 1897, 16,405–7.
Inoculations in Upper Daman only, 27,207.
Inoculation of all the inhabitants of the Fort, Upper Daman, including about 140 soldiers, and though rats died there was no plague among men, evacuation being resorted to in addition to inoculation, 16,448–58, 16,476.
Inoculations performed at Daman, summary of results, 52–60, 72.
Investigation sheets, manner of preparing and checking, 27,211–3, 27,284–6, 27,297, 27,301.
Method of checking plague occurrences among the inoculated; in 1897, on account of the panic and confusion, the statistics are quite unreliable, 16,744–9.
Method of collecting and checking particulars for the preparation of investigation sheets and the Joint Report on Inoculations, 16,862–77.
Method of inquiry into the figures relating to Daman, 27,189–204, 27,274–9.
Mortality rates among the inoculated and the uninoculated at Daman, 27,280–302.
No death from cause other than plague among the inoculated in Daman, 25,446.
Report on plague in Lower Daman, and effect of inoculation there, by Mr. Haffkine and Surgeon-Major Lyons, I. 305–330.
Upper Daman: In 1898 333 inoculations, with 48 cases and 18 deaths among the inoculated, 16,460–3, 16,604–5, 16,611.
Watch kept upon the inoculated, to observe effects, 25,427–8.
Statistics relating to:
Concealment of cases common, so that in April 1897 the authorities took steps to ascertain the numbers of dead by special inquiries at burial and burning places, 16,415–9.
First epidemic in 1897, February to June, Lower Daman only affected, 2,000 deaths, I. 306, 16,371–2, 16,385.
In epidemic of 1898, 1,318 attacks and 1,224 deaths, 16,511.
Lower Daman: Out of a population of 7000, 3,000 died of plague, 15,959.
Lower Daman: Population 7,000, 16,846, I. 305.
Plague at first worst among Muhammadans, and spread to Machis, 16,351.
Plague spread from Machis to Banniahs, and thence to Muhammadans, 15,959.
Population, number of deaths, and extent of emigration from Daman estimated by guess-work, 27,214–42.
Sixty-eight per cent. of cases plague mortality, I. 382.
System of death registration in force in 1897–8, 16,640–2.
Upper Daman: 1,000 deaths from plague during the second outbreak, 15,959.
Upper Daman: No plague in 1897, 16,430.
Upper Daman: Population, 5,400, 16,345, 16,464, I. 305, 16,438.

DAMP:
Bombay, Byculla Jail: Observations on the effect of moisture and dryness on plague in, 17,113.
Bombay: In 1896, a fortnight after the heavy rainfall, the shady streets were still damp, 898.
Bombay: Water supply had to be cut off from houses in Mandvi to prevent their dampness, 916.
Dryness of inhabited places the best prophylactic measure, III. 9.
Life of Bacillus of Plague in:
Flourishes most luxuriantly when the air is moist, 18,638.
Microbe establishes itself mostly in the presence of damp, dirt, and darkness, 9683.
Vitality of the germ is diminished by the sun and increased by damp, 17,031.
See Rains.
,, Sanitary conditions
,, Subsoil water.
,, Water.

DEATH CERTIFICATE.

See Registration of death.

DEER:

Died of plague at Bangalore, 3601.

DESICCATION:

Effects on Bacillus of Plague:

Bacillus found to survive five days desiccation in hydrogen, II. 8.
Bacillus somewhat resistant to desiccation, II. 13.
Bacillus desiccated in glass rods in air lived 48 hours, 18,886.
Bacillus shown by experiments to survive four days ordinary drying, 26,344–5.
Bombay, Byculla Jail: Observations on the effect of dryness and moisture on plague in, 17,113.
Desiccation with calcium chloride kills bacillus of plague in 18 hours, 26,346.
Disinfection of crews kit and clothes by exposure to sun and air, 738, 814.
Drying and ventilation the best methods of disinfecting, 33,662.

See Air and Light.
,, Sun.
,, Ventilation.
,, Hot weather.

DETECTIVES.

See Informers.
,, Spies.

DIAGNOSIS OF PLAGUE:

Difficulty of:

Bacteriological examination the only means of certifying plague cases, 20,014, 22,352.
Easy when several cases occur together, 6905.
Special points in diagnosis, I. 352.
Very difficult, 1016.

Of Plague and other Diseases:

Alcoholic poisoning: Plague mistaken for alcoholic poisoning, I. 382.
Glandular fever: Seeing patient once would not show whether plague or glandular fever, 6987.
Glandular fever: Cases in Bombay mistaken for plague, 22,351.
Malaria: Distinctive symptoms between plague and malaria, I. 381.
Relapsing fever: Doubt whether cases plague or relapsing fever, 24,184.
Relapsing fever: How to distinguish plague from malaria and relapsing fever, I. 381.
Relapsing fever: Many cases sent to plague hospitals proved to be of relapsing fever and other diseases, 25,732.
Small-pox: Epidemic of small-pox and plague which developed together in three villages of the Satara district rendered diagnosis of plague at first doubtful, 21,657.
Typhus: Difference between plague and typhus fever, I. 381.

Of Types of Plague:

Bubonic plague: Bubo not alone sufficient basis for plague diagnosis, 7530.
Bubonic plague: Difficulties of diagnosing plague during life, even when glands are enlarged, 20,027, 20,032.
Non-bubonic plague: Most men now engaged in plague work are absolutely incapable of diagnosing case of septicæmic plague, 20,135.
Non-bubonic plague: Native practitioners fail to recognise plague, especially septicæmic or pneumonic, 1638.
Non-bubonic plague: Pneumonic and septicæmic cases may easily escape detection if attention is only directed to the evidence of buboes, 13,915.
Non-bubonic plague: Position of the corpse an indication in the absence of enlarged glands, 18,556.
Pestis minor: Of mild bubonic cases, difficult, 6907.

DHERS:

Dhers, although impure people, not very subject to plague, 5478.
Plague spread by the Dhers, 15,337.

DHOBIS:

Bombay: Statistics of cases among dhobis up to December 1896, I. 392, 3.
Bombay: Statistics regarding dhobis admitted to the Arthur Road Hospital, III. 466.
Calcutta: No plague among dhobis, 7665–7.
Karachi: An outbreak, 5 cases, in a group of 70 dhobis, 11,314.
Satara: Cases among dhobis, 21,269.

DIRT:

Life of Bacillus of Plague in:

Dies in dirt, 1003.
Establishes itself mostly in dirt, 9683.
Incapable of existing in dirt, 1003–4.
Virulence of bacillus of plague increased by filth, 18,635.

Opinions that Plague a Filth Disease:

Open air and cleanliness the best preventives against plague, 17,711.
Plague bacillus establishes itself and ourishes in dirt, 9683.
Plague is a disease of filth and dirt, 7836.
Plague a filth disease, and will most likely attack persons living under insanitary conditions, 12,018.
Plague a manifestation of septicæmia by a special virus of filth disease, 25,249.

Opinions that Plague not a Filth Disease:

Filth *per se* has little influence; Byculla Jail beautifully clean, but most virulent outbreak, I. 378.
Kumaun: People very dirty; they hold that dirt is a preventive against Mahamari by closing the pores of the skin, 6159, 8832.
Plague not due to dirt, 1003.
Plague not a filth disease, 16,524.
Very dirty classes, such as Dhers and Mangs, not very liable to plague, 5478.

See Sanitary conditions.
,, Scavengers.

DISCOVERY OF BACILLUS OF PLAGUE.

See Isolation of plague bacillus.

DISCOVERY OF PLAGUE:

Early discovery of Cases, importance of:

Early detection the best means of preventing spread of disease, 9518.
Early discovery of plague cases most important, 19,479.
Early information of plague cases, &c. most important measure in dealing with plague, 12,052, 16,967–71.
Early information and proper measures prevented the disease for a long time from becoming local in Hyderabad Sind, 18,897.
Early intimation the most important factor, 17,669.
Evacuation would become unnecessary if information could be obtained early enough to limit the zone of infection, 21,711.
Evacuation unnecessary if early notification be given, 16,026.
If plague discovered early, ordinary measures stamp out the disease, 14,062.
Natives should be employed as voluntary agents for obtaining early information 26,730.
Partial evacuation sufficient if early intimation of first case obtained, 13,797.
Plague accidentally discovered at Backergunge, 7426.
Report of villages where early discovery of cases and suitable measures at once stopped the disease, 14,062.
The more perfect the organisation for obtaining early information the better results may be expected from plague measures, 21,708.

DISCOVERY OF PLAGUE—*cont.*

See Concealment of plague cases.
,, House searches.
,, Informers.
,, Notification of plague.
,, Registration of death.
,, Spies.
,, Staff: plague.
,, Volunteer agency.
,, Ward system.

DISINFECTANTS.

See Air and Light,
,, Boiling.
,, Burning.
,, Calcium Chloride.
,, Carbolic Acid.
,, Chlorine.
,, Chloro-Nitrous Ozone.
,, Copper Sulphate.
,, Electrolysed Water.
,, Izal.
,, Jeyes' Fluid.
,, Lime.
,, Lysol.
,, Naphthaline.
,, Nitric Acid.
,, Nitric Peroxide.
,, Oxydising Agents.
,, Perchloride of Mercury.
,, Permanganate of Potash.
,, Phenyle.
,, Reducing Agents.
,, Sulphur.
,, Sulphuric Acid.
,, Sun.

DISINFECTION:

Attitude of the People towards

Bangalore: Great opposition to disinfection, 2719.
Calcutta: Disinfection strongly disliked, 7520.
Calcutta: Disinfection carried out in Cross Street with difficulty by personal influence of officers, 7521.
Calcutta: Chemical disinfection supposed to injure the health by the residents of the Bara Bazar, and therefore resisted and hindered, 7520.
Calcutta: Natives begin to disinfect their houses of their own accord, 7661.
Calcutta: Objection to segregation but not to disinfection, 6545.
Calcutta: People might be persuaded to leave bustees for disinfection for three hours if camps were provided, 7718–20.
Cause of irritation in rural districts, 19,515.
Dharwar: People have no belief in disinfection, but apply for it as a means to return to their houses, 23,776.
Hubli: When disinfection of houses was gratuitous, people used to bribe the "coolies" to mark houses as disinfected without disinfecting them, 23,366, 23,399.
Hubli: People at a late stage in the epidemic paid for disinfection of houses, and they resorted to disinfection through rivalry to see who would spend most on it, 23,454.
Karachi: Opposition to disinfection, 11,651.
Natives object to disinfection by chemicals, 9740, 9823.
Native States: Great difficulties in disinfecting villages, 9739.
Objectionable from the Indian's point of view, 16,232.
Opinion that the system of drenching the houses whilst people were in them to a certain extent responsible for the epidemic, 16,137.
People not yet convinced of the usefulness of disinfection of houses, 14,763.
Surat: Chemical disinfection led to concealment of cases, 16,137.

Difficulties in carrying out Disinfection:

Bombay: At beginning of epidemic no apparatus, no stoves, no special disinfecting staff, 1147–50.
Bombay: Disinfection very imperfectly carried out in Bombay, 25,650.

DISINFECTION—*cont.*

Bombay: Unsatisfactorily carried out in, owing to want of supervision, 416.
Calcutta: Almost impossible in bustee houses, 7689–90.
Calcutta: Disinfection almost impossible in the insanitary parts of the town, 7519.
Calcutta: The construction of bustee houses is the great impediment to their thorough disinfection, 7709.
Chemicals useless in mufassil towns on account of the construction of the houses, 24,316.
Cleansing of the evacuated houses by the natives themselves a cause of renewed infection, 23,174.
Coolies cannot be taught to do their work efficiently, 16,987, 21,095–7.
Coolies careless: impossible to be sure of efficient disinfection of houses, 11,273, 24,342.
Difficult in hospital through patients spitting anywhere, 7085.
Disinfection of houses very little practicable, 23,187.
Disinfection of houses in India useless on account of the rats, which are not affected, and keep up the infection, 8578, 11,460–2, 22,864.
Hubli: The natives were made to pay the costs of disinfection with consequence that they took care the work was not shirked, 23,368.
Impossible to reach every crevice in houses with disinfecting solution, 20,161.
In native houses very difficult. So many crannies, 7856, 15,181, 24,342.
Of surface in houses at Karachi useless on account of their structure, 11,461.
Surat: Disinfection unsatisfactorily done 24,450.

Houses: Extent of Disinfection:

Ankleshwar: Only those streets disinfected where plague cases had been found, 14,234.
Baroda: Perchloride of mercury used for all houses in which any death had occurred. The others thoroughly cleaned, whitewashed, and opened for ventilation, 14,631.
Belgaum: Disinfection of houses and burial parties the only plague measures employed, 22,807.
Belgaum: No attempts possible to disinfect other than infected houses, 22,830.
Bombay: Houses at first, though not later, whitewashed in advance of the probable line of plague, 1214.
Bombay: In infected houses, only the room of the patient and the two adjoining rooms disinfected, 25,640.
Calcutta: Only the room of the patient disinfected, 6591.
Dharwar: Disinfection of houses in villages impossible with staff available, 2145–6.
Disinfection should not be confined to plague-infected houses, 14,439.
Karachi: Disinfection of all plague-infected houses, 12,789.
Malegaon: Every house disinfected, 18,080.
Mysore villages: No disinfection done, 3324.
N.W.P.: Whole town at Hardwar, Kankhal, and Jawalapur systematically disinfected, 9045–8, 9090, 9115, 9175.
Poona, Sadashiv Pet: Every house disinfected, 21,589.
Roof ought to be disinfected as well as rest of rooms, 7857.
Roofs should be disinfected as well as walls and floors, 20,163.
Satara: Restricted to infected houses, 22,298.
Sholapur: No disinfectants used in the villages round, 24,037.
Whole of wall might well be disinfected, 5785.

Imported Cases and Disinfection:

Disinfection without evacuation recommended for imported cases, 22,892.

Instances in which no Cases followed House Disinfection:

Ankleshwar: No instance known of plague contracted in a twice-disinfected house, 14,416.

DISINFECTION—*cont.*

Bombay: In Koli Wadi and a chawl in Parel disinfection effective, 20,431–7.
Bombay: Not a single case among contacts after thorough disinfection of room where patient had died, 25,803.
Bulsar: Re-occupation of houses 10 days after disinfection not followed by plague, 19,957.
Calcutta: Disinfection inefficient, notwithstanding no cases recur, 5615, 7662, 7664.
Calcutta: From the time when disinfection became efficient plague began to abate, II., 534.
Calcutta: No case known of plague reappearing in disinfected house, 7294, II. 534.
Guntakal Junction: Stops plague, 4183.
Kankhal: Good results of disinfection of every room and of every house, 9045–51.
Panjab: No instance known of recurrence of plague in a house known to be infected after its disinfection, 10,047.
Poona: Value of disinfection exemplified in the case of Wanaori Bazaar, and by fact that cases occurred on re-entering houses only limewashed, but not those chemically disinfected, 10,970, 10,972.

Instances in which House Disinfection did not stop Plague:

Belgaum: Disinfection did not stop plague in, 22,867.
Hardwar: Alone not sufficient to stop progress of plague, 9035.
Houses are still dangerous, even if disinfected, for re-occupation after only one or two weeks, 20,158.
Igatpuri: Complete disinfection does not stop the spread of plague, 19,455–67, 23,317, 29,196.
Jawalapur: Insufficient to stop course of disease at, 9175.
Lanauli: Disinfection not effective in, 20,184, 20,197.

See Recurrence.

Opinions regarding Value of Disinfection:

Bacillus dies in time without disinfection in about four months, 22,663.
Bacillus dies out without disinfection in two months, 22,713.
Bacillus of plague not killed by disinfectants, but by lapse of time, 11,531.
Bacillus survives application of disinfectants to infected floors, 5159.
Chemicals are very costly and do not give absolute results, 22,546.
Chemical disinfection not worth the money spent on it, 11,273, 24,448.
Chemical disinfection no use for Indian houses, burning the only cure, 15,178.
Chemical disinfection proved by experience to be unnecessary, ventilation and evacuation sufficient, 24,036–40.
Chemicals have not produced results to warrant the cost, 19,515.
Chemicals not to be relied on at all, 22,327.
Chemical disinfection: The fact that the plague germ can be killed by experiments in laboratories does not guarantee its destruction under natural conditions, 22,325.
Comparison between the number of bacteria found in a house before and after its disinfection no test as to its value, 26,369.
Comparison of the growth of colonies obtained from wall scrapings before and after disinfection showed no great difference. 26,350.
Details of experiments to determine the efficacy of the methods used for plague-infected houses, III. 345.
Disinfection by chemicals only useful in so far as the houses are cleaned out and aired in the process, 22,864.
Disinfection of clothing useful, but of houses impracticable, 23,187–8.
Disinfection of house by chemicals inefficient 20,158–61.
Disinfection by various fluids does more harm than good, 24,935.
Disinfection without evacuation sufficient for imported cases, 22,82–96.

DISINFECTION—*cont.*

Disinfection unnecessary if houses vacated for three months and ventilated, 24,040.
Evacuation and opening up of houses for a month more effective than chemical disinfection, 24,449.
Evacuated houses left unoccupied for two or three months proved safe without chemical disinfection, 19,955.
Even when most carefully done not always adequate to destroy the infection, 16,236.
If a thoroughly satisfactory measure, it ought to destroy the germ at once, 23,191.
No use when badly done, 18,768.
Not complete if any micro-organisms can be discovered after, 5188.
Of houses in towns not worth the enormous expense it entails, 11,266.
One process not sufficient, 10,325.
Repellant, not a destructive agent, 22,162.
Scientific disinfection of large areas an impossible measure, 12,757.
Sun does the work much better than chemicals, 24,342.
Thorough disinfection of every house in the place necessary measure in cases of plague, 9436.
To be effective must be lasting, II. 13.
Valuable if quite efficient, but difficult to get it properly done, 7991–4.
Ventilation as effective as disinfection by chemicals, only slower, 22,059.
Very little use if done several days after the infection has had a start, 25,130.

Opinions regarding Disinfection combined with other Measures:

Evacuation, combined with disinfection, ought to take the first place in plague measures, 18,422, 19,952.
Evacuation must be combined with the disinfection of the infected area and persons, 10,363.
Evacuation, cleansing, and disinfection the most important measures in dealing with plague, 16,969–71, 16,987.
Evacuation, segregation, and disinfection valuable in the order named, 23,032.
Failure when not combined with segregation, 18,426–8.
Useless without evacuation, 14,441, 19,484.

Rules for and matters connected with the carrying out of Disinfection:

Bangalore: Directions for disinfection of houses in, 2707.
Bombay Presidency: General procedure in, 8, 9.
Compensation paid at Hardwar for destruction of property, 9312.
Government compensation in the N.W. Provinces for loss by disinfection, 8841.
Houses should be opened up before use of chemical disinfectants, 24,124.
More efficient if done by fire engines, 8269.
More penetration when steam or strong hand pumps are used, 915.
Mysore: Rules for, in the Mysore State, 25,099.
Panjab: Rules for the disinfection of houses, 9984, 9996, 10,014–42.
Panjab: General mode of disinfection in the villages of the, 10,475–9.
Poona: Method of house disinfection, 21,289.
Steam engines used to flush down houses, 945.
Ten days interval before disinfecting houses, because dangerous before, 5529.
The rule to allow an interval of 10 days between evacuation and disinfection of no practical value, 10,044, 10,566, 10,788.
To give time for epidemic among rats to cease, disinfection not begun till 10 days after evacuation, 10,473.

See Burning.
,, Camp, *length of period of vacation of houses.*
,, Clothes.
,, Floors.
,, Grain.
,, Kiln burning.
,, Lime.

DISINFECTION—*cont.*

See Perchloride of mercury.
,, Period for which plague bacillus can survive.
,, Persons.
,, Phenyle.
,, Recurrence of plague in the same house or locality.
,, Unroofing.
,, Untiling.
,, Ventilation.
,, Walls.

DISINFECTORS:

Plague Cases among:

Ahmadnagar: Coolies employed in unroofing contract plague, 24,210.
Ankleshwar: No cases from whitewashing houses disinfected with perchloride of mercury, 14,237.
Cases among disinfecting gangs, 7220.
Cutch Mandvi: Attacks among disinfectors, 13,480.
Hyderabad State: Sweepers employed in cleaning infected houses frequently attacked, I. 442.
Jamalpur: 3 of the 34 cases were in disinfectors, 9158.
Jawalapur: 29 cases connected with disinfectors, 8929, 9178, 9217.
Kankhal: 4 of the 38 cases were in disinfectors, 9221.
Karachi: Cases common among disinfectors before their inoculation, 11,769, 22,668.
Kotri: 5 out of 100 cases in disinfectors, 12,647.
Palanpur: Disinfecting parties attacked, 13,100.
Panjab: Cases among disinfectors, 10,603–25, 10,625.
Panjab: Disinfectors attacked in every village before inoculation applied to them, 10,681–6.
Panjab: Disinfectors did not suffer from pneumonic plague to any marked degree, 10,340.
Panjab: 50 cases among disinfectors (total cases 3407, 9981), 10,001.
Panjab: Lime washers attacked, 10,323.
Panjab: List of some cases among disinfectors, 10,384.
Panjab: Mahlgahla—disinfectors attacked, 9987, 10,139, 10,349–41.
Punjab: Precautions taken with disinfections, 10,003–14.
Panjab villages: Disinfectors attacked, 10,143, 10,145, 10,146, 10,468.
Poona: No attacks in disinfecting gangs, 21,414.
Rohri: No plague among disinfectors, 12,089.
Satara: Attacks of disinfecting coolies, 22,720–7.
Sukkur: A large number infected by a plague case in a temple, in which five disinfecting coolies also died, 18,403.
Surat: A carpenter washing on disinfection with a wound on his foot attacked, 16,007.
Thana: Two men sent to clean a house where dead rats had been found got plague and died, 220.
Umreth: Two plague cases among Golas sent from segregation camp to limewash their houses, 14,067.

Risks run by Disinfectors, and Precautions against:

Boots and clean hands prevented disinfecting staff from taking infection, 16,007.
Digging up floors: Danger minimised by first white washing, 5554.
Disinfection dangerous work, many disinfectors attacked, 10,000.
Disinfection not dangerous to the gangs with proper precautions, 10,790–1.
Good results of inoculation among coolies on disinfecting gangs, 10,006–8, 10,681–6, 10,690–4, 11,769, 22,686.
No cases of mercurial poisoning among disinfectors, 10,020, 13,632, 19,939.
Panjab: Precautionary rules for the workers on disinfecting gangs, 10,003.
When careless of precautions disinfecting gangs as liable to plague as other persons, 22,721.

DISINFECTORS—*cont.*

Supervision and efficiency of Staff:

Bombay: At beginning of epidemic no special disinfecting staff. 1147–50.
Bombay: Staff trained to disinfection work, 1680.
Coolies shirk disinfecting work and require strict supervision, 7862, 21,096, 24,450, II. 13.
Disinfection requires continuous supervision, 8055, 23,189.
Disinfectors in villages open to bribes and careless about matters they consider unimportant, 21,154, 21,174.
Hubli: Disinfectors found to be taking bribes to shirk their work, 23,366, 23,399.
Just as well in hands of instructed laymen as of doctors, and no waste of power, 7970–3.
Native agency inefficient, 7862.
No staff to carry it out in villages, 2145.
Ought not to be left to inexperienced hands, 15,229.
Panjab: The Commissioner of the division issues the rules for the disinfection of infected areas, 10,661.
Poona: Disinfection always done under European supervision, 21,397–8.
Requires a great deal of supervision, 17,656.
Requires European supervision, 21,097.
Requires trained staff, 1680.
Should be entrusted only to trained hands 14,438.
Staff Corps officers supervise disinfection, 7971–6.

See Flying Columns.

DOG:

Dog died of plague in Daman, I. 378.
Dog seen with plague in Daman by Mr. da Cunha, and several heard of, 16,546.
Dog taken ill in Calcutta after taking up dead rat, 7187, I. 474.

DOMES:

Domes are natives of a certain caste who do not object to touch dead bodies, 6988.
Domes: Calcutta; get clothes of dead and assist at cremations, but had no plague, 7666.
Domes infected when assisting at post mortems, 6923–5, 6988–95, 6462, 6493, 6503–6, 6921.

DORMANCY OF PLAGUE.

See Interval between introduction of plague and outbreak.
,, Recurrence of plague in infected towns and villages.
,, Recurrence of plague in the same house or locality.

DRAINAGE:

Ahmedabad: Scheme for the drainage in course of execution at, 13,961.
Bombay: Mandvi, drains are bad, 17,934.
Bombay: More than a quarter of the island without drains, 25,741.
Bombay: Shone system of drainage introduced in certain districts, 25,752.
List of places where drainage and waterworks have been completed, I. 277–8.

Plague and Drainage:

Ahmednagar: Plague broke out in the most unhealthy part the town, where there was no drainage system, 22,264.
Bombay: Sewer pollution not the cause of plague, 19,437.
Karachi: Drainage system not responsible for the severity of the outbreak, 22,651.

DURATION OF PROTECTION AFFORDED BY INOCULATION.

See Inoculation with Haffkine's plague prophylactic.

DURATION OF SICKNESS IN PLAGUE:

Average duration of fatal cases three to four days, 2361.
Duration of disease averaged 5½ days in 17 fatal cases, I. 381.

DURATION OF SICKNESS IN PLAGUE—*cont.*
Instance of plague case of only 14 hours' duration, 17,580.
Minimum duration of plague case under 18 hours, 9607.
Mortality very high during first five days; highest on the second day of attack, 10,207.
Non-fatal cases, 10 to 15 days, 2362.
Table of mortality from day to day of the disease in the Jullundur district, II. 96.

DUST.
See Soil.

EARTH.
See Soil.

EFFECTS OF INOCULATION WITH HAFFKINE'S FLUID.
See Inoculation with Haffkine's prophylactic fluid.

EFFECT OF PLAGUE ON OTHER DISEASES:
Decrease in malarial fever during plague, 12,993, 16,505–7.

ELECTROLYSED WATER:
Plague bacillus killed by freshly electrolysed water, with chlorine in solution, 3632.

EMIGRANTS:
All emigrants at Mombassa were detained at Kiamari, 11,989–92.
No emigrants under contract allowed to depart from Karachi at all, 12,233.

ESTABLISHMENT.
See Staff, Plague.

EURASIANS, CASES AMONG:
Bombay: Death-rate from plague among Eurasians 43 per cent; among Europeans 32 per cent., I. 382.
Calcutta: Small percentage of Eurasians die at, 6870.
Death-rate among Eurasians at St. George's Hospital, Bombay, I. 391.
Poona: Sassoon Hospital, six deaths among 16 Eurasians in, 12,931.

EUROPEANS, CASES AMONG:
Bombay: Death-rate from plague among Europeans 32 per cent. of cases; among Eurasians, 43 per cent., I. 382.
Bangalore: Case of a European, 2600.
Bangalore: West Riding Regiment, cases among the men of, I. 173–4.
Course of disease the same among Europeans and natives, but the former showed more resisting power, 12,935.
Death-rate among Europeans and Eurasians at St. George's Hospital, Bombay, I. 391.
Few cases among Europeans, 330, 332.
Immunity: Almost entire for Europeans, 17,685.
Immunity: Europeans not so immune in former times as now, 22,263.
Immunity: Europeans have no racial immunity, 296.
Immunity: Europeans probably have racial immunity, 6969.
Immunity: Europeans on Malabar Hill escaped, whilst their servants contracted the disease, 1447, 18,189–99, 20,478, 20,539.
Immunity: Fresh air accounts for immunity of Europeans, 22,426.
Immunity: Healthy Europeans relatively immune because they sleep in beds and wear boots, 19,447.
Immunity: Ventilation accounts for immunity of Europeans, 917, 7168.
Notes of cases among Europeans, 7220.
Panjab: Sanitary inspector, death from plague of European, at Mahlgahla, 10,348.
Poona: One death among 17 Europeans in Sassoon Hospital, 12,928.
Poona cantonment: Cases among European soldiers, 21,293.
Three cases among, 7569–77.
Yersin's serum ineffective in plague case of a European girl, 1468.

EVACUATION:
Arranged under the following subheads:
Attitude of the People towards Evacuation.
Beneficial results of Evacuation: Instances and opinions in favour of.
Compulsion and Evacuation.
Difficulties connected with Evacuation.
Early Evacuation: Opinions and Instances connected with the Advantages of.
Length of Period of Evacuation.
Limit of Population which can be Evicted.
Limit of Population, 5,000–20,000.
Limit of Population, 20,000 and upwards.
Limit of Population, no limit.
Partial Evacuation: Instances and Opinions regarding.
Seasons favourable for Evacuation.
The Rains and their Effect on Evacuation.
Time taken to effect Evacuation.
Total Evacuation: Opinions and Instances regarding.
Unnecessary or Unsuccessful: Opinions and Instances that Evacuation is.

Attitude of the People towards Evacuation:
Agriculturists go out into evacuation camps without difficulty, 22,986.
Bandra: Impossible owing to the opposition of the Muhammadans, 18,929.
Bangalore: Evicted people will not go into camp and overcrowd other districts, 2580.
Baroda: The majority of the population are now convinced that evacuation is a beneficial measure, 14,635, 15,115–6.
Bhiwandi: People refused to comply with the orders for evacuation, 14,522.
Bombay: Parsees go readily to camp, 8294.
Bombay: The trouble of removing real cause of the objections to camps in, 18,261.
Co-operation of the people in reporting cases in camp necessary, 14,421.
Dharwar: Majority of people left town for camp of their own accord, 1612.
Dharwar: The most remarkable results from using no force but working with the people, 26,543.
Ghoti: The residents who last year forcibly resisted evacuation, are now thoroughly convinced of its efficacy, and went out and stayed out themselves in the rains, 14,758.
Guntakal: Takes time because of Muhammadan objections, 4145, 4161.
Hyderabad State: Villages around go out of their own accord, 5455.
In districts that have had experience of plague, people evacuate villages of their own accord, 9.
Kankhal: People settled down very easily and quietly to their temporary life in camp, 9343.
Kankhal: No opposition offered to evacuation, 9096, 9109.
Karachi: People as a rule do not like to go into segregation camps, 11,635.
Mahamari: People vacate villages of their own accord on appearance of, and will probably act similarly when they get to know plague, 26,575.
Muhammadan population makes evacuation impossible, 5462.
Muhammadans have an almost insuperable objection to, 3414.
Mysore State: Villagers evacuate their houses of their own accord on the appearance of plague, 3303.
Mysore State: Villagers evacuated villages willingly, 3304–5.
Natives themselves convinced of its advantages and will take initiative, 9, 11,378, 15,116, 15,494.
Native gentlemen use their influence and secure evacuation, 1345.
Panjab: Cordons and evacuation the measures natives most dislike, 10,310.
Panjab: Palewal: Evacuated under most favourable conditions through influence of the principal inhabitants, 10,725, 10,728.
People mostly object to go to segregation camps because of the trouble of removal, 18,261.

EVACUATION—*cont.*

Property: People object to go to camp from fear regarding safety of property left in their houses, 3321, 4970, 5007.
Ryots go out quite readily, but traders mostly object, 22,092.
Satara: Great difference in the attitude of the people between first and second epidemic, 22,558.
Satara: Opposition to evacuation mostly from Brahmans, Banniahs, and Sahukars, 22,623.
Satara: People frequently evacuate their villages of their own accord, 22,089.
Segregation camps popular with the people, 12,522.
Sholapur: Caused discomforts, but no complaints, 20,265.
Shopkeepers and weavers have to be frightened out, 5456.
Sind: Two villages voluntarily evacuated, 12,086.
Surat: People object chiefly to evacuation and segregation, 15,865.
Thana district: No opposition to evacuation, segregation very unpopular, 18,788.
Umreth: Only slight opposition offered by the people to the orders of evacuation, 14,040.
Villagers recognise usefulness of evacuation, 22,635.
Villagers now thoroughly convinced that evacuation is a beneficial measure, 14,635.

Beneficial results of Evacuation: Instances and opinions in favour of:

Ahmednagar: Sanitary improvements carried out in cantonment while people were in camp, 24,191.
Ankleshwar: Plague practically stopped within a month after evacuation, notwithstanding the rainy season, 14,403.
Ankleshwar: Table showing results of partial and total evacuation, II. 244.
Ankleshwar Taluka: Particulars of cases before and after complete evacuation, II. 250.
Banga: Statistics relating to evacuation, II. 97*a*.
Banga: Table showing the effect of evacuation in, II. 112.
Banga town: Table showing the effect of placing people in camp at, II. 99.
Bangalore: Of complete streets very successful, 3396, 3399, 3440.
Bangalore: Effect of regimental evacuation, 3396, 4889.
Baroda: Evacuation stopped plague in villages which were reoccupied without recrudescence, except Savali, 14,630, 14,658.
Belgaum: Good effects of removal to health camps noted in the case of a particular family, 23,041.
Beneficial effects of camp life under supervision, 16,016.
Beneficial in forcing the people to live in the fresh air, 19,498.
Bholana: Two families left in got plague; no cases in camp except from contact or visits to village, 24,545, 24,561.
Bobleshwar: No plague in people who went to camp; one family that remained in got plague, 19,513.
Chipagiri: One hundred and fifty go out from infected area. No cases in camp; two persons who refuse to go out die of plague, 4967–70.
Conclusions from the results of evacuation, 20,238.
Cutch State: Table showing results of evacuation in the villages, II. 409–10.
Cutch State: People who left villages got no plague, but the Jadejas who remained in contracted it, 13,449–50.
Dharwar district: Good effects of evacuation well marked in the villages, 1699, 23,168, 26,543–4.
Dharwar: Statement of plague during evacuation and subsequent to it, III. 355.
Dharwar district: Table of results of evacuation in, III. 214.
Disinfection should be supplemented by evacuation, 14,441–19,484.
Effect of health camping at Poona was magical, 21,320–1.

EVACUATION—*cont.*

Effects immediate; no measure so effective, 20,441.
Evacuation and opening up of houses for a month more effective than chemical disinfection, 24,449, 24,456.
Evacuation brings down mortality at once, 23,202.
Evacuation, combined with thorough disinfection, ought to take the first place in plague measures, 10,361–3, 18,422, 19,952.
Evacuation completely successful, chief measure in Bombay Presidency, 5, 9.
Evacuation essential, 13,191, 16,967–71.
Evacuation excellent in small places, but impossible in large cities, 7854, 18,585.
Evacuation first and most important measure, 22,288–9.
Evacuation frequently stopped plague in a week, 15,124.
Evacuation lessens mortality and decreases the duration of the epidemic, 26,543.
Evacuation stops the epidemic in the hot weather in some villages, 9988.
Evacuation stops the epidemic from extending, 10,464, 10,757, 25,186.
Evacuation successful in Gulamshah village, where plague and small-pox at the same time in the Gulamshah camp, 11,138.
Evacuation the best measure for combating plague, 11,849–50, 13,452, 14,396, 15,221, 15,764, 19,684, 22,031, 22,532, 22,595, 22,891, 23,507, 24,312–3, 26,672.
Evacuation the most effective remedy, as plague seems to be principally a disease of locality, 8791, 17,666, 26,672
Evacuation the real remedy, 1061, 10,634, 11,248–51, 11,482, 15,115.
Evacuation, segregation, and disinfection valuable in the order named, 19,639–41, 23,032.
Evacuation with opening up and cleaning houses the best remedy, 24,363.
Excellent measure, if combined with segregation, 11,482, 12,051, 21,667.
Followers of 4th Hussars moved with entire success, 2839.
General course of disease after evacuation, 9986.
Gharibabad part of Sukkur: Most successful (1,000 inhabitants) in, 3725–37, 3813–6, 4275, 11,162–70, 18,405–8.
Health of the people from camp life, beneficial effects on the general, 15,169.
Health, segregation camps exert a most beneficial influence on the general, 21,719.
Hinganghat: Very successful, 6032–6.
Hoshiarpur district: Table showing effect of evacuation on the epidemic, II. 82.
Immediate benefit following, 2.
Instances of beneficial results of, 13,449–51, 22,737.
Inoculation, combined with complete evacuation, the ideal plague measures, 22,408.
Jalgaon, mill hands all put out at once at, with the result that not one case occurred among them, 24,563.
Jullundur district: Table showing effect of evacuation on the outbreak, II. 80, 81.
Jullundur and Hoshiarpur districts: Table showing period between the date of the total evacuation of the villages and the date of the last case of plague, II. 83.
Kaira district: Statistics relating to evacuation in villages, II. 424–40.
Kaladgi: Complete evacuation caused the plague to subdue within two weeks, 19,475.
Kalankot: Successful, 4273.
Kalankot: Good results, 11,144.
Kankhal: People who were allowed to remain in evacuated quarters got plague, while those evicted benefited, 9217.
Karachi: Good results of camps in healthy spots with efficient supervision, 11,314.
Karachi: Good effects of evacuation, 11,959–60, 12,755.
Khandraoni: Evacuation successful at, 9661–4.
Khari: Successful results, 18,936.
Kotri: Table showing the beneficial result of evacuation, 12,654.

EVACUATION—*cont.*

Mysore: Barr Sepoys attacked and marched out into camps; disease stops at once, 3,397.
No fair test to compare the number of cases before and after evacuation, 9992.
North-West Provinces: Good results of evacuation, 9225.
Palanpur: Results obtained very strongly in favour of entire evacuation, 13,160.
Panjab: Statement showing effect of evacuation in the villages, II. 80–3, 112, 117, 403.
Panjab: Table to show effect of evacuation of villages on the epidemic of plague, II. 83, II. 117.
Plague always went down after evacuation, 3306, 5926, 6036, 14,662, 18,582, 19,482, 22,046, 24,549.
Porbandar: Table showing effect of evacuation on the different communities, II. 222.
Quick and certain method of stamping out plague, 10,361.
Removal from the vitiated air the most beneficial result of, 23,035.
Second outbreak after disinfection easily checked by evacuation of the re-infected houses, 10,053.
Sholapur: Effects of evacuation at, 20,233, 23,928–33.
Sirohi State: Success of evacuation in villages, 9825–9, 9830–3, 9862–6, 9873–7.
Stopping the plague, hygienic conditions of camp life and release from dread of plague, powerful agents to, 11,894.
Success depends on efficiency of staff, 10,297.
The more complete the evacuation the sooner the spread of the disease is stopped, 22,032.
Though evacuation of a place may do harm, by causing refugees to carry plague to neighbouring places, less harm will be done by resorting to evacuation than by not resorting to it, 23,336, 26,546–7.
Total evacuation the most effective plague measure, 11,248, 15,220, 15,764, 18,082, 19,684, 20,441.
Total evacuation with disinfection most satisfactory, 18,421–2.
Unkal: Successful, 2194.
Utmost importance to evacuate thickly populated infected area, 4292.
Wagdari: Effect on death-rate, 5926.

Compulsion and Evacuation:

Bandra: Evacuated by force, 18,570–3.
Bangalore: Refusal to enter Government camps at, 2551.
Broach: Compulsory evacuation of infected houses, 15,489.
Dharwar: Difficult to get people to camp provided by Government, 1934.
Municipal and Government camps, objection to, 12,527.
Official interference objected to much more than evacuation, 16,137.
Sholapur: All compulsory camps at, 20,241.
Surat: People at, refused to go to Government camps, but made camps of their own, 15,914.
Total evacuation should not be persisted in where people object, 26,546.
Wholesale compulsory evacuation of infected areas a most useful measure, 11,849.

Difficulties connected with Evacuation:

Arrangements in Broach to carry on the mills while people in camp, 15,484.
Bombay: Little done in the way of evacuation during the first epidemic owing to the difficulty of providing temporary accommodation, III. 362.
Bombay: Necessity for camps built near centres of work, with free passes given on railway to workmen living in the camps, 8273.
Easy at Cutch Mandvi, 466.
Fires occurred in the private camp at Jawalapur, by which much property was destroyed, 9343.
Found possible in Jalgaon to carry on the business of the town with the people in camp, 24,563–4.
Houses evacuated sometimes burned, 23,675–7.
Inconveniences of evacuation greater among the better classes than the poor, 14,457.

EVACUATION—*cont.*

In voluntary camps at Karachi thefts frequent, 11,883.
Property left in evacuated houses sometimes stolen, 5455, 22,639.
Rohri: Done in sections in order to keep trade going, 12,039.
Rohri: Plague continued a month after evacuation, as the people remained with their sick concealed in the date plantations, 12,107–19.
Trades and several occupations of the people a great factor in the difficulties of evacuation, 10,296.
Trade in large towns paralysed by evacuation, 13,1956.
Zanana chief difficulty in evacuation, 5463.

Early Evacuation: Opinions and Instances connected with the Advantages of:

Alibag: History of plague at, most instructive in showing the advantage of early evacuation, 17,611, 17,614.
Co-operation of the people and efficient assistance of subordinates indispensable for the good results of early evacuation, 14,421.
Cutch Mandvi: Good results in most villages turned out early, 13,452.
Evacuation by instalments not so definitely beneficial as prompt evacuation, 23,202.
Gives good results if effected in time and combined with early information, 26,675.
Instances of the benefits of immediate evacuation, 22,737.
Instances in which the spread of the disease was arrested by early evacuation, 14,419, 18,582–4, 21,639.
Kaladgi: Excellent results of prompt evacuation, 23,635.
Most effective when immediately applied, 18,764.
Must be complete, and undertaken at the first stage of the epidemic, 24,312–4.
Partial evacuation failed in many cases because not early enough, 19,638, 23,536–43.
Promptness of evacuation most important, 9281–3, 10,616, 21,667, 22,599, 22,737, 23,169, 23,589, 24,199, 24,545.
Upper Daman: Early evacuation stops the disease, 16,466.
Wasind: Evacuation before the first indigenous case stopped the disease at once, 19,654.

Length of Period of Evacuation:

Time of absence has to depend on circumstances, 575.
At Poona, street by street inhabitants back after disinfection within a week, and had no further plague, 15, 10,928–37.
Period 10 days, sometimes a month or more, 8301.
At the commencement of outbreak in Rohri, infected houses evacuated for 10 days, and all their inhabitants segregated, 12,027, 12,033.
Plague cases re-appear in disinfected houses if only left vacant for 10 days, 25,198.
People might be allowed to return to their houses 10 days after efficient disinfection, 10,378.
Bombay: People allowed to return after 10 days, 10,957.
Poona: People allowed to return after 10 days, 10,929.
Sweepers' quarter at Belgaum evacuated and re-occupied in 10 days, when more cases, and residents were then cleared out indefinitely, 2090–7.
Infection not stopped in 10 days, if return to infected areas is allowed, 11,902.
If incubation period is accurately known, three weeks a sufficient time for keeping people out of their villages, 10,789.
Three weeks the minimum period of evacuation in the Panjab, 10,570.
Period of evacuation ought not to be less than three weeks after last cases of plague, 10,054, 10,266–70.
Palanpur: People kept out three or four weeks, 13,134.
Time requisite, with disinfection of houses, at least three weeks, 10

EVACUATION—*cont.*

Evacuation and opening up of houses for a month more effective than chemical disinfection, 24,449, 24,455.
People should be allowed to return one month after deaths stop in camp, 5451.
People kept out for one month in Dharwar villages, 2141, 23,748.
Maintained at Sukkur for one month, 3727.
Period of one month after disinfection at Hubli, 1563.
Villages should be evacuated at least a month; the longer the better, 13,456.
Re-occupation within six weeks after evacuation and disinfection unsafe, 16,247, 20,158.
Baroda: people kept out between six weeks and two months, 14,687.
Two months evacuation quite sufficient, 11,380, 11,493, 12,763, 12,791, 22,714.
Villagers kept out for two months in Hyderabad, 5926.
Evacuation without disinfection of houses should last two or three months, 10.
Houses left unoccupied for two or three months proved safe without chemical disinfection, 11,379–80, 19,955.
People of Kotri kept out 2½ months, 3759.
Should extend to three months after the last case, 22,841.
Sholapur kept empty from December to March, 23,916.
Return to houses safe if they have been vacated three months and ventilated, 24,040.
Three months the longest period in which people were kept in camp in the Ankleshwar Taluka, 14,422.
Kankhal: people out from end of October to middle of February, 9102, 9117.
Houses should remain evacuated for about four months, 22,533.
Karachi: Evacuation into voluntary camps began in April. People had all gone back to town by the end of August, 11,876.
Bangalore: people allowed back only when house considered free of infection, 2580–2.

Limit of Population which can be Evicted:

Complete evacuation impossible for very large places, 8798.
Evacuation in large towns is really a matter of finance, 21,803.
Impossible in a big city; only leads to concealment of cases, and frightens people away, 7854, 8272.
Much easier carried out in agricultural villages, where the people are tied to the spot by their occupations, 24,072.
Of large numbers comparatively easy when the infected place is surrounded by open country, 18,586.
Total evacuation impossible in Bombay, 18,588.

Limit of Population, 5,000–20,000:

Banga and Garhshankar, 5,000 inhabitants each, evacuated, 9980.
Complete evacuation of Kotri (6,000 people), 3752.
Impossible to control 18,000 persons out in the fields, 24,556.
Impossible to control village of 4,500 with the staff available, 24,065–70.
Malegaon, about 12,000 people in health camp at, 18,074.
Possible in most towns of 10,000 inhabitants, 10,296, 18,787.
Rahon, population 8,000 to 10,000, evacuated, 10,459.
Six thousand or 7,000 successfully evacuated at Kowloor, 5453.
Total evacuation of Malegaon, with a population of 18,000, in about a fortnight, 13,777.

Limit of Population, 20,000 AND UPWARDS:

Can be applied to a town of 60,000 inhabitants, but impossible in large towns like Bombay and Surat, 11–12.
Could be done easily up to 100,000, 26,546.
Cutch Mandvi: between 30,000 and 35,000 evicted in second epidemic, 13,424.

EVACUATION—*cont.*

Evacuation of Karachi on second epidemic, population, 40,000, 3773.
Impossible in large cities, 7854.
Karachi: about 30,000 in camp at one time, 11,318–20.
Maximum population evacuated 70,000, 5460.
Surat, about 35,000 at one time in the municipal and health camp in, 24,420.
Surat: Thirty-four thousand persons evacuated at one time, 15,766.
Town of 70,000 Hindu inhabitants easily evacuated, impossible if the population is Muhammadan, 5462.

Limit of Population, NO LIMIT:

No limit to, if done in sections, as far as population is concerned, if accommodation can be provided, 26,689.
Under favourable local conditions no limit to, but that of expense, 18,586, 18,784.

Partial Evacuation: Instances and Opinions regarding:

Ahmedabad: Evacuation of the infected locality and disinfection proved sufficient, 13,937.
Ahmedabad: The dryness of the atmosphere probable cause of partial evacuation proving there more successful than at other places, 13,937.
Ahmednagar: Partial evacuation resorted to unsuccessfully, and town at last totally evacuated, 24,234, 24,253–5.
Bangalore: About 75 per cent. of population evacuate their houses themselves, 3481.
Bangalore City: Evacuation at first confined to infected houses, 3232.
Bangalore: Complete street evacuated and very successful, 3396, 3399, 3440.
Baroda City: Partial evacuation having failed, whole town was evacuated by March 1898, 14,613–4
Baroda: Inoculated persons not forced to evacuate their houses in the villages of the Baroda district, 15,084.
Bombay City: Evacuation not attempted on a large scale, 11–13, 1345.
Bombay: Evacuation among the Parsees in the Fort restricted to houses in which plague cases occurred, or dead rats were found, 25,247.
Broach: Partial evacuation resorted to, 15,486–91.
Calcutta: Houses adjoining infected houses not emptied, 6580.
Chipagiri: Partial evacuation succeeded, 4964–73.
Cutch Mandvi: Partial evacuation checked but did not stop plague, 13,453.
Evacuation by instalments not so definitely beneficial as prompt evacuation, 23,202.
Evacuation of the whole village preferable to its disinfection block by block, 10,349.
Guntakal: Partial evacuation unsuccessful, 4144.
Hardwar: Only partly evacuation resorted to successfully, though all but 500 people left the town voluntarily, 9039–44.
Hubli: Clearing and burning down streets did not check progress of disease across the town, 1645.
Hubli railway chawls, 2403.
Hyderabad: Village evacuated, people lodged in railway waggons, 19,468.
Instances in which partial evacuation was not successful, and instances where it proved sufficient, 23,241–4.
Jawalapur: Partial evacuation failing, was totally evacuated, 9173–7, 9276–8.
Kankhal: Evacuated in sections, had to be totally evacuated, 9090–4, 9101–2, 9113.
Karachi: Evacuation done in sections; a very great undertaking, 26,676.
Karachi: Methods of evacuation and segregation modified in the course of the epidemic, 11,143, 11,219–23.
Karachi: Joria Bazar evacuated on 7th April 1898; people allowed to go where they pleased, went to other parts of the town, 11,240.

EVACUATION—*cont.*

Karachi: Very large part of native town evacuated in May 1898, 11,242–4.
Kotri: Dhobi quarter evacuated at beginning of outbreak, 12,551.
Kotri: Does not stop disease at, 3755.
Malegaon: Partial evacuation unsuccessfully tried, 18,070–5, 23,241.
Molagavelli: Partial evacuation failed, and whole village was evicted, 4992, 4999.
Nasik: Evacuation began with the Musulmans and Kalals, 24,481.
Nasik: Gradual evacuation of infected areas until three-fourths of the town were evacuated, 13,751, 13,771, 16,929–44, 23,241.
Nominal evacuation no use, 22,041, 22,045.
N. W. P.: Partial evacuation succeeded in villages where first case detected, 9134.
Palanpur State: Partial evacuation insufficient in the villages, 13,124, 23,241.
Panjab: Banga: Muhalla system of evacuation very unsatisfactory, 10,172, 10,232–5.
Panjab: Bilron: Partial evacuation and cordoning of the area successful, but the village re-infected from outside, 10,741.
Panjab: Partial evacuation in villages failed, 9977–9.
Partial evacuation a danger to the surrounding villages, 20,237.
Partial evacuation did not stop progress of plague, 24,431.
Partial evacuation inadequate, 10,231, 11,126, 19,459.
Partial evacuation may succeed if infection is in a block of houses separate from rest of town, 18,783.
Partial evacuation sufficient if early intimation of the first case is obtained, 13,797.
Partial evacuation very little good. Disease gets across open space, 1710.
Poona: No evacuation during first epidemic, but rigorous plague measures, 21,690.
Poona: Partial evacuation in first epidemic, 10,921.
Satara: Only partially evacuated—plague kept on, 22,297.
Sholapur: Partial evacuation not successful, 20,231-3.
Sinnar: Partial evacuation proved inadequate; total evacuation ordered, 13,790.
Sirohi State: Partial evacuations failed, and villages totally evacuated, 9821–2.
Sukkur: Success with 600 people, 3816.
Surat: Adopted in, during second epidemic, 15,734, 15,883, 24,430–1.
Timmencherla: Partial evacuation successful, 4124–8.
Trade not affected by piecemeal evacuation, 17.
Umreth: Partial evacuation ineffective; town completely evacuated on November 24, 14,041.
Undhera: Partial evacuation tried at, proved inefficient, 14,648.
Wardha: Nearly the whole evacuated, 6120.

Seasons favourable for Evacuation:

Ahmednager: People complained both of the cold and heat in camp, but ultimately preferred camp life, 18,996.
Evacuation successful in every season of the year, though more troublesome in winter than in summer, 10,361.
May and June the most favourable months of the year for evacuation in the N.W. Provinces, 8804.
Panjab: People complained of heat in hot weather, but suffered little with camps under trees, 10,239.
Plague can always be controlled without much expense and without compulsion in up-country districts during the dry season, 26,544.
Sirohi: People objected to go into camp on account of the cold, 9912.

The Rains and their Effect on Evacuation:

Ankleshwar: Plague stopped within a month after evacuation, notwithstanding the rainy season, 14,403.

EVACUATION—*cont.*

Bandra: Impossible, owing to the rains and the opposition of the Muhammadans, 18,929.
Bangalore: People driven out from the health camps by rains, 3267.
Cutch Mandvi: Salaya and Bada, people return to villages from camp on account of rains, with recrudescence of plague, 13,456.
Dharwar: Byahatti and other villages: Epidemic extremely severe where evacuation was impossible on account of the rains, 2168–93, 2192.
Dharwar: Villages in which plague was severe, as evacuation was impossible on account of the rains, 1601, 1719, 23,518, 1. 92–3, 2168–2200.
During rains, entails great sufferings, 22,761.
During rains evacuation unsuccessful because huts built too solid and without ventilation, 22,048, 22,607–9.
During rains, exposure to cold and damp in the camps renders people more susceptible to plague, so that evacuation then fails, 22,737.
During rains, makes people more liable to the disease, 23,273.
Financially impossible to provide adequate shelter for the evacuated during the rainy season, 16,079.
Flat roof of houses the best place to camp people in black soil country if plague occurs during the rain, 23,591.
Hardships much greater in the rains in some parts than in others, 22,990.
Hubli: Wholesale evacuation not attempted, because impossible on account of the rains, 1943, 2001.
Igatpuri: Heavy rainfall interferes with evacuation, 16,887, 23,196.
Impossible in black soil, during the rains, without great hardships to the people, 24,560.
Impossible in the rainy season unless accommodation be prepared beforehand, 15,148.
Impossible on clay soil during rains, 5879.
Impossible to carry out completely in the rains, 5525, 22,606–10.
No increase in the mortality from other diseases, notwithstanding evacuation took place during the rains, 24,033.
Of rice-growing district in rainy season requires great and costly preparation, 22,196.
Panjab: Chak Kalal people driven back to village from camp by rains; increase of plague, 10,603 (8).
Panjab: In some parts evacuation on a large scale is possible during the rains, 10,237, 10,572.
Panjab: Mallupota: Great discomfort, half the camp under water, 10,236.
People will not stay out of their houses during the rains, 22,041, 22,077.
Possible during rains on rocky or sandy soil, 5878.
Prevention from leaving the town, the alternative in black soil country during the rains, 26,545.
Rains make supervision and evacuation impossible, 1588–9.
Sholapur: Little complaint by villagers against evacuation in the rains, 22,058.
Surat: Rains interfered with evacuation in second outbreak, 15,739.
Surat: Second outbreak during the rains; evacuation impossible, 24,417.
Thorough evacuation impossible in rainy weather, on a black cotton soil, 22,076.
Vathar and other villages: Evacuation unsuccessful, as, on account of rains, people kept on returning to the village from their huts, 22,041, 22,737.
Where rains interfered, percentage of deaths very high, 2169.
See Recurrence of plague.

Time taken to effect Evacuation:

Ahmednagar: The Sadr Bazar evacuated in 24 hours in, 24,145.
Dharwar, When arrangements in working order, villages usually evacuated in three days, 23,163.

EVACUATION—*cont.*

Duration of epidemic depends upon the time the evacuation occupies, 22,600.
Evacuation done in one month at Unkal, 2194.
Guntakal: Evacuation takes one month, 4164.
Hardwar: The time necessary to prepare a camp depends on the supply of materials and facilities of carriage, 9104.
Hinganghat: Five hundred people or more removed in six hours, 5986–8.
Jagjitpur, N.W.P.: Population 1,200, evacuated in three days, 9144–5.
Malegaon: Total evacuation, with a population of 1,800, in about a fortnight, 13,777.
Molagavelli: Takes four or five days, 5009.
Nasik: Evacuation commenced on November 11th, 1897, and completed early in January, 1898, 14,733.
Rahon: Total evacuation, with a population of 8,900, took 20 days, 10,460.
Rohri (5,000 inhabitants): Evacuated piecemeal; operations commenced 17th April; town emptied 30th June, 12,038–41.
Two months necessary for evacuation of 60,000 people, 23,903.
Wardha: Evacuation commenced 6th December 1898; whole town evacuated by end of December, 6218–9.

Total Evacuation: Opinions and Instances regarding:

Ahmednagar: Evacuated November 1897, 2.
Ahmednagar: Evacuation successful, 2008.
Ankleshwar: Totally evacuated by August 23, 14,222.
Backergunge: Inhabitants of village evacuated in a country boat, and entirely cut off from communication with their houses, 6448.
Baroda: Total evacuation of, completed in March 1898, 14,614.
Baroda: Total evacuation of villages in the Baroda district as soon as an indigenous case was discovered, 14,647.
Kankhal: Ultimate evacuation of the whole town, 9094.
Panjab: Total evacuation of a village as soon as more than one case appeared, 10,451.
Sholapur: Description of evacuation, 5547.
Sholapur: Wholesale evacuation considered too drastic a measure at the beginning of outbreak, 20,229.
Total immediate evacuation the great remedy, 20,238, 22,595.

Unnecessary or Unsuccessful: Opinions and Instances that Evacuation is:

Belgaum: Disease already on the decline when evacuation was commenced, 22,910.
Daman: Failed, except in the case of the Machis, who segregated themselves, 16,368.
Disinfection without evacuation recommended for imported cases, 22,892, 22,896.
Even without evacuation, plague spends itself and disappears in six or seven weeks from first attack, 16,250.
Instance in which evacuation was not successful in staying the disease, 22,038.
Not necessary if the house be good, 1069.
Removal of people to open places without comfort or protection from weather is as harmful as the disease itself, 24,935.
Rohri: Evacuation did not stop plague, as people took no precautions to keep away from cases after evacuation, and position of huts insanitary, 12,081–2, 12,120–1.
Rough and ready method, 16,016.
Satara: At Limb and Rahmatpur evacuation did not at once stop plague as people went to badly ventilated huts, 22,290–6.
Unnecessary if early notification be given, 16,026.
Uran: Evacuation failed to check plague quickly as huts overcrowded and people revisited houses, 22,752–87.
Worthless unless combined with searching, 21,667.
Would become unnecessary if information could be obtained early enough to limit the zone of infection, 21,711.
See Rats: *Death of rats regarded as a warning of plague, and action taken upon.*

EXCRETA:

All may convey plague bacillus out of the body, I. 374.
Bacillus present in, as a rule, I. 378.
Infection by excreta and discharges, 9418, 18,130, 19,036–40.
Mode of exit of plague bacillus from the body, I. 374.
Rats, excreta of, supposed to cause infection, 15,459.
Rats becoming diseased, taint articles with dung or urine, 992.
Should be disinfected, 318.

See Fæces.
,, Scavengers.
,, Sputum.
,, Urine.

EXODUS OF PEOPLE FROM PLAGUE-INFECTED PLACES:

Causes of:

Ahmednagar: One thousand out of 3,000 run away from fear of plague measures in, 2011.
Bombay: In 1896–7, 300,000 people said to have left the City, 1034.
Bombay: Exodus in 1899 not due to fear of plague measures, 26,563.
Bombay: Plague measures the principal cause of the general flight at the commencement of the epidemic, 20,911.
Calcutta: Panic caused principally by the fear of segregation in, I. 468.
Calcutta: 150,000 to 200,000 people out of 700,000 left the city, chiefly from fear of segregation, 6540–5.
Dharwar: Eighteen thousand persons left in one week for fear of segregation and inoculation, 23,746.
Dharwar: One-third of the people went out, but they were of the agricultural class, 1956.
Enforcement of restrictive measures led to the running away of the people, 17,864.
Evacuation impossible in a big city, only frightens people away, 7854, 8272.
Fear of plague as well as of plague measures determine the exodus at the beginning of the epidemic, 20,249.
Fear of the plague drove the people from the suburbs into the date plantations, 12,079.
Ghoti: Deserted after the riot, 16,901.
Karachi: People in first epidemic encouraged by the authorities to leave, but in the second not allowed to scatter, 11,130–5, 11,303.
Karachi: People running away equally from fear of the plague and the plague measures, 11,695.
Nasik: About 5,000 people left from dread of segregation, 13,832.
Segregation practically impossible, drives inhabitants away, 7043, 20,237.

Details of:

Ahmednagar City: 16,000 people left the place when city was evacuated, 24,358–9.
Ankleshwar: People leaving in numbers at the commencement of the outbreak from fear, 14,538.
Bangalore City: 40,000 people left the city, 3504–5.
Bangalore Civil and Military Station: 25,000 people fled, 2639, 2666.
Belgaum: 12,000 people left, being encouraged by the authorities to leave, 22,824.
Bhiwandi: 7,000 out of 14,000 people fled during plague, 14,549, 23,359.
Bombay City: Details regarding exodus, 26,079–91, 26,559–66.
Broach: About 18,000 people left the place, about 3,000 for camps near the town, the rest for other places, 15,502.
Calcutta: Graphic description of panic when it became known that the Bombay plague had reached the town, I. 467.
Cutch Mandvi: During the first epidemic people fled from the infected houses leaving the sick and dead, 13,494.
Daman: Large number of people left by land before Surat cordon imposed, 15,997–9.
Gadag: Flight of traders, the landowners remaining, 23,680.

EXODUS OF PEOPLE FROM PLAGUE-INFECTED PLACES—*cont.*

Karachi: Population reduced through both epidemics by over 25,000, 11,716.
Karachi: 25,000 people allowed to leave Karachi in January 1897, 11,130–6.
Mysore City: Exodus less than at Bangalore, but extent not determined, 24,977.
Palanpur: 3,000 or 4,000 people fled to villages around, 13,084.
Panjab: Difference between the census population and the number of people ascertained by the roll-call in the villages of the Jullundur and Hoshiaspur districts, 9940–4.
Poona Cantonment: Normal population, 24,000; reduced to 15,000 or 12,000 during plague, 10,903, 10,906, 21,314.
Poona City: Second epidemic, population reduced from about 118,000 to 60,000 or 80,000 by exodus, 21,782–4.
Risk that evicted people will go to other villages, 21,147.
Sholapur: People in easy circumstances the first to leave the town, 20,250.
Sholapur: About 35,000 people left the place, 20,245.
Sholapur: People left the town before proceedings taken for general evacuation, 20,237, 23,989.
Shopkeepers scatter on being evicted, but landowners do not, 5891, 24,067–14.
Surat: About 10,000 people left with passes, 24,461.
Surat: Population increased by 20,000 through influx from Bombay, 15,863.

Prevention of:

Concessions connected with inoculation have good effect in stopping flight at Bangalore, 2637–8.
Concessions given to inoculated prevented the running away at Bulsar, 16,291.
Exodus at Bangalore not checked by guarding roads and pass system, 2641–3.
Hubli: Inoculated remain in larger proportion than uninoculated, I. 399.
Hyderabad: Stringent measures adopted to prevent the people flocking to the district, 18,116.
Mysore: Lenient measures prevented flight, and consequently there was very slow spread of plague in the town and neighbourhood, 24,970–8.

Spread of Plague through:

Bangalore: Villages in the neighbourhood infected by refugees, 2669.
Dharwar: One-third of those who left the town encamped near it, but about 19,000 people must have scattered over the country and spread plague, 23,767–70.
Karachi: People left in great numbers in beginning of 1897, and carried infection to Cutch Mandvi, 3,708.
Neighbouring villages infected by people flying from plague in towns, 24,016.
Sholapur: Fifty-five thousand refugees infected every village between that town and Hyderabad, 5153.
Sholapur: Refugees from, spread plague, 23,338.
Sholapur: At least 20,000 people left, carrying the plague with them over the country, 5425, 23,332–8, 23,912, 24,016.

See Fear of Plague.

EXPERIMENTS ON ANIMALS:

Experiments on animals of no value, except on those of the same class, 27,370.
The test of the efficiency of a method should be made directly on the species of animal which it is intended to benefit, 26,515–22.

See Inoculation with Haffkine's plague prophylactic.

EYE:

Clinical Features in Plague:

Complications of the eyes a clinical feature of plague, 20,897–901.
Conjunctiva: hyperæmia of, a clinical feature of plague, 12,398.

EYE—*cont.*

Inflammation of the cornea a clinical feature of plague, 16,768.
Ophthalmia a clinical feature of plague, 10,215.

Infection through:

Infection by microbes entering the body through the corners of the eyes and nostrils, III. 42.
Infection entered through the conjunctiva in one case, 7211, 7215.
Infection through the mucous membrane of the eye, 20,530.

Sequel of Plague:

Complete disintegration of the orbits and disease of the eyes followed plague, 16,770, 16,822, 16,833, 20,896–901.

Sight, Loss of:

Loss of sight through careless handling of bubonic discharge, 17,587.

FÆCES:

Life of Bacillus of Plague in:

Bacillus of plague dies in 2½ hours in fæces, 3611.
Bacillus not recoverable from fæces after death, 1437.
Can only be found in the excreta of fæces and urine when they contain blood, 22,367.
Cannot be cultivated from fæces, 1436–7.
Found in fæces, 3556–64.
Negative results as to discovery of plague bacillus in four cases examined, and positive result in a fifth case where matter examined was almost pure mucus, 25,363–4.
Negative results as to discovery of plague bacillus in cases examined, 17,271.
Unsuccessful attempts at isolating the bacillus from fæces ascribed to the rapid growth of contaminating micro-organisms, 26,391.

See Excreta.
„ Rats, *discovery of bacillus in*, and *infection by.*

FAKIRS:

The fakirs at Bhiwandi remained entirely free from plague, 22,128–9.

FEAR OF PLAGUE:

Deccan Brahmans are very cautious with regard to plague, 14,367.
Fear, &c. of plague drove the people at Rohri from the suburbs into the date plantations, 12,079.
Fear of plague is growing on the people, 26,568.
Fear of plague shown in Ankleshwar, 14,566.
Fear of plague makes people neglect to dispose of their dead, 7,420, 9,895–8, 13,491, 18,569.
Fear of plague did not at first move people at Dharwar to take any steps, 1614.
Great fear of infection among the natives in the hills, 8829, 26,615.
Great fear of plague shown in Bangalore, 2625–6.
Native dread of plague, 9898.
People more afraid of plague officers than of plague, 15,130.
People showed much fear of the plague in the Panjab, 10,263.
People who are afraid of the disease most liable to be affected by it, 10,857.

See Exodus of people from plague-infected places.

FEMALES.

See Inoculation with Haffkine's fluid. *After Effects, Bars to Inoculation* and *Dosage.*
„ Sex.
„ Pregnancy.

FEVER, RELAPSING:

See Relapsing Fever.

FLEAS:

Fleas, no plague microbes found (Hankin), 8600.
Fleas in Bombay hospitals do not convey infection, 20,640, 20,999.
Fleas in the plague infected villages of the Kaira district, 14,081.

FLEAS—*cont.*

Fleas, plague microbes said to be found in, 151.
Fleas, independent of plague, Baroda noted for its colonies, 14,642.
Fleas and bugs possible agents for carrying infection, 9,500–4.
No evidence that fleas carry infection, 20,640.

LIGHT OF PEOPLE FROM PLAGUE-INFECTED PLACES.

See Exodus of people from plague-infected places.

FLOORS:

Disinfection of:

Ahmedabad: Digging up of floors resorted to as disinfecting method, 13,923.
Ammonia and albumenoid substances in floors hindrances to chemical disinfection, 20,217–20.
Bacillus of plague survives disinfectants in infected floors, 5159.
Danger in digging up floors minimised by whitewashing it, 5554, I. 456.
Dharwar: No plague after ventilation, &c., of dug-up floors, 23,778.
Experiments on best method of disinfecting floors, II. 10–3.
Experiments on effect of perchloride of mercury on infected floors, 18,828–63.
Experiments showing that the number of micro-organisms in floors is less after than before disinfection, 18,801–27.
Guntakal: Upper two inches of floor dug up and burnt to disinfect, 5117.
Jawalapur: Digging up of floors, 8934.
Khandraoni: Floors dug up, 9659.
Mud floors very difficult to disinfect, 8558.
Palanpur: Digging of floors resorted to as disinfecting measure, 13,091–4.
Panjab: Taking up the floors of infected houses a dangerous and expensive process, but resorted to, 10,039.
Rohri: Disinfection by digging up floors, 12,029.
Stone floors much easier to disinfect than cow dung, 8269, 8562.
Stone floors cannot be removed for disinfection, 6442.

See Burning.
,, Cow-dung.
,, Kiln burning.
,, Perchloride of mercury.

Infectivity of:

Bacillus of plague on aerobic species cannot live far under the surface of floors, 10,040.
Bacillus of plague extends to a certain depth by growth, 11,457.
Bacillus of plague generally located in floors and walls, 9682.
Bacillus of plague may be disseminated by rain and carried into buildings by soil adhering to the feet, 6900.
Cases frequent in those who sit on the ground, 5097.
Eating food in infected localities off the ground, a cause of infection, 18,108.
Fact that women are, as a rule, more affected by plague than men would point to the floor as seat of infection, 16,216.
Floor, infection confined to, 23,249.
Floor one of the only factors which has proved infective, 21,717.
Floors principal source of infection, 24,311.
Floor probable medium of infection, 9617.
House with pukka floors remained immune, while cases frequent in houses with earthen floors, 19,442.
Houses with cemented and tiled floors less attacked than houses with mud floors, 22,650.
Infection spread by earth and cowdung floors, 21,725.
Infection thrives better in houses with mud floors than with stone floors, 16,222–8.
Infection of plague mostly found in mud houses, 10,373.
Instance to show greater infectivity of mud floors, 17,288–91.
Parsee houses, Poona, plague cases mostly on ground floors, 25,244.

FLOORS—*cont.*

People living in huts and on ground floors of mud are more susceptible than those living in the top floors of wood, 19,100.
People who live in upper stories escape most, 18,569.
Plague cases in Bombay tabulated according to the floor on which they occurred, 916.
Plague cases not so numerous in upper as lower floors of chawls in Bombay, 2784, 28,422.
Poona: Most cases on ground floor, which are more crowded and worse ventilated than upper floors, 21,519–22.
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See Beds.
,, Boots.
,, Habits of the people affecting liability to plague.
,, Soil.

FLYING COLUMNS:

Composition of the flying columns, organised to carry out disinfection in places where special staff was urgently required, 16,992.
Organisation of flying columns, 17,158–9.
Staff and duties of flying columns, 21,618–29.

FOOD:

An acid re-action in articles of food kills the bacillus in a very short time, 8544, II. 7.
Infection by the alimentary canal is possible, 7076, 7211.
Natives may be infected by eating food directly off infected ground, 18,108, 22,021.
No post-mortem evidence of infection by the alimentary canal or stomach, I. 368, I. 375.

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FORMIC ACID:

Result of experiments, II. 7.

FORMS OF PLAGUE:

See Atypical forms of plague.
,, Bubonic plague.
,, Bullæ.
,, Clinical features of plague.
,, Pestis minor.
,, Pneumonic plague.
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FOWL.

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FUMIGATION:

Fumigation with resin and sulphur in Daman native villages used as a disinfecting measure, 25,448.

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FUNERALS:

Backergunge: Cases of plague contracted by attendance at funerals of pneumonic patients, 7331–9, 8463–9.
Baroda, Gungaur: Epidemic due to attendance at the funeral of a pneumonic patient, 15,182–5.
Bombay: Three or four friends of a man, dead of plague pneumonia, got plague by attending his funeral, 18,212.
Bombay: Plague contracted from contact with the corpse of a pneumonic patient, 19,099.
Calcutta: Funerals are powerful features in spreading the plague, II. 538.
Clothes distributed at a funeral might be calculated to spread plague, 9802.
Hubli: Woman with plague at Hubli in February 1897 contracted it by attending a funeral in Bombay, 2396.
Kakhindi: Plague due to attending a funeral, 19,513.
Sirohi: Spread of plague after attendance at funeral of first patient, 9702–9, 9794–802.
Surat: Plague brought to Umra by people who had attended a plague funeral elsewhere, 2054.
Surat: Plague kept alive in camps because people got infected at funerals and gatherings to lament over the dead, 24,441.

FUNERALS—*cont.*

See Acharj.
,, Corpse bearers.
,, Domes.
,, Habits of the people affecting liability to plague.

GANCHI:

First cases of plague at Ankleshwar among the Ganchi, 14,209.
Houses of Ganchis in Surat very dirty: Sunars fairly clean, 2048.
Mortality in Government health camp accounted for by it being occupied by Ganchis, who hired themselves out as corpse bearers, 24,422.
Severe outbreak among the Ganchis in Surat, 2047.

GANJA:

Immunity of ganja smokers from plague, III. 43.

GARHWAL:

See Bombay: *Introduction of plague into.*
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GEOLOGICAL CONDITIONS AND THEIR RELATION TO PLAGUE:

Alluvial Soil:

It is doubtful whether alluvial soil is favourable to plague, I. 378.

Black Soil:

Chemical composition of black cotton soil, 8324.
Plague rages most fiercely in black soil, 23,649.

See Evacuation: *The rains and their Effect on Evacuation.*

Deccan Trap:

Plague cases almost entirely over Deccan trap and crystalline rocks; rare in alluvial belt, I. 295.
The connection between Deccan trap and plague merely due to infection spreading from Bombay over neighbouring part of the country of this formation, I. 295.

Geological Formation of Plague Areas:

Description of geological formations of the plague-stricken areas, I. 294–6.
No proof that geological formation has any effect on plague except by its physical formation, 8311, 8326.
Rocky and sandy soil and climate of Hyderabad Sind favourable for combating plague, 18,142.
Something in the environment of Calcutta inimical to the spread of the plague in epidemic form, 7627.

Laterite:

Laterite-supporting rocks coincide with chief plague areas, 8315.
Plague practically occurs in all soils of India and at present in the soil directly covering laterite, 8317–8.
Water passing through laterite would dissolve iron, which may be favourable to pathological organisms, 8319.

Porous Soils:

Medical opinion holds that porous soils are favourable to the spread of epidemics, 8326.
Porosity of the soil has probably some connection with plague, 8311.

GLANDS:

Clinical Features of, in Plague:

Description of typical bubo, I. 379.
Diffused discolouration over the bubo and ecchymosis underneath the skin in plague patients, 9573.
Mumps in early stage sometimes resemble buboes, 10,235.
Necrosis of; four instances, two recover, 6940.
Suppuration generally occurs, 2371.

Diagnosis of Plague:

Bubo alone not sufficient, 7530.

See Pneumonic plague: *Clinical features.*
,, Septicæmic plague: *Clinical features.*

GLANDS—*cont.*

Disappearance after Death:

Cases of bubonic plague in which the bubo disappeared after death, 20,010–6.

See Corpse inspection. *Efficacy of Corpse inspection in identification of plague.*

Discharge, Character of:

Loss of sight through careless handling of bubonic discharge, 17,587.
Peculiar character of the slough, 20,733.

Discovery of Bacillus of Plague in:

Found in bubo during life, 7189.
Found in bubo, spleen, liver, and lungs of plague corpses, 7189.
Found in enormous numbers in buboes, I. 370.
Found in glands, kidney, spleen, liver, and blood, I. 370.
Found in the bloody discharge from the nose in patient with sub-mental buboes, 26,259.
Gradually disappears from bubo as soon as suppuration sets in, 20,613.
Invariably recovered from buboes before suppuration has taken place, but none after, 25,370.
Not extracted from pus of bubo, 1530.
Not found in pus of buboes, 1,530.
Other than plague bacilli found in pus from buboes, 26,265.
Plague bacilli not found in lymph of bubo in patients whose temperature had gone down, 26,396.

Fatality with reference to:

Cases with multiple buboes generally fatal, 14,853.
Fatal forms of plague do not develop buboes, 10,199.
Sixty per cent. of multiple bubo cases recovered among the inoculated, 14,997.

Frequency of:

Bubonic cases represent about 75 per cent. of the whole number of cases, 9389.
Enlarged glands without pronounced plague a common occurrence among plague workers, 18,065.
German Plague Commission report cases of swollen glands unknown to patient, 7003.
Lymphatic glands enlarged in about half the cases of primary carbuncles and buboes, 12,487.

Number in Plague:

Generally one; highest number seen, 17, I. 370, 2368.

Position of:

Axillary buboes represent about 14 or 15 per cent. of the total cases, 9389.
Glands, affected by proximity to spot of entry of infection, 1410.
Glands directly related to the seat of inoculation, bubo not necessarily situated in the group of, 9566.
Groin, generally in the, 298.
In buboes of the neck local lesion generally found on the surface of the tonsil, 11,580.
Notes on the relative frequency of the buboes in different situations, 12,395–6.
Observations on the appearance and position of buboes in the patients of the Sassoon Hospital, 12,897.
Table showing the position of buboes in 3,128 cases, II. 95.
Table showing the site of the glands in 101 plague cases, II. 196.
See Inoculation with Haffkine's fluid: *After-effects—Buboes.*

Post-mortem Appearance:

Enlargement and injection of the mesenteric glands and enlargement of the epiploic vessels constant features in plague cases, 9549.
Lymphatic and mesenteric glands generally found enlarged in cases where no external buboes appeared, 10,419.
Post-mortem appearance of glands, 1419–20.

GLANDS—*cont.*

Prognosis of Plague:

Prognosis bad when cases with small buboes, 21,652.
Prognosis bad with slight glandular enlargement, which indicates fatal septicæmia, 6986.
Prognosis is good if the gland is confined to the femoral region, 13,025, 15,308, 16,544.
Suppuration of bubo a good prognosis, 1529.

Sexes, in:

In females mostly axillary, 18,051.
Women in C. Ward, Bombay, suffered more from axillary buboes than men, 18,049.

Time of Appearance:

Attack, during, may develop, 12,398.
Beginning of fever, have been noted before, 12,398.
Convalescent, may appear when patient is, 2,370.
Details of cases showing period between beginning of illness and appearance of buboes, 20,027, 20,032.
Early symptom, buboes a very, 10,204.

See Glandular fever.

GLANDULAR FEVER:

Bacteriological Examination:

Cultivations of the blood taken from hospital patients of the Shropshire regiment remained absolutely sterile, with one exception, where a strepto-coccus was found, 11,035.
Cultures from the blood of two men of the Shropshire regiment injected into rats and guinea-pigs produced no effect whatever, 26,859.
Microscopical examination for the malaria plasmodium in patients of the Shropshire regiment gave no result, 11,017.

Clinical Features of:

Cases with or without buboes common and long known in Calcutta with 40 per cent. mortality and not clinically to be differentiated from plague except, perhaps, by look of tongue and infectiousness, 8209, 8213–17, 8220–31.
Clinical features of glandular fever have nothing in common with those of bubonic plague, 6974.
Clinical record of cases of lymphatic gland enlargement in the military hospital, Mian Mir, 11,065.
Difficult to tell glandular fever from plague, 1161–4.
Fever and swollen glands possible apart from plague, 6958.
Glandular enlargements from causes other than plague not uncommon, 613–4.
Plague quite distinguishable from ordinary fever with swollen glands, 6975–9.
Several men of the Shropshire regiment ill with dysentery developed buboes, 11,023, 11,053–4.
Typical symptoms of, I. 521.

In:

Bengal: At certain times of the year people suffer from enlarged inguinal glands, ascribed to climatic influence, I. 488, I. 521.
Bengal: Glandular fever very common in poorly fed low caste natives, who are constantly exposed to malaria, 7636.
Bombay: Glandular fever is not plague. No cases seen during epidemic, I. 382.
Bengal: No bubonic fever resembling plague in Bengal, 7322.
Calcutta: Cases of, may be found at any time, 8180, 8372–5.
Calcutta: Cases seen all amenable to treatment, 7846.
Calcutta: Appearance in 1858. Very fatal, 8107–8.
Calcutta: Details of cases of bubo and fever among the soldiers since 1894, 11,020.
Calcutta: Record of cases at Station Hospital, I. 495–6.

GLANDULAR FEVER—*cont.*

Calcutta: Seen every year from 1858 to 1898, 8114.
Mian Mir: Cases of glandular fever like those observed at Calcutta observed in Panjab, 11,050.
Navy: Cases of non-venereal bubo, I. 495, 27,005–8.
Poona: Many cases decidedly not plague, 7636.
Shropshire regiment: Cases of bubo and fever among the men at Calcutta during the summer of 1896 attributed to malaria, 11,017.

Infectivity of:

Non-infectious and non-contagious, 8121.

See Calcutta; *Plague in* 1898 and *Suspected cases of Plague in* 1896 *in Calcutta.*

GOATS:

Effects of Virus on:

Goats wasted away under repeated doses of plague virus, 133.
Secure local immunity after repeated dose of plague virus, 115.

Immunity from Plague:

Unaffected by plague, I. 378.

Serum of:

Serum of goats used for inoculation, 130–3.

GOLAS:

First detected cases in Umreth in the Gola quarter, 14,027.
Mortality in Government health camp accounted for by it being occupied by Golas who hired themselves out as corpse bearers, 24,422.
Refused to move to other sites, municipality too poor to apply the Land Acquisition Act, 24,459.
Spread the infection to the other quarters in Surat, 16,064.
Suffered most from plague in Surat, 16,108.

GRAIN:

Discovery of Bacillus of Plague in:

Experiments on, with several kinds of grain and seeds, II. 16–17.
Found in grain, I. 442.
Failure to isolate bacillus from grain, 619–20, 635, 995.
Not found in supposed infected grain bags, grain, &c., 635.
Series of experiments for the recovery of the bacillus from grain, 26,386.

Disinfected by Sun:

Grain sundried at Hinganghat to disinfect it, 6066.
Grain infected by the excreta of rats disinfected by exposing to the sunlight, 9409.
Grain exposed to the sun for six hours, 10,317, 10,935.

Infection due to:

Damaged grain source of contagion to rats, thence to men, III. 42.
Danger of infection through clothes, much less than through bags containing grain, in which rats would nestle, 994.
Dharwar Jail: Possible introduction of plague in grain, 2241, 2289.
Grain bought from infected Banniah's shop by woman; she got plague; no other source of infection traced, 23,005–7.
Grain is eaten unparched as well as parched, 9455.
Grain may be infective if infected by rats, 9399.
Grain stolen from a house evacuated by Banniahs; plague followed among the Mangs who stole it, 23,004.
Infection by grain, 23,004, 23,005, 13,814, 13,834.
North-West Provinces: Infection supposed to be imported at Kankhal by infected grain from Hardwar, 8774.

GRAIN—*cont.*

North-West Provinces: Jawalapur, possible introduction of plague by grain, 9153.
Possible introduction of plague at Reri, by grain and eatables brought from infected areas, 12,447.
Rats and grain somehow responsible for spread of plague in Bombay, 8277.
Rats, clothing, and some unknown agent in the retail grain shops spread plague, 11,278, 11,430.
Rotten grain suspected at Bombay, 54.
Theory that grain kept long generates the plague bacillus, 17,019–20.
Sholapur: Disease said to be due to importation of infected grain from Karachi, 1000.

See Mahamari: *Food and Grain and Mahamari.*

Infection not due to:

Grain not a factor in the dissemination of the disease, 16,316, I. 377–8.
Not through grain, 20,636.
Not carried by grain or articles of food, 7511.
Rats proved not immune by subsequent injection eat with impunity artificially infected grain, 635, 642.

Life of Bacillus in:

Bacillus not recovered after 7 days in grain artificially infected and dried, 26,386.
Dies in sterilised solution of rotten grain, 3669.
Experiments to show that the bubonic microbe derived from pure cultures perishes within 13 days after being added to specimens of grains and seeds, II. 17.
Results of experiments with several kinds of grains and seeds on life of bacillus of plague, II. 16, 17, 18.
Virulence of the microbe in grain only persistent for a few days, 8535.

GRAIN DEALERS:

See Banniahs and Traders.

GRANARIES:

See Banniahs and Traders.

GROWTH OF BACILLUS OF PLAGUE:

See Bacteriological characteristics of bacillus of plague.

GUINEA PIGS:

Discovery of Bacillus in:

Easily discovered in the bronchial contents of guinea-pigs in laboratory experiments, 27,341.
Found in the intestines of a guinea-pig, 27,336.

Involution Forms of Bacillus of Plague in:

Involution forms of plague bacilli seen after death at the seat of inoculation in a guinea-pig, 26,227.

Post-mortem Appearances:

Greater variety in those of guinea-pigs than in those of rats, 26,409.

Virulence of Bacillus of Plague in:

Virulence of bacillus of plague increased by a series of intra-peritonial passages through guinea-pigs, 26,434.

GUNNY BAGS:

Discovery of Bacillus of Plague in:

Microbes not found in sacks or other article of merchandise, 995.
None found in gunny bags in the Joria Bazar at Karachi, 11,210.
Not found in, 635.
Rats proved not immune by subsequent injection, eat with impunity supposed infected bags, 635–642.
Series of experiments for the recovery of the bacillus from artificially infected grain and gunny bags, not recovered after seven days, 26,386.

Disinfection of:

Grain, sun-dried at Hinganghat, and the bags boiled, 6066.

GUNNY BAGS—*cont.*

Gunny bags imported from infected places sent to Kiamari camp for disinfection by steam, 12,017.
Only cargo subject to disinfection at Karachi are the gunny bags which are sent back empty, 12,212.

Infection by:

Bhujpur: Infection said to be imported in gunny bags from Bombay, 13,462–71.
Idea that bags carry infection through rats, and that Karachi was infected by them in the second epidemic, 817.

HABITS OF THE PEOPLE AFFECTING LIABILITY TO PLAGUE:

Habits connected with Animals:

Jains and Banniahs averse from killing any animal or insect, 13,354.
Jains: Animal life sacred among the, 17,681.
Jains averse from anything involving the death of an animal, 7893.
Jains: High death-rate among, due to their refusal to destroy animals, particularly rats, 17,681.
Jains, Banniahs, and Marwaris will not take animal life, 17,741.
Jains, Banniahs, and others would sooner run the risk of plague than take animal life, 910.
Marwaris object to the slaughter of rats, I. 464.

Other Habits:

Attendance on sick: Natives kiss and wipe the mouths of plague patients, 13,013, 25,229.
Attendance on the sick: Plague spread by handling sputum, 17,785–18,511.
Attendance on sick: Spread by native habit of receiving patient's sputum into their hands, I. 379.
Attendance on sick: Sputum caught from plague patients in the hands of sympathetic friends, I. 408.
Barbers: Disease communicated by razors, hair-brushes, and aprons of barbers, III. 43.
Barbers: Infection through natives having their armpits shaved, III. 44.
Barbers use their razors for cutting the buboes of patients, 17,962.
Barbers who went to infected villages and brought clothes to their own villages introduced plague there, 10,051, 10,088.
Bare feet: Cracks on feet of natives common, 15,198.
Bare feet: Going barefoot, a common habit, is dangerous, 24,311, 24,349–51.
Bare feet: Infection through feet not common in North India, as no stones there to cut the feet, 9391.
Bare feet: Kumaun: No shoes or stockings generally worn, 26,573.
Bare feet: Kumaun: Shoes often not worn except in higher parts, 26,619–24.
Bare feet: Natives infected by their feet through cuts, 1002.
Bare feet: People go barefoot and have cracks in their skin, 532.
Bare feet: People go barefoot and sit on the ground, 17,370.
General remarks on influence of habits, 17,681–6.
Ground: Cases frequent in those who sit on the ground, 5097.
Ground: Infection by food in infected localities eaten off the ground, 18,108, 22,021.
Ground: Natives sit with their hands on the floor, and have many scratches and lesions by which infection may enter, 20,644.
Infection by mud used for cleaning purposes after deeds of nature, 18,113, 25,778.
Infection by mud used to clean brass vessels: cleaning with infected earth may communicate plague, 18,049, 18,112.
Infection by visiting the sick, 19,369.
Infection through funeral ceremonies, 17,993.
Infection through certain remedial ceremonies, 10,104.

See Beds.
„ Boots.
„ Funerals.
„ Leeches.

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HÆMORRHAGES:

Clinical features:

Hæmorrhages in the bowels and stomach, 20,854.

Post-mortem Appearance:

Description of internal hæmorrhages, 6898.
Hæmorrhage in every part of the body, 20,972.
Small hæmorrhages in Peyer's patches seen in two cases, 6899.

Prognosis:

Sign of bad prognosis in plague cases, 12,413.

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Constituents of Haffkine's fluid.
Re-sterilisation of Haffkine's fluid.
Sterility of Haffkine's fluid.

See also, Inoculation with Haffkine's Plague Prophylactic, *under which the entries are given under the following subheads:*

Adoption of Inoculation as a general Plague Measure.
After Effects of Inoculation with Haffkine's Fluid.
Age and Inoculation.
Animals, Experiments on.
Attitude of People towards Inoculation.
Bars to Inoculation with Haffkine's Fluid.
Caste, Religion, and Race; Inoculation with reference to.
Certificates of Inoculation.
Dosage.
Duration of Protection afforded by Inoculation.
Effects of Inoculation with Haffkine's Fluid—
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STERILITY OF HAFFKINE'S FLUID.

Contaminated:

Animals: Eight out of 38 died from the effects of inoculation of the prophylactic, which was found to be contaminated, 24,589–601.
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Micro-organisms in fluid appear to give a certain amount of immunity, 5708.

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Micro-organisms found in two out of 12 bottles of Haffkine's fluid, 10,653.
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Organisms, the contents of six properly-sealed bottles left to grow at the natural temperature of the room showed a growth of living, 5811.
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Results of bacteriological examination of Haffkine's fluid at Calcutta, 8380–9.
Results of examination of old bottles of prophylactic in Haffkine's laboratory, 17,481.

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Sterilisation by Carbolic:

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Plague bacillus cannot possibly live in the fluid, because of the carbolic acid, 8411.
Probably affected by heat but not by carbolic, 119–20.

Sterilisation by Heat:

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Sterilisation, Importance of:

The use of a microbe of known virulence and an absolutely aseptic vaccine the necessary factors in, 26,488.

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Health Officers not necessarily members of the Indian Medical Service, 7939.
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Health Officers appointed by Municipality, 7937.
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Attitude of the People towards:

Bangalore: Very unpopular, 3282.
Bangalore: People thought Government were poisoning patients in hospital, 2628.
Bombay: Arthur Road Hospital threatened with destruction by the populace, 26,638.
Bombay: Patients go more readily now to hospital, 444, 7863.
Bombay: People had no belief in hospitals, 17,950.
Calcutta: Manicktollah Hospital, patients will not go to, 7717.
Calcutta: Natives say they are not properly treated at Manicktollah, 7722.
Calcutta: Patients go fairly readily to Mayo Hospital, 7717.
Calcutta: Very friendly relations with relatives of patients, 6966.
Dharwar: Patients come readily, 1863.
Dread of Government hospitals and of being carried there on stretchers, 22,656.
Hardwar: Great objection of the natives to go to hospitals, 8800.
Hindu notions averse to taking people to hospital, 22,581.
Karachi: People voluntarily brought sick to hospital huts in camps Trans Lyari, 11,246.
Mortality in hospitals the reason for their unpopularity, 26,550.
Muhammadans rather troublesome in hospitals, 13,532.

HOSPITALS—*cont.*

Objection to hospital from fear of being poisoned, 18,574.
Poona: Relatives willing to send patients to hospital as soon as definite symptoms of plague develop, 25,842.
Preponderance of males in hospitals partly due to the objection to bring the females there, 12,347.

Caste Prejudices and Caste Hospitals:

Bangalore: Caste prejudices respected as far as possible, 3483.
Calcutta: Caste prejudices respected in hospital, I. 471.
Cutch Mandvi: Caste hospital much approved of by the people at, 13,443.
Cutch Mandvi: No difficulty to get people to caste hospitals, but considerable objection to be taken to Government hospitals, 27,191.
Surat: Caste prejudices considered in the Hindu Hospital, 16,137.
Surat: Concealment punished in Surat by removal to general instead of caste hospital, 24,417.

Friends and Relatives of Patients, Arrangements for:

Broach: Families should never be separated in hospitals; good results of system, 15,500.
Dharwar: Friends allowed to stay with patients if they like, 1867.
Each hospital should have a house attached for some of the friends of patients, 21,544.
Friends encouraged to come to the hospital with the sick, 6963.
Hubli: Friends allowed to accompany patients to hospital, 1656.
Poona: A friend of the patient taken to hospital with him, 10,915, 21,259.
Surat: Accommodation for friends of patients in Hindu Hospital, 16,137.

Infection in:

A properly made plague hospital does not require disinfection every time a patient dies, 19,515.
Attendants and friends of patients escape the disease in hospitals because they are under much better hygienic conditions, 14,713.
Bangalore Civil and Military Station: Of 16 attendants and ayahs, four got plague, 2737–8.
Bangalore Civil and Military Station: Friends of patients allowed to accompany them and attendants do not suffer, 2897–900, 2922–3, 3483.
Bombay: Arthur Road Hospital: very few attendants attacked, 20,973, 20,989.
Bombay: Cases among attendants at Bombay, 7218–9.
Bombay: No cases noted of infection of hospital servants by Major Deane while in Bombay, 2789.
Bombay: No disinfection at first of plague cases in Jamsetjee Hospital, Bombay, but no bad results, 1439, 1516.
Bombay: Staff and attendants not often infected, 22,489.
Broach: No infection in hospital, 15,500.
Calcutta: Antiseptics used but particular precautions not taken by nurses, 7083, 6961–2.
Calcutta: Disinfection carefully attended to, 7022.
Calcutta: Medical College Hospital not an ideal place, ventilation leaves much to desire, 7082.
Calcutta: No infection in the Medical College Hospital, 6921, 6961.
Calcutta: No infection among 74 servants in the Chandni or Mayo Hospitals, 7020.
Calcutta: No infection in the Medical College Hospital, 7081.
Clothing destroyed and no further infection in Bangalore Hospital, 2894.
Clothing disinfected in Anand Hospital by steam, 14,127.
Clothing not very infectious to hospital attendants, 237.
Contact with plague patients in plague hospital said to be means of conveyance of plague, 19,031.

HOSPITALS—*cont.*

Cutch Mandvi: Fourteen plague nurses working in Mandvi Hospital during first epidemic; very few casualties among the attendants or relatives, 13,527, 13,533–6; I. 351.
Dharwar: 30 relatives attending on patients developed plague, and two bubonic and four pneumonic cases occurred among hospital staff; pneumonic infected from pneumonic cases; bubonic not from pneumonic cases, as there were none such in hospital at the time of their infection, 1896–1901.
Dharwar: Attendants on the sick not very often attacked, 1953.
Freedom from infection due to good sanitary conditions, 1440–1, 10,376.
Gadag: Hospital staff and attendants not infected; cases that occurred contracted outside, 23,684–91, 23,757.
Hubli: Few cases amongst hospital attendants at Hubli and Dharwar, 1704.
Panjab: Hospital attendants usually not attacked, 10,488.
Patients spit about everywhere, 20,628–7085.
Plague cases among doctors, 7220.
Plague cases among nurses, 7220, 7661.
Plague mortality among those connected with hospitals, 7220.
Poona: People in hospitals free, disease not very infectious, 12,876–7, 12,937, 21,539.
Porbandar: Few cases in hospital, where only six pneumonic cases treated, 13,724–7, II. 484.
Sirohi: Few cases among attendants and plague officials, 9878–80, 9906–9.
Sirohi: High mortality among the sick attendants accounted for by their being in incubation stage on arrival at hospital, 9907.
Surat Government Hospital: Few attacks among staff and attendants, 15,792–6.
Surat: One-fourth of the staff and attendants in the Hindu Hospital were attacked; no shoes were worn, the floors were bad, and disinfectants not properly used, 15,797, 15,825.
Surat: Only 19 cases among 1,200 relations of patients in the Hindu Plague Hospital, Surat, 16,149.
Umreth: No deaths among the attendants in the Plague Hospital or the village hospitals, II. 233.
Very slight infectiousness in a properly kept hospital, 1439.

Mortality in Hospitals:

Ankleshwar hospitals: table showing, with results, admissions to the, II. 246.
Bangalore City: 66.7 per cent., 25,024.
Bangalore City: Statistics regarding, 3483.
Bangalore Military Hospitals: 71·4 per cent., III. 286.
Bangalore, North Camp Hospital: 63·4 per cent., 25,024.
Bangalore, South Camp Hospital: 60·2 per cent., 25,024.
Baroda: 71 per cent., 15,266–8.
Bombay, Arthur Road: 71·4 per cent., 21,050, III. 457.
Bombay: Arthur Road and Parel Hospitals, I. 391.
Bombay: Arthur Road Hospital: Statistics of cases treated from September 1896 to February 1899, III. 457–60.
Bombay: Parel Hospital, I. 257.
Bombay: Statement showing number of plague patients admitted to all plague hospitals, public and private, from March 1897 to March 1899, III. 313.
Calcutta: 80 per cent., 6550.
Calcutta: Mayo Hospital, description of plague cases in, 7015.
Calcutta: Medical College Hospital, I. 244.
Dharwar Hospital: Case mortality, 58 per cent., 1857.
Gadag: Statistics relating to, 23,694–5.
Karachi Civil Hospital: From March till August 514 cases treated with a mortality of 60 per cent., 12,342.
Mysore: 49 per cent., 25,024.
Poona, Sassoon Hospital: 74·4 per cent., 12,896.
Umreth Plague Hospital: Forty-eight deaths out of the 64 cases admitted to, II. 233.

HOSPITALS—*cont.*

See Medical treatment, European.
,, Removal of the sick to hospital.
,, Removal of moribund patients to hospital.

Private Hospitals:

Ahmedabad: Private hospitals established, 13,890.
Bombay: Private local hospital started in the Telang property whilst the blocks were disinfected, 17,865.
Calcutta: Large number raised by private subscriptions, and licensed, I. 469.
Calcutta: Particulars of private hospitals, I. 470.
Calcutta: Private hospitals little resorted to, II. 533.
Karachi: Private hospitals started by several communities, 12,523.
Porbandar: Hospitals started by private persons, 13,702.
Private plague hospital and segregation camp popular with the people, 12,522.
Surat: Hindu Hospital maintained entirely by private contributions, 16,137.

Provision, Organisation, and Staff of Hospitals:

Ahmedabad, list of plague hospitals, 13,890.
Anand: Plague hospital built of millet seed stalks, lined with bamboo matting which was whitewashed, 14,111.
Bangalore: The details of the camp hospitals at, I. 117–9.
Bombay: Hospital arrangements, accommodation for 1,000 cases, 251–71, 276–84, 417–8.
Bombay: Nursing poor at first, now satisfactory, 259.
Bombay: Organisation satisfactory, 250.
Bombay: Very inadequate staff in plague hospital, 25,508–32.
Calcutta: Arrangements for hospitals in different wards of the town, 6756.
Calcutta: Manicktollah Hospital, description of, I. 469.
Dharwar: Staff (beds 76), 1852, 1854, 2068, 2071.
Hardwar: Old temple used as plague hospital, 9332.
Igatpuri: Municipal school turned into hospital, 16,887.
Inspector-General of Civil Hospitals not responsible for supervision of municipal hospitals, 276–8.
Inspector-General of Civil Hospitals responsible for inspection of municipal hospitals, 7742.
Plague hospitals should be located at a distance of at least half a mile from the town limits, 13,928.
Poona: Sassoon Hospital, special building for plague cases, 12,941.
Porbandar: Eleven plague hospitals, 13,702.
Rural areas: No hospitals in, 1661.
Surat: Native hospitals good at, 5.
Surat: Proper hospital system introduced by Lord Sandhurst, 16,137.
Surat: The Parakh Dharamsala turned into a municipal hospital, 24,417.
Sweepers and police on duty at the plague hospitals at Hardwar relieved every 10 days, 9337.
Tents as hospitals better suited for cold than hot weather, 12,943.
Ward hospitals more suitable than one central plague hospital, 21,544.

HOT WEATHER:

Abatement of Plague during:

Ahmedabad: The dryness of the atmosphere probable cause of partial evacuation proving there more successful than at other places, 13,937.
Bombay Presidency: During the hot season the plague declined in, 2.
Broach: The habit of the people to sleep outside their houses in hot weather the reason why plague has remained dormant in May and June, 14,385.
Heat prevents plague from spreading fast, 5307.

HOT WEATHER—*cont.*

Hot winds appear to act as natural disinfectants, 8.
Hyderabad: The dry and very hot climate and rocky and sandy soil favourable conditions in combating the plague, 18,142.
Kolaba: Plague occurred in each of the infected towns in, in the cold weather, and dropped as the hot weather came on, 17,650.
Panjab: In hot weather plague becomes milder in type, 10, 332.
Plague decreases in hot weather, I. 378, 1035.
Sirohi State: Relation between temperature and epidemic very marked in, 9902, App. XXXII.
Table of maximum and minimum temperature during epidemic in Baroda City, II. 283.
Temperature of 80° to 90° the predisposing conditions to plague, 17,354.
Temperature in Bengal before outbreak of plague below normal, 6874.
Variable temperature; a rise in the plague attacks coincides with a fall in the temperature, in places with a, 22,433.

Epidemic of Plague during:

Cutch Mandvi: Plague begins in hot weather, 2.
Hot weather seems to have no influence on the virulence of the epidemic, 25,682.
Kankhal: Plague did not spread in May on account of the hot weather conditions, people being much out in the open and sleeping out, 9071.
Karachi: Heat did not affect plague in, 1700.
Sukkur: Epidemic at its height when temperature was 115 degrees in the shade, 11,446.
Sukkur: Rise in temperature no influence on the disease in, 18,441.
Temperature: Has nothing to do with plague epidemics in India, 11,444.

HOUSE.

See Disinfection.
,, Evacuation: *Length of period of evacuation.*
,, Floor.
,, Incubation period: *Incubation period in plague cases contracted by visits to infected places and houses, or contact with the sick.*
,, Infection by locality.
,, Soil.
,, Recurrence of plague in the same house or locality.

HOUSE SEARCHES:

Adoption of and Experience in:

Ankleshwar: House to house visitation gave good results, 14,218–9.
Bangalore City: House searches not adopted on account of the attitude of the people towards them, 3279.
Bangalore Civil and Military Station: System of house searches adopted, 2572.
Baroda: House to house inspection at the commencement of second outbreak very unsatisfactory, 14,601.
Bombay: House to house visitation by native gentlemen unsuccessful, 17,954.
Bombay: On account of large chawls and shifting population house searching of little use, 16,987.
Bombay: Opposition during the first epidemic to search parties, 18,465.
Bombay: Raids only made at odd times in worst quarters, 10,949–51.
Bombay: Search parties resented in, and cases concealed, 487–9.
Bombay: Search parties with European agency resulted in plague being driven out of one district but only into another, 18,464, 25,794.
Bombay: System of house searching abandoned, 8257.
Cutch Mandvi: Search parties instituted towards the end of April 1897, 13,375.
Karachi: Search parties employed with good results in camps, though results in the town were bad, the people scattering, and no information being obtained, 11,370–2.

HOUSE SEARCHES—*cont.*

Kolaba: In the second epidemic no resistance offered to the search parties by the people, except in 5 Muhammadans' houses, 18,465.
Kolaba: Search parties were good friends with the people, as they never interfered with caste prejudices, 18,466.
Poona: House searches successfully carried out, staff strong enough to prevent concealment, 10,908–13, 21,216, 21,259, 21,269.
Poona: Second epidemic, house searching by volunteers with help of soldiers, 21,494, 21,526, 21,576.
Shikarpur: People induced to use precautionary measures to avoid search parties, 18,288.
Surat: House to house search of no practical utility, 16,004, 24,417.
Umreth: House to house visitation every morning, 14,039.

Agency for:

Karachi: Objection to use of military in, 12,527.
Palanpur State: Darbar Sepoys employed for search parties, 13,069.
Poona: Search parties of European and native soldiers employed, 10,908, 21,259.
Voluntary organisations for the detection of plague more successful than military search parties, 18,387.

Opinions and Recommendations regarding:

House to house visitation for discovering plague cases absolutely intolerable to the people, 25,554.
House to house visitation impossible without considerable annoyance to the people, 16,987,
In large places only possible methods are searching, &c., 10,963, 16,987.
Room to room visitation in tenements impossible on account of the irritation it would cause, 7619.
Search parties did more harm than good at Surat, 24,417.
Search parties do more harm than good, 18,978.
Search parties not a good thing, too unpopular, 8259.
Searching ought to be the last means resorted to on account of the objection of the people, 10,964.
Ward system with superintendence a better measure than search parties, 18,975.

HOUSING:

Existing Conditions:

Ahmednagar: Contains houses in which air and light had never had access since they were constructed ages ago, III. 253.
Ahmednagar: Number of houses condemned in, re-building on a particular pattern, ventilation, big doors, windows, and height of roofs specified, 24,191.
Bangalore: Overcrowding in houses, 3258.
Bangalore: Statistics of housing, 3386, I. 128.
Baroda: Dwellings insanitary, 15,193.
Belgaum: Description of houses in, 22,810.
Bombay: Conditions vary in different parts, 8295–6.
Bombay: Conditions very bad, 17,692, 26,638.
Bombay: Description of houses and difficulty of keeping them clean, 833–91.
Bombay houses: Practically no light, 851.
Bombay: Kamatipura, 70 per cent. of houses insanitary, 972.
Calcutta bustees more airy than Bombay chawls, 7439.
Calcutta: Bustees dreadfully insanitary, 7632.
Calcutta: Bustee houses very difficult to disinfect, 7689–91.
Calcutta: Construction of bustees, 7708.
Calcutta: Housing conditions, 6861.
Dharwar houses: No light, 1711, 2307.
Dharwar: Houses of the agriculturists in the villages are very dirty, ill-ventilated, and dark, 23,518.
Dharwar: Style of houses one-storied with bamboo roofs laid on beams covered with earth easily opened up, 23,175.
Dharwar: Ventilation and light very bad, 23,185.

HOUSING—*cont.*

Hardwar: Houses well built, well ventilated, and not overcrowded, 9005–7.
Hinganghat: No light or air in houses, 6249.
Hubli: Except for ventilation, houses excellent for an Indian City, 20,144, 20,207–10.
Hyderabad: Houses badly lighted and ventilated, 5446.
Kankhal: Houses very large and sparely occupied, except on the occasion of the Kumbh fair, 9013.
Karachi: Houses in old town very bad, 11,080–6, 11,971, 12,456–63, 22,649.
Kumaun: Sleeping rooms, for warmth, just over cattle stalls, and also used as store-room for grain, some of which is gathered wet, II. 354, 360.
Nasik: Description of houses, 13,786–8, 16,980–4.
Panjab: Description of houses, 10,370.
Surat: Houses of Ganchis very dirty; Sunars fairly clean, 2048.

Housing of Cattle:

All villagers inclined to keep cattle and manure close to their houses, 7922.
Baroda: Cattle and men live all under same roof, 15,193.
Cattle in one enclosure with living rooms at Pali, 6423.
Cows kept in filthy state in Kumaun dwelling-houses, 6163.
Cutch Mandvi: Cattle kept by the people of, in the lower rooms of the houses, 13,353.
Dharwar district: Kept in the dwelling-houses in all the villages in the, 23,180, 23,431, 23,518.
Housing of cattle in human dwellings a confirmed habit which cannot be altered, 7922.

Improvements in Housing:

Bangalore: Scheme for improving housing and rebuilding part of city, 3261, 3481.
Bombay: Houses marked unfit for human habitation and removed; number removed, 25,666.
Bombay: Improvements effected in respect of insanitary houses, 946–71.
Bombay Municipal Act is likely to improve the housing, 869.
Bombay: Re-building of houses proposed by Bombay Government at cost of four crores, 1101.
Bombay: Re-housing cannot be carried out at once, 965.
Improvements required in conditions prevailing within the houses, 8066.
Intended to destroy the worst houses, but this was found impossible, 26,682.
Poona: A considerable number of bad houses demolished, 21,290.
To secure light and air in houses, the whole of India would require to be rebuilt, 8067.

No Relation between bad Housing and Prevalence of Plague:

Bombay: Mandvi quarter, houses where plague broke out first roomy and airy, 19,437.
Bombay: Plague, though often found in insanitary houses, severe in many sanitary houses in Mandvi, 17,691.
Bombay: Plague infected houses in Mandvi, many of them good, 19,437.
Bombay: Several cases at Mandvi in well-lighted and well-aired rooms, 1512.
Calcutta: Bustee houses expected to be the scourge of the place have been its salvation, 7443.
Calcutta: Some cases in good houses; disease appearing in good or bad houses wherever the contagium morbi was carried, 6659–60.
Habitation and surroundings not the exclusive factors in giving immunity from plague, 6070.
Incidence of plague does not vary with character of house, 6285.
Poona: Infection for a time raged very high in houses on raised platforms with plenty of air and light, 21,583, 21,608–11.

HOUSING—*cont.*

Sholapur: Cases equally frequent in bad and better class houses, 23,959–62.

Relation between bad Housing and Prevalence of Plague:

Ahmedabad: Considerable difference between the houses in the chief plague-infected areas and the other areas, 13,975, 13,978, 13,980, 13,985.
Baluchistan: Unfit houses pulled down, plague does not come, 8096.
Bombay: Bad housing responsible for plague remaining, 8273.
Bombay: Plague occurred in bad houses; no plague in good houses, 26,638, 26,672–4.
Calcutta: Incidence of attacks in dwellings at, I. 4474.
Dharwar: Plague in bad houses, while well ventilated houses escaped, 2307–9.
Karachi: Really good houses in the worst infected parts of the city remained free, 22,649.
Karachi: The wretched construction of the houses in the market quarter, and their insanitary condition, enabled rats to spread the disease, 22,649.
Most cases where sleeping rooms badly lighted and ventilated, 19,515.
Nasik: Plague cases chiefly in dark, dirty, and badly ventilated, and overcrowded houses, 24,524.
Nasik: Plague not worst in worst quarters, but houses in which cases occurred generally defective in lighting, ventilation, or overcrowding, 16,971–86.
Plague chiefly in low, damp, dirty, and ill-ventilated habitations, 11,848.
Plague very common in houses with no light and air, 2784.
Poona: Bad houses most exposed to attack, 21,505.
Poona: Sadashiv Pet: Particulars of houses affected, III. 157.
Poona: Well ventilated spacious houses escaped plague, 21,547.
Poona: The great mortality in certain parts of the city ascribed to the insanitary conditions of the houses, 12,880.
Rats die in well ventilated sanitary houses without plague following, 1147, 21,538.
Surat: Dirty houses attacked, 2048.
The poorest who live in damp, filthy localities are most susceptible to, 25,256.
Virulence and extension of disease dependent upon dirt, over-crowding and want of ventilation, 11,281.
Virulence of epidemic varies with badness of housing, 8298.
Worst cases of plague in ill-ventilated and overcrowded houses, 11,228.

See Air and light.
„ Overcrowding.
„ Sanitary conditions.

HUBLI:

Introduction and course of plague:

Attempt to confine plague to the chawls by a cordon failed, and first cases in town were those of persons who passed through the cordon from the chawls, 1583, 2407, 2436–8, 20,151.
Epidemic slight in beginning of 1898, and did not become severe till May, 1582, 1587, 20,168.
First epidemic in railway chawls, first case coming to notice on 15th October 1897, 1576, 2396–9, 19,971.
Infection said to have been first imported by persons who came with the disease from Sholapur, 2398.
Infection said to have been first imported by railway employés who had been to Poona and contracted the disease there, 20,141–2.
Plague attacks and death-rates, I. 412–6.
Plague may easily have been reimported in May 1898 by sick persons, or the infection already imported into the town may never have died out, 20,170.

HUBLI—*cont.*

Population at beginning of epidemic 57,000, 1570.
Statistics of attacks, deaths, and inoculation, 1598, 1615, 25,151.
Suggestion that the increase of plague in May 1898, after apparent cessation for a short period, was due to the reimportation of the infection in merchandise, 2413, 2441.
Woman who returned from Bombay in February 1897 developed plague, but disease did not spread, 2396.

Inoculation with Haffkine's prophylactic:

Both inoculated and uninoculated left the town, 3085.
Fifty-eight thousand and eighteen operations in 15 weeks I. 407.
History of inoculation at, 70.
Inoculations: Began on 11th May 1898, 1936.
Inoculations: Discrepancies in statistics, 3056–65.
Mortality from plague and other causes in inoculated and uninoculated, May to November 1897, 1819–24.
Report on inoculations at Hubli, I. 398–409
Statistics relating to inoculations, I. 65.
Statistics (corrected) relating to inoculation, 25,151.
Whole population inoculated in Hubli, except sick and aged, 1689.

See Census.

HYDROCHLORIC ACID:

Result of experiments, II. 8.

See Perchloride of mercury.

HYDROGEN:

Growth of Bacillus of Plague in:

Experiments on the growth of the plague microbe in air with admixture of hydrogen, 18,893.
Plague bacillus survives 5 days' desiccation in hydrogen, II. 8.

Isolation of Bacillus of Plague in:

Failure of attempt to isolate the plague bacillus from earth in an atmosphere of hydrogen, 26,387.

IDENTIFICATION OF BACILLUS OF PLAGUE.

See Isolation of bacillus of plague.

IDENTIFICATION OF INOCULATED PERSONS.

See Inoculation with Haffkine's plague prophylactic.

IMMUNITY FROM PLAGUE:

Classes of Persons alleged to be immune from Plague:

Alcohol consumers, III. 43.
Bhang consumers, III. 43.
Ganja smokers, III. 43.
Lepers, 917.
Opium eaters and smokers, III. 43.
Prostitutes, 917.
Syphilitic persons, 8693–4, 22,487, III. 43.

General Remarks regarding Immunity from Plague:

Immunity not racial, 19,448.
No such thing as complete immunity, 27,375.
Observation on immunity in different classes of people and castes during the present epidemic, 22,264–7.

See Europeans.
„ Inoculation of plague virus.

IMPORTATION OF CASES: *Instances in which Imported Cases were Numerous:*

A large number of importations or contacts are necessary in order to establish plague in a place, 16,000.
Ahmedabad: 77 plague cases (the majority bubonic) discovered at Ahmedabad Station among passengers from Bombay between October 5th, 1896, and February 16th, 1897, 13,886, 13,906, 13,908.

IMPORTATION OF CASES—*cont.*

Ahmednagar: 1896, 26 imported cases and no spread of infection, 24,216, 24,225.
Bhuj: Cases imported into, but no epidemic, 13,407.
Cutch Mandvi: Local cases began in March 1897; up to end of February 1897, 47 imported cases detected, 13,360–2, 13,519, 13,760.
Cutch Mandvi: 26 cases imported in January to April 1898 in period before second outbreak and when indigenous cases were very few, 13,411.
Hyderabad (Sind): 34 or 35 imported cases before infection became local, 18,087.
Mundra: 26 imported cases, with 17 indigenous cases, up to 6th June 1897; the main outbreak commenced later in August, 13,401.
Nasik: 26 imported cases, mostly bubonic, before outbreak commenced, 14,705–11.
Nasik: 35 cases of plague imported before disease became indigenous, 23,319.
Poona: 63 cases imported by railway before indigenous cases occurred in first outbreak, 12,856–9–62.
Poona: 30 imported and no indigenous cases between second and third outbreaks, 21,687–8.
Rohri: 22 cases imported from Sukkar between February 22nd and April 17th, 1897, 12,021–5.
Surat: Series of imported cases, 33 occurring before plague became indigenous, 15,556, 15,561–3.
Surat: Between March 1898 and February 1899, 16 imported, and no indigenous, cases, 15,750.
Wai: Notwithstanding influx of people from infected quarters, Wai remained entirely free from plague, 17,605.

INCUBATION PERIOD OF PLAGUE:

Arranged under the following subheads:

Incubation Period in Cases of Plague contracted from direct Inoculation.
Incubation Period in Cases that may be cited in connexion with the above.
Incubation Period in First Cases on Introduction of Plague.
Incubation period in Plague Cases contracted by direct infection from Rats.
Incubation Period in Plague Cases contracted by Visits to Infected Places and Houses or contact with the Sick.
Opinions regarding the Incubation Period.
Plague Cases of exceptionally long alleged Incubation Periods.
Plague Cases in Segregation Camps and Hospitals showing the Number of Days after Admission on which they developed.

Incubation Period in Cases of Plague contracted from direct Inoculation.

A compounder at Hubli was bitten on the thumb by a pneumonic patient and fell ill 3½ or 4 days after, 4567.
A Dome died 48 hours after assisting at a post mortem, 6921.
A Dome who assisted Major Gifford in the post mortem last-mentioned, died of plague; his relatives said he was taken ill 3 days after the post mortem, 6503–6.
A medical student who assisted at a post mortem, and made smear preparations, got plague 14 days after. Between the post mortem and his illness he worked regularly in the laboratory with plague material, the usual laboratory precautions against infection being taken, 6888, 6927–32.
A man who assisted at a plague post mortem on 3rd June is said to have injured his hand. Major Green was informed on 8th June that he was ill of plague, and had had fever for 2 days, 6894 (7220, D 28).
A student in the Jamsetjee Hospital was assisting in the post mortem of a case of plague, and he pricked himself in the left thumb, a very slight prick, which he did not notice at the time. About 3 days afterwards, he got a bubo in the corresponding left axilla and died of plague, 1411.
At the post mortem of first discovered case of plague in Calcutta, on 17th April 1898, at 8 a.m., Badri Dome scratched his hand. At noon on 19th he was found to have fever, and plague declared itself, 6462, 6493.

INCUBATION PERIOD OF PLAGUE—*cont.*

Dome, who assisted at post mortem, and did so on 27th and 29th April, and 1st May, on last date got a scratch; fever was noticed in the evening of the 2nd, and the man died of plague on the 3rd, 6923–5, 6988–95.
Dr. Sticker, of the German Plague Commission, had several times cut or otherwise wounded his left hand without any evil consequences at post mortems made during the second week in March 1898. On March 26, he performed three post mortems, and one at 3 p.m. on March 29th, without wounding himself. At 6 p.m. on the 29th, on drying his hand after a bath, he noticed over the metacarpo pharyngeal articulation of the right thumb a small, slightly painful, vesicle. This received a blow the same evening, and the arm at once became painful and plague developed, the bacilli of plague being recovered afterwards from the vesicle, 4562–9.
During post mortem on pneumonic corpse, a Hospital Assistant pricked a finger of his left hand at 3 p.m. on March 14. On the evening of the 16th he felt ill, and plague set in, 7218.
On October 7th 1897, an imported case of plague died at Poona; on Thursday Major Gifford made a post mortem; on Sunday Major Gifford felt unwell, on Monday he he had to go bed, and went through an attack of plague, 6496, 6502.
On 17th January 1898, Hospital Assistant got scratch on back of right hand at post mortem of pneumonic patient; plague manifested itself in 36 hours, fever setting in on afternoon of January 19th, 9393, 9566.
On 21st December 1897, about 5 p.m., Captain Leumann scratched himself at a post mortem. On 24th December he had a feeling of malaise all day; in the evening suffered from a sort of mental aberration; got fever, and an enlargement of glands in the armpit, on the same side as the scratch, and then went through a slight attack of plague, 4542–55.
Sputum was accidentally introduced into the eye of a nurse in Parel Hospital on the 15th February. On 18th feverishness set in, and next day the plague symptoms were marked, 7213–5.

Incubation Period in Cases that may be cited in connexion with the above:

A nurse at Poona who was attending on pneumonic patient died of pneumonic plague, as proved by bacterial examination of her sputum. She had not been long in attendance on the patient, perhaps 2 or 3 days, 7576.
A patient came to hospital and was there two days till he died. A friend came from an uninfected place to nurse him while in hospital. She fell ill of plague three days after the death. Other similar cases noted, 4316–9, 4323–5.
A phthisis patient in the police ward of the Civil Hospital, Karachi, who had been in since 12th January, got a sudden rise of temperature on 6th February and developed plague. A case admitted on 1st February probably infected him, that case having developed buboes and died on the 4th, 12,387.
Man got plague and was in hospital from 31st March to 9th April, when he died with secondary pneumonia. His brother went to hospital to nurse him, and developed plague on the 13th, 13,003–15.
Policeman A. died of plague of the pneumonic form in the lines on 18th April. On 26th April Policeman B. got a wound on his right foot at noon, was admitted to hospital that night, and developed plague next morning, 12,380.

Incubation Period in First Cases on Introduction of Plague:

A man went to Guntakal when it was infected. The day after his return home his daughter sickened of plague; she died the following day. The man was well until her death, but he died of plague shortly after. Infection

could have been communicated to the girl only through her father, 4481.
In Kelwa 5 days after the arrival of people from Bombay, who stayed 3 days, plague broke out among men, rats having died before, 23,207.
In Kersova a case was imported on 30th, proving fatal on 31st January. The schoolmaster who lived in the Talati's house visited the patient, and on 2nd February the Talati's nephew sickened, 23,207.
People from Bombay stopped 3 days in Ahmedabad; 3 days after they left plague occurred among the people with whom they had stayed, 15,202.
The first case in Shriguppi was a member of the family of a man who went to an infected place. This case was attacked about 4—not more—days after the man's return. The man himself suffered later, 2107–14.
The first imported case at Bangalore Civil and Military Station was taken ill on his return on 12th August after 3 or 4 days in Hubli, 2538.
Two servants belonging to Banniahs in Sharafi and Reri went to the Joria Bazar on 16th May and returned on the 19th; one got plague on the 21st in Reri and the other on the 23rd in Sharafi; 4 of their masters, probably infected by them, got plague on 24th May, 12,446.

Incubation Period in Plague Cases contracted by Direct Infection from Rats:

A listless rat was sound on the 5th March, and 8 days after a servant boy, who is said to have killed and burnt a rat, was attacked with plague, 17,996.
An imported case, Khandwa, developed plague 25th October, and died 29th. Dead rats found on 30th and 31st October. Boy, who was playing outside the verandah of the room where the man died, and must have been infected from the first case by the rats, attacked 2nd November, 5958.
A woman got a dead rat on her hand in taking out rice, and next day she got plague, 19,023.
Fitter in Karachi who played with a rat got plague three days after, 11,418.
Four cases in Customs' Lines after finding of dead rats and pending removal to huts, and no cases after, 11,976.
In Areh dead rats were found on 1st and 2nd October in 2 houses; on 3rd October plague broke out in both houses, and those adjoining, 22,223.
In Bhiwandi plague broke out in a house 7 days after dead rats were found there, 22,223.
In Jegapur 2 or 3 days after a man returned from Dharwar rats died, and 3 days after his daughter was attacked, 23,207.
New Kurla: Dead rats found 4th April; plague occurred 8th, 23,207.
On the fourth day after dead rats were found in the Karad Post Office the postmaster's daughter was attacked, 14,363.
On 19th November rats were found in a house; one old lady threw them away; other rats were found on the 24th; on the 29th the old lady got plague, 1180.
Pali: Dead rats found 6th or 7th January; plague occurred 13th, 23,210.
Santa Cruz: Dead rats found 12th February; plague occurred 15th, 23,207.

Incubation Period in Plague Cases contracted by Visits to Infected Places and Houses or contact with the Sick:

A compounder attacked with plague on May 4th was probably infected on the previous evening by a visit to a non-disinfected house where he went to get an article, 10,588–91.
A disinfected family that re-occupied a disinfected house had four cases of plague (including one death) within five days of re-occupation, 11,271.

After evacuation had stopped the plague in Pademal, people were, on 29th June, allowed to re-visit their houses for disinfection, and on 4th July cases began again, 22,737.
After the people of Trimbak in camp were perfectly free, 2 cases suddenly occurred; the patients had gone in the previous evening to an infected temple to get grain, 13,814.
Andumbar: Three cases of plague occurred the same day or the next day among people who had re-visited their houses for disinfection, 22,732.
An infected room was left vacant till 15th April. A man then went to live in it, and got plague on 18th, and later on, on 14th May, another man, who had used the place for cooking for 2 or 3 days only, got plague there, 11,266.
A man who assisted in carrying a plague corpse got plague 4 days after, 19,038.
A prostitute who about 23rd April re-occupied a disinfected house getting plague 4 days later, 11,276.
Chak Kalal: Series of cases on occupying infected houses, 10,109–10, 10,189, 10,603.
In Bada, after 17 days freedom, people were allowed in for 4 days on occurrence of rain; immediately they came out again there were 4 cases. Same happened in Salaya, 13,456.
In Hinganghat from 26th November to 1st December there was little plague, only one attack. On 2nd December plague started again with 6 cases. On the 28th people were prematurely allowed to re-visit their shops; they did so on 29th November, and this led to the cases of 2nd December, 6006–11, 6024.
In Joo people left their houses for some time on dead rats being found, but returned prematurely: three days after their return plague appeared among them, 23,216.
In 2 cases men from Kankhal who went to Jawalapur to see after the disinfection of their houses got plague in 3 or 4 days after, 9226.
Mahlgahla: Cases after occupying infected houses, 10,348.
On 20th April a disinfected and vacated house was re-occupied; plague occurred there again on 24th, 11,276.
Poona: European editor of paper went out with a search party the first day such parties went out. He lived in an uninfected part, and was not otherwise in contact with plague. The same day he developed the disease, 12,883–8.
Succession of cases connected with the Backergunge epidemic, 6835–9, 7350–94, 7409–13.

Opinions regarding the Incubation Period:

About a week, 3375–6.
About 8 days, 13,016.
Appears to be from 2 to 10 days, 45.
Eight to 10 days, 1899.
Five to 10 days, 4457.
From 3 to 5 days. Longer periods mostly mild cases, 25,485.
Incubation period from 2 to 4 days, 8640.
Majority of cases in camp developed in 48 hours after admission, 17,368.
May extend to 15 days, 3139.
Most difficult to ascertain period of incubation when the infection is through clothing, 8640, 13,023.
Ordinarily from 24 hours to a week, 3375–6.
Over 10 days, 1903.
Period of incubation from less than one day to 10 days, 10,407–12.
Period certainly up to 10 days, 10,196.
Period in some cases under 2 days, 1198.
Some cases have incubation period over 10 days, 12,505.
Ten days a liberal allowance, probably shorter, 1619, 1796, 2833.
Usual period 4 or 5, extending to 10 days, 226.
Varies from 1 to 8 days, I. 351.
Varies from 36 hours to 10 days, I. 399.

INCUBATION PERIOD OF PLAGUE—*cont.*

Plague Cases of exceptionally long alleged Incubation Periods:

Cases in which incubation period was beyond 10 days, II. 184.
Dharwar: Contacts got plague up to 18 days, 1902.
Details of cases in plague hospital, Baroda, in which the period of incubation appeared to be more than 10 days, II. 285–6.
On 3.12.96, a Parsee child arrived in Poona from the Fort, Bombay, where there was plague. She got plague 14 days after, on 17th December. There were suspicious cases in Poona City, but the child had not access to the city, 12,889–92; 21,658–63.
Statement of a number of cases that occurred more than 10 days after removal to segregation, as proof of a period of more than 10 days but these cases may have been due to a surreptitious visit to an infected house, 22,330, 22,414–22, 22,456–80.
Statement of 7 cases said to have a period of over 10 days; the histories are not detailed enough to show exact source of infection and exact length of period, 15,293.
Syce who came from Bombay to Baroda 5 weeks before was attacked by plague, 15,187.
Ten days after wearing a pair of shoes that gave her a sore on her heel the day she wore them, a Parsee lady developed plague. She lived in a non-infected quarter, 20,658.
Woman develops plague on 10th day at Bandra, 8240.

Plague Cases in Segregation Camps and Hospitals showing the Number of Days after Admission on which they developed:

Anand, 14,182, 1291.
Anand detention camp, one case on the 11th day, 14,138.
Ankleshwar, 14,215.
Bangalore Civil and Military Station, 25,030.
Bhusawal, 1,291.
Bombay (Wari Bundar Camp), 301, 17,367, 20,442.
Dharwar, 1,926.
Gadag, 23,693.
In Kalankot and Gharibabad, evacuation stopped plague 8 days and 2 days after evacuation, 11,145–6, 11,163–6.
Poona: 1st and 2nd epidemics, 21,835.
Poona: 3rd epidemic (to March), 25,827.
Porbandar, 13,722,
Porbandar: Dates of developing plague after arrival in observation camps, ordinary period not more than 9 days, 13,662, 13,744–7.
When two members of a family were attacked the other members were segregated; one of the persons segregated developed plague 9 days after segregation, 4320.

See Inoculation with Haffkine's plague prophylactic: *Effect on incubating plague.*

INFECTED PLACES; *Miscellaneous particulars regarding:*

Ahmedabad: Bombay, plague introduced from into, 13,855.
Ahmedabad: First plague cases among the mill hands occupying a dehla outside the Sarangpur gate, 13,918.
Ahmedabad: Table showing number of plague cases, imported and indigenous, from October 1896 to April 1898, II. 228.
Ahmednagar Cantonment: First case in September 1897, 24,136.
Ahmednagar City: First cases in, October 1897, evacuated November 1897, 2, 24,237.
Ahmednagar City: Four hundred and seven cases and 315 deaths from October 1897 to March 1898, 24,238–9.
Ahmednagar City: Plague travelled in a line from house to house and ward to ward, 24,234.
Alibag: Latest outbreak among Brahmans, former among the Kolis, 17,621.
Ankleshwar; Population, 10,700: First cases among the Ganchis in April 1898, 14,205–6.
Ankleshwar: Table showing monthly mortality from April to November for the years 1896, 1897, and 1898, II. 247*a*.

INFECTED PLACES—*cont.*

Ankleshwar: Table showing weekly attacks and deaths from plague in 1898, 14,214.
Ankleshwar Taluka: Table of weekly attacks and deaths in the villages during the year 1898, II. 239.
Backergunge: Epidemic at, 6836–43, 7331–475.
Backergunge: Plague bacillus not found in sputum of pneumonic patient at, as examination could not be made in time, 7471–4.
Bantwa: Population 8,500; Epidemics August and September 1898, deaths 602 in 944 cases, 17,549–55.
Baroda Cantonments: Table of deaths from causes other than plague, from August to December, 1898, II. 289.
Baroda City: First outbreak began April 1897, second began October 1897 and ended April 1898, 14,584, 14,596, 14,628.
Baroda City: Population 112,500, 14,582.
Baroda City: Table of mortality from plague and other causes, January 1897 to January 1899, 14,983.
Baroda City: Influx of people to Baroda from infected places, especially Surat, 14,599.
Baroda City: Table of gross mortality three months before outbreak, II. 274.
Baroda City: Table of weekly gross mortality from plague and all other causes, II. 274.
Baroda City: Table showing progress of plague from January 2nd to April 9th, 1898, II. 287.
Baroda City: Table showing weekly number of plague cases from October 1897 to January 1899, II. 253.
Baroda City: Table showing weekly returns of mortality from October 1897 to January 1899, II. 252.
Baroda State: Dhamdachha: first epidemic among the Kolis and Pattidars; the second epidemic began among the Kolis, 15,120.
Baroda State: Ratio of number of attacks to population in the several divisions, II. 283.
Baroda State: Table showing ratio per 1,000 inhabitants of plague deaths in different villages, II. 282.
Baroda State villages: Statistics regarding the plague outbreaks and inoculations, 14,793–4, 14,834–981, II. 496, 502, II. 503–21.
Belgaum: Account of the introduction of plague in Belgaum, 23,028, 23,031.
Belgaum: First epidemic October 1897 to June 1898, 23,124.
Belgaum: Plague began among the Muhammadan Kolis, 1715.
Belgaum: Population about 28,000; 23,122.
Belgaum: Second epidemic began July 1898, 23,125.
Belgaum: Sweepers first infected in Belgaum, 2086.
Belgaum: Table showing number of plague cases from October 1897 to end of January 1899, III. 210.
Bhiwandi: Plague epidemic from May to September 1898, 22,111–3.
Bhiwandi: Population 15,000, 22,108.
Bhiwandi: Table showing attacks and deaths from plague, 14,482.
Bombay Presidency: Deaths from plague, 61,915 from May 27 to November 25, 1898, 3.
Bombay Presidency: Deaths from plague to May 1897, 28,737; to May 1898, 61,219; and in 1898–9, to 3rd March, 94,000, 26,536–7.
Bombay Presidency: One-half of the plague mortality in Bombay Presidency occurred in the districts of Dharwar and Belgaum, 26,537.
Bombay Presidency: 94,000 deaths from plague in the Presidency during the first three epidemics, 26,536.
Broach: Infected in September 1898, arrangements made, 15,484.
Broach: Lower classes and Banniahs suffered most, 15,470.
Bulsar: First epidemic in early part (February) of 1897, 19,940.
Bulsar: Plague probably did not spread from Dhebras to Tais in Bulsar, 16,179.
Bulsar: Second epidemic began in April 1898, 16,159.
Cutch Mandvi: Account of the outbreak at Rawapur in 1897, 13,364.

INFECTED PLACES—*cont.*

Cutch Mandvi: First cases, probably about February 1897; most virulent, 455.
Cutch Mandvi: First epidemic, 4,298 cases, with 4,224 deaths, 13,384–5.
Cutch Mandvi: First imported cases in October 1896, 13,358.
Cutch Mandvi: History of cases among the Megwals, 13,408–10.
Cutch Mandvi: History of former epidemics, 13,356.
Cutch Mandvi: Khatris suffered most during first outbreak at Mandvi, 13,417.
Cutch Mandvi: No cases among the Waghris, who live in grass huts outside Mandvi, 13,480.
Cutch Mandvi: Not visited by plague since 1817 until now, 13,351.
Cutch-Mandvi: Particulars of deaths from causes other than plague from February 20th to March 9th, 1898, II. 218–9.
Cutch Mandvi: Report of an outbreak of disease in Cutch Mandvi, II. 405–8.
Cutch Mandvi: Salaya: Two hundred and thirty-three cases and 176 deaths during the second epidemic, 13,433.
Cutch Mandvi: Second epidemic from March 1898 to January 1899, 992 cases and 748 deaths, 13,412.
Cutch Mandvi: Second epidemic severe from March to end of May 1898, with 288 cases and 202 deaths, 13,411.
Cutch Mandvi: Severe epidemic of fever in 1878, 13,357.
Cutch Mandvi: The town being a mile square contained 40,000 people before the outbreak, 540.
Dharwar district: Attacks and deaths, I. 72.
Dharwar district: Plague introduced into the villages from Hubli, 23,499.
Dharwar district: Statement of attacks and deaths, I. 72.
Dharwar district: Statement of the course of the epidemic in Byahatti, 2192.
Dharwar district: Statistics of very severe plague in certain villages in which measures could not be properly carried out, 1601, 1719, I. 92–3, 23,518.
Dharwar Jail: Plague in, 2227–303.
Dharwar Town: Measures applied to plague infected houses, 1610, 1943–5, 2328–37, 23,747.
Dharwar Town: Plague began among the Muhammadan Kolis, 1715.
Dharwar Town: Population 32,000 at census, 38,325 in August 1898, 1571, I. 67.
Dharwar Town: Statistics of attacks, deaths, and inoculations from middle October 1898 to middle December 1898, 1608, 25.151.
Guntakal: Course of infection around, 5039–47.
Hinganghat: Daily mortality, 6096.
Hinganghat: In spite of precautions taken, plague imported, and escaped detection till November 19, 1898, when it had been some time in the town, 5948–59, 5977, 5980, 6001, 6083–8, 6,200, 6289, 6395–9.
Hinganghat: Plague mortality, 6197.
Hinganghat: Suggestion that plague was introduced by two mill hands from Bombay who came to the town, but did not themselves develop plague, 6200, 6240–7, 6293.
Hinganghat: Table of death-rate, 6,200.
Hyderabad (Sind): Population 65,000; slight increase by arrivals from Karachi before plague broke out; no great decrease after plague broke out, 18,114–7.
Hyderabad State: Plague imported from Sholapur, and first cases in January 1898; measures adopted, 5,153.
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Ilkal: Table of plague cases and deaths, III. 90.
Kaira district: No direct evidence to show how plague was introduced in the, 14,025.
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Madras Presidency: Infection first appeared about August 1898 in the Anantapur district, 3882–93, 4101–19.
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Panjab: Measures taken on appearance of plague, 9934.
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Poona Cantonment: Two hundred and ninety-eight deaths in 1897 epidemic, 21,316.
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Poona City: First epidemic December 1896 to May 1897, 21,674, 21,679.
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Rohri; One hundred and fifty-one cases and 92 deaths, 12,043–4.
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Sukkur: First case in February 1897, 3723.
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,, Calcutta.
,, Daman.
,, Hubli.
,, Karachi.
,, Kirki.
,, Lanauli.
,, North-West Provinces.
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,, Floors.
,, Recurrence of plague in infected towns and villages.
,, Recurrence of plague in the same house or locality.

INFECTION, MODES OF:

By Lesion of the Skin:

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Alimentary canal: Produces abdominal form of plague, very rare, 20,388, 22,325.
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Lungs: Infection chiefly through inhalation, 22,360.
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Sexual intercourse: Cases probably due to, 21,293, 22,264.
Stomach: No post-mortem evidence that infection contracted through stomach, I. 368, I. 375.
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See Boots.
,, Carbuncles.
,, Floors.
,, Food.
,, Infection by locality.
,, Merchandise.
,, Pneumonic plague.
,, Rats.
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INFECTIOUSNESS OF PLAGUE:

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Among monkeys: did not spread among monkeys confined in open cages at Hardwar, 8739–45.
At Jamalpur plague mostly communicated from person to person in families, 9165.
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Course of cases from person to person in Calcutta, 7509.
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From person to person infection rare, 18,146, 18,789.
Infection not often communicated by touching plague patients, 22,554.
Infection in Kotri seemed to spread by contact of persons not from houses, 12,650.
Infection carried in Cutch chiefly by people, personal infection, 13,462.

INFECTIOUSNESS OF PLAGUE—*cont.*

Not infectious in the early stages, 1913.
Not a very infective disease, 26,714–7.
Not very infectious or contagious, 12,876.
People observed hugging and kissing plague patients without getting infected, 25,229–31.
Plague cases in camps and hospitals do not spread infection to any important extent, 10,374.
Spread of plague from a single case not noted in Bombay, 1269–71.
Very little infection from person to person, 17,722–3, 21,500, 21,538, 21,569.

Instances in which Infection has been severe in individual Families or Houses:

In Calcutta in a family of 15, 8 succumbed to plague, and in another case 11 persons died in one hut, II. 537.
In Hubli 9 persons out of a family of 13 died before the existence of plague among them became known to the authorities, 23,383.
In one family eight deaths occurred and in one compound 13 deaths out of 18 people, 3698.
In one house in Karachi no less than 21 people among 35 to 40 inmates caught the plague from one another, 12,333–4, 12,479–80.
Instance of a family of 13 members (Mr. Shelke's) in which infection spread and 9 uninoculated members all died, 1725.
One case in Kajurgi, Hyderabad State, introduced the infection and started an outbreak, 9 or 11 persons dying in one family, 5498–506.
Three persons out of five infected in a week in a small room, 18,321–3.
When ventilation openings closed, 20 cases at once occurred in a fairly sanitary chawl, which, though in the middle of plague, had before remained free, 22,382.

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INFORMERS:

Cutch Mandvi: Poor people giving information of cases paid small reward, 13,436.
Hyderabad State: Reward of 10 rupees offered for information of an infected village. System worked well, 5439–42.
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Karachi: Rewards paid for the reporting of plague cases, 13,436.
Panjab: Good results of the system of rewards for information of newly infected villages, 9934–6.
Panjab: 50 rupees paid to informer who first gave notice of newly infected village. System worked well, 9934–6, 10,826.
Panjab: Banga, system of reward for notification of plague very successful in, 10,249.
Panjab: Banga, people under the belief that the first case was imported for the sake of the reward, 10,235.
Sukkur: Small rewards paid to informers, 18,385–6.
Surat: System of rewards to private persons, Sanitary Inspectors and police for information, 15,575–9.

See Spies.

INFORMATION OF OCCURRENCE OF PLAGUE.

See Concealment of plague cases.
,, Corpse inspection.
,, Discovery of plague cases.
,, House searches.
,, Informers.
,, Notification of plague.
,, Registration of deaths.
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Confers special local immunity as well as general, 194.
Doses of virus necessary to produce the initial reaction must be increased in order to produce the same reaction subsequently, 27,376.
Immunity conferred by inoculation with plague cultures, living and dead, 27,355–7.

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Immunity—Local and general immunity in animals produced by repeated inoculations of plague virus, 115–7.
Post-mortem appearances the same in animals partially protected by inoculation with plague virus and those dying without previous inoculation, 27,363.
Sterilized cultures of plague bacilli have not given the satisfactory results obtained with microbes of cholera and typhoid, 27,358.

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Method of testing for the presence of plague bacilli by inoculation of suspected substances into rats, an exceedingly sensitive method, 153.
Method used by Dr. Gibson, 619-20.
Not a good method, and gives doubtful results, 8522–4, 8588–9.

Test of Virulence of Plague by:

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See Experiments on animals.
„ Inoculation with Haffkine's prophylactic.

INOCULATION WITH CURATIVE SERUM:

See Haffkine's curative serum.
„ Lustig's curative serum.
„ Yersin's curative serum.
„ Serum.

INOCULATION WITH HAFFKINE'S FLUID.

Arranged under the following subheads:

Adoption of Inoculation as a general Plague Measure.
After Effects of Inoculation with Haffkine's Fluid.
Age and Inoculation.
Animals, Experiments on.
Attitude of People towards Inoculation.
Bars to Inoculation with Haffkine's Fluid.
Caste, Religion, and Race; Inoculation with reference to.
Certificates of Inoculation.
Dosage.
Duration of Protection afforded by Inoculation.
Effects of Inoculation with Haffkine's Fluid—
Attacks; on Plague—
Clinical Symptons of Plague; on—
Incubating Plague; on—
Mortality: on Plague—
Other Diseases; on—
Spread of Plague; on—
Extensive Application of Inoculation: when resorted to and General Effects of.
History of Inoculation in particular places.
Identification of Bodies of Inoculated Persons.
Inducements to Inoculation with Haffkine's Fluid.
Investigation Sheets.
Method of performing Inoculations.
Period for which Protective Effect exerted.
Persons recovered from Plague; Inoculation of.
Reaction after Inoculation of Haffkine's Fluid.
Re-inoculation.
Registration of Inoculated Persons and Statistics regarding Inoculation.
Standardisation of Dose.
Strength of Haffkine's Fluid.

See also Haffkine's Plague Prophylactic, *arranged under the following subheads:*

Character and preparation of Haffkine's fluid.
Constituents of Haffkine's fluid.
Re-sterilisation of Haffkine's fluid.
Sterility of Haffkine's fluid.

Adoption of Inoculation as a general Plague Measure.

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Can never prepare a district against invasion of plague, 3872.
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Value in preventing interruption of trade, 1956, 23,680.
Prevents panic and the scattering of the people, 3482, 18,430, 24,977–9.
Bose, Dr., formerly an opponent, converted by results, 7542.
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Lawrie, Col., thinks in no way to be recommended, 5161.

After Effects of Inoculation with Haffkine's Fluid:

Differ in different persons, 4748, 10,711, 10,799–802.

Abscesses:

Occur occasionally at point of, 4743, 4781, 11,705, 16,568.
Due to defect in antiseptic precautions and neglect, 1753, 4781, 10,714.
Ten in 2,500 cases, 1779; 2 abscesses, 1806; 8 in 27,000 cases, 1836; 12 in 75,000 cases, 3034.
None noticed, 664, 14,519, 16,317.

Alcoholism: One death at Bangalore after inoculation due to, 4636.

Apoplexy: Death 1½ months after due to, 14,819.

Boils: Attributed by population to, I. 477.

Buboes:

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Have occurred, not near point of, 16,327.

Cellulitis: Of whole arm form seat of inoculation to wrist, 4853.

Chorea and Tetanus: In a child follow, 4774.

Collapse: In a girl follows, 11,707.

Consumption: Attributed to, 201, I. 340.

Deaths:

Apparently due to inoculation fever, 4605, 4973, 2388.
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Death shortly after, in inoculated mostly occur in first few days after inoculation, 3185
Plague bacillus found in 41 cases which died shortly after inoculation at Hubli, 3074.

Debility: General, caused by, 14,813–5.

Delirium follows: 2762, 6381–2, 16,592.

Depression: Great, caused by, 20,458.

Diarrhœa:

Followed, 4742.
Did not follow, 16,322.

Endocarditis: Death from, ascribed to, 20,458.

Erysipelas:

No case known to M. Haffkine, due to, 204.
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INOCULATION WITH HAFFKINE'S FLUID—*cont.*

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High fever, cases of, 5160, 6387–8, 23,705.
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Two cases die of simple fever, 5123.

Gastric Irritability: caused by, 20,458.

Headache: Caused by, 14,519.

Hydro-nephrosis: Woman suffering from, died after, 4763.

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Leprosy: Alleged, 19,314.

Leucoderma: Alleged, 19,314.

Malaria: Followed, 6361.

Neuralgic Pains:
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Follow, 14,813.

Pains in Joints and Muscles: Follow on, 23,707.

Permanent evil Results: None, 202, 705, 1481, 2759, 4635, 10,154, 10,645, 10,698, 14,518, 16,278, 16,322, 19,051.

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Severe Symptoms from:
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In Europeans, 5124–6.
In the case of Colonel McConaghy and an Assistant Surgeon, 12,944–59.
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Surgeon-General Harvey's case, 5160.

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Tetanus and Chorea: In a child followed, 4774.

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Effect of inoculation on children, I. 400.
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Baroda: Inoculations encouraged at Undhera and elsewhere; number of inoculated persons in the State, II. 517 (R).
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Dharwar district: Fees paid for inoculation, 170.
Dharwar: 18,000 people left in one week for fear of inoculation, 23,746.
Dharwar: People get inoculated to secure certificate and privileges, 1963.
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Gadag: General rush for inoculation to avoid segregation, 23,680.
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Guntakal: Fear of inoculation because of sudden death of a man after the operation, 5068.
Hinganghat: People inoculated merely to secure privileges, 6046.
Hubli: Fee paid by the people for the benefit of inoculation, 23,871.
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Jawalapur: No inoculation done; strenuously objected to, 9211.
Madras City: In spite of official encouragement very few people got inoculated when outbreak of plague was threatened, 3872.
Madras Presidency: Adopted before appearance of plague, 170.
Mysore City: Inoculations were pressed; about one-seventh of the population inoculated before plague broke out, 25,034.
Native States averse to inoculation or any treatment at all, 9803–4.
Panjab: One village fully inoculated before plague broke out, owing to influence of an interested leading man, 10,723–8.
Panjab: People at Banga inoculated on advice and example of their leaders, 10,172.
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Satara: People at first averse from but influenced by their doctors or officials with regard to inoculation, 22,391–400.
Satara: Strong objection against inoculation, 22,155.
Surat: Inoculation mostly of Bombay refugees who were anxious to return, 15,879.
Unpopular at first, much less unpopular later, 922, 3163.

BARS TO INOCULATION WITH HAFFKINE'S FLUID.
Anæmia, persons refused who suffer from, 16,588.
Diabetes, phthisis, old age, and marked plague debar from, 1759–62.
Fever or marked plague disqualifies for inoculation, 3068.
Fever; no person with temperature of 100 degrees should be inoculated, I. 400.
Infirmity and Bright's disease bars to inoculation, 1790–1.
Malarial fevers, refused to persons with, 16,588.
None, except infants, who applied rejected at Karachi, 11,737.
Pregnancy, weak health, or certain diseases as diabetes, form a bar to inoculation, 1689–95.

INOCULATION WITH HAFFKINE'S FLUID—*cont.*

Sickly and weak persons and pregnant women not inoculated, 16,273.
Sickly infants, pregnant women, and people with fever not inoculated, 1812–4.
Weak persons dissuaded from, 12,967.

See Age and inoculation.
,, After effects of inoculation: *Pregnant women.*

CASTE, RELIGION, AND RACE, INOCULATIONS WITH REFERENCE TO:

Baroda: Table of inoculated and uninoculated according to castes and social position, II. 276.
Bajwa: Table of inoculated and uninoculated according to castes, II. 72.
Calcutta: Caste and sex and social position of inoculated, 6557.
Dhamdacha: Numbers of inoculated and uninoculated per castes, II. 268.
Europeans do not, as a rule, apply for inoculation, 1815.
Hindus persuaded to take it, 1363, 1366.
Jains, on account of their objection to anything which involves death of animals, are difficult, 7892.
Karachi: Weekly number of inoculations according to caste, II. 376.
Koili: Table of inoculated and uninoculated in the different castes, II. 270.
Low Class Cultivators: Inoculation applied for at Ankleshwar principally, 14,314.
Muhammadans offered themselves first for inoculation in Hubli, 3052.
Muhammadan soldiers at first object, 2819.
Muhammadans readily come at Calcutta, I. 476.
Muhammadans slow to take it up, 4016.
Muhammadans: Among those inoculated at Bangalore only 2163, 3482.
Muhammadans: Inoculation not resorted to at first, later more frequently resorted to in Bangalore, by 2636.
Muhammadans: Come forward to be inoculated in Calcutta, I. 476.
Muhammadans: Favour inoculation in Broach, 15,511.
Muhammadans: Set example of being inoculated in Calcutta, 6643.
Parsee gentlemen assisted in inoculation operations in Daman, I. 306.
Parsees favour inoculation in Broach, 15,511.
Parsees inoculated at Broach, II., App. LIII.
Undhera: Table of inoculated and uninoculated according to castes, II. 266.

CERTIFICATES OF INOCULATION:

Are occasionally transferred from one person to another, 25,504.
Certificates of inoculation liable to be taken from dead by relatives, 2480, 2482–7.
Certificates of single inoculation likely to be used by persons other than those to whom granted, to secure advantages of double inoculation, but not successfully, 23,872–4.
Certificates rarely sold, 2513.
Certificates very rarely transferred, 3089, I. 408.
Occasionally transferred, but nothing of the kind of a regular traffic in certificates existent, 1966–7.
Occasionally transferred, but not likely that a person should get inoculated in order to give his certificate away, 3089.
Prosecutions stop transference of certificates, 1965.
Refusal of relatives to return certificates of dead patients, 2647, 2714–5.
Thumb-marking for identification difficult in practice and an unnecessary precaution, 1965.
Transfers of certificates would, without doubt, take place if the transferor could make anything by it, 2676.
Transfers suspected in Mysore City in rare instances only, 25,040.
Where the register is kept properly there is little room for personation; it may only come in afterwards by the transfer of certificates, 1964.

DOSAGE.

Amount injected, 32–34, 72, 141, 180, 185, 1832, 10,705–6.
Average dose fixed at 2½ c.c. 33, 4002, 4577.
Children stand a larger dose comparatively, 1748, 3009, I. 400.

INOCULATION WITH HAFFKINE'S FLUID—*cont.*

Children stand proportionately much larger doses, 23,819.
Dose one-fifth more than standard dose given by Miss Corthorn, 23,812.
Experiments for obtaining a permanent immunisation by a succession of doses rapidly increasing from the minimum dose, 19,287.
Fighting weight the most important factor in standardising the dose, 3010.
Half dose used at Belgaum, 5627.
Larger dose given in cases where patient had not reacted on the first, 1751.
Uniform doses very important, 3156–7.
Uniform, dose should be more. Would save much trouble, 4064, 7889.
Use of a dose definitely known, the most important point in every vaccination, 26,449.
Women bear quite as strong doses as men, 23,813–5.

DURATION OF PROTECTION AFFORDED BY INOCULATION.

Details of cases to show that period of immunity is uncertain, II. 282.
Duration of protection probably at least 6 months, 1938.
Effects upon people living in good houses and fresh air last about 2 years, III. 43.
Investigations into the length of protection afforded by the prophylactic rendered impossible by the eagerness of the people to be inoculated over and over again, 26,436.
Newspaper statement that immunity from inoculation wears off in about 5 months, 11,760.
Period of immunity very uncertain, 15,438.
Period of immunity 6 months in the majority of cases, 23,723.
Table of plague cases among inoculated in Baroda cantonments, II. 289.
Table showing date of inoculation and of attack in Baroda, II. 282.
Table showing percentage of mortality from plague at Bulsar after inoculation had been introduced, II. 319.
Two cases admitted to Poona Hospital of bubonic plague 8 and 9 months after inoculation, 18,896.

EFFECTS OF INOCULATION WITH HAFFKINE'S FLUID:

Attacks, on Plague:

Alibag: Instances of good effects in individual cases in, 25,879.
Baroda: Table showing date of inoculation and of attacks, II. 282.
Belgaum: List of persons inoculated and list of attacks and deaths among them, III. 211–2.
Belgaum: Two cases after inoculation in the 26th Madras Infantry, 25,932.
Billimora: List of plague cases inoculated, with particulars, II. 262.
Billimora: Percentage of attacks on the total population higher among the inoculated than the uninoculated, 14,835.
Bulsar: Number of attacks among the inoculated and uninoculated about the same, but the mortality greater among the uninoculated, 19,989–92.
Cases to show the effects of the prophylactic in actually preventing plague, 19,066.
Child, the only uninoculated person in a compound, attacked by plague at Belgaum, and dies, 4021.
Daman: Persons attacked in 1897, recovered, inoculated and attacked a second time a month later, 16,554.
Did not check the disease among the Tais at Bulsar, 16,280.
Disinfecting coolies and staff inoculated with good results, 10,006–8, 10,681–6, 10,690–4, 11,769, 22,668.
Family of 13, 4 inoculated, the other 9 died of plague, 1752.
Good results of inoculation among coolies on disinfecting gangs in Karachi, 22,618.
Good results of inoculation in chawls on the Telang property, Bombay, 17,878.
Great safeguard, 18,230.
Increases resisting power, 1788.
Karachi: List of all inoculated persons who developed plague, II. 395.
Karachi: Only one case among 900 inoculated, some of them contacts, 22,671.

NOCULATION WITH HAFFKINE'S FLUID—*cont.*

Low figure of attacks and mortality in inoculated, 15,424.
No attacks among inoculated at Calcutta, 6557.
No case in 1,500 inoculated near Bellary, 4466–7.
No deaths from plague among the Barodias, Bhois, Kachhias, and Mahrattas at Billimora, 14,857.
Not a single attack among 200 inoculated kept under observation for five months, 18,519.
Of 2 men, working in a disinfecting gang under the same conditions, the uninoculated died of plague, the other, who was inoculated and nursed the patient, remained well, 10,681.
One person only out of 500 inoculated developed plague symptoms, 17,907.
Only uninoculated person in family of sweepers gets plague, 2776.
Percentage of recoveries, inoculated and uninoculated, in pneumonic plague, 2382.
Plague cases among inoculated 6 days after injection, 15,421.
Poona: No cases of death from plague among the inoculated, 12,971.
Preventive against plague, 10,866.
Protective remedy, 1059–60.
Protects those in contact with plague cases, I. 399.
Report of cases of plague among disinfecting gang inoculated with Haffkine's fluid, 10,006.
Summary of observations on 2 groups of inoculated and uninoculated persons, 23,809.
Two tables showing particulars of cases after inoculation, II. 153.
Table of plague cases among inoculated in Baroda cantonments, II. 289.
Table showing percentage of mortality from plague at Bulsar, after inoculation had been introduced, II. 319.
There is no such thing as complete immunity, 27,375.
Three cases in which plague appeared 10 days after, 14,941.

Clinical Symptoms of Plague, On:

Cases among the inoculated of a milder type, 1843.
Cases of inoculated so mild as to be almost unrecognisable, 69.
Clinical history of 3 inoculated plague cases. Two recover from very mild attacks, 7236.
Delirium and aphasia very rare in cases among inoculated, 14,977.
If plague follows, symptoms among inoculated are of much milder type, 1869, 23,696.
Inoculation seems greatly to modify the symptom of subsequent plague, 69.
Plague among inoculated milder than in uninoculated, 14,997.
Plague among the inoculated at Bulsar was of an attenuated type, 16,307.
Plague cases milder in inoculated, 75, 1869, 2892.
Pneumonic plague little influenced by inoculation, 2382.
Temperature not high and less exhaustion in inoculated cases, 1843.
Two cases describing course of disease in inoculated, I. 383.

Incubating Plague, On:

Cannot arrest symptoms developed very soon after inoculation, 44.
Favourable effect on incubating plague, 1772.
May influence for good course of disease when injected during incubation, if infection has taken place 3 or 4 days previously, 45.
Powerless to arrest incubating plague, 36.
Probably harmful in plague cases of short incubation, I. 400.

Mortality, On Plague:

Apparently saved a number of people from dying, but did not prevent plague from spreading, 24,447.
Bangalore: Relative mortality of inoculated and uninoculated patients in hospitals, 2917–8.
Bulsar: Number of attacks among the inoculated and uninoculated about the same, but the mortality greater among the uninoculated, 19,989–92.
Keeps down the mortality of plague, 16,282.
Miss Corthorn's table of plague death-rate among inoculated and uninoculated at Hubli, I. 76–7.

INOCULATION WITH HAFFKINE'S FLUID—*cont.*

Multiple buboes: In Baroda 60 per cent. of multiple bubo cases recovered among the inoculated, 14,997.
Rate of mortality reduced, 15,426.
Reduces mortality from 80 to 90 per cent., 78.
Sixteen out of 18 cases recover in Bombay after inoculation, 49.

Other Diseases, On:

Appears to protect from disease generally, I. 338, I. 400.
Asthma relieved, 4872, 17,963, 19,303–5.
Cases to show the beneficial effect on the general health, 19,055.
Dyspepsia, improves appetite, 4870.
Eczema relieved, but lupus and tuberculosis aggravated, I. 400.
Effects of inoculation on other diseases, 198–9, 201, I. 340, III. 43.
Fever, chronic, relieved, 17,916, 17,963.
General health much improved after inoculation, 25,443–5.
Gout milder and at longer intervals after inoculation, 25,459.
Headache, chronic, cured, 17,809.
Hemicrania, relieved, 16,631.
Instance to show its effect on other disorders, 17,912–5.
Leprosy, anæsthetic, marked improvement, 14,827.
Lumbago relieved, 199, 17,916.
Malarial fever, good effect on quartan, 199, 14,824.
May have general prophylactic effect against other diseases, 198–9.
Neuralgic pains, dissipated, 201, 14,824.
Old complaints revived, 201, 16,627.
Parametritis, revived by, 16,628.
Polyuria, good effect on, 14,826.
Rheumatism revived, 201, 16,627.
Ringworm disappeared, 14,827.
Statistic evidence does not prove that the plague prophylactic confers immunity against other diseases, 26,441.
Venereal diseases benefited, 22,402.

On Spread of Plague:

Alleged spread of plague in Daman owing to the use of Haffkine's fluid, 18,698–707.
Alleged spread of plague in Dharwar owing to use of Haffkine's fluid, 18,684–98.
Alleged spread of plague in Undhera owing to use of Haffkine's fluid, 18,707–13.
Infection carried through an inoculated person who himself remained free, 10,753.
Inoculated persons are not a source of danger to the uninoculated, 14,977.
Inoculated persons do not spread the disease, I. 408.
Not prejudicial to the uninoculated, 24,112.
Produces an increase in mortality among the uninoculated, 18,703.
Table in support of allegation that increased mortality at Daman followed inoculations, III. 65.

EXTENSIVE APPLICATION, WHEN RESORTED TO, AND GENERAL RESULTS OF:

At first applied only where epidemic was severe, 165.
Cannot be done quickly enough to stop epidemic, 7990.
Cannot be carried out on same scale as vaccination, 7982.
Five hundred inoculations a day a fair average, 4079.
Produces good results mainly through the feeling of safety it provides, 23,680.
Inoculation in Karad and Belgaum commenced when plague was practically over, 21,121, 25,930.
Not general until plague becomes epidemic, 88.
People refrain until panic-stricken, 4476.
Prevents disturbance of trade, 1956.
Useful as a means of allaying the dread of plague, 16,096.

HISTORY OF INOCULATION, IN:

Alibag: Inoculations in the beginning of 1899, with particular instances of its protective effects in individual houses, 25,878–91.
Ankleshwar: No positive results as to the value of inoculation at, plague dying out when commenced, 14,250.

INOCULATION WITH HAFFKINE'S FLUID—*cont.*

Ankleshwar: Of 645 inoculated persons in, 4 died from plague, II. 242.
Ankleshwar: Statement of inoculations with Haffkine's fluid at, II. 240.
Baroda Cantonments: Plague occurrences among the inoculated and the uninoculated, 15,426.
Baroda Cantonments: Native troops refused inoculation, 15,435.
Baroda Cantonments: Register of persons living in cantonment maintained: total population 4,000: two thousand and forty-eight were inoculated: the balance reckoned as uninoculated, 15,401, 15,406, 15,475.
Baroda City: Figures relating to inoculation in, 14,986–8.
Baroda State: Evidence and report on inoculations, II. 261–78, II. 503–21.
Belgaum: Eleven thousand persons inoculated in I. 202–3.
Belgaum: Inoculations began in December 1897, 3,447 persons inoculated in the district, results of inoculation, 23,131–56.
Belgaum: Major Forman's evidence regarding inoculations among his servants, 23,787–9, 23,102–9.
Belgaum: Major Forman's evidence regarding inoculations among hospital servants, 23,797–9, 23,809.
Belgaum: Number of plague casualties among inoculated not ascertained by inquiries at the time but by reference to registers which were very defective, 22,929–36.
Belgaum: Registers very inaccurate; instances of omissions, 23,079–102.
Belgaum: Two thousand five hundred and sixty-five persons inoculated in the city, 23,133.
Belgaum: 26th Madras Infantry: Plague occurred on 12th November 1897, and inoculations commenced on 23rd December, when outbreak near its close: only two inoculated attacked, 25,927–33.
Belgaum: 26th Madras Infantry: Second outbreak: On reappearance of plague the regiment re-inoculated at the men's request between 18th and end of August, only men away being uninoculated, 25,940–2.
Belgaum: 26th Madras Infantry: Second outbreak: Lieutenant Lethbridge with the regiment from 5th September to 15th October 1898, and inoculated everyone in it, 5617–21.
Belgaum: 26th Madras Infantry: Second outbreak: Re-inoculations began 10th September, and were done in 10 days, 5625–6.
Belgaum: 26th Madras Infantry: Second outbreak: Lasted from July to December: Major Bannerman's details of cases, 25,934–8.
Belgaum: 26th Madras Infantry: Second outbreak: Lieutenant Lethbridge's statement of cases in September and October, 5621–6.
Broach: Encouraged by native practitioners, 15,504, 15,510.
Broach: Inoculations, chiefly among the Parsees, 15,511; II. 522–3.
Broach: Kantiajal: Good results of, at, 15,505.
Bulsar: Census taken and statistics carefully checked, 16,288, 16,296–306.
Bulsar: Comparison between inoculated and uninoculated Tai wards, 16,280–1, 19,970–94.
Bulsar: Comparison between inoculated and uninoculated Ghanchi wards, 16,287.
Bulsar: History of inoculations in Bulsar, 16,255–341.
Bulsar: Inoculations performed to see to what extent they would stop plague, 75.
Bulsar: Inoculated did not leave the place but uninoculated persons did so, 16,291.
Bulsar: Table of results of inoculation, 16,256, 16,277; II. 320.
Dharwar: Cases among inoculated and uninoculated in camp recorded only if found in the town, 1988, 1991, 1996, 2351–8.
Dharwar: Deaths among people who left the town and camped outside not included in inoculation returns, 1976.
Dharwar: Inoculations recommended on the outbreak of plague, 1974.
Dharwar: Most people in the town inoculated, the people in camp being the large majority of the town population not inoculated, 1956, 1974–9, 1991–4, 2351.

INOCULATION WITH HAFFKINE'S FLUID—*cont.*

Dharwar: Report on plague cases among inoculated, I. 429–38.
Dharwar: Statistics of inoculation and plague, I. 67.
Dharwar: Statistical comparison by Miss Corthorn of inoculated and uninoculated, 1763–71.
Dharwar: Whole population inoculated in, except sick and aged, 1689.
Dharwar District: Inoculations at Huilgol, II., 226.
Dharwar District: Inoculations at Lingdhal, III., 225.
Dharwar District: Villagers desired inoculation and were permitted to be inoculated, 1958–61.
Dharwar Jail: All prisoners in inoculated successfully, 2263–4.
Gadag-Betigeri: Classification of doubtful cases as inoculated or uninoculated done by Mr. Foy, 23,837, 23,863.
Gadag-Betigeri: Inoculations done on a large scale, 23,680.
Gadag-Betigeri: Miss Corthorn's figures obtained from official statistics supplemented by independent inquiry, 23,837–9.
Gadag-Betigeri: Miss Corthorn's statistics of inoculation, 23,831.
Gadag-Betigeri: Mr. Foy's statistics of inoculation, 23,727.
Hyderabad State: Not introduced at, 5159, 5511, 18,141.
Madras: Efforts to introduce in, 3871–2.
Mora: Statistics of inoculation, 52, 27,310–8.
Mysore City: Census not taken, and extent of exodus not determined, 24,977.
Mysore City: Large numbers inoculated before plague broke out, 3482.
Mysore City: Statement of inoculations at, III. 277.
Mysore City: Statistics checked by inquiries but not very accurate, 25,037.
Mysore City: Statistics of attacks and deaths among inoculated probably correct, 25,112–5.
Mysore State: Evidence on inoculations of the native troops, I. 177–9.
Panjab: Banga: Details of, in, 10,172–7.
Panjab: Banga: Prophylactic inoculations done in, on a large scale, 10,235.
Panjab: Garhshankar: Inoculations in, 10,674–8, 10,861.
Poona Lunatic Asylum: Inoculations performed partly as a preventive and partly as an experiment, 12,841.
Satara: Gave very good results, 22,156–61, 22,390.
Satara: Seven hundred and eight persons inoculated, 22,390.
Satara: Six hundred and ninety-one persons inoculated in the city, 22,154.
Satara: Statistics of inoculation in the district, 1898–99, III. 190.
Surat: Not tried at, 2061.
Umreth camp: Inoculations on a large scale in, but the existing conditions offer no fair test of its efficiency, 14,088.
Wardha: Evidence on plague cases and inoculations at, I. 225–7.
See Bangalore.
,, Bombay.
,, Byculla Jail.
,, Calcutta.
,, Daman.
,, Hubli.
,, Karachi.
,, Khoja Community.
,, Kirki.
,, Lanauli.
,, Umarkhadi Jail.
,, Undhera.

IDENTIFICATION OF BODIES OF INOCULATED PERSONS:

Bangalore City: Identification of dead thrown into the streets difficult, 3522.
Bangalore Civil and Military Station: Identification in doubtful cases difficult, and depends on inquiries, 2615–6.
Hubli: Dead bodies classed as inoculated or not after careful inquiry; tendency of people to assert that uninoculated dead have been inoculated, 1726.
Hubli: Half the dead found in the streets had inoculation certificates on them, 3093.

INOCULATION WITH HAFFKINE'S FLUID—*cont.*

Hubli: Inoculation register had no index, 2528.
Hubli: Method of registering deaths. Over 95 per cent. of bodies identified I. 400, 409.
Hubli: No difficulty in identifying the dead as inoculated or not by register and certificates, 1964–5.
Hubli: The unidentified dead classed as inoculated or not according to the proportion of each class shown by the census to be in the town, 3082–4, 3131–7, 3181–2.
Hubli: Statistics assume a man inoculated if no certificate is found, 2473–7, 2515–6, 2530.
Mysore City: Inquiries made and certificate recovered if possible, but margin of error remained, 25,038–9.

INDUCEMENTS TO INOCULATION WITH HAFFKINE'S FLUID:

Alibag: Inoculates allowed to remain in their houses and go where they like, 25,880, 25,883.
Bangalore City: In houses, where all inmates inoculated, neither sick nor contacts removed, 3292.
Bangalore City: Privileges for inoculated same as at Hubli, 3298–9, 3315–6.
Bangalore Civil and Military Station: Contacts from going to camp, inoculation exempts, 2637.
Bangalore Civil and Military Station: Contacts not evacuated at Bangalore if inoculated, 2576.
Bangalore Civil and Military Station: Inoculated contacts allowed to remain in their houses, if good, otherwise to go where they please, 2637, 2661–4.
Bangalore Civil and Military Station: No restrictions on the movements of the inoculated, 2637, 2661.
Baroda State: Inoculated persons not forced to vacate their houses in the villages of Baroda State, 15084.
Baroda State: Passes offered in the Naosari district as an inducement, 15,071.
Belgaum: Privileges to inoculated at Belgaum, 23,132.
Bombay: Certificates given to inoculates which passed them on railways. Not treated as contacts, 1366.
Bombay: Contacts given the option between inoculation and removal to camp, 17,878.
Bulsar: Concessions in the way of treatment of sick in their own houses, &c. given to the inoculated, 16,291.
Calcutta: Inoculated persons practically free of all plague measures, 6537–9.
Calcutta: Segregation, inoculated families promised freedom from, I. 468.
Dharwar: Concessions granted to inoculated persons in the matter of return to evacuated houses, 2337, 23,746–7, 23,763–6.
Discomforts of plague measures, people would submit to inoculation to avoid the, 10,727.
Hubli: Certificate to enable travelling without detention and short time of segregation induced the people in Hubli to submit to inoculation, 2428–34.
Hubli: Inoculated persons re-admitted to evacuated areas after 10 days, 1955.
Hubli: Inoculation encouraged by moral suasion and grant of privileges, 2471–2, 3055, 19,889.
Hubli: Method of persuading people to inoculation at Hubli, I. 398–9.
Hubli: No rule prohibiting non-inoculated from re-occupying their houses, but they could not travel by road or rail, 3158–9.
Hubli: People induced to submit to inoculation by the offer of privileges, 19,826–8, 19,888.
Hubli: People not allowed to return from segregation to infected places unless inoculated, 2432–3.
Hubli: The once inoculated had no privileges in segregation, 3164.
Hubli: Twice inoculated persons allowed to return to their houses at once after disinfection, 23,375.
Karachi: Inoculates not segregated, II, 423.
Karachi: Only inducement that inoculates were not turned out of their houses, 11,756.
Mysore City: Number of people probably inoculated in order to leave the place, as facilities for travelling granted to inoculates, 25,108.

INVESTIGATION SHEETS:

Banga: Tables of inoculated and uninoculated persons in the houses attacked with plague, II. 91–93.

INOCULATION WITH HAFFKINE'S FLUID—*cont.*

Baroda State: Manner in which investigation sheets prepared, 15,040–54.
Belgaum: Investigation sheets of four houses in Belgaum prepared by Colonel Peters, I.M.S., 23,108–15.
Billimora (Baroda State): Table of inoculated and uninoculated in the same houses, II. 263.
Daman: Particulars of households where inoculation was applied and plague occurred, I. 310–30.
Hubli: Investigation sheets prepared from cases selected by Lieutenant Keelan as good cases, 19,877–88, 19,899–906.
Hubli: Investigation sheets selected from 200 cases collected and investigated by Lieutenant Keelan, 19,829–35.
Hubli: Investigation sheets, 60 cases appended to Leumann's Report selected by him from 200 cases picked and chosen for him by Lieutenant Keelan, 1725.
Hubli: Plague cases (house by house) in inoculated houses, I. 417–28.
Kantharia (Baroda State): Investigation sheets synopsis, II. 506.
Karachi: Details of plague cases in houses inhabited by inoculated persons, II. 428–42.
Koili (Baroda State): List of cases with particulars among inoculated, II. 269.
Koili (Baroda State): Table showing the results of inoculations, II. 277.
Undhera: Cases among inoculated and uninoculated, house by house, I. 333–6.
Undhera: Summary by houses of plague cases, I. 331.

METHOD OF PERFORMING INOCULATIONS:

An operation requiring very little skill, 178.
Place generally in left arm, rarely flank, 1,746–7.
Precautions for cleaning inoculating instruments, 1752.

PERIOD AFTER WHICH PROTECTIVE EFFECT EXERTED:

Acts in from 12 to 24 hours, 144.
Affords some protection from first day, 1774.
Beneficial within 24 hours, 47.
Protection begins in 24 hours, 4024.
Rapidity of action, 47.

PERSONS RECOVERED FROM PLAGUE; INOCULATION OF:

Reaction does not differ from that in persons who have not had plague, 1537.
Recovered plague cases not inoculated, 2504.

REACTION AFTER INOCULATION OF HAFFKINE'S FLUID:

Amount of:

Above 103° not observed after inoculation, 14,319.
Above 104° not observed after inoculation, 14,520.
Average temperature, 103°, 16,571.
Average resulting temperatue, 101·8°, 778.
Between 100° and 103°, 12,398.
Higher in men than in women after inoculation, 23,813.
Highest 24 hours after inoculation, 10,796.
Inoculation of the 26th Madras Infantry gave so little discomfort that they submitted willingly to a second, and were anxious for a third, 25,942.
Maximum after inoculation, 102½°, 32.
No excessive reaction observed, 6338.
Reaction temperature after inoculation ought not to be less than 102°, 3006.
Subnormal after, 20,471.
Some of the doses did not give enough febrile reaction, 2996.
Temperature aimed at after inoculation 102½, 32.

Animals, In:

Effect of injection into animals, 28, 29.

Europeans, In:

Europeans suffer more than natives from, 4858–9.
Produces stronger reaction in Europeans than in natives, 4858–9.

Old Men, In:

Small doses given to old persons and little children give very little reaction, 25,891.

Particular Instances, In:

Bangalore: Maximum temperature resulting 104°, 4837.
Baroda: Highest temperature 104, 15,417.

Inoculation with Haffkine's Fluid—*cont.*

Bombay: Byculla Jail: Did not react on several of the prisoners in, 20,365.
Bombay: Byculla Jail: Prisoners suffered severely from, 20,364.
Bombay: Umarkhadi Jail: Its discomforts did not last beyond 36 hours, 20,315.
Calcutta: Highest temperature observed, 105°, 6663.
Hubli: Temperature obtained 106°, 2000.
Karachi Jail: Highest temperature 104°, lowest 99° after, 11,563–4.
Mr. Haffkine: Effect of inoculation on, 32.
Wardha and Nagpur: Report of 201 inoculations in, with special reference to the reaction produced, I. 460–1.

Persons recovered from Plague, In:

Reaction in those recovered from plague not in any way different, 1537.

Re-inoculation, After:

No diminution in the severity of the reaction after three subsequent inoculations, 19,050.
Second inoculation, effect more severe, 6626.
Temperature reaction about the same as in the first, 23,820.

Variety in:

Great variation in the general symptoms after, 11,573.
Great variety in different views, 6330.
Reaction sometimes too slight, sometimes excessive, in Calcutta, 6622–4.
Standard dose does not produce the same symptoms in each healthy individual, 5095.
Varies with individuals after same doses; ideal temperature 102°, 76, 10,797–800, 17,490–5.
Varying after inoculation of the inmates of the lunatic asylum, 12,963.

See After effects of inoculation, *fever and severe symptoms from.*

Re-inoculation.

Attitude towards:

People generally object to second inoculation, 10,166.
Second inoculation would make process unpopular, 4062.

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In Animals:

Animals can die of plague without the microbe being discoverable in their bodies after death, 8613.
Ants: Not found in ants, 635, 8530.
Cattle: Diplo-bacteria similar to those found in plague rats discovered in the blood of cattle during the epidemic at Calcutta, 1897, 26,942.
Cat and guinea-pig: Involution forms of plague bacilli seen after death at the seat of inoculation in a cat and guinea-pig, 26,227.
Cats: Found in cats with typical plague symptoms, 20,424.
Fleas: Not found in fleas, 8600.
Fleas: Said to be found in fleas, 151.
Guinea-pigs: Found in the intestines of a guinea-pig, 27,336.
Guinea-pigs: Easily discovered in the bronchial contents of guinea-pigs in laboratory experiments, 27,341.
Recoverable from body of dead animals up to 21 days after burial, 27,382.
Leeches: Isolated from leeches which had been applied to buboes, 22,287.
Mice: Spleen and liver of mouse dead of plague found full of plague bacilli, 26,324.

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In the Human Body:

Bronchi: Found in the bronchi, 6916.
Kidneys: Found post mortem in kidney I. 370.
Lesions: Found in four cases in small lesions of the skin, 1393.
Lesions: Cultures obtained from small lesions; blood remained sterile, I. 374.
Liver: Found in liver of plague corpses, 7189.
Liver: Found post mortem in liver, I. 370.
Nose: Found in the bloody discharge from the nose in patient of sub mental buboes, 26,259.
Saliva: Found in saliva of ordinary bubonic cases occasionally, once 10 days after apyrexia, 25,355.
Spleen: Found in spleen of plague corpses, 7189.
Spleen: Found post-mortem in spleen, I. 370.
Sweat: Never discovered in sweat, 26,393.
Throat: Found in the throat 10 days after fever had subsided, 25,312.
Tongue: Found twice in the fur at the back of the tongue out of 20 cases, 27,055.

See Blood.
,, Carbuncles.
,, Fæces.
,, Glands.
,, Lungs.
,, Sputum.
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In small-pox and other diseases natives recognise value of, and enforce isolation of the sick, 7,038.
Isolating the sick most necessary, 12,053–5, 13,191, 21,540.
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Izal 1 in 300 capable of killing the plague bacillus in 3 minutes, 27,401.

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Jains among the first to suffer in Bombay, 911.
Jains had the highest mortality rate in Bombay, this being due to the fact that animal life is sacred among them, 17,681.
Jains, on account of their objection to anything which involves the death of animals, are difficult to deal with with respect to inoculation, 7,892.

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,, Habits of people affecting liability to plague.

JEYES' FLUID:

Disinfection by:

Clothes of passengers entering Karachi by sea disinfected with Jeyes' fluid, 4243.
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KARACHI:

Inoculation with Haffkine's Prophylactic:

Inoculated left the city, but not more than uninoculated, 11,756.
No roll call or anything of that sort in the voluntary camps, 11,374, 11,879.
Population during period of inoculation arrived at by approximate estimate, 11,713–7, II. 424, 11,804.
Report on the effects of inoculations, II. 375–97.
Results on prisoners negative, as plague ceased in the city at the time of inoculation, 11,548.
Summary of results of inoculation, 11,758, 11,765, II. 424–5.
Statement of results of inoculation among the people, 11,758-65.
Statistics of inoculation to be accepted with caution as population of Karachi during plague not accurately known, 11,765, II. 425.

See Khoja Community.

Introduction of plague into:

First case came to notice on 10th December 1896, other cases being detected immediately afterwards, and no connexion between the earliest cases being traced, 11,092–3, 11,099, 11,101, 11,846.

KARACHI—*cont.*

First case occurred in a good quarter, in a person who had had no direct communication with Bombay, 11,096, 12,984, 13,216.
Gunny bags: Asserted to have been the means of importation of plague into Karachi, but no proof of assertion, 817.
Gunny bags: Examined, but no traces of plague found in them, 635.
No plague in Karachi between the first and second epidemics; authorities confident that the city was quite free, 11,183.
Rats or cargo probable means of first importation of plague by sea from Bombay to Karachi, 12,984.
Sea: Disease must have come to Karachi in 1896 by sea from Bombay, 11,338.
Sea: Strict precautions taken against importation of infection by sea between the first and second epidemics at Karachi, 11,598, 12,151, 12,218, 12,317–8.
Second epidemic: First cases at end of March 1899, no connexion traced, 11,181, 11,512–22.
Second epidemic: First case in quarter not infected in first epidemic, in person of a man working at the Native Jetty, hence assumption that infection imported in cargo, 13,215–6, 13,220.
Second epidemic: Rats died before the first case came to the notice of the authorities, 11,198, 11,308.
Second epidemic: Joria Bazar infected, no connexion traced with first case occurring in a different part of the town and no source of infection ascertained, 11,188, 11,197–200, 13,344.
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Statistics of death-rate for the period between the two epidemics compared with three other towns in Sind, II. 142*a*.
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Khoja community: The low rate of mortality from general causes in the inoculated probably due to unconscious selection of cases for inoculation, 26,500–3.
Khojas in Poona inoculated at H.H. the Aga Khan's house, 12,965.

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Comparability of the inoculated and the uninoculated, 199.
Deaths and inoculations at different ages in the Khoja community of Bombay, 17,845.
Figures for the Khoja community, Bombay, obtained from the Jamaat books which are most accurate, 17,846–51.
Inoculations among the Khojas of Bombay, 17,793–822.
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List of inoculated Khojas treated in the Khoja Hospital, Bombay, III. 404–6.
Nominal roll of inoculated and uninoculated Khojas in Bombay who died of plague and other diseases from April to December 1898, III. 399–403.
Report by Mr. Haffkine on inoculations in the Khoja community of Bombay, I. 337–344.
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Karachi Khojas:

Census of Panjabhai Khojas, showing occupation, &c. in Karachi, accurate, 11,718–24, 11,734.
Gross mortality among Panjabhai Khojas at Karachi from October 1898, to January 1899, 11,780.
Gross mortality among Panjabhai Khojas in period before plague at Karachi, 11,728.

KHOJA COMMUNITY—*cont.*

Mortality among Panjabhai Khojas at Karachi during plague period from causes other than plague, 11,740.
Mr. Kaka's report on inoculation among the Khojas and other persons at Karachi, II. 423–45
Mr. Kaka's statistics checked by special house to house inquiries by Khoja headmen, 13,233.
Mr. Kaka's statistics of attacks and deaths derived from comparison of nominal rolls of casualties and inoculates, 13,227–39.
No explanation of difference between death-rate of inoculated and uninoculated children among Panjabhai Khojas, Karachi, 11,747–54.
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Possible that errors made in classifying dead persons as inoculated or uninoculated, 11,752–4.
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Of kidneys, 1421.
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Thana: Used in, 19,673.
Umreth: Golas quarters disinfected by kiln burning, 14,037.

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Approved by all village officers and by villagers themselves, I. 443.
Kiln method employed at Hyderabad popular, 5159.

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Apt to give infection to persons employed in digging up the floors, 24,022.
Kiln burning a dangerous process on account of the carelessness of the coolies, 22,727.
Previous lime-washing important to lay dust and secure completeness, I. 456.

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Effect of kiln method on mortality in Hyderabad villages, I. 459.
Fresh cases of plague in village disinfected by kiln burning process, 22,716–32.
In the Nizam's territory, used with great success, 5449.
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Doses used at Lanauli 184–190.
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Bangalore: Privies generally in cow yard, 4912.
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Dharwar: Five public latrines and two trenches capable of holding 20 seats, 2306, 2327.
Dharwar: Quite inadequate for the large population, 2313.
Hinganghat: Public latrines outside the town; fairly well-to-do people have privies in their back yard, 6262–5.
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Only one case in Bombay among lepers, 917.

LESIONS:

Discovery of Bacillus in:

Plague bacillus found in small lesion near bubo; four cases, I. 374, 1393.

Frequency of:

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,, Caste.
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Poona: Great opposition to the floors being spread over with quicklime, 21,590.

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Chloride of lime, best in powder, 1226.
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Doubtful whether whitewashing is a sufficient precaution against plague, II. 6.
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Baroda: Uninfected houses cleaned and whitewashed in, as disinfecting precaution, 14,631.
Bombay: Umarkhadi Jail: Disinfection with boiling quicklime and carbolic dilute failed to prevent succession of cases in the particular place disinfected, 20,344.
Danger in digging up floor minimised by whitewashing it, 5554.
Dharwar: No plague after whitewashing, &c. of dug-up floors, 23,778.
Guntakal: Lime applied to roof and walls as disinfectant in, 5099, 5100.
Kalindri: Resorted to as disinfectant, 9894.
Mandvi: Quicklime and sulphur used as disinfectants considered quite as effective as more expensive agents, 13,458.
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Poona: Use of lime alone as disinfectant found valueless, 10,972.
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Clinical Features of Plague:

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Liver in bubonic plague somewhat large and congested, 10,421.

Discovery of Bacillus of Plague in:

Found in liver of plague corpses, 7189.
Found post-mortem in liver, I. 370.

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Climatic Conditions and Mahamari.
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Epidemic Diseases in Kumaun and Garhwal other than Mahamari and Sanjar.
Food and Grain and Mahamari.
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Grain not the cause of infection, App. XXV., sect. 254.
Grain the cause of the disease because rats, mice, and snakes die, II. 362 (4), II. 363 (11).
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No connexion between the disease and food grains, as people who take their grain with them into the jungles remain free after evacuating their villages, 8831.
Not due to grain, 26,588.
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Habits of the People of Kumaun and Garhwal:

People dirty and never wash, 6108–9.
People do not wear shoes, 26,573.
People never wash themselves, thinking that dirt is a preventive of mahamari, and they also have cuts on their feet frequently, 8832.

Immunity of particular Places and Persons from Mahamari:

Dasauli: Though surrounded by affected tracts, has remained immune, except for a small outbreak in two villages in 1864, II. 366.
Kidarnath: Pilgrims have never had mahamari, II, 366.
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Before coming over into Garhwal, Thibetans make special enquiries about the existence of mahamari, and take precautions against it, II. 369 (d).
Importation improbable, 26,617–8.
No mahamari heard of in Thibet, II. 369 (d).
Possible importation from Yunnan by communication with countries north of the Himalayas, 26,574.
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Incubation Period of Mahamari:

Incubation period less than 24 hours, II 362 (3).
Incubation short in both mahamari and sanjar, 8827.

India, Communication of Mahamari to, from Kumaun and Garhwal:

About 50 years ago the disease was introduced from the Hills into Bijnaur and Moradabad, 8836.
Communications are bad, the trade small, and the disease is usually found to occur several days' march from the plains, 8834.
Infection not imported from Kumaun to Hardwar, 8765, 6185.
Inter-communication with India slow and difficult, II. 370.

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Introduction from Okhimath into Bombay very improbable, 8836.
Mahamari was once imported from the Hills to the plains, 6181–2.
The only danger from pilgrims and from the Panda Brahmans of the hills who pass the winter in India, II. 370.

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Locality and Mahamari:

Cause of disease lies in the houses, as a child or woman generally suffered first, 26,575, II. 360 (250).
Chuni, 1895, a case was attacked in the jungle, after the people had left the village, II. 371 (4).
Contracted by visits to mahamari houses, II. 369 (6c).
Disease contracted by return to houses vacated, 26,576.
In Balt all remained well until a fall of snow drove the people, after evacuation, back to their houses, when the disease re-appeared, II. 339.
Infection kept alive by visits to infected houses, App. XXV., sect. 263.
Infection most potent in the house in which a case has occurred, less so in the open air, App. XXV., sect. 261.
In many cases the disease is restricted to localities or houses, App. XXV., sect. 247.
Semi, 1897, all five cases were of people belonging to one house, II. 371 (3).
Suini outbreak, 1893, two cases directly due to return to infected houses, II. 368.

Measures, Precautions, and Rules relating to Mahamari in Kumaun and Garhwal:

Concealment for any long time impossible, and villagers take steps to guard themselves, 8834.
Most important measure is improved sanitation, during periods of attention to sanitation, outbreaks were few, II. 361 (256).
No person of an infected village dares to pass into an uninfected village, II. 339; II. 341.
People favour the burning of mahamari houses, 26,576.
Rules prescribed by Government for dealing with mahamari, 8836–41; II. 338 (104–5); II. 377.
Villagers evacuate their villages on the death of rats, 8839–40.

Mild Forms of Mahamari and Sanjar:

Dr. Planck knows no epidemic with low mortality, 26,589.
Mild cases few, 26,581.
Neither sanjar nor mahamari patients can walk about, 8962–5.
No evidence of existence of less fatal and atypical forms, II. 369 (6c).
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Monkeys and Mahamari:

Chuni, 1895, monkeys swarm, but none sickened, 371 (4).

Mortality from Mahamari and Sanjar:

Case mortality very high, II. 369 (6c).
Extremely fatal: in one instance two houses affected, and all inmates died, in one house five, and in the other six, 6193.
In 1834, fatal termination in 3 to 4 days, App. XXV., sect. 95.
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In 1876–77, in 291 cases, case mortality 95 per cent., App. XXV., sect. 244.
Mortality in an outbreak of mahamari 75 per cent., 8903.
Mortality in an outbreak of sanjar 20 per cent., 8900, 8962.
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Native Names for Mahamari:

Disease called mahamari, golarog, and phutkiarog, II. 368 (6).
Name sanjar includes relapsing fever, II. 368 (6).
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An epidemic of sanjar found to be relapsing fever, the diagnosis being confirmed by an examination of the blood, II. 368 (6).
Dr. Pearson and Assistant Surgeon Mukerji, in 1851, thought that mahamari was typhus fever, App. XXV., sect. 101.
In 1850, Dr. Renny thought the disease was not plague, but malignant typhus with buboes, the appearance of which was an unfavourable sign, II. 336.
In 1852, Dr. Pearson and Assistant Surgeon Mukerji came to the opinion that mahamari was plague, App. XXV., sect. 102.
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Mahamari is true plague (Planck), 26,573.
No skin eruption in sanjar, which has been wrongly called typhus, 8957–9.
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Outbreaks of, History and Continuity of :

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Two-thirds of cases seen in corpse inspection not seen by any doctors during life, 22,007.
Very few cases in which examination of the sick has been objected to, 21,340.
When plague was rife only diagnosis was whether the case was one of plague or not; afterwards the disease from which the sick person was suffering was diagnosed, 21,346–7.

Opinions and Recommendations regarding:

Agency for, very unreliable in India, no one compelled to send in a report except in the municipalities, 6083.
Cannot be accurate without enormous increase of qualified practitioners, 7792.
Employment of Assistant Surgeons, but not of Hospital Assistants, in the Sanitary Department would improve notification of disease, 3964–83.
Employment of Assistant Surgeons or Hospital Assistants in the Sanitary Department would probably improve notification of disease, 23,303.
Law of notification of contagious diseases as it is carried out in England would be impossible in India, 21,855.
Notification of diseases should be made compulsory, 17,747.
Notification of sickness not possible, as at present 90 per cent. of the people die without being seen by a doctor, 21,856.
Special forms of reports of sickness recommended, 23,298.
Voluntary system of registration of sickness should be established in Bombay and other large places, 10,990.

See Medical Treatment, European: *Attitude towards, in general.*
,, Native practitioners.
,, Voluntary agency.

Systems of:

Ahmedabad: No regular system of notification of diseases, 13,873.
Bombay Presidency: Notification of deaths not general; chiefly a matter of police, 17,229.
Calcutta: Supposed to be compulsory; not effective, 6506.

NOTIFICATION OF DISEASE—*cont.*

Cholera promptly notified, 7818.
Dharwar: System of notification of deaths, 1623–40.
Headman of village only means of information of deaths and sickness for rural areas, 22,959.
Municipalities: Penalty for the non-notification of disease never enforced, 23,311.
No notification law in India, 6089.
No system of notification of infectious disease in Bombay, 17,228.

NOTIFICATION OF PLAGUE:

Attitude towards:

Bombay: Cases not notified owing to difficulty of recognising plague, 1050.
Dharwar: No information to be obtained, 6133.
Difficulty of getting information owing to the temper of the people, 7598.
Errors in notifying plague in rural district owing to the difficulty of recognising the disease, 23,289.
Hubli: Cases notified mainly after death, 19,836.
N.W.P.: Cases not reported from sheer ignorance, people failing to recognise plague, 8867.
Objection to plague measures so great that people will not report cases, 7824.
Surat: Compulsory declaration of plague easily evaded, 15,582.

Early Notification, Importance of:

Early notifications impossible in India without very large increase of the existing machinery, 10,499.
Early notification most important, 10,497.
Early notification of primary importance, 22,945.
Early notification the most important thing, I. 408.
First cases rarely come to official notice anywhere,
N.W. Provinces: Prompt reporting of cases the object of some of the plague regulations in the N.W. Provinces, 8749.
Panjab: Early notification of disease encouraged by a system of rewards and punishments, 10,826.

Efficient Notification secured:

Belgaum: The fear of Government measures being removed, people willingly give notification of plague, 23,138.
Bombay: In some of the districts in Bombay people voluntarily notify plague, 17,868.
Enemies of patients give information of cases, 6598.
Hyderabad (Sind): People give early notification of cases, 18,394.
Karachi (Kiamari division): Ready reporting of plague cases by people, 11,980.
Ready compliance with orders for notification the results of former experience, 24,519.
Sukkur: At first notification not prompt, but improved later, 18,394.
Surat: Dread of segregation works as pressure on neighbours to give early notification, 16,030.
Surat: Incumbent on head of family to report sickness within 24 hours, 15,836, 16,030.
Surat: Concessions regarding disinfection and segregation induced people to notify sickness, 15,715, 16,004, 16,031–2, 16,036.

Notification not Efficient:

Bombay: Mild cases of plague escape notification, 1192.
Cutch Mandvi: Muhammadans opposed to, 13,581.

Special Rules for Notification of Plague:

Bombay: Measures adopted to obtain early information of plague and their result, 25,574–617.
Bulsar: Notification of plague and corpse inspection compulsory, 16,169.
Calcutta: Native practitioners found to report plague to Plague Officer, 6791–3.

NOTIFICATION OF PLAGUE—*cont.*

Calcutta: Under plague regulations all cases to be reported, under penalty, 6791.
Cutch Mandvi: Penalty of fine imposed for neglecting to report cases, 13,552.
Palanpur: System introduced in the villages ensured daily reports and early information of all sickness and deaths, 13,128.
Rohri: Notification of all fever made compulsory, 12,026.

NUTRIENT MEDIUM IN HAFFKINE'S PLAGUE PROPHYLACTIC.

See Haffkine's plague prophylactic, *character and preparation* and *constituents of.*

OBSERVATION CIRCLES:

Inspectors visit every village in Madras observation circles once a week, 3954–5.
Madras: General rule that no one shall be detained more than 10 days, 3961.
Observation system in Madras obtains prompt discovery of cases, 3881.
Observation circles on Madras frontier, I. 145–9.
Observation circles 10 miles drawn in double rows along Madras frontier, 3865.
Observation system of Madras unique as regards rural areas, 3916.
People arriving within the circles in Madras report themselves for 10 days; few omissions, 3929–35.
Village headmen bound to question and report persons arriving in Madras observation circles and without pass, 3946.
Where plague has broken out, number of Madras observation circles increased and their area contracted to five miles diameter, 3947.
See Surveillance.

OCCUPATIONS:

Influence of Occupation on Liability to Plague:

Incidence of plague on various occupations at Bombay, I. 392–3.
Karachi: One thousand seven hundred and five cases of plague classified according to the occupation of the sufferers, 11,727.
Preponderance of plague cases in men due to their occupations exposing them more to infection, 12,349.

See Acharj.
,, Banniahs and Traders.
,, Corpse bearers.
,, Dhobis.
,, Disinfectors.
,, Domes.
,, Scavengers.

Influence of Outdoor Life on Liability of Plague:

Beggars at Satara, living all day in the open air, escaped plague, 22,267.
Bhils suffered little on account of their leading an open air life and residing on the outskirts of the town, II. 246.
Herdsmen and shepherds have almost a perfect immunity, 5473.
Open air life and occupation of people living in fishing villages near Karachi keep them free from plague, 12,278–86.
Outdoor occupations do not make people less liable to plague. Instance, the people at Salaya, 13,476.
People working in air all day and sleeping out all night have few cases, 17,711.
Rice cultivators at Satara, living and working in the open, escaped infection, 22,127, 22,184.
Waghris and gypsies live in grass huts outside Cutch Mandvi and suffered little from plague, 13,480.

See Air and light.

ŒDEMA:

Clinical Feature of Plague:

Common symptom in the lungs in pneumonic plague, generally observed on third day of illness, 20,823.

ŒDEMA—*cont.*

Never observed œdema on the chest with pneumonic plague, but in many cases of inguinal and axillary bubonic plague, 27,179.
No œdema at the anterior portion of the body observed in plague patients in the hospital at Ahmedabad, 13,944.
No exceptional œdema of the thorax or abdomen noticed in patients in the Sassoon Hospital, 12,924–5.
Œdema never seen confined to the anterior part of the body, 12,407.
Œdema of the surface very rare, 12,403.
Several cases of œdema on the anterior and posterior wall of the body, 18,059.

In Animals:

Œdema on front of trunk noted in three monkeys died of plague, 9577.

Post-mortem Appearances:

Doubt, till bacteriological examination, whether case with œdema was plague or not, 9552–3.
No œdema in the anterior wall of the chest after death from pneumonic plague, 15,926, 18,158–61, 20,021.
No œdema of the surface either before or after death, 18,557.
No œdema in plague corpses except in contiguity of buboes, 10,643.
No œdema in cases of exclusively inguinal buboes, 10,347.
No œdema in front of thorax and abdomen seen independent of buboes on neck, 10,379–83.
No œdema observed at post-mortem in cases of pneumonic plague, 8669, 9891.
Œdema never observed in the subcutaneous tissues at the front of the body, 10,424–9.
Œdema of the thoracic surface invariably observed at post-mortems, but never in face, hands, or feet, 9544–9.
Œdema observed in several cases of pneumonic plague, 10,346.
Œdema of the chest and upper limbs very common after death, 10,345.
Œdema occurring in the last hours of life never observed in autopsies other than plague, 9546.

OPIUM:

Immunity from plague of opium smokers and eaters, III. 43.

ORIGIN OF PLAGUE, THEORIES REGARDING:

Mr. Davur's theory of the origin, spread, and treatment of plague, III. 42–5.
Mr. Desai's theory of the origin of plague, 14,767.
Mr. Ghadiali's theory that plague is merely the child of other causes more powerful than the simple bacillus of plague, 24,935.
Opinion that the plague bacillus does not originate plague, but is merely a symptom of the disease, 8314.
Prof. Gajjar's theory of the origin and causes of plague, 18,622–49.

OTHER DISEASES:

Decrease in malarial fever during plague, 12,993, 16,505–7.

See Inoculation with Haffkine's plague prophylactic.

OVER-CROWDING:

Existing Conditions:

Ahmedabad: Very much over-crowded, 100 persons to the square acre in, 13,938.
Bangalore: Much over-crowded; some divisions contain a population of 8,000, while there is only room for 3,000, 2560.
Bombay: Average population of rooms in Bombay, 5 persons; Muhammadan quarters less over-crowded, 844.
Bombay: Density of population varies from 7 square yards to each person to over 800 square yards in the extreme suburbs, Bombay, 25,674.

OVER-CROWDING—*cont.*

Bombay: Excessive over-crowding and dirt in the chawls, 16,985, 17,711.
Bombay: Houses in Bombay contain several families of outcasts in one room, 833.
Bombay: In one chawl 2,000 to 2,500 persons in 300 rooms, 17,289.
Bombay: Kamatipura: Houses low and badly ventilated as well as over-crowded, 17,834.
Bombay: Mandvi: Houses large and well-ventilated, but much over-crowded, 17,934.
Bombay: Muhammadan quarters less over-crowded, 844.
Bombay: Outcasts living in Moorland Road in very crowded rooms, under conditions favourable to plague, 25,784.
Bombay: Over-crowding in Bombay a blot on its municipal system, 17,688.
Bombay: Over-crowding in Bombay, new municipal regulations, 133, 878.
Bombay: Over-crowding in the suburbs not so bad as in the town, 25,677.
Bombay: Over-crowding worse than any seen anywhere else, 26,644–5.
Bombay: Statistics regarding density of population, 883, 25,667.
Calcutta: Bustees occupied by 200 people or more, 7707.
Calcutta: Great overcrowding in parts, 6861.
Cutch Mandvi: Very much congested even in ordinary times, 13,349.
Hardwar: Inspection of lodging-houses very strictly carried out and number of occupiers limited, 9007.
Hinganghat: Over-crowding, but no worse than in other parts of India, 6278.
Hubli: No remarkable over-crowding, 20,209.
Karachi: Over-crowding exists to a terrible extent, especially among the weavers, 22,649.
Nasik and Malegaon: Over-crowding a tendency of the poorer classes, 13,785.
Nasik: Not much over-crowded, 16,979–84.
Surat: Houses of Golas, who suffered most, over-crowded, 16,107–8.

Improvement of Conditions of Over-crowding:

Bangalore: Over-crowding to be limited by law, 3265.
Bombay: Lord Sandhurst's scheme for the restriction of over-crowding highly approved, 17,688.

Influence of Over-crowding on Plague:

Ahmedabad: Plague worst where crowding least, though houses bad, 13,963–85, 13,990–1.
Bangalore: Relationship of over-crowding to mortality, 2590.
Bombay: Epidemic in Bombay increasing during cold weather owing to the people sleeping more indoors, 26,104.
Bombay: More sparsely populated districts of Bombay City most severely attacked, 8707.
Bombay: Mortality from plague in the most densely populated portions of Bombay less than in small villages in the suburbs, 25,675.
Bombay: Mortality in the sanitary Byculla Jail greater than in some over-crowded chawls, 25,682.
Bombay: Suburbs attacked have old houses and were crowded at time of attack, 25,690–8.
General vitiation of the air by over-crowding, &c., the predisposing conditions to plague, 17,354.
Great difference in the intensity of plague in ill-ventilated, over-crowded houses and well-ventilated houses, 14,390–1.
Karachi: Fishing villages close to Karachi where people live in open air and huts not over-crowded remained free, 12,279.
Karachi suffered most in the over-crowded weaver quarter, 22 649.
Northerly and easterly winds, by causing over-crowding, have an unfavourable influence on mortality in Bombay, 26,103.
Over-crowding and bad ventilation the causes of the disease, 14,716.
Over-crowding and plague diminished in the hot weather because the people sleep out of doors, 14,385.

OVER-CROWDING—*cont.*

Over-crowding, lowering of the vitality, habitual disregard of hygienic rules cause plague, 25,263.
Over-crowding principal factor in the propagation of the disease, 14,388.
Over-crowding the great question in Bombay in connexion with plague, 17,688.
Virulence and extension of disease dependent upon dirt, over-crowding, &c., 11,281.
Worst cases of plague in over-crowded houses, 11,228.

See Housing.

OXYDISING AGENTS:

Microbe resistant to reducing agents, sensitive to the action of oxydising agents, 8551, II. 8–10, 13.

PASS SYSTEM:

Bandra: Abandonment of the pass system and of inspection led to the introduction of plague in, 19,431.
Bangalore: Passengers forbidden to leave without pass, 2586.
Bangalore: Pass system failed; unworkable without enormous staff, 2641–3.
Baroda: Pass system very carelessly administered, 18,614–7.
Belgaum: Native resident plague authority distributed passes to villages in the district in, 23,000.
Bellary district: Valuable, 4934.
Billimora: Passes given to the uninoculated if no plague had occurred in their houses or streets, 15,066.
Daman: Frontier pass system, 15,966.
Dharwar: Pass system in force, 19,852.
Hubli: At one time no one could leave the town by rail or road without a pass, 19,812–6.
Hubli: Passes for travel by road or rail given only to persons having twice inoculation certificates dated 10 days previous to issue of pass, 3159.
Hubli: Pass system practically useless, as far as the road was concerned, 19,825.
Jalgaon: Pass system and patrolling roads failed to check spreading of plague by refugees, 25,544–6.
Karachi: No one allowed to change residence at Karachi without pass from plague superintendent, 12,277.
Karachi: People allowed to leave the place on passes, 3778–9.
Kotri: System of passes, 12,620.
Madras frontier, I. 145–9.
Madras: Headman of village notified of arrivals, 3929.
Madras: No complaints of blackmail, 3956.
Madras: One hundred and fourteen out of 5,500 with passports do not present themselves, 4224.
Madras: Pass system deficient at first, now working smoothly, 4236.
Madras: Statistics of evasion of pass system, 3870.
Madras: Very few evasions and little dislocation of trade, 3867.
Madras: Works well, 3866.
No special passes given to people travelling with large families, 12,222.
N.W.P.: Pass system adopted in, 9337.
Panjab: System of passes, 10,069.
Panjab: People allowed to leave infected places after spending 10 days in quarantine camp, 10,258.
Surat: Passes insisted on to prevent people leaving the place and spreading plague, 24,426–7, 24,461–3.
System of passes worked better with the village people than with the town people, 13,653.
Under the passport system people have to present themselves at one of the district offices for 10 days after they arrive in town, 4234.

See Camps.

PERCHLORIDE OF MERCURY:

Action on the Bacillus of Plague:

An absolutely effective disinfectant if used properly, 10,930.
Bacillus in infected floors survives the application of disinfectants, 5159.

PERCHLORIDE OF MERCURY—*cont.*

Cow-dung: Perchloride of mercury, details of experiments with, in acid solution on, 18,828–63.
Eight gallons of the 1 in 725 Bombay solution would be required to vitally damage the plague microbe at the surface of a 100 square feet cow-dung floor, 18,862.
Experiments showing efficiency in killing plague, 26,347, 26,434A.
Floor, not affected by 1 in 725 Bombay solution, infective material below, 18,862.
Floors adequately disinfected by solution of 1 in 500, 26,350.
Growth of plague bacillus stopped in plague cultures after half an hour's exposure to perchloride of mercury, 1 in 20,000, 26,347.
Increase in number of microbes, perchloride of mercury in neutral solution led to apparent, 8550, 8579.
Inert, bacillus of plague not killed, only rendered, 18,653.
Killed more rapidly in perchloride of mercury than in anything else, 7918.
Neutral solution led to apparent increase of microbes, but acid solution a reliable disinfectant, 8553–61, 8579–80.
Results of comparison with nitric peroxide, II. 258.
Result of tests with metallic salts, II. 6, 18,828.
Strong disinfecting action, sublimate in an acid solution has a, 8,554, 8561, II. 13.

Attitude of Natives towards use of:

Better classes averse to disinfection of their houses by perchloride of mercury, 15,495.
Disinfection by sublimate not objected to, 21,590.
Natives very sceptical about benefit of perchloride, 22,205.
Villagers often bribe the coolies to save their clothes from being put in perchloride solution, 21,174–5.

Disadvantages and difficulties connected with the use of:

Albuminous material is present, loses its disinfectant power, when an, 18,662.
Brass tubes, disinfecting power destroyed by applying it through fire engines with, 16,137.
Corrosive action of perchloride of mercury on metals disadvantage to its use as disinfectant, 10,018.
Expense: Kiln system cheaper than disinfection with perchloride, 19,677.
Expense: Perchloride of mercury expensive, 13,192.
Floor: Question whether in practice sufficient quantity of perchloride could be used to properly disinfect, 8578.
Lime applied every day to the rooms in which patients were left to be treated at home in Bombay, perchloride being applied from time to time on top of the lime, 25,798.
Lime: Good effects of perchloride of mercury liable to be counteracted by addition of, 20,212.
Lime: Mixed with, at Guntakal, before application, 5100–8.
Lime salts contained in many waters throughout India, perchloride solution rendered inert through the, 10,016.
Limewash neutralises disinfectant effect, mercury and acid upon, 8658.
Mercurial poisoning, generally not noticed among disinfectors, 10,020, 18,632, 19,939; but said to have occurred in 2 cases from clothes disinfected in perchloride, 16,117, 16,120–32.
Methylene blue added to perchloride useless for controlling the work of disinfectors upon mud walls, 9997.
Mud introduced into and weakened the solution in Satara because the coolies would place the pumps used on the ground, 22,323.
Poona: The native salt procured from the bazar found very impure, 21,305–10.
Purity of perchloride used in Bombay shown by analysis, 25,535.

PERCHLORIDE OF MERCURY—*cont.*

Disinfection, used for:

Bangalore: Perchloride of mercury used at, 2698.
Bombay: Perchloride of mercury chiefly used in strength of 1 in 1000, 911, 940, 1345.
Calcutta: Perchloride used with small pumps at, 7297, I. 471.
Calcutta: In acid solution, perchloride of mercury adopted at, 6528.
Calcutta: Perchloride of mercury adopted instead of carbolic acid at, 6528.
Calcutta: Mayo Hospital chiefly disinfected with perchloride at, 7022.
Jawalapur: Disinfection by perchloride used at, 8934.
Kalindri: Resorted to as disinfectant, 9894.
Kotri: Disinfection by perchloride, 12,536, 12,562.
Madras: Perchloride used in, 3937.
Palanpur: Perchloride resorted to as disinfectant, 13,091–4.
Panjab: Perchloride first used, but given up for phenyle, 10,016.
Poona: Perchloride of mercury solely used as disinfectant at, 10,933.
Rohri: Disinfection by perchloride, 12,029.
Umreth: Disinfection by acid perchloride still going on, 14,049.
Use of, prescribed by the Government of India, 7859.

Disinfection of Clothes by:

Clothes disinfected with perchloride for five or six hours, 1873–4.
Clothes soaked in perchloride to disinfect, 2707.
Clothes steeped in perchloride for disinfection at railway stations, 1315.
Clothing of people detained at Ahmedabad station disinfected by immersion in a solution of perchloride of mercury, 1 in 1,000, to which common salt was added, 13,894.
Dharwar: Clothing of patients leaving hospital disinfected with perchloride, 1872–4.
Dharwar: Clothing of persons from infected houses disinfected with perchloride, 2329.
Lanauli: Clothes disinfected in perchloride, 20,200.
Perchloride of mercury forced on clothes and houses with Chinese pumps at Dharwar, 2329–32.
Poona: Acid not used in the solution as all clothes disinfected, and acid spoiled clothes, 10,931.

Disinfection of Houses by:

Ankleshwar: Houses disinfected with perchloride of mercury in a hydrochloric solution, 14,258.
Baroda: Perchloride used to disinfect infected houses only, 14,631.
Bombay: Used in, 911.
Calcutta: Acid solution used for house disinfection, 6528–31.
Dharwar: Perchloride forced on houses with Chinese pumps, 2329–32, 2345–50.
Guntakal: Perchloride applied to roof and walls as disinfectant, 5099, 5100.
Hardwar: Houses disinfected with perchloride, 9030.
Karachi: Infected houses, and houses where dead rats had been found, disinfected by perchloride 1 in 1,500 in acid solution, 11,448, 11,647.
Poona: Perchloride solution used in disinfecting houses from roof to floor, 10,927.
Satara: Perchloride pumped on to walls, roof, and ceilings, 22,322.
Surat: Houses opened up, swept out, and doused with perchloride solution, 24,434.
Surat: Perchloride of mercury used only for infected houses, 16,011.
Umreth: Golas quarters disinfected by perchloride and kiln burning, 14,037.
Wardha: Houses saturated with perchloride 6124.

Disinfection of Persons by:

Patients on leaving hospital at Dharwar disinfected by pouring over them a warm solution of perchloride 1 in 1,000, 1875–6.

PERCHLORIDE OF MERCURY—*cont.*

Persons in Poona camps bathed in perchloride of mercury baths, 19,937.

Efficacy of:

Belgaum: Thorough disinfection with perchloride stops epidemic at Kagwad, 23,044.
Carbolic acid given up in favour of perchloride solution, 6528.
Disinfection with perchloride only so far useful as it cleans out the place and lets in fresh air, 22,864.
No cases at Ankleshwar from whitewashing houses disinfected with perchloride of mercury, 14,237.
No cases in Poona in houses disinfected with perchloride on return of people from camps, 10,937.
Perchloride of mercury most efficacious, 245, 10,930, 13,192.
Table showing number of plague cases after disinfection of houses with perchloride of mercury and with nitric peroxide, II. 258*b*.

Inefficacy of:

Karachi: No effective results of disinfection with perchloride of mercury, 22,658.
Lanauli: Disinfection with perchloride of mercury under careful supervision ineffectual, 20,184.
Reliance not placed solely on perchloride of mercury, 570.

See Recurrence of plague in the same house or locality.

Strength and Mixture in which used:

Acid not used in Poona on account of its effect on clothes, 10,931.
Formula of Bombay solution of acid perchloride disinfectant, 18,832.
Hydrochloric acid used in Calcutta, perchloride of mercury with, 6530.
Hydrochloric acid used in disinfecting processes at Umreth, perchloride of mercury with, 14,051.
Hydrochloric acid used instead of common salt to make solution of perchloride of mercury, 14,113.
Igatpuri: Solution made and used 10 times too weak, 23,317.
Perchloride of mercury used in solution of 1·1000, 940.
Strength of disinfectants not always accurately made up in Bombay through press of work, 915.
Strength of solution not always accurate, 24,450.
Tests of strength and acidity of perchloride disinfecting mixture used in Bombay, 25,540–7.

PERIOD FOR WHICH INOCULATION AFFORDS PROTECTION.

See Inoculation with Haffkine's plague prophylactic.

PERIOD FOR WHICH PLAGUE BACILLUS CAN SURVIVE:

In Air and Light:

Dies in a few hours when exposed to air and light, I. 447.
Dies very soon in the open air, in articles taken from infected districts, 9296, 9297.
Grows when covered up from the light in laboratory experiments, 25,304.
Microbe establishes itself most in darkness, 9683.

In Animal Organisms and Substances:

Bacillus dies more quickly when obtained from blood than when obtained from urine, 26,337.
Bacillus disappears very quickly in the blood of rats, 17,500.
Bacillus recovered from bodies of animals up to 21 days after burial, 27,382.
Bacillus recovered from saliva 10 days after apyrexia, 25,355.
Bacillus recovered from sputum of pneumonic case up to 28 days after development of lung

PERIOD FOR WHICH PLAGUE BACILLUS CAN SURVIVE—*cont.*

symptoms, and three days after apyrexia, 25,342–6.
Bacillus recovered from throat 10 days after fever ceased, 25,312.
Disappearance of the microbe after death, the acidity developed in the bodies of persons dead of plague, may aid in causing the, II. 7; 8655, 8681–4.

In Cow-dung:

In laboratory experiments the plague bacillus lived several months in sterile cow-dung, 18,877.
In unsterilised cow-dung the microbe did not live more than six days, 18,880.

In Clothes:

Kept alive for indefinite period in clothes, 10,094.

In Dirt:

Dies in dirt, 1003.
Establishes itself mostly in dirt, 9683.
Incapable of existing in dirt, 1003–4.

In Fæces:

Bacillus of plague dies in 2½ hours in fæces, 3611.

In Grain:

Dies in sterilised solution of rotten grain, 3669.
Experiments to show that the bubonic microbe derived from pure cultures perishes within 13 days after being added to specimens of grains and seeds, II. 17.
Not recovered after seven days in grain artificially infected and then dried, 26,386.
Results of experiments with several kinds of grains and seeds on life of bacillus of plague, II. 16, 17, 18.
Results of experiments on survival of bacillus in grain mixed with the sputum of a human plague patient, II. 18.
Virulence of the microbe on grain only persistent for a few days, 8535.

In Gunny Bags:

Longest period after which recovered from artificially infected gunny bags, seven days, 26,386.

In Milk:

Bacillus of plague grows slowly in milk, 156.
Dies in milk when sour, 3666.

In Soil:

Dies in a few hours in earth when exposed to air and light, I. 447.
Experiments on the growth of the plague microbe in soil, 18,893, 26,132.
Non-sterilised garden mould, can live from four to six days in, 18,867.
Sterilised garden mould, can have a continued existence for 13 days in, 18,865.

In Water:

Lived 44 days in sterilised tap water, 19,086.
Survives application of water and disinfectants in infected floors, 5159.
The bacillus flourishes most luxuriantly when the air is moist, and in damp surroundings, 9683, 18,638.
The microbe does not remain long alive in water, 8540.

When subjected to Drying:

Desiccation with calcium chloride kills bacillus of plague in 18 hours, 26,346.
Exposing a plague culture for four days to ordinary drying does not kill the microbe, 26,345.

When subjected to Heat:

Killed by exposure for five minutes to a temperature of over 62° or 63° C., 27,379.
Plague dies at 45° C., 102.

When subjected to the Sun's Heat and Light:

Growth could not be recovered in any instance from cultures exposed to sunlight for four hours, 26,342.

PERIOD FOR WHICH PLAGUE BACILLUS CAN SURVIVE—*cont.*

Heat of the sun sufficient to destroy plague microbes, 814.
Plague-infected calico rendered sterile by exposure to sunlight, 26,342.
Three hours' exposure to strong sunlight of infected flannel, cotton wool, and cloth, greatly retards the development of the bacilli, 26,342.
Vitality of the bacillus of plague diminished by the sun, 17,031.

Vitality of Plague:

The life of the plague organisms is indefinite, 10,597.
The plague bacillus dies out without use of chemicals, 22,058.
The plague bacillus dies in time without disinfection in about four months, 22,663.

See Acids.
„ Air and Light.
„ Calcium Chloride.
„ Clothes.
„ Cow-dung.
„ Damp.
„ Dirt.
„ Desiccation.
„ Disinfectants.
„ Fæces.
„ Grain.
„ Gunny Bags.
„ Hot Weather.
„ Milk.
„ Soil.
„ Sun.
„ Water.

PERIOD OF EPIDEMIC:

Epidemic ceased in villages after three months without evacuation or disinfection, 22,845–8.
Normal length of the epidemic where no, or insufficient, measures are taken, 9982–3.

PERMANGANATE OF POTASH:

Efficacy of as a disinfectant for plague:

An extremely potent destroyer of the bubonic microbe, II. 9.
Cowdung, 1 gram, deoxidises ·04 grammes permanganate, II. 10.
Result of tests with, II. 9–10.
Useless in practice, as it is readily destroyed by organic substances, 8628.

Use of, in connexion with cholera:

When used in well water can check a water-born cholera epidemic, II., 10.

Use of, in connexion with plague:

Bombay: Permanganate of potash used for flushing houses with steam-engines, but given up later for perchloride, 911–2.
Khandraoni, used at, as disinfectant, 9659.
Too expensive and inconvenient for practical use, II. 10a.
Well for camp use at Anand treated with, 14,148.

PERSONS, DISINFECTION OF:

A bath of soap and water without disinfectants would equally answer the purpose, 4298, 4308.
Females: Women are attended to by a female. They are made to sit down in a tub and the disinfectant is poured over their head, 4314.
Persons disinfected by soap and water bath, 13,896.
Personal disinfection most necessary, 12,053–5.
Personal disinfection more disagreeable to the natives than the disinfection of clothes, 21,510.
People objected very strongly, 4298, 4300.
Poona: Arrangements for disinfection at, 4027.
Rules regarding disinfection of emigrants at Karachi, 12,231–3.

See Camps, *disinfection in.*
„ Carbolic Acid.
„ Jeyes' Fluid.
„ Perchloride of Mercury.
„ Phenyle.
„ Railway Measures.

PESTIS MINOR:

Cases observed:

Bombay: Mild cases described, I. 380.
Bombay: Cases of mild plague in 1898 in Bombay, 18,039–46.
Bombay: Mild plague, with enlarged glands frequent among plague workers, 18,063–5.
Bombay: Pestis minor, cases observed in Bombay, 20,516–22.
Bombay: Pestis minor, observed in Bombay, symptoms described, hard to diagnose; no case of infection from such cases noted, 20,556–69, 20,573–6.
Bombay: One mild case observed at Parel, 22,349.
Bombay: Parel Hospital: Case of pestis minor in attendant in post mortem room, 22,489.
Bulsar: Observed at Kosumba in Bulsar, 26,989.
Calcutta: Pestis minor, resulting from cut at post-mortem of plague case, 7,003–9.
Hyderabad Sind: Mild cases of plague, non-infectious, observed in Hyderabad Sind, 18,096–103, 18,146–52.
Kankhal: Mild case observed in a child, 9467–78.
Khandesh: Possible cases of mild plague, 24,542.
Khandraoni: Pestis minor, case of, in child at Khandraoni, 9,678.
Pali epidemic: Observed during the Pali epidemic of 1836, 27,012.
Panjab: Boy at Gobindpur, Panjab, got mild plague from an ordinary case, 10,369.
Panjab: Mild cases at Dasanjh and Aur in the Panjab, and in other villages, 10,337–9, 10,548–50.
Panjab: Epidemic of *Kanedas* (swellings behind the ears) before plague at Garhshunker, 10,832.
Poona: Pestis minor, two mild cases observed; other cases probably malarial might be confused with plague, 7,636–52.
Poona: Mild cases, with neuralgia, among plague workers in Poona, 21,951–7, 21,967–73.
Rania: Observed at Astrachan, in Resht, 26,989.

Cases resembling Mumps:

Banga: Epidemic of mumps resembling plague before outbreak of plague, 10,235.
Bombay: Two cases in the Umarkhadi Jail, Bombay, 69 (I. 9), 7236–8.
Bombay: Mild case in Byculla Jail, Bombay, with pain in lower jaw, III., 659 (*f*).
Kankhal: Case at Kankhal, 9077, 9253.
Poona: Mild cases in Mahuli village and in Poona city, cases of mumps, of which one later developed plague symptoms, preceded the outbreak of plague, probably infective as so many children had mumps, 21,653–6, 21,664–5.
Poona: Mild cases of pestis ambulans and cases resembling mumps observed in Poona, 21,961–2.
Umreth: A case in Umreth, II. 487.

Infectiousness of Pestis Minor:

Coryza associated with plague, 21,006.
Coryza: Plague infection, spread by cases of coryza, 20,394–401, 20,532–6.
Mild case at Gobindpur, spread the disease and a case infected from a mild case was a serious one, 10,099.
Mild case, instance of possible infection from, 18,059–62.
Mild case, instance when infection was carried by a, 24,117–8.
Mild cases probably infectious, 7649, 9422–26.
No evidence of mild cases being infectious, 14,749.
Possibility that mild cases act as a connexion between epidemics, 27,000.

PETECHIÆ:

Clinical Feature of Plague:

Petechiæ not a characteristic sign of plague, 20,833.
Petechiæ not observed in pneumonic form, I. 369.

PETECHIÆ—*cont.*

Post-mortem Appearances:

Petechiæ always found post-mortem, 1420.
Petechiæ sometimes on the capsule of the kidneys in bubonic plague, 10,421.
Petechiæ of the stomach a noticeable feature in post mortems after bubonic plague, 10,419.
Petechiæ not seen on the skin, frequently on internal viscera, 9572.

Prognosis, As:

Petechiæ sign of bad prognosis, I. 352.

PHENYLE:

Action on Bubonic Microbe:

Action of phenols and their allies on the bubonic microbe, II. 4*a*, 5.
Doubtful whether phenyle and izal are capable of killing microbe under natural conditions, II. 5.
Phenyle more active than carbolic acid, II. 5.

Attitude towards:

Banga: Phenyle favoured by people, 10,306.
Disinfection of clothes by phenyle objected to by owners, 10,307, 10,817.

Clothing disinfected by:

Clothing dipped in phenyle and spread out in the sun used as disinfecting method, 10,048–9.
Disinfection of clothing by a five per cent solution phenyle, 11,452.

Houses flushed with:

Bombay: Infected buildings flushed with a solution of phenyle or permanganate of potash, 911.
Bombay: Given up for perchloride in, 911.
Calcutta: At first used for spraying houses, but afterwards given up for perchloride, 6520–28.
Khandraoni: Used for houses at, 9659.
Panjab: Phenyle, 1 in 200, used for disinfecting houses, 10,022.

Persons disinfected by:

Bombay: Convalescents bathed in phenyle on leaving hospital, 559.
Panjab: Before leaving evacuation camps, inmates bathed in phenyle, 10,049, 10,235.
Panjab: Disinfecting gangs bathed in phenyle on leaving work, 10,003.
Poona: Persons bathed in phenyle and similar disinfectants, 4027, 10,235, 21,273.
Strong solution of phenyle stings the skin so much that people cannot stand it, 4300.
Tungabadhra: Persons bathed in phenyle at, 4513.

PIGEONS.

See Birds.

PIGS:

No appearance of sickness in pigs, although great numbers were at large in the streets of Daman, I. 378.
Pigs were affected with plague in Daman, 16,355, 16,548.

PILGRIMS:

Cashmere pilgrim ship left Karachi carefully inspected; no cases on board, 12,262.
Nasik a great centre of pilgrimage, 13,751.
No pilgrims sail from Karachi to form caravans to Mecca, 12,266.
Pekin pilgrim ship left Bombay in December 1896, before pilgrim traffic stopped; plague occurred on board, 752–4.
Saharanpur: General inspection of pilgrims from Bombay and Sind, 9021.
Suggested infection of Bombay by pilgrims coming from Kumaun to Nasik Fair, 1545, 1551.

PHOTO-MICROGRAPHS OF BACILLUS OF PLAGUE.

Photo micrographs of bacilli made in connexion with cases of suspected plague in Calcutta in 1896, III. 369–376.
See Microscopic appearance of bacillus of plague.

PLAGUE PROPHYLACTIC:

See Haffkine's plague prophylactic.
,, Inoculation with Haffkine's plague prophylactic.
,, Yersin's curative serum.

PNEUMONIC PLAGUE:

Clinical Features:

Acute pneumonia a complication of plague, 10,212.
Brought to notice first in September 1896, 19,170–4, I. 375.
Course of typical pneumonic plague, I. 381.
Details of pneumonic cases, II. 63*b*, 64*a*.
No buboes in cases of, 12,918.
Pneumonic plague rarely with buboes, 2378.
The plague bacillus always recoverable from blood of patients dead of pneumonic plague, 17,258.

Diagnosis of:

Absence of inflammation of the pleura a marked difference between croupous pneumonia and plague pneumonia, 9568.
Cases may readily escape detection if attention directed only to presence of buboes, 13,945.
Diagnosis of plague pneumonia impossible without bacteriological or at least microscopical examination, 12,476.
Difficult to distinguish between plague and ordinary pneumonia, 12,469.
No certainty of pneumonic plague without post-mortem examination, 9435.
Pneumonic cases discovered by the stethoscope and physical examination of the lungs, 12,911.

Efficacy of Inoculation in:

Percentage of recoveries, inoculated and uninoculated, in pneumonic plague, 2382.

Fatality of:

All pneumonic cases in Anand Hospital died, 14,120.
Children: Pneumonic plague always fatal in, 12,413.
Great mortality in pneumonic form, 13,040, 15,176.
Non-bubonic type characterised by pneumonic engorgement of the lungs and accompanied by extreme prostration most fatal, 15,274.
Percentage of recoveries, inoculated and uninoculated, in pneumonic plague, 2382.
Pneumonic plague generally fatal in 48 hours, 2379.

Infection: mode of:

Pneumonic form infectious through the air, 12,359, 17,775, 18,111, 18,174.
Pneumonic plague infectious through the air by inhalation, I. 351, 20,622, I. 369.

See Sputum, *spread of infection by.*

Infectiousness of Pneumonic Plague:

Direct infection from patient to patient probable in pneumonic plague, 8663.
In rooms where cases of pneumonic plague treated, nurses and attendants suffer, except in properly arranged hospitals, 18,008–11.
Many instances of infection from pneumonic cases noted, 504–8.
More than one case of pneumonic plague noted in the same house—is the more contagious form of plague, 19,201–4, 19,218–20.
Plague pneumonia easily given to monkeys by the Russian Commission by spraying solution of virus in the air of the monkeys' cages, 22,362.
Pneumonic cases specially dangerous and give rise to pneumonic cases, 14,717–9, 14,750.
Pneumonic cases the infectious cases, attendants on other cases not attacked, 9586.
Pneumonia form spreads disease most, 4370.
Pneumonic plague infectious even in hospitals; *cf.* case of Dr. Manser, 1,439, 1,514–6.
Pneumonic plague highly infectious, 10,201.
Pneumonic plague noted for its spread of infection, 11,163.

Pneumonia and Mahamari:

No suspicion in the Hills of the existence of pneumonic plague until the outbreak at Hardwar, 8833, 8955.

PNEUMONIC PLAGUE—*cont.*

Pneumonia and Plague of the Bubonic type:

Bubonic case in one instance gave rise to combined bubonic and pneumonic case, 17,988–9.
Case of bubonic plague may become very infectious if pneumonia sets in, 6918.
Instance of infection of members of a family in Hardwar, one bubonic case followed by three bubonic and two pneumonic cases, 9558–60.
Mixed bubonic and pneumonic case gave rise to two pneumonic cases, 13,179–82.
No instance in which bubonic plague gave rise to pneumonic case, 19,208.
No instances of pneumonic plague from bubonic case or *vice versâ*, 14,123, 14,709, 17,785–7, 17,890, 18,206–7, 18,440, 19,075–6.
Pneumonia developed in bubonic cases, 14,121.
Pneumonic plague contracted from bubonic case, one instance, 16,048.
Pneumonic plague got by contact with bubonic cases, 12,728.
Six instances in which bubonic cases gave rise to pneumonic cases, 15,280.

Pneumonic Cases contracted from Pneumonic Cases:

Four cases of infection from pneumonic cases in Dharwar Hospital, all the persons infected getting pneumonia, and one having a bubo as well, 1891–2.
Four people got pneumonic plague, one after the other in one house, where there was no bubonic case, 15,454–8.
Infection of one type of plague from another type or of pneumonic cases from pneumonic cases not observed, 17,372–5.
Infection of 11 persons from one case of pneumonic plague in Cutch, 530, 13,366.
In one case one pneumonic patient infected ten people with pneumonic plague, in another one pneumonic patient infected two others with pneumonic plague, 18,207–13.
Nurse at Poona contracted pneumonic plague from a patient, 7576.
One case of pneumonic plague gave rise to another in attendant, 7515.
One case of pneumonic plague gave rise to two others, 18,509–10.
Outbreak of pneumonic plague at Backergunge: Details regarding, 6836–43, 7331–407, 7444–75.
Outbreak of pneumonic plague at Backergunge: Dr. Amulaya Charan Bose suffered from pneumonia but not from plague, 7067.
Person who slept in bedclothes of pneumonic patient contracted pneumonic plague, 19,208–35.
Pneumonic cases give rise to pneumonic cases, 14,717–9, 17,788.
Pneumonic cases gave rise to pneumonic cases only, four in one family and two in another, at Kankhal and Jawalapur, 9523–35.
Pneumonic plague: Cases of a laboratory assistant, Dr. Muller, and Nurse Pecha at Vienna, 7220, (17), (22), and (58).
Pneumonic plague contracted by nurse attending Dr. Manser, when he had pneumonic plague, 1439.

Pneumonic Plague communicates Bubonic as well as Pneumonic Plague:

Bubonic case contracted by medical attendance on pneumonic case, 9566.
Bubonic case contracted from bite of pneumonic patient, 4567.
Bubonic plague communicated by inoculation from pneumonic case, 7218.
Bubonic plague from pneumonic cases, two instances, 16,048.
In Gangaur from one case of probable pneumonic plague 37 deaths followed, both bubonic and pneumonic, 13 of the deaths being among the relatives of the first case, 15,182–5.
In one instance a pneumonic case gave rise to both bubonic and pneumonic cases among eight persons, who all died, 17,990–1.
No case of bubonic plague traced to contact with a pneumonic case, 12,729.

PNEUMONIC PLAGUE—*cont.*

No instances of bubonic cases from infection by cases of pneumonic plague or *vice versâ*, 14,123, 14,709, 17,785, 17,890, 18,206–7, 18,440, 19,075–6.
One case of pneumonic plague at Manora gave rise to one bubonic and three pneumonic cases, 12,997–8.
Pneumonic case gave rise to a bubonic case, 13,177.
Pneumonic plague communicates the bubonic form as well as the pneumonic, 1517.
Pneumonic plague, in one instance, gave rise to two bubonic cases, 18,214–6.
Six instances in which pneumonic cases gave rise to bubonic cases, 15,280.

Pneumonic Plague contracted from Bubonic Cases:

No instance in which bubonic plague gave rise to pneumonic case, 19,208.
No instances of pneumonic plague from bubonic case or *vice versâ*, 14,123, 14,709, 17,785–7, 17,890, 18,206–7, 18,440, 19,075–6.
Pneumonic plague contracted from bubonic case, one instance, 16,048.
Pneumonic plague got by contact with bubonic case, 17,728.
Six instances in which bubonic cases gave rise to pneumonic cases, 15,280.

Prevalence of:

Pneumonic cases most frequent in the height of the epidemic, 12,915, 13,104, 18,204.
Pneumonic form of plague not very frequent, 9507.

See Œdema.
,, Lungs.
,, Post-mortem appearances in plague.
,, Sputum.
,, Types of plague in particular epidemics.

PORT REGULATIONS:

Measures adopted and Staff:

Bombay: Evidence on port regulations, I. 34–8.
Bombay: Inspection applied to all crafts, 717.
Bombay: Notwithstanding the heavy work thrown on the Port Health Officers it was carried out in a most thorough manner, III. 363.
Bombay: Number of staff under Port Health Officer at Bombay, 716.
Madras: Procedure on lines of the Venice Convention, 3865.

Measures at Infected Places to prevent the Exportation of Plague:

Bombay: After inspection neither passengers nor crew may go ashore again, 732.
Bombay: All out-going crews and passengers inspected by health officer assisted by staff and two lady doctors, 26,638.
Bombay: Any person suffering from unexplained rise of temperature or from chronic enlarged glands detained, 725–6.
Bombay: Crew and passengers examined on shore if bound out of India, 720.
Bombay: Difficulty in collecting crew long enough before-hand for inspection, 794.
Bombay: Examined for second time on day of sailing, 723.
Bombay: In case person removed from a ship develops plague, telegram is sent to first port of call of ship, 742.
Bombay: Method of inspection, 721, 723.
Calcutta: Passengers and crew inspected on shore. All suspicious articles disinfected, 6455–6.
Cutch Mandvi: People own boats plying to Madagascar, African Coast, and Persian Gulf. These are not examined, and take no bill of health, 13,347, 13,513–5.
Karachi: All emigrants to Mombassa were detained at Kiamari, 11,989.
Karachi: All persons from detention camps sent to the steamers under police escort, 12,225.
Karachi: At one time 2,400 people in detention camp at Kiamari, 11,992.

PORT REGULATIONS—*cont.*

Karachi: Measures applied to out-going passengers, 12,222–25.
Karachi: Rules regarding emigrants, 12,231–3.
See Ships.

Measures at Uninfected Ports to prevent Importation of Plague:

Baluchistan ports: All passengers inspected. No cases seen, 8078–93.
Bombay: Arrangements for keeping under observation persons from infected ports or ships, 772.
Bombay: Inspection of vessels arriving started April 1897, modified April 1898, and applied to infected ports only, 768, 781–93.
Bombay: Medical officer goes on board all ships from infected ports, 784.
Bombay: Quarantine rules for ships coming from Hong Kong and Canton, 775.
Cutch Mandvi: All passengers, without exception, segregated for 20 days at Cutch Mandvi, while plague was very virulent in Bombay, 27,121.
Cutch Mandvi: Precautionary measures against Bombay and Karachi in quarantine camp at, 13,507–12.
Cutch Mandvi: Quarantine for passengers from Bombay and Karachi started in Sept. 1897; very successful, 13,344.
Cutch Mandvi: Stringent measures for passengers from Bombay and Karachi, 27,118.
Karachi: Guarded by harbour boats, 12,318.
Karachi: Native crafts from Bombay kept 10 days, 12,317.
Karachi: Passengers coming by sea from Bombay detained in Kiamari camp, 11,893, 12,151.
Karachi: Quarantine for passengers coming by sea unsuccessful, 4239–50.
Karachi: Quarantine tried for a year, but found too severe a measure and raised opposition, 12,194, 12,527.
Karachi: Regulations for the inspection of passengers arriving in port, 12,139.
Ratnagiri: Quarantine an essential measure as all traffic comes by sea, 20,042.

Measures for Disinfection of Clothing and Personal Effects:

Bombay: Disinfection of crews' kit and clothes by exposure to sun and air, 738, 814.
Bombay: Disinfection of every article taken on board ship impracticable, 811.
Bombay: Kits and baggage of persons free from plague not disinfected as a rule in December 1898, 741.
Bombay: Present rules for disinfection of all kit of crews at pilgrim shed, III. 299.
Calcutta: All suspicious articles disinfected, 6455.
Karachi: Clothing of passengers entering by sea disinfected with Jeyes' fluid, 4243.
Karachi: Clothing of passengers from Bombay not disinfected during first epidemic, 12,989.

Statistics of Results of Measures taken at Ports:

Bombay: List of vessels having left in 1897 and 1898, I. 361–2.
Bombay: Plague cases detected chiefly among native passengers departing for local ports, 749.
Bombay: Table of medical examination of crews and passengers on board ships, 25,479–83, App. X. in Vol. I., and App. LXXV. in Vol. III.
Bombay: Total number of ships examined, 50,177, 771.
Bombay: Very few genuine cases have gone on ships bound from Bombay for foreign ports, 767.
Bombay: 85 per cent. of cases reported as plague from ocean-going vessels recover; 85 per cent. of plague cases in Bombay die, 726.
Bombay: 149 plague cases among outgoing passengers detained discovered by inspection at Bombay, 747.
Bombay: 16,798 people detained at Bombay, 746.

PORT REGULATIONS—*cont.*

Bombay: 43,477 incoming passengers detained in observation camps, 48 cases of plague, 771–2.
Bombay: 87,571 crafts inspected at Bombay from January 1st to November 30th, 1898, 745.
Cutch Mandvi: 15,988 passengers examined from October 1897 to July 1898. Twenty-two cases sent to plague hospital, 27,122.
Karachi: Details of plague cases in detention camp, 12,982.
Karachi: In 1897 nine cases of plague was imported by sea, and in 1898 seven cases, 12,995–6.
Karachi: Of 35,360 passengers detained in camp five had plague, 12,161–2.
Karachi: Only one mild case of plague among 69,366 passengers and crew on board of vessels between August 1st, 1897, and December 1st, 1898, 12,145
Karachi: Seven cases of plague detected among in-coming passengers at Kiamari, 11,998.
Karachi: 7,310 people leaving by sea detained in April, May, and June, 12,237.
Karachi: 35,360 incoming passengers detained in detention camp from August 1st, 1897, to December 31, 1898, at Kiamari, 11,987.

See Bombay: *Introduction of plague into:*
,, Ships.

POST MORTEMS:

Done in all suspicious cases at Hubli for about three months in 1898, 3077–8, 20,007.
Objected to in India, 4615, 10,343, 19,531.

See Corpse Inspection.

POST-MORTEM APPEARANCES IN PLAGUE:

In Animals:

Greater variety in organs of guinea-pigs than in those of rats, 26,409.
Pathological appearances in animals experimented upon with plague, 26,407.
Post-mortem appearances the same in animals partially protected by inoculation with plague virus and those dying without previous inoculation, 27,363.

In Plague Patients:

Appearance of the several organs, III. 44.
Notes on 54 autopsies in Arthur Road Hospital, 20,972.
Post-mortem appearances of cases of bubonic plague, II. 65.
Post-mortem appearances of cases of internal bubo with hæmorrhages, II. 64*ab*.
Post-mortem appearances of cases of typical plague pneumonia, 6913.
Post-mortem appearances of pneumonic plague, 1418–33, I. 368–9.
Post-mortem appearances of septicæmic case, I. 439–40.
Post-mortem appearances of the septicæmic variety of plague, II. 64*b*.
Post-mortem of intestinal plague in which the mesenteric glands were found much enlarged, 9371.
Special appearance of septicæmic form after death, 21,365.
Summary of post-mortem examination of the different organs, 1419–26.

Post-mortem appearance:

Hæmorrhages: Description of internal hæmorrhages, 8898.
Hæmorrhage in every part of the body, 20,972.
Hæmorrhages: Small hæmorrhages in Peyer's patches seen in two cases, 6899.
Of alimentary system, I. 367.
Of brain: Engorgement of the vessels, but no softening or actual hæmorrhage, 1421.
Of genital system after death from plague, I. 368.
Of heart: Soft and flabby, 1421.
Of kidneys, 1421.
Of kidneys: Only in two case of post mortem kidneys found to be affected, 9622.

POST-MORTEM APPEARANCES IN PLAGUE—*cont.*

Of kidneys: Sometimes petechiæ on the capsule of the kidneys in bubonic plague, 10,421.
Of liver, I. 367.
Of nervous system in the bubonic form, I. 367.
Of peritoneum, I. 368.
Of spleen, I. 367.
Of urinary system, I. 367.
Petechiæ always found post mortem, 1420.
Petechiæ not seen on the skin, frequently on internal viscera, 9572.
Petechiæ of the stomach a noticeable feature in post mortems after bubonic plague, 10,419.
Petechiæ sometimes on the capsule of the kidneys in bubonic plague, 10,421.

See Blood.
,, Glands.
,, Hæmorrhage.
,, Kidneys.
,, Liver.
,, Lungs.
,, Petechiæ.
,, Œdema.
,, Stomach.
,, Spleen.
,, Urinary System.

PRECAUTIONARY MEASURES.

See Measures, Plague.

PRECAUTIONS, SANITARY.

See Measures, Plague.

PREGNANCY:

Abortion in a plague patient a short time before death, 12,987.
Pregnancy and miscarriage, prognosis bad, 15,308, 16,544.
Pregnant patients generally aborted on the first or second day of attack, I. 380.
Pregnant women miscarry when attacked by plague, 23,733.
Premature confinement, supposed due to plague, 12,818.
Recoveries rare in pregnant women, 20,864.
Woman died after confinement, but child was not attacked, 1451.

See Inoculation with Haffkine's plague prophylactic: *After effects* and *Bars to inoculation.*

PREPARATION AND CHARACTER OF HAFFKINE'S PLAGUE PROPHYLACTIC.

See Haffkine's plague prophylactic.

PROGNOSIS IN PLAGUE CASES:

Bad:

Aphasia, 13,030.
Buboes: Cases with small buboes generally fatal, 21,652.
Coma, 12,413.
Diarrhœa generally just before death, 20,850.
Exudation, rapidity of, 20,725.
Hæmorrhage, 12,413.
Lung symptoms, 19,078.
Petechiæ, I. 352.
Pregnancy and miscarriage, 15,308, 16,544.
Pregnant women rarely recover, 20,864.
Presence of bacilli in the blood, 17,402, 20,805.
Pulse intermittent, dicrotic, weak and irregular, 12,413.

Good:

Bubo, suppuration of, 1529.
Bullæ after the acute stage favourable, 16,816.
Disappearance of bacilli from the blood, 20,807.
Favourable and unfavourable symptoms, I. 382.
Gland if confined to the femoral region, 13,025, 15,308, 16,544.
Nervous symptoms, doubtful, 12,413.
Pulse and speech improvement, 15,310.
Survive the first five days, patients if, 12,415.
Urea and uric acid, free excretion, 20,860.

Children:

Better prognosis among children than among grown-up people, I. 352.

See Temperature.

PROPHYLACTIC, HAFFKINE'S.

See Haffkine's plague prophylactic.
,, Inoculation with Haffkine's plague prophylactic.

PROSTITUTES:

General belief that prostitutes under syphilis are immune from plague, 917, 8693, III. 43.

PULSE:

Clinical Feature of Plague:

Character of pulse in plague, I. 380.
Pulse feeble and dicrotic, but of varying character in different patients and different stages of an illness, 20,800.
Pulse similar to that of aortic regurgitation, 21,035.
Pulse small, easily compressible, and frequent, 12,398.
Sphygmographic charts of patients treated in the Arthur Road Hospital, Bombay, III. 547.

Prognosis:

Improvement in pulse good prognosis, 15,310.
Pulse intermittent, dicrotic, weak, irregular, 12,413.

QUARANTINE:

Baroda: Not successful in keeping plague out, 15,378.
Bhuj: A 15 days' quarantine maintained through the whole time of the epidemic, 13,407.
Bhuj: Plague kept out by the strict quarantine, 13,544.
Bulsar: Strict quarantine kept out plague for a long time, 24,447.
Cutch: Quarantine arrangements started in September 1897; very successful, 13,344.
Cutch: Ten days' quarantine for passengers coming by the land route, 13,344.
Karachi: Quarantine tried for a year but found too severe a measure, 12,194.
Satara: Quarantine, rules at, 22,262.
Villages: Difficulty of keeping relations of influential people out from infected villages, 22,993.
Villages: People learn to guard themselves against reintroduction of the disease by keeping outsiders away, 10,976.
Villagers quarantine strangers of their own accord, 3954.

Opinions regarding:

Quarantine a most essential precautionary measure, 20,042.
Quarantine an important measure, 22,380.
Quarantine cannot stop plague unless it can be extended to rats and other animals, 22,446.

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RABBITS:

Haffkine's plague prophylactic, effects studied in rabbits, 28–9.
Haffkine's plague prophylactic requires to be injected in very large doses into rabbits to afford protection, 5201–4, 5685.
Experiments in the Hyderabad State on rabbits for the identification of the plague bacillus in earth and in order to test the effects and sterility of Haffkine's prophylactic, 5164–78, 5215–92, 5293–414, 5590–1, 5713–7, 5744–79, 5915–25, 24,588–820, 24,917–21, 24,932–3, Vol. I. App. XVIII; Vol. III. Apps. LXIX. and LXX.
Experiments in the Hyderabad State on rabbits to test the presence of plague infection in raw cotton, 24,580–7, 24,820–94, 24,922–5, 25,903–9, Vol. III. App. LXVIII.
Presence of plague infected in houses tested in the Hyderabad State by inoculation of earth from the houses under the skins of rabbits, 5670–3.
Rabbits relatively resistant to plague, 8585.

RACE AS AFFECTING LIABILITY TO PLAGUE:

African: No African suffered from plague in Calcutta though many, and poor, 6970.
Bangalore: Statistics of plague according to race, I. 109, 25,015.
Bombay hospitals: Mortality from plague by race, I. 391.

RACE AS AFFECTING LIABILITY TO PLAGUE—*cont.*

Burmese and Chinese seem to possess some kind of immunity from plague, 6863.
Chinese: Not a single case of plague among Chinese or Japanese at Bombay during the three epidemics, III. 43.
Chinese: Only one Chinaman dies at Calcutta, 6862.
Chinese wear boots in Calcutta, 6863.
Distribution of plague cases among nationalities, 7073.
Influence of race on plague, 17,681–86, 22,264–7.
Japanese: Not a single case of plague among Japanese at Bombay during the three epidemics, III. 43.

See Inoculation with Haffkine's plague prophylactic.

RAILWAY MEASURES:

Adopted in:

Ahmednagar: Excellent railway precautions in, 24,217.
Bangalore: Adopted at, 3442.
Bombay: Commenced early in November 1896, I. 52.
Bombay: Statement of measures for the protection against plague at the several stations of the railways, I. 53–4.
Poona: System of railway precautions, 21,769.
Surat: Railway precautions, 15,539.
Variations in the plague regulations of railway stations most inconvenient to travellers, 21,616.

Detention and Observation Camps:

Ahmedabad: On November 3, 1897, observation camp established at, 13,891.
Anand: Detention camp opened on November 24th, 1897, and closed May 17th, 1898, arrangements in, 14,106–57, 14,165–93.
Bandra: Camps for observation of people leaving Bombay; each family had a separate hut, and castes were kept together: no spread of plague in camp, 8236–43.
Bengal: Detention at Khana till 10 days complete after leaving infected place, 6711–2.
Bombay: Detention camps employed till middle of 1898. New rules then instituted, including medical examination, disinfection, inquiry as to destination, and detention of suspicious persons, 1285.
Bhusawal: Observation and plague camps erected for the protection of Calcutta, the Central Provinces, and Bengal, I. 52.
Dharwar: Observation for people from infected localities, 1927.
Hubli: Ten days detention for all arrivals in, 2429.
Hyderabad State: Strict detention on railway and patrols on the roads leading from infected villages have kept plague from the city, 29,916.
Kaira district: Precautionary measures at the railway stations, 14,018.
Kotri: Only railway passengers with high temperature detained at, 12,666.
Malir: Camp for observation of people leaving Karachi; arrangements, 4256–62, 12,440–5, 12,498.
Malir: Ten days strict detention in Malir segregation camp, 12,495.
Rajputana: Detention at first for 10, then for five days, 9729–34.
Rajputana: Interpretation of the word "suspicious" by the Government of India and the Native States, with regard to detention on railways, 9786.
Rohri: Passengers from Karachi and intermediate stations detained at, 12,065.
Sibi: All passengers detained for not more than 10 days on coming into Baluchistan, 8086–91.
Surat: Passengers for, detained or kept under surveillance, 2062.
Tungabhadra: Detention for 10 days of suspicious passengers, 4490–515.

Detention Camps, Efficacy of:

Detention, if efficiently carried out, prevents the importation of plague beyond the camp, 14,189.

RAILWAY MEASURES—*cont.*

Observation camps and inspecting stations best means for keeping out the plague, 9728–9.

Detention Camps, Plague Cases in:

Bandra: Twenty cases discovered in passengers from Bombay, 8238.
Bengal: Six cases of plague detected, 6712–9.
Bombay detention camps: Number of plague cases in, 1291, 26,537.
Dadu: Twenty-seven cases taken out of trains developed plague, 12,672.
Malir: Few cases of plague out of thousands examined, 4263–5.
Malir camp: 108 cases of plague among the travellers by rail from Karachi, 12,430.
Poona: Twenty-three cases among 13,606 passes, 21,539.
Rajputana: 38 plague cases, 9729.
Rohri: One case developed in 266 detenus, 12,065, 12,072, 12,097, 12,100.
Sibi: One plague case in detention camp, 8092.
Tungabhadra: No plague case, 4510, 4516.

Detention Camps, Mortality in:

Table showing number of deaths among detained in Anand camp from causes other than plague, 14,187.

Detention Camps, Number in:

From 5,000 to 6,000 people in Anand detention camp at one time, 14,176.
From end of November 1897 to end of May 1898, about 58,000 people detained in camp at Anand, 14,177.
Hyderabad State: Number of passengers examined and detained by railway inspection from February 1897, 5789, 25,912.
Seven thousand nine hundred and twenty-five persons detained in Malir camp during April, May, and June, 12,429.

Detention Camps, attitude of the People towards:

Objection to detention camps on railways, but not to examination; attempts at evasion, 1298.
Objection to Malir camp on account of it interfering with trade, 12,528.
People object to detention and try to evade it, 1298.

Disinfection on Railways:

Bombay: Disinfection of railway baggage, 1285.
Disinfection at frontier stations only of third-class passengers who look dirty, 1285, 1312.
Goods trains: No disinfection of goods trains, 7298.
Infected carriage either disinfected at once or locked up till it can be, 1334.
Khana Junction: Method of disinfection adopted at, perfect but no proper supervision, hence clothes, &c. passed through without disinfection, 7494, 7497.

See Persons.
,, Clothes.

Medical Examination on Railways:

Believed to have prevented infection by railway into Calcutta, 6887.
Deterrent effect on travelling of plague cases or people who are probably infected, 1297.
Examination keeps away many who are sickening, 824.
If medical examination were taken off, the country would be flooded with plague cases immediately, 26,544.
Medical examination acts twofold; it discovers cases which go out, and prevents a great number from leaving, 26,538.
Railway inspection first line of defence against introduction of plague in a town, 21,767.
Stopping suspicious cases as efficacious as old system of stopping all passengers, 1302.
Train inspection a most salutary measure, 8753.
Under the precautions of careful medical examinations the exodus of the population from an infected town offers but slight risk, 26,537.

RAILWAY MEASURES—*cont.*

Value of railway examination, 26,537.
Value of railway inspections small, 4355.

Medical Examination, on Railways; arrangements for:

Ahmedabad: Railway inspection of passengers from Bombay commenced on October 5th, 1896, 77 cases detected to February 16th, 1897, 13,869, 13,886.
Anand: System of medical inspection of railway passengers, 14,107–8.
Baluchistan: Two cases detected by examination, 8092.
Bangalore: Medical examination of passengers leaving, and regulation forbidding departure without pass, 2586.
Bombay: Results of medical examination to middle of March 1898, 26,537.
Bombay: Results of railway inspection on the routes to, 1291.
Bombay: 221 posts of inspection gradually established at the principal stations and junctions, III. 363.
Calcutta: 41,854 people detained and six probable cases of plague resulted, 6715.
Central Provinces: About 20 plague cases stopped by examination by December 1898, 5952–6.
Female passengers examined by lady nurses, 12,865.
Guntakal: Inspection, I. 185–6.
Guntakal: 69,050 examined, 32 detained, one developed plague, 4500–2.
Guntakal: 111 suspicious cases removed from train, four developed plague, 5130–1, 5140.
Karachi and Malir: In first Karachi epidemic no cases detected, but 13 detected at Malir in second epidemic, 4256–66, 12,425–45.
N.W.P.: Two plague cases detected, 8753.
Occupants of infected carriage, if in good health, not detained, 1337.
Poona: Examination at first not strict, 12,863.
Poona: Forty cases detected in 1896–7, 12,850–3.
Poona: Medical examination of passengers departing in 1896, 2393–6.
Results of examinations at various railway junctions, I. 53–4.
Surat: Nineteen passengers detained, of whom 11 developed plague, 15,554–7, 15,871.
Table of results of medical examinations at certain stations for varying periods, I. 55.
Temperature made test for detention. Anyone over normal detained for 24 hours' observation, 1286–8, 1301.
Tungabhadra: 31,272 inspected, no plague cases, 4508–16.
Wardha district: Several hundred detained, about 20 cases, 5951–5.

RAINS:

Bangalore: Plague measures stopped on account of the rains, 2551.
Bengal districts: Rainfall is deficient during early months of 1898, 6874.
Bombay: Abnormal monsoon before outbreak in, 892.
Bombay: Heavy rainfall, increasing dampness of and rise of sub-soil water level preceded the present epidemic in, 25,268.
Cutch Mandvi: Epidemic influenced by damp weather and heavy dews, 13,485.
Germs become less virulent after rains, 22,356.
Hubli: Rains interfered with the work of plague staff, 1589.
Kolaba: Epidemic ceased during rains in, 17,650.
Kotri: Heavy rains and inundations before second outbreak, 12,593.
Rains assist spread of infection at Hubli, 2.
Rains assist spread of infection round Poona in 1897, 2.
Rains make preventive measures almost impossible, 2.
Rains make supervision and evacuation impossible, 1588–9.
Rains prevented plague measures in Bhiwandi, 22,176.
See Evacuation: *The rains and their effect on evacuation.*

RATS:

Arranged under the following subheads:

Bacillus of Plague, discovery of:
in bodies of dead rats.
in the blood of rats.
in the fæces of rats.
in the spleen of rats.
in the urine of rats.
Death of rats:
Before an outbreak of plague among men.
During and after Outbreak among Men.
Not followed by Plague among Men.
Not noticeable in connexion with Plague among Men.
Regarded as a warning of Plague and action taken upon..
Destruction and disappearance of rats.
Infection of Plague communicated and spread by rats.
Liability of rats to Plague.
Manner in which rats contract Plague.
Movements of Plague-infected rats.
Post-mortem Appearances.
Opinions regarding influence of rats in spreading plague:
Opinions that rats important Agents in spreading plague.
Opinions that rats not important Agents in spreading plague.

Bacillus of Plague, discovery of:

in bodies of dead rats:

Bacillus discovered in dead rats at Bangalore, 3592.
Bacillus found in dead rats at Bombay, 8705.
Bacillus found in dead rats at Hubli, 3117.
Bacillus of plague not found in mummified rats or any other material from the infected houses in the Joria Bazar at Karachi, 11,210.
None found in dead rats, gunny bags, or any material from the infected houses in the Joria Bazar at Karachi, 11,210.
Not found in supposed infected rats, 635.

in the blood of rats:

Invariably found in the blood of dead rats, if examined at once, 17,499.
None found in the blood of rats, 17,248.
None found in the blood of rats inoculated with earth from plague-infected rooms, 26,157.
Microbe disappears very quickly from the blood of dead rats, 17,500.

in the fæces of rats:

Found in the intestinal contents, 8602.
Not found in the excreta, 17,248, 17,410.
Reported as found by the German Plague Commission in the dung, 994.

in the spleen of rats:

Culture from the spleen of a dead rat gave typical plague bacilli, 10,235.

in the urine of rats:

Found in the urine, 17,248, 17,410.
Not found in the urine, 8602, 17,505.
Reported as found by the German Plague Commission in the urine, 994.

Death of rats:

Before an outbreak of plague among men:

Ahmednagar: Rats found dying before outbreak in 1898, 24,276.
Alibag: Many die before recrudescence of plague, 17,619.
Balutji: Die seven or ten days before first plague case, 5518.
Bangalore Lunatic Asylum: Discovered just before outbreak, 3510.
Baroda, Dave Muhalla: Rats died on importation of infected clothes, and persons died after, 15,170.
Baroda: Seen dying near spot where plague first broke out, 15,347.
Belgaum Jail: Dead rats found before outbreak of plague in which seven cases occurred in seven days, 22,870, 22,924-5, 23,047, 23,073.
Bombay: Dead rats found in godowns containing Chinese goods at Mandvi a fortnight before cases of plague occurred, 17,772.

Rats—*cont.*

Bombay: Death precedes outbreak among people, 1002, 1168, 8288, 10,961.
Bombay: First cases in August, rats die in September, 1176.
Bombay: Malabar Hill: Death of rats usually precedes outbreak of plague, 1375-7.
Bombay: Mandvi Ward: Die in large numbers where plague breaks out, 601.
Bombay: No striking mortality among, before epidemic, 997.
Bulsar: Found dead a week before outbreak among Dhebras, 16,164.
Calcutta: Dead rats preceded plague in seven out of 32 cases inquired into, 7188.
Calcutta: Rats die before human beings in certain houses in, 6451, 7187-8.
Calcutta: Human cases occur where dead rats had been previously found, I. 467.
Calcutta: Found dead before 1898 plague cases, I. 464.
Calcutta: Heavy mortality in March 1898 7503.
Calcutta: Epidemic among, 10 days before first reported case, 6468.
Calcutta: Sickness and death from plague were associated with unusual mortality among rats in houses affected, 7184.
Dead rats found near grain mart before epidemic, 3242.
Dead rats found in two houses 10 days before people were attacked, 10,454.
Death of rats usually, but not always, precedes plague, 25,218-20.
Death of rats precedes plague, as they spread the disease, 25,794.
Die in houses just before outbreak of plague, 1
Die before inmates in infected houses, 3425.
Found dead between an imported and indigenous case, 15,199.
Ghoti: Dead rats found in several parts before plague was discovered among human beings, 14,695.
Guntakal: After the first case rats died and probably spread the infection to the later cases, 5048-62.
Hale Tegur, and Mandihal: Plague followed infection of the rats, 23,558-9.
Hinganghat: Die in houses before outbreak, 6200.
Hyderabad State: Rats generally die before plague breaks out, 5854.
Hyderabad: Great mortality among rats between the imported cases and the first local case, 18,106.
Kankhal: Mortality among rats observed before the epidemic, 8774, 9074.
Karachi: Found dead in large numbers before second outbreak, 3785-90, I. 29.
Karachi: Great mortality among rats, on second outbreak rats beginning to die a week or two before first human case, 11,198-205.
Karachi: Kiamari: Plague cases appeared in the Customs lines soon after some dead rats had been found in the lines, 11,976.
Karachi: Mortality among rats precedes plague cases in houses, 11,313, 11,835-7, 11,974, 22,646.
Khandwa: Rats died on importation of infection, and a human case occurred later, 5958-61.
Kotala: Deaths before outbreak of plague, 4167, 5097.
Kotur: Rats died before spread of plague and spread was due to them, 23,612-25.
Ladghar: Rats died on importation of infected clothes, and men died afterwards, 20,066-73.
Local infection among human beings occurs generally three days after rats begin to die, 23,563.
Mortality in rats in individual houses preceding mortality among human beings in those houses, 18,005.
Mortality among rats invariably precedes that among men, 8906, 19,650-3.
Mortality among rats in 22 villages preceded outbreak of plague by an average of three days, 23,563.

RATS—*cont.*

Mortality in rats usually precedes human cases by about eight days, I. 464.
Mundra: Before plague became epidemic many dead rats found, 13,401.
Nasik: Dead rats found three days before indigenous cases, 14,705.
Rats seen in a dying condition in a house in Sotran where plague broke out five days later, 10,082.
Sukkur: Affected before outbreak, 290.
Vinzol: Discovery of dead rats followed after a fortnight by plague cases, 14,073.

During and after Outbreak among Men:

Ankleshwar: Several monkeys, rats, and squirrels died of plague, 14,243.
Bangalore: Great mortality after outbreak of plague, 2653.
Bantwa: Many found, particularly in a house with 16 cases, some of which occurred after disinfection, 17,565, 17,589, 17,595.
Baroda: Rats died in, 14,641.
Belgaum: No case in any village in which rats not also affected, 22,898.
Bhiwandi: Many rats and squirrels were found having died of plague. 14,493, 22,124–6.
Bombay: Mandvi: Rats died in large numbers when plague increased in virulence, 8704, 14,437, 19,159.
Die at same time as and after human beings, 7184. After, 7185.
Garshankar: Die in infected houses; not found while disinfection going on, 10,743.
Guntakal: Die in large numbers but not before first plague cases, 4139, 4169, 4171, 4177.
Hubli: Dead rats found in great numbers under the floor of a goods shed, 3117.
Hubli: Dead rats discovered in chawls and town after epidemic had begun, 2442.
Hubli: Hundreds dead in goods shed, 3117.
Jamalapur and Kankhal: Death of rats, 9189.
Karachi: Dead rats very numerous, 11,198–205, 22,646–7.
Kiamari: Rats found in numbers in the railway godowns during plague, 11,974.
Poona: Dead rats in 1899, 21,503, 25,831.
Satara: Dead rats found, 22,530, 22,681.
Sholapar: Dead rats found in every infected village, 24,048.
Sukkur: Heavy mortality at time of plague, 2269–73.
Timmencherla: Death of, in plague-infected houses, 5047, 5051.
Umreth town: Many dead rats found in villages during the epidemic, 14,076.

Not followed by Plague among Men:

Bombay: Dead rats found on ground floors and upper storeys of well-ventilated houses without the subsequent occurrence of plague among men, 25,220.
Bombay: Malabar Hill: Death of rats in a good house not followed by plague, 1447.
Bombay: Sherli village: An epidemic among rats in a village where no cases occurred among human beings, 18,966.
Daman Fort: Many found dead, no case among men supervening, 15,960, 16,449.
Doongri: Rats died without plague among men, 14,377.
Hyderabad State: In several cases rats died in villages where plague did not follow, 5854.
Poona: Death of rats in good houses not followed by plague, 21,538.

Not noticeable in connexion with Plague among Men:

Little evidence of rats mortality in Panjab in the autumn of 1897, but rats died everywhere next spring, 10,127.
Mortality among rats not an incident of plague at Nasik, 13,819, 16,962–4, 16,998, 24,512–4.
No dead rats at Ahmedabad, 13,929.
No dead rats at Rohri, 12,057.
No dead rats in first Belgaum epidemic, 23,068–72.
No dead rats in certain Dharwar villages where infection not so general as elsewhere, 23,571, 23,578.

RATS—*cont.*

No great rat mortality in Ahmednagar, 24,090–1, 24,241, 24,276, 24,332.
No great rat mortality noted in Surat, 15,887, 16,062.
No mortality of, observed at Karachi, before first outbreak, 11,098, 11,201, 11,202.
No mortality among rats or other animals at Khandraoni, 9675.
No mortality among rats in Kajurgi, where 22 plague cases occurred, 5504–6.
No rats died at Hardwar, 8913, 9448.
None found at Chipagiri; none at Molagavelli, 4411, 4445, 5011.
Not affected in Cutch during epidemic, 13,461.
Not affected in civil station at Karachi, where there was no plague, 11,345–52
Rat mortality not noticeable at Poona to February 1899, 7567, 10,923, 21,723.
Rat mortality not noticeable at Sholapur, 23,953.
Very few dead rats in Cutch Mandvi, 13,461, II. 480.
Very few rats in Nagar town during epidemic, 18,992.
Very few dead rats in Palanpur, 13,153–5.

Regarded as a warning of Plague and action taken upon:

Evacuation of village on appearance of dead rats and subsequent plague among the people probably contracted by visits to the evacuated houses, 23,215–24.
Good effects of evacuating villages in Dharwar on appearance of dead rats, 23,583–4.
Hamirpur: Evacuated on death of rats, no outbreak, 10,400–4.
Inhabitants of one village leave because of dead rats; no plague cases, 2116.
In the Broach district the health inspectors are required to report the discovery of dead rats even before the appearance of plague, 14,432.
Nasik: Dead rats cause evacuation of village, 9.
Only houses were evacuated in which plague cases occurred or dead rats were found in fort in Bombay among the Parsees, 25,247.
Palanpur State: Villages evacuated on appearance of dead rats. Plague appeared among the people of these villages after evacuation, 13,146–52.
Voluntary evacuation of Lasundra and Bahutha as soon as rats were found dying in the villages, 14,664.
Voluntary evacuation in N.W. Provinces following an outbreak of epidemic among rats, 8839.
Wasind evacuated on appearance of dead rats, and epidemic stopped, 19,654.

Destruction and disappearance of Rats:

Abnormal increase in number in Bombay in 1896, much damage to grain, 892.
Ahmednagar: Rats entirely disappeared after disinfection and evacuation, 24,330.
Bombay: Rats practically exterminated in the locality of the docks since 1896, 19,247, 19,329.
Half-anna paid for destruction; 50,400 paid for in 1898, 1367.
Khandraoni: Rats disappeared from the villages after plague, 9904.
Lascar's microbe killed white mice but not black rats in laboratory experiments, III. 324.
Marwaris object to their slaughter, I. 464.
Poisoning of, approved, 10,975, 16,251.
Poona: Reported all to have died, 7563.
See Habits of the people.

Infection of Plague communicated and spread by Rats:

Areh: Plague in village in houses in which dead rats found, 22,223.
Bandra District: Plague spread from village to village by rats and occurred on occupation of houses infected by rats, 23,207, 23,210.
Banga: Found infected, and infect the village, 10,235.

RATS—*cont.*

Bangalore: Only European who had plague had many dead rats and squirrels in his house, 2600.
Bhaynder: Infected by rats from Utan, 5 miles distant, 14,377.
Bhiwandi: Shop full of dead rats the centre of infection, 22,125.
Bite of diseased rat communicated plague, 1181.
Bombay: After dead rats found in a house, every one vacated it except one person, who remained, and 10 days after got plague, 1180.
Bombay: Dukar Gali: Plague in a boy after handling plague rat, 17,996.
Bombay: Granaries at Mandvi attracting the rats, the source of infection is continually renewed, 17,837.
Bombay: In a house in the Fort, on rats dying, and no precautions being taken, the servants and some of the European residents got plague, 20,416.
Bombay: Infected by sick rats from Hong Kong, 19,189.
Bombay: Malabar Hill: Instance of plague cases by sleeping in room where dead rats had 10 or 12 days previously been found, 18,190, 18,196.
Bombay: Rats probably spread disease all over Mazagaon and Byculla, 25,794.
Boys who killed rats in Mandvi fell ill, 997.
Case of individual contamination by dead rats, 11,418.
Children who eat dead rats in Kumaun not affected, II. 334.
Convey infection in an evacuated village, 10,141.
Coolies who cleared out dead rats from Bombay go-downs were not affected, 19,437–41.
Direct communication of plague to persons handling dead rats, 19,021.
Diseased rats taint articles with dung or urine, 992.
Evidence of infection being transmitted by, 10,109–18.
Excreta of rats supposed to cause infection, 15,459.
Fairlie Place: Mortality co-incident in one house with attack of nine workmen, 7182.
Hardwar and Kankhal: The epidemic among rats the connecting link between the outbreak, 8775, 8913.
Holihosur: People out for a month without plague; on their return plague occurred, rats having died in the village, 22,943.
Hunsikatti: Plague apparently spread by rats through the village while evacuated, 23,215–24.
Infection from infected rats through insects to human beings, III. 44.
Karachi: Plague brought by rats, 12,984.
Karachi: Rapid spread of disease through the voluntary evacuations and diffusion of rats in, 11,413.
Karachi: Three people got plague in house in which shortly before rats died, 11,421.
Karad: Plague in postmaster's house after rats died, 14,363.
Man removed dead rat and burnt it, and got plague after, 20,418.
Panjab: Banga: plague probably spread by rats, 10,235, 10,256.
Panjab: Chak Kalal: Plague on premature occupation of houses infected by rats, 10,109–10, 10,189, 10,603.
Panjab: Hamerowal, alleged to have been infected by rats, 10,113, 10,400.
Panjab: Mahlgahla: Plague on occupation of houses infected by rats, 9987, 10,348.
Panjab: Punian: Rats the source of infection, 10,077, 10,401–4.
Panjab: Rats appear to have spread the plague in several villages, 10,152.
Panjab: Rats probable agents of spread in Lidhar Kalan, Bika, and Garkshankar, 10,454, 10,456, 10,844–54.
Parli: Rats took plague from house to house, 19,647.
Plague case follows on a dog laying a dead rat on bed, I. 474.

RATS—*cont.*

Plague case follows on dead rat being trodden on, 16,513.
Poona Cantonment: Rats supposed to carry infection, but not very numerous, 10,923.
Poona: Cases in 1899 after rats died, 25,831.
Sirohi State: Rats probable source of infection in the, 9722, 9785, 9904.
Spread the plague by contaminating houses, 11,415.
Suspected of causing outbreak in Calcutta in 1898, 7118–23.
Thana: Two men sent to clean a house where dead rats had been found got plague and died, 220.
Three persons in one house got plague, one after the other, from sleeping on the only part of the floor attacked by rats, 23,598.
Umbowda Budruk: Plague follows on death of rats in a house, 22,223.
Woman contracted plague by throwing out dead rat and scrubbing with her hands the place where it was found, 17,926.

Liability of Rats to Plague:

Bombay rats frequently found immune, 26,311.
Rats observed in all parts of Mandvi running about as if drunk, 19,437.
Squirrels seem to die off more quickly than rats during the epidemic, 3120.

Manner in which Rats contract Plague:

Apparently not infected by other rats suffering from plague, 696.
Appearance of lungs in rats dead of plague suggest infection by inhalation, 17,247.
Calcutta: Instance showing that rats may have contracted the disease by gnawing body of a man dead of plague, 7185.
Contract infection before men owing to their connexion with soil, 17,366.
Damaged grain source of contagion to rats thence to men, III. 42.
Eat with impunity supposed infected bags, and artificially infected grain and bags, 635–42.
No data to prove that rats contract the disease from one another, 8906.
Somewhat doubtful whether rats affected through mucous membrane of the respiratory passages, 8703, 26,311.
Take infection from rat-case in Bombay laboratory, 17,498.
Very virulent plague would infect rats if fed with it, 8691, 8733.

Movements of Plague-infected Rats:

Dead rats found in Belgaum in the huts to which evicted people moved, 23,071.
Dead rats found in houses at Surat that had been closed for months, 2042.
Definite proof that rats carried infection along a distance of 600 yards, 22,262, 22,281, 22,437.
Evidence in favour of infection being carried by rats from one part of an evacuated village to another, 22,065–72.
Instance in which plague followed on the appearance of rats in one village and not in another, 14,377.
Instance in which plague was carried by rats to a considerable distance, 14,363.
Instance in which rats carried plague one mile, 23,215.
Kalindri: Plague carried by rats to a well outside the village, 9718–24, 9784–7, 9859–61.
Migrate from infected area to distance of two miles, 17,566.
Migrate on the occurrence of the disease, 1244.
Movements across Bombay from grain stores bring plague, rats moving in advance of and spreading the disease, 998, 1168, 1260–1, 8277, 8288.
No evidence obtained in Panjab that rats migrate in numbers from plague villages, 10,107.
No migration of rats observed in the villages near Baroda, 14,645.
Observed migrating over the causeways from Bombay, 19,237.

RATS—*cont.*

Opinion on observations in Dharwar that rats never move out of a village, 23,596.
Rats carried plague from Dharwar to a village ½ mile away, 2116–20.
Rats followed villagers into camp and from one camp to another in two villages in Satara, 23,012.
Rats found in a health camp just outside a village in the Panjab, 10,120.
Rats migrated ¾ mile to Bhagda from Bulsar and took plague, 16,190.
Rats present, found alive and not dead, in Arthur Road Hospital, Bombay, which they have not left on account of plague, 21,008–10.
Rats seen to go from house to house in Karachi, 22,646–7.
Sirohi State: Rats migrated from village to village carrying plague, 9784,
Some having died, others leave house, 16,473.

Post-mortem Appearances:

Greater variety in those of guinea-pigs than in those of rats, 26,409.
Pathological appearances in rats experimented upon with plague, 26,407.

Opinions regarding influence of rats in spreading plague:

Opinions that Rats important Agents in spreading Plague:

Animals do not seem to spread plague except, perhaps, rats, I. 378.
Danger of infection through clothes much less than through bags containing grain, in which rats would nestle, 994.
Disinfection of houses in India useless, as they are infested with rats which keep up the infection, 11,460–2.
General belief of epidemologists that rats are the principal carriers of infection, I. 464.
Infection chiefly spread by rats, 998.
Infection conveyed by rats, 17,251, 25,794.
Karachi: The wretched condition of the houses in the market quarter and their insanitary conditions enabled rats to spread the disease, 22,649.
Karachi Disease spreads very rapidly in the market quarter because plague broke out there and was assisted by unsanitary condition and also spread by the rats in the granaries of that quarter, 22,645–6.
Karachi: Rapid spread of disease through the diffusion of rats in, 11,413.
Little doubt that rats carry infection after it has been introduced by human agency, 22,693.
Mandvi, Bombay City: The granaries attracting the rats, the course of infection is continually renewed, 17,837.
Opinion that imported cases do not give rise to epidemics unless the rats are affected (Cutch Mandvi) II., 477 (*k*) (3).
Plague spreads by proximity through the agency of rats, 10,456.
Plague spreads by the agency of rats, but it also seems to travel of itself, as there are very few rats in some villages attacked, 22,618.
Poona: Undoubted infection by rats, though a localised one, 25,831.
Rats an important agency, 22,064–6.
Rats chiefly carry the plague from house to house in villages, 22,618.
Rats have much to do with spread; in two villages died 15 to 20 days before local cases, 17,779.
Rats more important agents in spread than men, 17,925.
Rats important agency; spread plague near grain market in Bangalore. 3229, 3240.
Rats chiefly, and also infected persons, spread plague, 3361.
Rats, infected before man, spread the disease, 14,072–8, 15,199, 18,107, 18,792–5, 19,650, 22,922, 24,044.
Rats and grain somehow responsible for infection, 8277.
Rats, clothing, and some unknown agent in the retail grain shops spread plague, 11,278.

RATS—*cont.*

Rats must convey contagion, for they are always found where plague is found, 17,891.
Rats play an important part in the propagation of the epidemic, 11,835.
Rats the most powerful source of infection, 19,028.
Result of experiments to determine the importance of rats in the spread of the epidemic, III. 324.
Spread infection where helped by insanitary conditions, 1446.

Opinions that Rats not important Agents in spreading Plague:

Did not convey infection to ships at Bombay, 17,677.
Doubtful whether they spread infection, 5498.
Epidemic at Palanpur cannot be traced to rats, 13,170.
Hubli: No epidemic noticed before outbreak of plague, 1579.
Infection not traced in Hubli district to rats, 1651.
Influence of rats in spreading plague has been exaggerated, 25,831.
No evidence how far they spread the disease, 5853–4.
Not charged with infection in Hubli district, 1651.
Plague spreads along lines of human communication, not by rats, 16045.
Rats become infected simultaneously with man, not before, 18,436–7.
Rats have little influence in disseminating plague, 17,678.

See also Ships.
„ Spread.
„ Virulence of bacillus of plague.

REACTION DUE TO INOCULATION.

See Inoculation with Haffkine's plague prophylactic.

RECRUDESCENCE OF PLAGUE.

See Recurrence of plague in infected towns and villages.

RECURRENCE OF PLAGUE IN INFECTED TOWNS AND VILLAGES:

Due to cause not traceable:

Recrudescence cannot be prevented by exertions in any big city, 7868, 7908–11.
Ahmednagar: Possible instances of villages infected in one year from infection that had lain dormant in neighbouring villages infected the year before, 24,099–106.
Alibag: Second epidemic started spontaneously in, probably from last year's germs which remained dormant, 17,620, 25,878.
Bandra: Recrudescence after 24 months, 19,086–94.
Baroda: Second epidemic began in October 1897, in the same part of the town as the first epidemic, 14,596, 15,335.
Bombay: Second epidemic, December 1897, 2.
Cutch Mandvi: Recrudescence began among the Salats, who escaped in the first outbreak, and was not due to imported cases, 13,411.
Dhamdachha: Without apparent cause of infection, fresh outbreak at, 15,119.
Kanaswadi: Supposed recrudescence in, 24,542.
Kotri: Local origin of the second outbreak, 12,710.
Kotri: Recrudescence without any obvious cause at, 3795, 3820.
Kotri: Date of last case in first epidemic, May 9th, 1897; first recognised case in second outbreak, October 28th, 1897, 12,580, 12,581.
Kotri: First diagnosed cases of plague in second outbreak dotted all over the town, 12,595.
Sirohi State: Recrudescence of plague, 9713, 9810.
Surat: First case in recrudescence had lived long in the town, and first four cases occurred together, their origin being untraced, 15,594.
Thana: Bassein: Recrudescence due to a permanent source of infection present, 18,778.

RECURRENCE OF PLAGUE IN INFECTED TOWNS AND VILLAGES—*cont.*

Due to Re-infection:

Most generally re-infection is due to re-importation, 22,717.
Nasik: Out of 200 cases only five in houses where plague appeared during first epidemic, 24,510.
Panjab: Recurrence of plague in villages due to re-infection, 10,051–2.
Poona: Infected a second time from Kirki, 21,579.
Poona, &c.: Second outbreaks in, may have been due to re-infection, 7911.
Thana: Recrudescence generally traceable to second infection, 18,797.

Severity of Plague on its:

Bandra: Recrudescence with increased virulence in, 18,926.
Bandra: Type of plague most virulent in third epidemic, 19,084.
Bombay: Third epidemic in, more virulent than the two first, 18,472, 17,888.
Daman: Second outbreak less severe than first, 58.
Karachi: First outbreak spread very slowly, second very fast, 11,105, 11,221.
Karachi: Second epidemic more virulent than the first, 3692.
Poona: Course of second epidemic, 21,683.
Poona: Third epidemic entirely different in its aspect from the former two, and very much more virulent and more rapid in spreading, 25,860.
Second epidemic more severe than the first in all localities, 7868.
The disease seems to increase in virulence with every repeated outbreak, 25,860.

RECURRENCE OF PLAGUE IN THE SAME HOUSE OR LOCALITY.

Adumbar: Outbreak of plague on people visiting houses to disinfect them, 22,372.
Ankleshwar: Instance of a person contracting plague by sleeping in an evacuated disinfected house, 14,412.
Bandra: Recurrence in the same houses visited by plague during first and second epidemic, 19,090.
Bangalore: More than one case occurred in 297 houses, 25,031.
Bantwa: Increase of plague when people driven back to the town from camp by a storm, 17,558–9.
Bantwa: Sixteen cases occurred in house in, some after disinfection, 17,589–92, 17,595.
Baroda City: Plague decreased on evacuation, but increased when people returned to city on account of rains, 15,234.
Baroda: Savali: Cases again occurred on return of people to their houses on account of rain, 14,658.
Bassein: Recurrence of epidemic in the same spot notwithstanding repeated disinfection, 18,779.
Belgaum: Sweepers' quarter evacuated and reoccupied in ten days, when more cases: residents were then cleared out indefinitely, 2090–7.
Belgaum: Several cases in disinfected houses, 22,835.
Bombay: After disinfection of chawls, plague always recurred in them on reoccupation, 25,790.
Bombay: A good house, 45, Argyle Road, continually attacked in spite of disinfection, 19,437.
Bombay: Cases appear after disinfection, 1211.
Bombay: Cases recurred in 1899 in same houses and same rooms as were infected before, 25,227.
Bombay: Large number of cases suddenly occurred in a particular chawl, 22,382.
Bombay: Many houses reinfected on reoccupation after disinfection, 20,439.
Bombay: Umarkhadi Jail; series of cases in persons sleeping in a particular part of the jail, 20,331–8.
Bulsar: Plague recurred on reoccupation in houses disinfected with perchloride and of which roofs were opened up, 16,236.
Cutch Mandvi: Bada, and Salaya: Plague on revisiting infected houses, 13,431, 13,456.
Cutch Mandvi: Numerous cases in same houses in first epidemic when people not removed; similar series of cases not noticed in second epidemic when houses vacated, 13,440.
Hinganghat: People from camp come to fetch grain from infected huts. Revives plague, 6008, 6024, 6055, 6069.
Hubli: A man who went to his infected house from camp sickened of plague within 11 days and died, 20,076.
Igatpuri: Failure of disinfection, and recurrence of plague in houses, 23,196.
Instances observed at Bulsar and Surat of recurrence of plague in disinfected houses, 24,451.
Karachi: Case of plague through surreptitious occupation of police lines when evacuated, 14,412.
Karachi: Plague on premature occupation of disinfected houses, 11,266, 11,268, 11,271, 11,276.
Karachi: Recurrence of plague cases in disinfected houses if only left vacant for 10 days, 25,198.
Kaira: Common occurrence of plague cases among persons returning to houses which were not infected at the time of evacuation, 14,070.
Kankhal: Attacks due to visits to Jawalapur for disinfection of infected houses, 9226.
Kotri: Death from plague in the only house which had escaped disinfection, 12,648.
Kotri: Case of recrudescence in disinfected house, 12,559.
Mysore City: Particulars of houses in which plague cases recurred, 25,031.
Nasik: Persons attacked at Trimbak on revisiting infected temple, 12,812.
Pademal: Plague on revisiting infected houses, 22,737.
Palanpur: Increase of plague on evicted people re-entering the town to visit temple for Diwali festival, 13,080.
Panjab: Compounder attacked after visit to infected house, 10,588–91.
Panjab: Plague in evacuation camps due to visits to infected houses, 10,758.
Poona: Cases often recurred in houses even after disinfection, 21,614.
Porbandar: Cases again broke out on premature return to the town on advent of rains, 31,678.
Satara: Jailor's house infected three times after thorough disinfection, 22,062, 22,137, 22,330, 22,384–5, 22,535, 22,549.
Satara: Ten cases observed of recurrence of plague in same houses, 22,549.
Sirohi State: Recrudescence of plague in, in houses disinfected by perchloride, 9712.
Sukkur: A large number infected by a plague case in temple, in which five disinfecting coolies also died, 18,403.
Surat: Cases during the second outbreak in the same houses and the same rooms where they occurred in the first, 18,473.
Surat: Plague reappeared in eight out of ten evacuated localities, 15,781, 15,818.
Surat: Recurrence of plague in thoroughly disinfected houses, 16,111.
Surat: Table showing the re-infected localities with period of freedom, II. 299.

See Camps: *Return of people from camps to evacuated houses, and spread of plague in.*
„ Evacuation: *length of period.*
„ Incubation period: *Incubation period in cases contracted by visits to infected places and houses or contact with the sick.*

RECURRENCE OF PLAGUE IN THE SAME PERSONS:

Baroda: A second attack of plague in a woman about a year after the first, 15,123, 15,133–42.
Belgaum: Case of boy who had two attacks, in November 1897 and February 1898; recovered, 23,103–7.
Bombay: Case of a man who died in Modikhama Hospital of his third attack, 19,348–54.
Bombay: Case of Ghella, who was twice attacked, 19,355–9.
Bombay: Case of Mr. Munchershaw, who had plague three times, 19,520–40.
Bombay: Dr. Davur's case, who died of plague in a second attack after recovering from a first attack and being inoculated, 18,017.

RECURRENCE OF PLAGUE IN THE SAME PERSONS—*cont.*

Bombay: Instance of a second attack of plague, 22,492.
Bombay: Instance of a person attacked by plague three times, 19,108.
Bombay: Mr. Champaklal Thakurdas twice attacked by plague, 19,541–96.
Bombay: Mr. Narayan Muljee, three distinct attacks of plague, 19,597–628, 20,525.
Bombay, Parel Hospital: Two cases, 7261.
Bombay, Parel Hospital: A sweeper who had plague in December got the disease again next March and died, 7,219 (3).
Bombay: Two cases recorded; one died, I. 381.
Cutch Mandvi: Case of second plague attack; recovered, 13,492, II. 471.
Daman: Person attacked in 1897; recovered, inoculated, and attacked a second time a month later, 16,554.
Daman: Case of fisherman who had three separate attacks within six months, 16,655, 16,704–40.
Daman: Case of a man who had two attacks, 16,652–4, 16,684–703.
Daman: Instances of persons attacked by plague a second time, 16,649–51, 16,673–83.
Panjab: A case of recurrence of plague in which the second attack was milder than the first, 10,830.
See Relapses.

REGISTRATION OF DEATH:

Opinions and Recommendations regarding:

Certificate of death, impossible in 90 per cent. of cases, 1126–40.
Death certificates would be very unpopular, 8014.
Decline of mortality from other causes during plague mainly due to defect in the report of the village officers, 23,289.
During the rains, reports take a long time to reach the Collector, 22,964.
Half the people are not attended by men qualified to declare cause of death, 7701.
Incorrect: Plague returned as chronic rheumatism, 8051–2.
Law should be passed making death registration compulsory within six hours, and removal penal, 10,990.
Native Assistant Surgeon class might be entrusted with the giving of death certificates, 26,761.
Obligation to report deaths generally imposed by Municipal byelaws but not enforced, 23,308–11.
Organised registration system expensive, but more beneficial than the present mode of dealing with plague, 10,993.
Returns forwarded to Deputy Sanitary Commissioner, 1629.
Returns take some time in getting from villages to Sanitary Commissioner, 7919.
Small towns, in, number of deaths approximately correct, but causes of death extremely incorrect, 8054.
Spread of plague among the uninoculated due, in Hubli, to removal of restrictions on the burial of patients without report of death before burial, I. 409.
System of death registration might be practicable in towns, but scarcely in the villages, 26,788.
System of death registration impossible to combat infective diseases, 26,732.
System of death registration without corpse inspection would be very valuable, 26,748.
Village officers inclined to report cases as plague to save themselves trouble, 3.
Village registers kept by the headman, 1627.
Village watchmen incompetent to ascertain cause of death, 10,283.

Particular remarks regarding, in:

Agra: Registration of births and deaths very inaccurate in, 10,280.
Ahmedabad; Death registration by counting the corpses which are carried through the gates, 13,877.
Ahmednagar: First case of plague discovered by death register in, 24,239.

REGISTRATION OF DEATH—*cont.*

Aminbhavi: Interval before increased death rate comes to official notice, 2201–10.
Ankleshwar: The burning grounds guarded by peons and patawalas, 14,298.
Bangalore: Only effective when paid agents were put on at, 2555.
Bombay and Bengal: Government trying to devise better scheme in, 7833.
Bombay: No system of death certificates, 1126.
Bombay: No system of registration of sickness or death, 10,962.
Bombay: Sole death registration at the cemeteries, 25,737.
Bombay: Notification of the cause of death obtained through the police, 17,899.
Bombay: The police and municipal registers of deaths quite correctly kept for the last 25 years, 17,751.
Bombay: Information obtained from friends at the cemeteries, 1115.
Bombay: If no evidence, plague is assumed as cause of death during epidemic, 1134.
Bombay: A large amount of under registration, but also an amount of over registration of plague, 907.
Bombay: Proposed system of improved death registration, 25,740.
Bombay: Allowing an office for every 5,000 inhabitants, 160 registration offices would be required, 10,991.
Calcutta: No system of death certificates at, 6512.
Calcutta: Hindus and Muhammadans dead against registration, 7549.
Calcutta: Number of deaths obtained at burning and burial grounds. Not cause of death, 6510.
Calcutta: Mortuary returns very unsatisfactory; they give an idea of the general number of deaths but not of the causes, 7522.
Calcutta: Many plague cases returned under false names, 7523–5.
Calcutta: Fever cases wrongly returned to avoid suspicion, I 471.
Central Provinces: Deaths in rural areas reported by policemen, 6139.
Cutch Mandvi: No death registration measures taken during the plague in 1897, statistics imperfect, 13,374.
Daman: Sepoys kept on the burying ground of the Mussulmans and cremation places of the Hindus to report the deaths, 16,417.
Daman: No regular compulsory death registration, 16,641.
Dharwar: Weekly return of deaths sent to each native magistrate of a village, who reports to collector, 1636.
Hardwar: System of registration organised during the presence of plague in the villages, 9205–9.
Hubli: A good many plague deaths escaped record, 1590.
Hubli: Death certificate system introduced during plague worked well, every case being seen and no burial allowed without certificate, 3075–7.
Hubli and Dharwar: System in, supervisors go round weekly, 1665–6.
Hubli and Dharwar: Ordinarily done by municipal authority; taken over by plague staff, 1623.
Hyderabad State: Death returns are kept with the village records by the headmen, police, and revenue patel, 5855.
Karachi: Death certificates from volunteers and municipal commissioners accepted for children under two years of age, 12,790.
Karachi: Death registration enforced by law, obligation to report in two days; period reduced to immediate report after plague outbreak, 11,290–1, 13,222.
Karachi: Death returns made by plague superintendent, 11,287, 11,386.
Karachi: Of death, very accurate at, 11,408.
Madras: Death returns as to number accurate, but not as to cause, 4353–4.
Madras Presidency: Method of, 3894–900.
Mysore: Most accurate, 24,257.
N.W. Provinces: The sweepers report deaths to the municipal office, 8855.

REGISTRATION OF DEATH—*cont.*

N.W. Provinces: No part of India in which the system of death registration is so efficiently carried out, 8858–62.
Panjab: Deaths registered on report by village watchmen to the police, 10,280.
Poona: a byelaw existed regarding reports of death, but was not enforced, 21,553–6.
Rohri: Death certificates required for disposal of dead; police observe burning huts and burial grounds, 12,026.
Satara: Punctual compliance with the order for notification of deaths within 72 hours, 22,566.
Surat: Death registration very deficient, 15,829–50.

REGISTRATION OF INOCULATED PERSONS.

See Inoculation with Haffkine's plague prophylactic.

RE-INOCULATION.

See Inoculation with Haffkine's plague prophylactic.

RELAPSES:

Case recovered on 15th November: recurrence of plague on 2nd December, 1229.
Gadag: woman who left the hospital cured after a mild attack got plague again in eight days, 23,710–7.
Relapse very common, 50 or 60 per cent. develop an iliac or pelvic bubo, 20,923.

See Recurrence of plague in the same persons.

RELAPSING FEVER:

Many cases sent to plague hospitals proved to be of relapsing fever in Bombay, 25,732.

See Diagnosis of plague.

RELIGION AND INFLUENCE OF RELIGIOUS HABITS ON LIABILITY TO PLAGUE:

Ankleshwar: Among Hindus (who are more numerous), majority of plague cases at, 14,953, 14,444, 14,448.
Bangalore: Plague and religion, 25,015.
Baroda: Statistics regarding, 14,989, 15,219.
Bombay: Hindus suffer worst at, because of their low standard of living, I. 352.
Bombay, Parel Hospital: Statistics of plague by religion, I. 257.
Christians in the village of Vadala sleeping in beds more immune than the Hindus sleeping on the floor, 19,446.
Christians show a high mortality at Calcutta, 6554.
Christianised Hindus, Bombay: The lower mortality among, attributable to their diet being better, 17,685.
Cutch Mandvi: More recoveries among Muhammadans than Hindus, 13,479.
Hindus more feeble than Muhammadans, and have less vitality, 27,041.
Hindus resist plague less than the Muhammadans, the resistance of lower caste being less than that of the higher, I. 349.
Influence of religious habits on plague, 17,681–6.
Karachi: Statement of cases by castes and religions, 11,827, 12,356.
Kotri: Hindus only attacked in first epidemics, 12,578.
Kotri: Of the 100 cases of the second epidemic, 77 were Hindus, 25 Muhammadans, 12,639.
Kumaun: Hindus very dirty, 6159.
Muhammadans not more liable to the disease than Hindus, 10,810.
Muhammadans suffered less than Hindus at Mandvi, 13,478.
Muhammadans singularly exempt at Surat, 15,635.
Muhammadans suffered most in Bangalore and Mysore, 25,016.
Muhammadans suffer severely at Bangalore, 2598.
Mussulmans suffer more than the other communities at Daman, 16,424.
Mysore City: Statistics of plague and race, 25,015.
Parsees in Surat, had 11 imported cases but no indigenous case, 15,632.
Parsees live in better parts of the town, and are clean in their habits, and suffer less than other communities, 12,358.

RELIGION AND INFLUENCE OF RELIGIOUS HABITS ON LIABILITY TO PLAGUE—*cont.*

Rohri: About 5 Hindus to every Muhammadan present during plague; 7 cases among Muhammadans and 111 among Hindus, 12,091–3.
Surat: Hindus suffered most, being more overcrowded than the Muhammadans; Parsees and Europeans living in good houses suffered little, 15,632–4, 15,637–8.
Surat: Statement of cases according to religions, 15,635, 15,642.
Well-fed Muhammadans and Memons little liable to infection and less attacked than Hindus at Karachi, 11,898.

See Inoculation with Haffkine's plague prophylactic.

REMOVAL OF MORIBUND PATIENTS TO HOSPITAL:

Condemnation of:

Bangalore: Removal of moribund patients considered an inhuman measure and forbidden, 2628, 2679.
Bombay: Moribund patients not removed 25,635, 25,560–1.
Chances of recovery reduced 50 per cent. if removed to hospital in dying condition, 12,494.
Patients should not be sent to hospitals when moribund, 2628, 7866.
People taken to hospital that would have recovered if let alone, 8126.
Poona: All patients except the moribund removed to hospital, 25,841.

Mortality in Hospitals of Patients within 24 hours of admission:

Ankleshwar Hospital: Of 141 deaths, 61 occurred within 24 hours of admission, 14,443.
Baroda: One hundred and forty-three out of 342 patients died in 24 hours, 15,266.
Bombay: Thirty-eight per cent. of cases died within 24 hours of admission in Arthur Road Hospital, Bombay, 25,734.
Calcutta: Many people taken to hospital in moribund condition, 6551.
Gadag: Of 334 admissions 60 died within 24 hours of admission, 23,692.
Karachi: Death-rate within 48 hours after admission to hospital, 60·4 per cent., 12,419.
Sassoon Hospital, Poona: 34·5 per cent. died within 24 hours of admission, 12,897.

REMOVAL OF THE SICK TO HOSPITALS:

Arrangements regarding:

Ahmedabad: Plague cases at once removed to hospital and contacts to segregation camps, 13,926.
Belgaum: Patients not removed to hospital unless voluntarily, 22,808.
Bombay: Many cases sent to plague hospitals proved to be relapsing fever in, 25,732.
Bombay: Measures directed chiefly to removal of patients to hospital, 17,865.
Bombay: Persons attacked with plague in, removed to hospital, 10,953.
Bombay: Policy to remove patients to hospital, 923–33, 25,559–61.
Calcutta: First epidemic; one of the principal measures, 6,520.
Calcutta: Second epidemic; policy of compulsory removal abandoned, II. 532 (6).
Concealment of cases threatened with removal to Government hospital, 13,157.
Daman: Plague patients removed to hospital, contacts left in the house, 16,369.
Karachi: Removal of the sick not enforced at first, 11,109.
Karachi: Many sick removed from infected to non-infected areas without permission, such removal prevented by making cases so removed liable to removal to hospital, 11,111–2.
Karachi: Sick segregated and removed to hospital later, 11,620–3, 11,952.
Poona: Sick in first epidemic removed, 10,914.
Surat: Disease reduced to 30 per cent. within one month by removal of sick cases to hospital, 16,005.

REMOVAL OF THE SICK TO HOSPITALS—*cont.*

Attitude of the people towards:

Bangalore: A source of dread and horror to the people, 3239.
Calcutta: Very bitter opposition to removal of patients, 6445.
Compulsion should not be used with regard to removal of patients to hospital or of contacts to camps, 25,095.
Daman: Carriage of sick to hospitals in bullock carts caused many deaths, 16,402.
Government hospitals: General dread of, and especial objection to be taken there on stretchers, 22,656.
Objection to remove the sick to Government hospital in Bombay, 17,865, 18,259.
Removal to hospitals disliked, 8260.
Removal to hospital an objectionable measure from Indian's point of view, 16,232.
Resentment owing to the manner people were conveyed to hospital, 17,952.
Satara: Removal of patients to plague hospital very offensive to people, 22,542.
Worrying the people about sending their patients to hospital leads to concealment, 25,806.

See Concealment of plague cases.
,, Isolation of the sick.
,, Removal of moribund patients.
,, Treatment of the sick in their own houses.

RESPIRATION:

Clinical Feature of Plague:

Grave cerebral and respiratory symptoms in cases without marked bubonic enlargement, 12,897.
Hurried and oppressive respiration marked in cases without bubonic swellings, 12,897.
Respiration generally hurried, voice disconnected and slurring, 12,399.
Respiration hurried, I. 380.
Respiration in pneumonic cases much more hurried than in other forms of plague, 12,914.

RETURN OF PEOPLE TO THEIR HOUSES.

See Camps. *Return of people from camps to evacuated houses.*
,, Evacuation. *Length of period of evacuation.*
,, Incubation period. *Incubation period in plague cases contracted by visits to infected places and houses or contact with the sick.*
,, Recurrence of plague in the same house or locality.

REWARDS.

See Informers.
,, Spies.

RIVERS:

Belgaum: Nearness of a place to a river has no connexion with the prevalence of epidemic, 22,954.
Dharwar: Plague spread along the river, but did not cross it, 23,227–40.
Satara: Epidemic confined to towns and villages along the bank of the river, 22,147.
Satara: Most villages in the affected talukan are on the river, 22,707.
Surat: River precautions, 15,543.

ROAD INSPECTION:

Baluchistan: All suspicious cases detained; no plague found, 8081, 8094.
Bijapur: Road posts to perfect Bagalkot from Kaladgi, 23,635.
Bombay routes guarded, and all but two closed to foot traffic, 1291.
Hubli and Dharwar: Road posts instituted, 1585.
Hyderabad State: Patrolling roads assisted in keeping out the plague, 25,916.
Hyderabad State: Roads watched, 5151.
Poona: Road patrols, 21,299–301, 21,327–8.
Sion Causeway: Seventy-three cases detected, 180 at Mahim Bandra Causeway, 1291.
Statistical results of inspection, 1291.
Surat: Road precautions, 15,541.

ROLL-CALL:

Ankleswhar: Daily roll-call at in order to detect cases, 14,273.
Bangalore: Census at Civil and Military Station during epidemic, I. 107.
Baroda: Residence in camp checked by roll-call, morning and evening, 14,620.
Baroda: Census of villages taken and checked, 14,651.
Bulsar: Census taken and carefully checked, 16,288, 16,296–306.
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Septicæmic plague gave rise to septicæmic plague in one case, 16,048.

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Grain infected by the excreta of rats disinfected by exposing it to sunlight, 9459.

Efficacy of Disinfection by Sunlight:

Benefits of disinfection by exposure to sun and air shown in Salaya, 13,474.
Exposure of infected area to sun and air the most beneficial measure of disinfection, 13,643, 17,646.
No plague after exposure of dug-up floors to sun, 23,778.
Sun does the work much better than chemicals 24,342.

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SUPERNATANT FLUID IN HAFFKINE'S PLAGUE PROPHYLACTIC.

See Haffkine's plague prophylactic, *constituents of.*

SURVEILLANCE:

Ahmedabad: Municipal inspectors to keep arrivals under observation are laymen, 13,871.
Bombay: System applied to persons landing from infected ports or ships, 772.
Broach: Guards at railway stations kept out people from infected areas; people coming in were kept under surveillance, 15,483.
Hardwar Union: Large area placed under an organised system of observation, 8869–70.
Nasik: Notification of arrivals from infected places within six hours, and inspection works well, 24,515.
Poona: System adopted at, 21,528, 21,769–71.
Surat: Passenger kept under surveillance, 2062.
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SURVIVAL OF BACILLUS OF PLAGUE.

See Period for which plague bacillus can survive.

SYPHILIS:

Belief that syphilis protects against plague, 8694.
General belief that prostitutes under syphilis are immune, 8693, III. 43.
People who have had syphilis survive a much more severe type of plague than a person who has not, 22,487.

TEMPERATURE:

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TEMPERATURE IN PLAGUE CASES:

In one case, 106, 1861.
Mild case, 99·4–101, 4550.
Pyrexia of plague remittent; of glandular fever intermittent, 6982.
Regular course much affected by complications. I. 379.
Remittent; difference of 1 or 1½ degree between morning and evening, 20,694.
Rises more quickly and is greater in children than in adults, 20,689.
Rises sometimes to 107 or more before death, I. 380.
Severe symptoms not always corresponding with a rise in temperature, 11,574.
Sudden fall generally associated with collapse and death, 20,699.
Temperature no criterion in plague cases, 18,312.
Temperature variable; no criterion of gravity of cases, 20,686–94.
Virulent cases 107 or 108, 20,686.
See Haffkine's curative serum.
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TEMPERATURE AFTER INOCULATION WITH HAFFKINE'S PLAGUE PROPHYLACTIC.

See Inoculation with Haffkine's Plague Prophylactic.

TONGUE:

Clinical Features of Plague:

Appearance of tongue varies with the character of disease, 20,831.
Appearance of tongue the only difference between the disputed cases at Bombay and Calcutta, 8214–5.
Condition of tongue most important, 8215.
Furred and patchy, 8222, 12,398, 12,937, 15,294.
Glossitis, 20,835.
No raw tongue seen at Bombay, 8224.
Paralysis of the tongue, 16,537.

Discovery of Plague Bacillus:

Found twice in the fur at the back of the tongue out of 20 cases, 27,055.

TRADERS.

See Banniahs and Traders.

TREATMENT OF THE SICK IN THEIR OWN HOUSES:

Ahmedabad: Isolated in the houses of the patients, first cases at, 13,866.
Bangalore: Measures at, relaxed to allow of home segregation, 3293.
Belgaum: Sick treated in their houses; epidemic increased, 23,815, 22,821–3.
Belgaum: In second epidemic patients allowed to remain in their own homes, with good results on reporting of cases and prevention of scattering of people, 23,138.
Bhiwandi: 1,625 sick treated in their own houses, and only 153 in hospital; results not good, 22,116–9, 23,359.
Bombay: Good results of isolating the sick in their own homes, 17,870.
Bombay: Isolation of plague patients in their own houses impossible in the chawls, 25,564.
Bombay: Under the present system of not removing patient to hospital, except by the desire of his friends, concealment of cases is the exception, 25,815.
Bulsar: Concessions, namely, treatment of sick in their own houses, given to inoculated, prevented the concealing of cases, 16,291.
Calcutta: Airy houses in Calcutta licensed as family hospitals with good results, 6588–9.
Calcutta: Second epidemic; policy of treating sick in their own houses adopted, II. 532.
Constant intercourse between the sick and healthy outside, the evil of home treatment, 12,178.
Home treatment does not tend to spread plague, and renders carrying out of disinfection easier, II. 537.
Home treatment of people sick with plague a very bad measure with regard to the spread of the epidemic, 12,332–6.
Home treatment ought not to be allowed in houses of a long row, 25,831.
Isolations in people's own houses under favourable conditions and proper precautions not dangerous, 22,510.
Permission to some people to treat their sick in their houses makes it more difficult to enforce isolation of others, 19,665.
Poona: Home treatment allowed with antiseptic precautions, 21,940.
Poona: In two cases where patients were allowed to be treated in their own houses, the results were so disastrous that the trial had to be given up, 25,828–9.
Poona: Parsees in Poona treated at home successfully, as they adopt hygienic measures while treating the sick at home, 12,879.
Treating patients in their houses a very bad measure, 22,118.
Treatment of sick in their houses at Mysore successful, as people reported cases, and were not impelled by fear to scatter and spread the plague, 24,960.
Unless a private house can be placed under the conditions of a hospital, home treatment of patients should not be attempted, 25,827.

TYPES OF PLAGUE:

See Atypical forms of plague.
,, Bubonic plague.
,, Bullæ.
,, Clinical features of plague.
,, Pestis minor.
,, Pneumonic plague.
,, Post-mortem appearances in plague.
,, Septicæmic plague.

TYPES OF PLAGUE IN PARTICULAR EPIDEMICS:

Anantapur district: Pneumonic cases not more than 4 or 5 per cent., 4462.
Ankleshwar: Very few pneumonic cases, 14,268.
Backergunge district: Course of pneumonic epidemic, I. 262–6.
Bombay: First epidemic bubonic, 19,067.
Bombay: In first epidemic cases easily distinguishable, 19,471.
Bombay: Second epidemic bubonic, and pneumonic from middle of epidemic in February and March, 1898, 19,067–70.
Bombay: In second epidemic no buboes or swellings, and cases hard to diagnose, 19,471.

TYPES OF PLAGUE IN PARTICULAR EPIDEMICS—*cont.*

Bombay: Third epidemic virulent, with much pneumonia 17,888.
Bombay: Third epidemic almost pneumonic in character, but with enlarged glands; in February 1899 mainly bubonic, very fatal, 19,071–5, 19,077.
Bombay: Epidemic 1898–9, at first primary pneumonia very frequent, at end of February very few cases, 21,043.
Bulsar: Non-bubonic plague appeared first in 1897, 16,314.
Chipagiri: Little pneumonic plague, 4461–2.
Cutch: At first of a virulent type, then chiefly pneumonic, then chiefly bubonic; in second epidemic more bubonic and less virulent, 13,488–93.
Cutch: In Salaya chiefly pneumonic and septicæmic, 13,490.
Cutch: In the epidemic at Sarli no pneumonic cases 13,619.
Daman: Epidemic of 1898; a form of plague noticed, accompanied by bullous inflammations, 16,794.
Daman: Pneumonic plague with other types of plague found, 18,794, 18,842.
Gharibabad: Was infected with pneumonic plague, 11,163.
Hardwar: Majority of cases bubonic, pneumonic cases noted after January 1898, 9170–1.
Hyderabad (Sind): In 1897 epidemic bubonic, with few pneumonic cases, 18,093–4, 18,151–3.
Hyderabad (Sind): A fairly large number of pneumonic cases, 18,439.
Kotri: Cases mostly bubonic, 12,700.
Nasik: Only 5 pneumonic cases which did not occur in the same house, 14,705–11.
Poona: Virulence of type of imported cases since February 1899, shown by the proportion of deaths, 25,827.
Porbandar: Only 6 pneumonic cases treated in hospital, 13,724–7.
Satara: Pneumonic plague cases not numerous, 22,153.
Sukkur: Pneumonic plague fairly common, 18,439.

Pneumonic Plague: Prevalence of Pneumonic Cases in Particular Epidemics:

Cases with lung symptoms vary in frequency at different periods of an epidemic, 21,042.
Cutch: Pneumonic cases most prevalent in, at height of epidemic 18,202–4, I. 352.
Palanpur: Pneumonic cases observed in, chiefly in the height of epidemic 13,104–5.
Poona: Pneumonic cases more frequent in, when the epidemic most severe, 12,915.

See Pestis minor.
,, Virulence of plague in different phases of an epidemic.

UMARKHADI COMMON JAIL, BOMBAY, PLAGUE IN, AND INOCULATION WITH MR. HAFFKINE'S PROPHYLACTIC IN.

A number of cases took place in one particular spot in spite of its disinfection with quick lime, 20,332–46.
Clinical records of two persons inoculated in the Jail and subsequently attacked by plague, one case resembling mumps, 69, 7236, I. and II.
Correspondence with Mr. Haffkine regarding the correctness of the inoculation returns relating to the Umarkhadi Jail, III. 658–61.
Inoculations performed in the Umarkhadi Jail, 69.
No selection of cases for inoculation in the Umarkhadi Jail, 20,115–31, 26,500.
Particulars of the inoculations in the Umarkhadi Jail in January 1898, 17,099–112.
Prisoners not selected for inoculation, 20,311–6.
Re-inoculations in the Umarkhadi Jail, 11,714–27.
Report by Lieut.-Colonel Waters, I.M.S., on the inoculations in the Umarkhadi Jail, 113, 117.
Results of inoculation, 20,317–31.
Statement of particulars regarding inoculation and plague in the Umarkhadi Jail, Bombay, III. 441–4.
Umarkhadi Jail much overcrowded during the epidemic of plague, 20,341.

UNDHERA, PLAGUE IN, AND INOCULATION WITH MR. HAFFKINE'S PROPHYLACTIC IN:

Ages of the inoculated, 14,910, II. 510.
Census made of Undhera, and mortality figures prepared by a subordinate Medical Officer on the spot, carefully checked house by house, statistics for inoculated and uninoculated, 4,035–61.
Census taken at the beginning of the epidemic, 14,892–900, II. 504.
Deaths among uninoculated most numerous directly after the inoculations were performed, 18,708–13.
Gross mortality for three months before the outbreak of plague, 14,882.
Gross mortality from plague and other causes after the introduction of inoculation, 14,912–5.
Gross mortality from plague and other causes and plague attacks, 14,908–9, II. 510.
Inoculations performed at Undhera, 69.
No selection of cases for inoculation in Undhera, 26,500.
Report by Mr. Haffkine on inoculations in Undhera, I. 331–6.
Statement of weekly gross mortality for three months before December 22nd, 1897, II. 266.
Statistics of plague and inoculations in Undhera, 26,535.
System of selection for inoculation, 14,902–7, 14,918–9.
Table of gross mortality before the first case of plague, 14,882.
Total number of uninoculated persons, 14,885–92.
Undhera figures carefully checked, results accurate, 7872–8.
Undhera, statistics checked and investigation sheets prepared, 161–3.

UNROOFING:

Bangalore: People object to removal of roofs, 3419.
Better class houses disinfected with chemicals, poorer houses unroofed, 23,924.
Bombay Presidency: Houses unroofed, 8.
Dharwar: Houses unroofed, 23,163, 23,748, 23,773.
Dharwar: In the villages chief reliance on unroofing, 1680–2.
Entire unroofing of infected houses the most effective measure of disinfection, 22,667.
Kalindri: Resorted to as disinfecting method, 9894.
Karad: Roofs taken off and opened up, 21,078.
Panjab: Entire roof removed and ventilation holes made, 10,027.
Poona: Houses unroofed or partially unroofed, 10,933, 21,289.
Satara: Sadar Bazar in first epidemic unroofed as a precaution, and escaped; in second epidemic not unroofed, and suffered, 22,273–4.
Satara: Strips of the roof, three feet broad, taken off, 22,157.
Sholapur: Poorer houses unroofed, 23,924.
Unroofing recommended for villages as the application of the measure can be easily checked, 21,156.
Unroofing the great thing as disinfecting method, 1681.

See Houses, disinfection of.
,, Ventilation.

UNTILING:

All small houses in Bombay untiled, 1240.
Calcutta: Untiling resorted to, 6532.
Cutch Mandvi: Tiles removed about a yard in width, 13,474.
Dharwar: Houses untiled, 23,434.
Disinfection by untiling roofs at Sholapur, 8.
Guntakal: Openings made in roofs, 5099.
Hubli: Houses untiled, 2404.
Palanpur: Untiling resorted to as method of disinfection, 13,091–4
Removal of tiles must be on generous scale, 6616.
Sirohi: Roofs untiled, 9911.
The removing of the tiles from the roofs only good in the case of basement houses, 13,474.
Untiling adopted in Bangalore and Mysore, 25,003.

UNTILING—*cont.*

Untiling useful both in admitting light and air, and also in driving rats out of roofs, 1220.

See Houses, disinfection of.
,, Ventilation.

URINE:

Clinical Features of Plague:

Urine diminished in quantity, of high specific gravity, and intensely acid, I. 380.

Discovery of Bacillus in:

Bacillus found in, before death, 3556–63, 3647–9.
Bacillus found in, in 2 cases before death, 7200–3, 7270.
Bacillus cultivated after death, 1436.
Bacillus of plague will only be found in urine when it contains blood, 22,367.
Bubonic plague found in the urine of patients dead of, 17,261.
Can be cultivated from urine after death, 14,367.
Found in urine during life, 3556, 3564, 7189.
Found in urine when it contains blood, 22,367.
Not found in urine in about 15 examinations, 25,358–62.
Not found in a number of cases examined, except once, when bacillus recovered from organs of animal inoculated with the urine, 26,268, 26,336.
Only twice found in urine in 20 or 30 cases, 7206–7.
Only twice isolated from the urine in 50 plague cases, 26,389.
Results of 50 analyses at Bombay Hospital, I. 396–7.
Results of 50 examinations of urine at Bombay Hospital, I. 396–7.

Life of Bacillus in:

Bacillus of plague grows more slowly when obtained from the urine than from the blood, 26,337.
Lives 24 hours in urine at least, 3611.

Prognosis:

Free excretion of urea and uric acid good prognosis, 20,860.

See Rats.

URINARY SYSTEM:

Discovery of Bacillus in:

Recovery from the urethra of a patient dead of bubonic plague, 17,258.

Post-mortem Appearances:

Of urinary system, I. 367.
Of urinary system in septicæmic case, I. 440.

VACCINATION DEPARTMENT:

Advantages of classifying native vaccinators, and promoting by merit, 7769.
Great advantages to have Hospital Assistants among vaccinators, 7770.
Organisation for vaccination, 7751.
Qualified Assistant Surgeons in Vaccination Department would be of material help in detecting cases of plague, 8884.
Scheme by which Assistant Superintendents of Vaccination would be Assistant Surgeons with a knowledge of sanitation, 8881.

VENTILATION:

Attitude of the People towards:

Ahmednagar: People close up windows and ventilation holes made, and enforced opening of windows necessary every morning, but people may in time come to appreciate ventilation openings, 24,203–9, 24,361–2.
Bangalore: People now ask for their houses to be ventilated, 3269.
Bangalore: People object to openings in their houses, 3419.
Baroda: The people, except the Machis, seem to approve of improvements in ventilation, 19,159–65.
Desire to secure privacy excludes ventilation, 867.

VENTILATION—*cont.*

Native servants close up all ventilation openings in their quarters, 20,539.
Panjab : People close up ventilation holes as soon as possible, 10,271.
People apt to close up the openings made in their houses when allowed to return, 12,077.
People close up ventilation openings at night, 19,515.
People plaster up holes made for ventilation, 23,404.
Satara : People more inclined to ventilation, 22,432-4.

Disinfection by :

Ahmedabad : Ventilation resorted to as disinfecting method, 13,923.
Ahmednagar : Openings made in houses to admit the sun, 24,274.
Baroda : Uninfected houses cleansed, white-washed, and ventilated as disinfecting precaution, 14,631.
Panjab : Ventilation holes made in roofs and side walls to disinfect, 10,481.
Surat : Houses opened up, and then swept out and doused with perchloride solution, 24,434.

Effect on Plague :

Bad ventilation the causes of the disease 14,716.
Bad ventilation one of the principal factors in the propagation of the disease, 14,388.
Cases less frequent among those who sleep in the open air, 7517.
Connexion between ventilation and the disease, 916.
Contagion lessened by ventilation, 12,881, 18,569.
Great difference in the intensity of plague in ill-ventilated and well-ventilated houses, 14,390–1.
Infective material retains its vitality for a long time in badly-ventilated houses, 9725.
No plague after ventilation of dug-up floors in Kalgeri, Dharwar, 23,778.
Over-crowding and bad ventilation the causes of the disease, 14,716.
Plague at Kiamari principally in badly-ventilated houses, 11,971.
Remarks on conclusions derived from experiments as to the efficacy of ventilation, II 13.
Satara Jail : Infection did not spread on account of good ventilation, 22,330.
Statistics of effect of ventilation on disease, 916.

In various Places :

Ahmednagar : Ventilation arrangements in houses bad, 24,275, 24,347.
Bangalore : Ventilation in houses bad, 3254,
Bombay : Houses badly ventilated, 26,645–8.
Bombay : Houses inefficiently ventilated from narrow galis, 835.
Bombay : No ventilation at night in bad houses, 847.
Bombay, Kamatipura : Houses low and badly ventilated as well as over-crowded, 17,834.
Cutch Mandvi : No ventilation in houses, except by the door, 13,355.
Dharwar : Ventilation bad, 1711, 23,185, 23,518.
Hubli : Houses remarkable for their bad ventilation, 20,144, 20,207.
Karachi : No free passage for air in houses at Kiamari, 11,971.
Karachi : Ventilation much improved since epidemic, 22,664.
Nasik : No ventilation except by the door, 13,786–7, 16,983.
Satara district : Many houses in the Taluka have not been ventilated for years, 22,734.
Surat : Most localities absolutely without ventilation, 15,937, 16,107–8.
Surat : Only ventilation often through door, 15,937.
Thana district : Houses poor, with no outside opening but door, 19,690–3.

Opinions regarding Ventilation as a Plague Measure :

Chemical disinfection unnecessary if vacated for three months and ventilated, 24,040.
Disinfection by ventilation, II. 13.

VENTILATION—*cont.*

Houses should be permanently improved by ventilators and windows, 22,667.
Ventilation as effective as disinfection by chemicals only slower, 23,259.
Ventilation best method of disinfecting, 23.662.
Ventilation holes in roofs very useful way of disinfection, 10,026.
Ventilation the most important sanitary measure to be enforced, 19,515.

See Air and Light.
,, Desiccation.
,, Sanitary Conditions.
,, Unroofing.
,, Untiling.

VIRULENCE OF BACILLUS OF PLAGUE :

Attenuated through the effects of involution, 8520.
Attenuated by being passed through rats, 8675, 26,311–4.
Change in the bacillus of plague after it has left the human body suggested to account for infection not spreading in camps, 10,488.
Increased by a series of intra-peritoneal passages through guinea-pigs, 26,434c.
Increased by filth, 18,635.
Increased by filth : Weak or attenuated germs require environment of filth to cause development of plague, 18,633.
Increased by passing through animals, 18,641.
Increased by passage through monkeys, 26,547-8
Increased by passage through rats, 17,423.
Increased by passing through mice, 8677.
Maintained by passages through rats, 26,464.
Plague germs become virulent after the rains, 22,356.
Plague germs may be of a non-pathogenic nature, and of different degrees of pathogenic viralence, 18,630.
Test of virulence by inoculation into the peritoneum surer than by subcutaneous injection, 17,413.

VIRULENCE OF PLAGUE IN DIFFERENT PHASES OF AN EPIDEMIC :

Lessened towards Decline of Epidemic :

Cases live longer towards the end of an epidemic, 17,714.
Disease becomes milder in form towards the end, 18,102.
Larger percentage of patients recover at the end of an epidemic, 21,047.
Most marked decrease in virulence at the end of the epidemic in Savali, 14,667.
Virulence abates towards end of epidemic, 15,907.
Virulence in Baroda lessened at close of epidemic, 14,242, 14,634.
Virulence of epidemic always subsides towards the end, 15,112.
Virus becomes weaker towards the end of the epidemic, 13,741.

Not Lessened towards Decline of Epidemic :

Cases as virulent during the subsidence as at the beginning, 12,512.
Does not decrease towards close of epidemic, as in cholera, 6938.

Statistics bearing on :

Ankleshwar, 14,404.
Igatpuri, 19,493.
Ilkal, 19,493.
Porbandar, II 484.

See Types of plague in particular epidemics.

VITALITY OF BACILLUS OF PLAGUE.

See Period for which plague bacillus can survive.

VOLUNTARY EVACUATION :

Advantages of :

Although evacuation is only partial in voluntary camps, it decidedly reduces the number and virulence of the attacks, 11,901.
Intelligent voluntary action of the people will soon reduce the expenditure involved in, 26,543.

VOLUNTARY EVACUATION—*cont.*

Securing of all contacts one of the advantages of voluntary camps, 11,821.
Voluntary camps of great help to the Government officials, no pressure being needed, 11,903.
Voluntary evacuation works much better than coercive measures, 11,265.

Arrangements for:

Baroda City: Forty thousand people in fields round the city, not in Government camps, 14,677.
Baroda: Unauthorised camp at, under no organised control or supervision, 14,627.
Broach: Have their own organisation and administration at, 15,503.
Broach: Camp arrangements at, voluntarily and successfully carried out, 15,484.
Cutch Mandvi: Borahs went into segregation camps in, and suffered little, although under no control, 473, 566.
Cutch Mandvi: People refused to go to Government camps, but 20,000 left the city, and, as they were prevented by guards from scattering, camped in gardens around, 13,378–9.
Karachi: Boundaries of voluntary camps at, 11,869.
Karachi: Conditions under which voluntary evacuation was allowed, 11,868.
Karachi: Largest voluntary camp at, that of the Baluchis, containing 2,851 persons, 11,911.
Karachi: Trans Lyari, good success of people appointing their own plague committee in the voluntary camps of the, 11,258.
Karachi: Thefts being frequent in voluntary camps, the people applied for regular police protection, 11,883.
Karachi: Twenty-five thousand people in voluntary camps, 11,260.
Karachi: In most of the voluntary camps some Banniahs' shops allowed at the extreme limit of camp, 11,882.
Kotri: Health camps started in second epidemic at, by Seth Vishandas and Muhammadans, 12,655.
Mat huts for the sick in voluntary camps, 11,888.
Not a good plan to have one large camp; the more broken up the better, 5459.
Parsees freely resorted to evacuation at Bombay, 8294.
Supervision: Inspection of the voluntary camp at Nagar by the Abkari police, 18,982.

Attitude of the People towards voluntary Evacuation:

Bangalore: Done promptly and voluntarily by villages around, 3303.
Baroda villages: Voluntary evacuation as soon as rats were found dying in the villages, 14,664.
Carried out by villagers of their own accord after some experience, 9.
Cutch Mandvi: 20,000 persons segregated themselves of their own accord in the gardens round, 13,383.
Karachi: Nassarpuris: Voluntary evacuation, 11,113, 11,120.
Karachi: Lyari, voluntary camps across the, popular, 12,525.
N.W. Provinces: Voluntary evacuation following an outbreak of epidemic among rats, 8839.
Palanpur: People of, built themselves huts in the Mango groves outside the town, 13,084.
Surat: People at, refused to go to Government camps, but made camps of their own, 15,914.
Twenty-six thousand at one time in camp at Karachi, 11,631.
Voluntary evacuation of a whole village at the first appearance of plague, 21,642.

Efficacy of:

Baroda: Mortality in the unauthorised camps at, much less than if people had remained in town, 14,680.
Cutch Mandvi: Not a single plague case among 1,000 persons camped for 2½ months along the seashore, 13,389.

VOLUNTARY EVACUATION—*cont.*

Impossible to prevent people in voluntary camps from going backwards and forwards to their houses, 11,679.
Karachi: Death-rate rapidly decreases in voluntary camps in, compared with the total mortality in the town, 11,784.
Karachi: Mortality in voluntary camps in, compare favourably with the mortality in the town, 11,682.
Karachi: The securing of all the contacts one of the principal advantages of voluntary camps in, 11,821.
Karachi: Table of total number of cases in the voluntary camps at, II. 149.
N.W.P.: Jawalapur: Voluntary evacuation not so effective as Government camps, as huts crowded and not ventilated, and property taken out without disinfection, 9179.
Rohri: The unsanitary condition of the ground in the date plantations the cause of the persistence of the plague in the camp, 12,120.
Surat: In the unauthorised camps at, huts crowded and plague as rife as in the town, 16,016.
Surat: Voluntary camps closed at, as supposed to spread infection, 24,419.
Surat: Plague at once ceased in the voluntary camps at, as soon as they were placed under the control of the ward system, 16,018.

See Camps: *Provision of Accommodation, Food, &c. by the People.*

VOLUNTEER AGENCY:

Bangalore: Volunteer agents failed altogether at, 2553, 3273–6.
Bombay: Native gentlemen serve on plague committees, 5, 1344.
Bombay: Nagpada, one of the best native committees, 1345.
Bombay: Plague committees organised by natives far from efficient in some districts, 1347.
Bombay: Reports from volunteers at present principal source of notification, 25,556.
Bombay: 300 Native assistants do valuable work; description of services, 1345.
Bombay: Voluntary system of reporting plague cases very imperfect, 25,553.
Calcutta: Formation of Vigilance Committee, I. 467.
Calcutta: Vigilance Committee fails to remove contacts, 7610.
Evidence of the relative merits of carrying out plague measures forcibly, and of leaving them to be carried out by the people under a voluntary system is unmistakably in favour of the latter, 24,980.
n large places only possible methods are enlisting the co-operation of the inhabitants themselves, 10,963.
Mysore: Voluntary system worked well at very small expense, 24,956, 25,091.
Natives should be employed as voluntary agents for obtaining early information, 26,730.
Poona: Best results of plague measures in those Pets where voluntary agency was largely represented, 25,866.
Poona: Military intimidation replaced by volunteer system during second epidemic, 21,699.
Poona: Native gentlemen entrusted with plague operations in each ward under supervision of a European officer, 21,528.
Poona: Valuable assistance from better class inhabitants, 10,965.
Satara: Voluntary vigilance system, 22,166.
Surat: Native gentlemen's assistance of great value, 5.
Surat: Vigilance committee of native gentlemen for the discovery of plague cases, 15,635.
Voluntary organisations for the detection of plague more successful than military search parties, 18,387.

See Ward system.

VULTURES.

See Birds.

WALLS :

Microbe generally located in walls, 9682.
Not found in walls at Hyderabad, 5781.
Not found in walls or roof more than an inch deep I. 442.
Not found in wall scrapings, 5199.
Walls burned up to two feet from the floor to disinfect at Hyderabad, 5781.
Walls scraped and burnt two feet up to disinfect, upper part untouched, 5849.

See Disinfection, *Houses : extent of disinfection.*
,, Soil, *Assertion, &c.*
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WARD SYSTEM :

Bangalore, Civil and Military Station: Ward system, 2572.
Bombay : Division and organisation of wards, 1344–9.
Calcutta : Arrangements for wards, 6757.
Hubli : Ward system successful in discovering cases *after* death, not very effective *before*, 20,167.
Hubli : Ward system worked well at first, when cases were few, 19,799.
Hubli : Ward system failed when the plague panic set in at, 19,803.
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68 70 72 74 76 78

32 30 28 26 24

SIALKOT
CHAMBA
DERA ISMAIL KHAN
AMRITSAR
HOSHIARPUR DISTRICT
LAHORE
KAPURTHALA
JULLUNDUR DISTRICT
GARHSHANKAR
SIMLA
LUDHIANA
QUETTA
DERA GHAZI KHAN
MOOLTAN
PATIALA
UMBALLA
HARDWAR (JAWALAPUR)
SIBI
SIRSA
BAHAWALPUR
MEERUT
DELHI
SHIKARPUR DIST
SHIKARPUR
SUKKUR
ROHRI
BAREILLY
BICKANEER
ULWUR
AGRA
JEYPORE
JODHPORE
AJMERE
HYDERABAD DISTRICT
PACHPADRA
GWALIOR
KHANDRAONI
KARACHI DISTRICT
KOTRI
HYDERABAD
JHANSI
KARACHI
2720
SIROHI
MOUNT ABU
OODEYPORE
GUNA
PALANPUR STATE
RANN OF CUTCH
CUTCH STATE
CUTCH MANDVI
PANCH MAHALS
BHOPAL
AHMEDABAD DIST
REWA KANTHA
KAIRA

PUNJAB
RAJPUTANA
BOMBAY AGENCY
CENTRAL INDIA

80 82 84 86 88 90

32 30 28 26 24

BRAHMAPUTRA

LHASA

SHIGATZE

JANGLACHE

KUMAUN

ITAL

NEPAL

KATMANDU

DARJEELING

OUDH

LUCKNOW

GORAKHPUR

COOCH-BEHAR

DURBHUNGA

TIRHOOT

SARAN

PATNA

MONGHYR

BENARES

ALLAHABAD

MALDAH

MYMENSINGH

MOORSHEDABAD

DACCA

FARIDPUR

HAZARIBAGH

BENGAL

BURDWAN

HOOGHLY

HOWRAH

BACKERGUNGE

CHYBASSA

BULPORE

AWNPORE

INCES AGENCY

RIVERS

GANGES RIVER

BRAHMAPUTRA RIVER

KATHIAWAR
RAJKOT
PORBANDA
BHAUNAGAR
BARODA
BARODA TERRITORY
BROACH
ANKLESHWAR
SURAT
SACHIN STATE
SURAT DISTRICT
BULSAR
DAMAN 1,000?
DHARAMPUR
THANA DIST
NASIK
IGATPURI
BASSEIN
BHIWANDI
BANDRA
BOMBAY 9091
PANVEL
ALIBAG
KOLABA DIST
POONA 1334
JANJIRA
BHOR
RATNAGIRI DISTRICT
SATARA DISTRICT
AUNDH
MASUR
KARAD
RATNAGIRI
KOLHAPUR STATE
TASGAON
KOLHAPUR
SAVANTWADI
GOA
KARWAR
KANARA DIST
BELGAUM
HUBLI
DHARWAR
GADAG
MANGALORE
CANNANORE
KHANDWA
KHANDESH DIST
JALGAON
MALEGAON
BHUSAWAL
AMRAOTI
WARDHA
NAGPUR
HINGAN
PALASKHEDA
MANMAR
AURANGABAD
AHMEDNAGAR DISTRICT
AHMEDNAGAR
BHIR
PERANDA
DHARASEO
TULJAPUR DISTRICT
NILANGA
SHOLAPUR DISTRICT
SHOLAPUR
NALDURG
KALIANI
GANJOTI
AKALKOT
GULBURGA.
HYDERABAD
BIJAPUR
AMRABAD
RAICHUR
MUDGAL DIST
LINGSUGAR DIST
KUSHTAGA DIST
YELBURGA DIST
KOPBAL DIST
KURNOOL
HOSPET
BELLARY
MADDIKERA
GUNTAKAL JUNC.
ANANTAPUR
CUDDAPAH
CHITALDROOG
HINDUPUR
TUMKUR
BANGALORE
KOLAR
HOSUR
MYSORE
OOTACAMUND
SALEM
CENTRA
HYDERABAD
MYSORE
20
18
16
14
12
68
70
72
74
76
78

Eyre & Spottiswoode, Lith.

No. I.

MAP OF INDIA

SHOWING

THE NUMBER OF RECORDED DEATHS FROM INDIGENOUS PLAGUE

From the Commencement of the Epidemic of 1896 to March 31st, 1897.

1–50 deaths were recorded at all places printed in red, but where a name is underlined the recorded deaths exceeded 1–50 by 50 for each dotted red line, by 100 for each thin red line, and by 500 for each thick red line.

Where recorded deaths exceeded 1,000, the actual numbers are given in connection with signs as follows:—

for deaths from 1,000 to 5,000.

,, ,, 5,000 ,, 10,000.

,, ,, 10,000 ,, 20,000.

,, over 20,000.

The number of deaths in districts is exclusive of the deaths in towns such as Karachi, Poona, Dharwar, Bhiwandi, &c., for which the state of plague is shown separately.

Scale

50 0 50 100 150 200 250 300 Miles

68 70 72 74 76 78

32 30 28 26 24 22

SIALKOT
CHAMBA
DERA ISMAIL KHAN
AMRITSAR
HOSHIARPUR DISTRICT
LAHORE
KAPURTHALA
JULLUNDUR DISTRICT
GARHSHANKAR
SIMLA
LUDHIANA
QUETTA
DERA GHAZI KHAN
MOOLTAN
PATIALA
UMBALLA
HARDWAR
HAWALAPUR
SIBI
SIRSA
BAHAWALPUR
MEERUT
DELHI
SHIKARPUR
SUKKUR
DISTRICT
ROHRI
BICKANEER
ULWUR
AGRA
JEYPORE
JODHPORE
AJMERE
GWALIOR
PACHPADRA
HYDERABAD DISTRICT
HYDERABAD
KOTRI
KARACHI
KARACHI
DISTRICT
JHANSI
KHANDRAON
SIROHI
MOUNT ABU
OODEYPORE
GUNA
RANN OF CUTCH
PALANPUR STATE
CUTCH STATE
CUTCH MANDVI
3815
PANCH MAHALS
REWA KANTHA
AHMEDABAD DIST
KAIRA
BHOPAL
INDORE
JAMNAGAR
ANAND JUNC
BARODA
RAJKOT
BARODA TERRITORY
KATHIAWAR

P U N J A B

R A J P U T A N A

B O M B A Y

A G E N C Y

C E N T R A L I N D I A

80
82
84
86
88
90
32
30
28
26
24
22
LHASA
SHIGATZE
JANGLACHE
KATMANDU
DARJEELING
COOCH-BEHAR
DURBHUNGA
GORAKHPUR
LUCKNOW
CAWNPORE
TIRHOOT
SARAN
PATNA
MONGHYR
BENARES
ALLAHABAD
MALDAH
MYMENSINGH
MOORSHEDABAD
DACCA
FARIDPUR
HAZARIBAGH
BURDWAN
HOOGHLY
HOWRAH
CALCUTTA
BACKERGUNGE
24 PARGANAS
CHYBASSA
JUBBULPORE
BILASPUR
NAINITAL
RHWAL
KUMAUN
BRAHMAPUTRA
GOGRA RIVER
GANGES RIVER
SOANE RIVER
HIMALAYAS
OUDH
BENGAL
AGENCY

L PROVINCES

SAMBALPUR

CUTTACK

GANJAM

VIZAGAPATAM

RAJAHMUNDRY

MASULIPATAM

MADRAS

ARCOT

PONDICHERRY

ANGHAT

ORE

80

82

84

86

88

20

18

16

14

12

No. II.

MAP OF INDIA

SHOWING

THE NUMBER OF RECORDED DEATHS FROM INDIGENOUS PLAGUE

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Scale

50 0 50 100 150 200 250 300 Miles

LONDON.

68 70 72 74 76 78

32 30 28 26 24 22

SIALKOT
CHAMBA
DERA ISMAIL KHAN
AMRITSAR
HOSHIARPUR DISTRICT
LAHORE
KAPURTHALA
GARHSHANKAR
JULLUNDUR DISTRICT
SIMLA
LUDHIANA
QUETTA
DERA GHAZI KHAN
MOOLTAN
PATIALA
UMBALLA
HARDWAR (JAWALAPUR)
SIBI
SIRSA
BAHAWALPUR
MEERUT
DELHI
SHIKARPUR
SUKKUR
ROHRI
BICKANEER
ULWUR
AGRA
JEYPORE
JODHPORE
AJMERE
GWALIOR
PACHPADRA
JHANSI
HYDERABAD
KOTRI
KARACHI
SIROHI STATE
MOUNT ABU
OODEYPORE
GUNA
PALANPUR STATE
RANN OF CUTCH
CUTCH STATE
1053
CUTCH MANDVI
PANCH MAHALS
BHOPAL
AHMEDABAD
REWA KANTHA
KAIRA
ANAND JUNC.
INDORE
JAMNAGAR
BARODA
1226
BARODA
RAJKOT

P U N J A B

R A J P U T A N A

B O M B A Y

A G E N C Y

C E N T R A L I N D I A

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32 30 28 26 24 22

BRAHMAPUTRA

LHASA

SHIGATZE

JANGLACHE

ARHWAL

KUMAON

NAINITAL

NEPAL

KATMANDU

DARJEELING

OUDH

GORAKHPUR

LUCKNOW

CAWNPORE

RIVERS

COOCH-BEHAR

DURBHUNGA

TIRHOOT

SARAN

PATNA

MONGHYR

BENARES

ALLAHABAD

MALDAH

MYMENSINGH

AGENCY

MOORSHEDABAD

HAZARIBAGH

BENGAL

DACCA

FARIDPUR

BURDWAN

JUBBULPORE

HOOGHLY

BACKERGUNGE

HOWRAH

CHYBASSA

CALCUTTA

24 PARGANAS

PORBANDA
BHAUNAGAR
BROACH
ANKLESHWAR
KHANDWA
TAPTI RIVER
SURAT 560
SACHIN STATE
SURAT DIST 1144
BULSAR
DHARAMPUR
DAMAN
KHANDESH DIST
BHUSAWAL
AMRAOTI
WARDHA
MALEGAON
PALASKHEDA
NASIK DISTRICT
MANMAR
NASIK
AURANGABAD
IGATPURI
BASSEIN
THANA DIST
AHMEDNAGAR DISTRICT
AHMEDNAGAR
BHIR
BANDRA
PANVEL
BOMBAY 13,564
POONA DIST 1316
KOLABA DIST
POONA 4778
JANJIRA STATE
BHOR STATE
PERANDA
DHARASEO
TULJAPUR
DISTRICT
NILANGA
SATARA DIST 7562
SATARA
SHOLAPUR DISTRICT
NALDURG DIST
KALIANI
SANJOTI
AUNDH STATE
SHOLAPUR 2191
AKALKOT STATE
MASUR
KARAD
GULBURGA DISTRICT
HYDERAB
RATNAGIRI
TASGAON
BIJAPUR DISTRICT
RATNAGIRI DISTRICT
KOLHAPUR STATE
AMRABAD
BELGAUM DISTRICT
RAICHUR
MUDGAL DIST
SAVANTWADI
BELGAUM
LINGSUGAR DIST
KUSHTAGA DIST
KURNOOL
YELBURGA DIST
GOA
GADAG
DHARWAR
KOPBAL DIST
HUBLI
HOSPET
BELLARY
MADDIKERA
KARWAR
KANARA DIST
GUNTAKAL JUNCN
ANANTAPUR
CHITALDROOG
CUDDAPAH
HINDUPUR
TUMKUR
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MANGALORE
HOSUR
MYSORE
CANNANORE
OOTACAMUND
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EYRE & SPOTTISWOODE.

L PROVINCES

SAMBALPUR

CUTTACK

GANJAM

VIZAGAPATAM

RAJAHMUNDRY

MASULIPATAM

MADRAS

ARCOT

PONDICHERRY

ANGHAT

HOOGHLY RIVER

20

18

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80

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LONDON.

68 70 72 74 76 78

32 30 28 26 24 22

QUETTA
SIBI
DERA ISMAIL KHAN
DERA GHAZI KHAN
SIALKOT
CHAMBA
AMRITSAR
LAHORE
HOSHIARPUR DISTRICT
KAPURTHALA
JULLUNDUR DISTRICT
LUDHIANA
GARSHANKAR
SIMLA
PATIALA
UMBALLA
MOOLTAN
BAHAWALPUR
SIRSA
HARDWAR (JAWALAPUR)
DELHI
MEERUT
SHIKARPUR
SUKKUR
ROHRI
BICKANEER
ULWUR
AGRA
JEYPORE
JODHPORE
AJMERE
GWALIOR
PACHPADRA
JHANSI
KOTRI
KARACHI
KARACHI 2513 DISTRICT
MOUNT ABU
PALANPUR STATE
OODEYPORE
GUNA
RANN OF CUTCH
CUTCH STATE
CUTCH MANDVI
PANCH MAHALS
REWA KANTHA
KAIRA
KAIRA DIST
ANAND JUNCTION
BARODA
BARODA TERRITORY
BROACH
JAMNAGAR
RAJKOT
KATHIAWAR 1095
INDORE
BHOPAL
BAREILLY

P U N J A B
R A J P U T A N A
B O M B A Y
A G E N C Y
C E N T R A L I N D I A
C E N T R

80 82 84 86 88 90

32 30 28 26 24 22

BRAHMAPUTRA

LHASA

SHIGATZE

JANGLACHE

RHWAL

KUMAUN

NAINITAL

NEPAL

KATMANDU

DARJEELING

COOCH-BEHAR

GORAKHPUR

LUCKNOW

CAWNPORE

DURBHUNGA

TIRHOOT

SARAN

PATNA

MONGHYR

BENARES

ALLAHABAD

MALDAH

MYMENSINGH

MOORSHEDABAD

DACCA

FARIDPUR

HAZARIBAGH

BURDWAN

HOOGHLY

HOWRAH

CALCUTTA

BACKERGUNGE

24 PARGANAS

CHYBASSA

JUBBULPORE

BILASPUR

OUDH

GANGES RIVER

JUMNA RIVER

GOGRA RIVER

SOANE RIVER

BRAHMAPUTRA RIVER

TWA RIVER

AGENCY

BENGAL

SURAT
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BULSAR
DAMAN
DHARAMPUR STATE
NASIK DISTRICT
KHANDESH DIST
JALGAON
MALEGAON
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AMRAOTI
WARDHA
PALASKHEDI
MANMAR
THANA DIST
NASIK
IGATPURI
AURANGABAD
BASSEIN
BHIWANDI
AHMEDNAGAR
BHIR
BANDRA
BOMBAY
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KOLABA DT
ALIBAG
POONA DISTRICT
POONA
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DHARASEO
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NALDURG DIST
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BIDAR DISTRICT
GANJOTI
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AKALKOT STATE
AUNDH STATE
MASUR
KARAD
GULBURGA DISTRICT
HYDERAB
RATNAGIRI
RATNAGIRI DISTRICT
TASGAON
BIJAPUR DISTRICT
KOLHAPUR
KOLHAPUR STATE
2763
BELGAUM DIST
895
MUDGAL DIST
RAICHUR
AMRABAD
SAVANTWADI
BELGAUM
LINGSUGAR DIST
KUSHTAGA DIST
KURNOOL
YELBURGA DIST
KOPBAL DIST
BELLARY DISTRICT
GOA
GADAG
DHARWAR
HUBLI
2477
HOSPET
BELLARY
MADDIKERA
DHARWAR DISTRICT
4478
KARWAR
KANARA DIST
GUNTAKAL JUNCN
ANANTAPUR DISTRICT
ANANTAPUR
PENNE
CUDDAPAH
CHITALDROOG
HINDUPUR
TUMKUR
KOLAR
BANGALORE
MANGALORE
HOSUR
MYSORE
CANNANORE
OOTACAMUND
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EYRE & SPOTTISWOODE

SAMBALPUR

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VIZAGAPATAM

RAJAHMUNDRY

MASULIPATAM

MADRAS

ARCOT

PONDICHERRY

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No. IV.

MAP OF INDIA

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50 0 50 100 150 200 250 300 Miles

LONDON.

68 70 72 74 76 78

32 30 28 26 24

SIALKOT
CHAMBA
DERA ISMAIL KHAN
AMRITSAR
HOSHIARPUR DISTRICT
LAHORE
KAPURTHALA
JULLUNDUR DISTRICT
SIMLA
LUDHIANA
QUETTA
DERA GHAZI KHAN
MOOLTAN
PATIALA
UMBALLA
HARDWAR (JAWALAPUR)
SIBI
SIRSA
BAHAWALPUR
MEERUT
DELHI
SHIKARPUR
SUKKUR
ROHRI
BICKANEER
ULWUR
AGRA
JEYPORE
JODHPORE
AJMERE
GWALIOR
PACHPADRA
JHANSI
KOTRI
KARACHI
MOUNT ABU
PALANPUR STATE
OODEYPORE
GUNA
RANN OF CUTCH
CUTCH STATE
CUTCH MANDVI
PANCH MAHALS
AHMEDABAD DISTRICT
KAIRA
REWA KANTHA
BHOPAL

P U N J A B
R A J P U T A N A
B O M B A Y
A G E N C Y
C E N T R A L I N D I A
W. R.

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BRAHMAPUTRA

LHASA

SHIGATZE

JANGLACHE

KUMAUN

NAINITAL

NEPAL

KATMANDU

DARJEELING

COOCH-BEHAR

OUDH

LUCKNOW

GORAKHPUR

CAWNPORE

RIVERS

DHARBANGA

TIRHOOT

SARAN

PATNA

GANGES

MONGHYR

MALDAH

BRAHMAPUTRA

MYMENSINGH

INCES

BENARES

ALLAHABAD

SOLANE RIVER

AGENCY

MOORSHEDABAD

RIVER

DACCA

FARIDPUR

HAZARIBAGH

BENGAL

BURDWAN

HOOGHLY

JUBBULPORE

HOWRAH

BACKERGUNGE

CHYBASSA

KATHIAWAR
RAJKOT
PORBANDA
BHAUNAGAR
BARODA
BARODA TERRI 1909
BROACH
ANKLESHWAR
BROACH DIST
SURAT
SACHIN STATE
SURAT DIST
BULSAR
DHARAMPUR
DHARAMPUR STATE
THANA DIST 1656
BASSEIN
BHIWANDI
BANDRA
BOMBAY 10,633
ALIBAG
KOLABA DISTRICT
JANJIRA
JANJIRA STATE
BHOR STATE
POONA
PANVEL
POONA DISTRICT
RATNAGIRI DISTRICT
SATARA DIST 6294
SATARA
AUNDH STATE
MASUR
KARAD
SHOLAPUR DISTRICT
SHOLAPUR
RATNAGIRI
TASGAON
BIJAPUR DISTRICT 1793
KOLHAPUR STATE 7974
BELGAUM DIST 10,596
BELGAUM 1448
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GOA
GADAG
DHARWAR
HUBLI
DHARWAR DIST 22,000
KARWAR
KANARA DIST.
MANGALORE
CANNANORE
OOTACAMUND
KHANDWA
KHANDESH DIST
BHUSAWAL
JALGAON
MALEGAON
NASIK DIST
MANMAR
PALASKHEDA
NASIK
IGATPURI
AHMEDNAGAR DISTRICT
AHMEDNAGAR
AURANGABAD
PERANDA
DHARASEO
TULJAPUR
DISTRICT
NILANGA
BHIR
NALDURG DIST
KALIANI
GANJOTI
AKALKOT STATE
BIDAR DIST
GULBURGA DISTRICT
HYDERAB
WARDHA DISTRICT
AMRAOTI
WARDHA
NAGP
HIN
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RAICHUR
MUDGAL DIST
LINGSUGAR DIST
KUSHTAGA DIST
YELBURGA DIST
KOPBAL DIST
BELLARY DISTRICT
HOSPET
BELLARY
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KURNOOL DISTRICT
GUNTAKAL JUNCN
ANANTAPUR
ANANTAPUR DISTRICT
CUDDAPAH
CHITALDROOG
HINDUPUR
TUMKUR DIST
TUMKUR
BANGALORE 5826
BANGALORE DIST 4180
HOSUR
KOLAR DIST
KUPPAM
MYSORE 1943
MYSORE DIST
SALEM DISTRICT
SALEM
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EYRE & SPOTTISWOODE

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SAMBALPUR

MAHANADI RIVER

HOOGHLY RIVER

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GANGHAT

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GODAVERI

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LORE

PONDICHERRY

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No. V.

MAP OF INDIA

SHOWING

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68 70 72 74 76 78

32 30 28 26 24 22

SIALKOT
CHAMBA
DERA ISMAIL KHAN
AMRITSAR
HOSHIARPUR DISTRICT
LAHORE
KAPURTHALA
JULLUNDUR DISTRICT
GARHSHANKA
SIMLA
LUDHIANA
QUETTA
DERA GHAZI KHAN
MOOLTAN
PATIALA
UMBALLA
HARDWAR (JAWALAPUR
SIBI
SIRSA
BAHAWALPUR
MEERUT
DELHI
SHIKARPUR
SUKKUR
ROHRI
BICKANEER
ULWUR
AGRA
JEYPORE
JODHPORE
AJMERE
PACHPADRA
GWALIOR
JHANSI
KOTRI
HYDERABAD
KARACHI 1583
SIROHI
MOUNT ABU
PALANPUR STATE
OODEYPORE
GUNA
PALANPUR
RANN OF CUTCH
CUTCH STATE
CUTCH MANDVI 1027
PANCH MAHALS
BHOPAL
AHMEDABAD
REWA KANTHA
KAIRA
INDORE
JAMNAGAR
KAIRA DIST
ANAND JUNC
BARODA
RAJKOT
KATHIAWAR
BARODA TERRITORY

PUNJAB
RAJPUTANA
BOMBAY
AGENCY
CENTRAL INDIA

80 82 84 86 88 90

32 30 28 26 24 22

ARHWAL
KUMAUN
NAINITAL
LHASA
BRAHMAPUTRA
SHIGATZE
JANGLACHE
NEPAL
KATMANDU
DARJEELING
OUDH
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LUCKNOW
CAWNPORE
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COOCH-BEHAR
DURBHUNGA
TIRHOOT
SARAN
PATNA
GANGES
MONGHYR
BENARES
ALLAHABAD
MALDAH
MYMENSINGH
AGENCY
MOORSHEDABAD
DACCA
HAZARIBAGH
BENGAL
FARIDPUR
BURDWAN
JUBBULPORE
HOOGHLY
BACKERGUNGE
HOWRAH
CALCUTTA
CHYBASSA
24 PARGANAS
BILASPUR

ANKLESHWAR
KHANDWA
SURAT
SACHIN STATE
SURAT DIST
KHANDESH DIST
BHUSAWAL
WARDHA DISTRICT
JALGAON
AMRAOTI
BULSAR
DHARAMPUR
MALEGAON
NASIK DISTRICT
PALASKHEDA
MANMAR
THANA DIST
NASIK
IGATPURI
AURANGABAD
BASSEIN
BHIWANDI
AHMEDNAGAR DISTRICT
BANDRA
BOMBAY 3585
KOLABA DIST
POONA DISTRICT
BHIR
POONA
JANJIRA
DHARASEO
NILANGA
PERANDA
TULJAPUR
DISTRICT
JANJIRA STATE
BHOR
SHOLAPUR DISTRICT
NALDURG DIST
KALIANI
GANJOTI
SATARA DIST
SATARA
SHOLAPUR
AKALKOT
AUNDH STATE
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KARAD
GULBURGA DISTRICT
RATNAGIRI
RATNAGIRI DISTRICT
TASGAON
BIJAPUR DISTRICT
KOLHAPUR
KOLHAPUR STATE
AMRABAD
BELGAUM DISTRICT
RAICHUR
MUDGAL DIST
SAVANTWADI
BELGAUM
LINGSUGAR DIST
KURNOOL DISTRICT
KUSHTAGA DIST
YELBURGA DIST
BELLARY DISTRICT
GOA
DHARWAR DISTRICT
KOPBAL DIST
HOSPET
MADDIKERA
HUBLI
BELLARY
GUNTAKAL JUNCN
ANANTAPUR
KARWAR
KANARA DIST
ANANTAPUR DISTRICT
CUDDAPAH
CHITALDROOG
HINDUPUR
TUMKUR DIST
TUMKUR
BANGALORE
KOLAR DIST
BANGALORE DISTRICT
HOSUR
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MYSORE
MYSORE DIST
SALEM DISTRICT
CANNANORE
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SALEM
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EYRE & SPOTTISWOODE

L PROVINCES

SAMBALPUR

CUTTACK

GANJAM

VIZAGAPATAM

RAJAHMUNDRY

MASULIPATAM

MADRAS

PONDICHERRY

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HOOGHLY

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No. VI.

MAP OF INDIA

SHOWING

THE NUMBER OF RECORDED DEATHS FROM INDIGENOUS PLAGUE

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Scale

50 0 50 100 150 200 250 300 Miles

NDON.

REPORT

OF THE

INDIAN PLAGUE COMMISSION

WITH

APPENDICES AND SUMMARY.

VOL. V.

Presented to both Houses of Parliament by Command of His Majesty.

LONDON:
PRINTED FOR HIS MAJESTY'S STATIONERY OFFICE,
BY EYRE AND SPOTTISWOODE,
PRINTERS TO THE KING'S MOST EXCELLENT MAJESTY.

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1901.

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UNIVERSITY OF EDINBURGH
USHER INSTITUTE

REPORT.

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INDIAN PLAGUE COMMISSION.

No. 4360–74.

EXTRACT from the PROCEEDINGS of the GOVERNMENT OF INDIA in the HOME DEPARTMENT (Sanitary / Plague),—under date SIMLA, the 11th November, 1898.

READ—

(1.) Despatch to Her Majesty's Secretary of State for India, No. 19, dated the 25th August, 1898.

(2.) Telegrams from Her Majesty's Secretary of State for India, dated the 28th September and 2nd November, 1898.

RESOLUTION.

Plague broke out in the City of Bombay during the rainy season of 1896, and has since visited all parts of the Bombay Presidency, the City of Calcutta, the district of Saharanpur in the North-Western Provinces, the districts of Jullundur and Hoshiarpur in the Punjab, several districts of the Madras Presidency, the Hyderabad and Mysore States, the Sirohi State in the Rajputana Agency, and the Gwalior State in the Central India Agency. No efforts have been spared in devising and prosecuting measures to stay the epidemic, and the Government of India think that, although the plague has been widespread, these efforts have been attended in many respects with marked success.

2. The Governor-General in Council is of opinion that the time has now come when it is possible as well as most desirable to obtain reliable scientific opinion as to the manner in which the disease is communicated, the causes of the outbreaks in different parts of India, and the circumstances influencing their growth and decline. The facts connected with the outbreaks of the disease are still very obscure, and no such opportunity has arisen—and it may be hoped that none will arise in the future—as now presents itself for their investigation. A number of interesting facts have already been collected and embodied in the different reports regarding the outbreak of the disease which has prevailed in India for the past two years; but there must be many valuable items of information to be ascertained from officers who have been employed on plague operations that have not been recorded in official reports; and there are also many circumstances connected with the outbreaks at different places which require to be locally examined by persons who can collate the facts and form conclusions which will be accepted by public as well as scientific opinion in respect of them.

The Government of India also regard it as very desirable that an authoritative scientific opinion should be obtained as to the value of prophylactic and therapeutic serum in dealing with plague.

3. The Governor-General in Council has accordingly decided, with the approval of Her Majesty's Secretary of State for India, to appoint a Commission to inquire into the following questions:—

(1) the origin of the different outbreaks of plague;

(2) the manner in which the disease is communicated;

(3) the effects of curative serum; and

(4) the effects of preventive inoculation.

The object with which the Commission is appointed is to conduct an investigation of a scientific character, the results of which will be of the greatest interest and importance not only to the Government of India, but beyond the limits of the Indian Empire.

4. The President of the Commission will be Professor T. R. Fraser, M.D., LL.D., F.R.S., Professor of Materia Medica in the University of Edinburgh. The members of the Commission will be Mr. J. P. Hewett, C.I.E., I.C.S., Secretary to the Government of India in the Home Department; Professor A. E. Wright, M.D., Professor of Pathology in the Army Medical School, Netley; Mr. A. Cumine, I.C.S., a Senior Collector in the Bombay Presidency; and M. A. Ruffer, Esq., M.D., President, Sanitary, Maritime, and Quarantine Council of Egypt. Mr. C. J. Hallifax, I.C.S., will be Secretary to the Commission.

5. The Commission will be formally constituted on the arrival of the members from England. They will first visit the City of Bombay, and will probably then desire to make inquiries in the other parts of India in which plague is or has been prevalent. The Governor-General in Council desires to leave the matter of method and procedure entirely to their discretion. The general conduct of the inquiry and the regulation of the course of business before the Commission will be entrusted to the President in communication with the members. The Commission, through their Secretary, acting under the instructions of the President, will correspond direct with Local Governments and Administrations (including Political Officers in direct relation with the Government of India) and departments of the Government of India and with any local authorities with whom direct communications may be authorised by Local Governments and Administrations as a matter of convenience and in order to save time. It is requested that all communications or requisitions for information emanating from the Commission may be treated as urgent and complied with promptly, and that, in the event of the Commission visiting a Province, they may be afforded every facility for their inquiries.

ORDER.—Ordered, that copy of this Resolution be forwarded to the Local Governments and Administrations for information and guidance, and to the Departments of the Government of India noted in the margin for information.*

Madras.
Bombay.
Bengal.
North-Western Provinces and Oudh.
Punjab.
Burma.
Central Provinces.
Assam.
Coorg.
Hyderabad.
Foreign } Department.
Public Works } Department.
Finance } Department.
Military } Department.

* *To Foreign Department*, and communication to Political Officers.

Ordered also, that it be published in the Supplement to the *Gazette of India* for general information.

[True Extract.]

A. H. L. FRASER,

Offg. Secretary to the Government of India.

CHAPTER I.

QUESTIONS REFERRED TO THE COMMISSION, AND PROCEEDINGS OF THE COMMISSION.

I.—Terms of Reference and Scope of the Commission.

1. The questions into which we were directed to inquire by the Reference from the Government of India are:—

(1.) The origin of the different outbreaks of plague;
(2.) The manner in which the disease is communicated;
(3.) The effects of curative serum of Yersin and Lustig respectively;
(4.) The effects of preventive inoculation.

Questions referred to the Commission by Resolution of the Government of India, No. 4360–74, dated the 11th November 1898.

We were informed that the object with which the Commission had been constituted was to conduct an investigation of a scientific character.

2. In the earlier stages of our inquiry we found that it was impracticable to examine witnesses regarding the origin of the different outbreaks of plague and the manner in which the disease is communicated without recording evidence relating to plague administration and its results. The witnesses who came before us were witnesses to facts, and the facts of plague origin and history were found to be inseparably interwoven with the facts of plague administration. At some of the earliest of our sittings we found evidence tendered on matters which were not strictly among those committed to us for investigation by the order of reference, but which we found it impossible to exclude from our consideration if we were to examine all aspects of the questions to which our attention had been directed on our appointment. We, therefore, on our arrival at Calcutta communicated informally to the Government of India the fact that we had recorded a considerable amount of evidence which could not be held to be strictly relevant to the consideration from a scientific point of view of the four questions referred to us, and the difficulties which we felt in excluding such evidence. We were informed by the Government of India that they accepted the necessity for taking evidence regarding the facts of administration. It was left to our discretion to determine how far it was necessary or desirable to comment in our Report on the facts so recorded in evidence. The Government of India expressed the opinion that the purely scientific side of our inquiry should be the main one, and that the object to be aimed at was that our Report should be a document for the information and use not only of the Indian Government but also of other nations generally. It appeared to the Government of India that differences of opinion between various authorities in India might detract from rather than enhance the value of the Report in this respect.

Necessity for taking evidence regarding facts of plague administration.

3. These instructions were communicated to us unofficially and no change was considered necessary in the formal orders regarding the scope of our inquiry. The record of our evidence accordingly contains much which is not of a scientific character or strictly relevant to the specific issues definitely communicated to us for consideration. In addition to the evidence which has been recorded and the papers which have been embodied in the appendices to that evidence, we have been favoured by the different Governments, particularly the Government of India and the Government of Bombay, with many other papers to assist us in our conclusions. We have also in the course of our tour in India endeavoured to supplement the evidence placed before us by personal observation. We have taken every opportunity of seeing plague measures in operation in the large towns, such as Bombay, Poona, and Bangalore Civil and Military Station; we have frequently visited plague-infected villages, and examined the village sites, and the villagers in their camps; we have seen the arrangements in many Plague Hospitals; we have seen the inoculation with Mr. Haffkine's prophylactic done; we have, during the course of our railway journeys, closely examined the system for the medical inspection of passengers and the disinfection of their personal effects, and during our residence in Bombay we, on several occasions, observed Major Crimmin's arrangements for the examination of passengers by sea from Bombay, and the disinfection of their clothes. We have had numerous opportunities of personal communication with the officers employed on plague measures, and of observing the attitude of the people. We desire to express our thanks to the Government of India and to the Local Governments as well as to individual officers in their employ

Report based on record of evidence and other sources of information.

for assistance in the conduct of our inquiry, which has been given to us in the most thorough and complete manner wherever we have been.

Synopsis of subject-matters treated of in this Report.

4. In view of the fact that part only of the evidence recorded by us bears on the scientific study of plague, we have prepared our Report in two parts so that the portion dealing with this question can, if the Government so pleases, be separated from that portion which deals with the evidence regarding the different administrative measures adopted from time to time.

The first part of the Report deals with :—

I. The history of plague in India with special reference to the spread of plague and the origin of the plague epidemics that have occurred in various places;

II. The clinical, bacteriological, and epidemiological data so far as they bear on the mode in which plague is communicated, and the mode in which it is spread from place to place: the clinical facts which have a less direct bearing on the communication and spread of the disease have been treated of in a separate Appendix by the President;

III. The preparation, character, and effect of the particular therapeutic sera which we were directed to study; and

IV. Preventive inoculation by Mr. Haffkine's method.

The remainder of the Report is devoted to a description and consideration of the results of the measures actually adopted to deal with outbreaks of plague, and to the question whether it is possible to make any improvement in them.

In addition to collection of information in general, scientific investigations on special subjects arranged for.

5. We arrived at Bombay on the 26th of November 1898 Our first task on arrival was to settle the programme for our future work. We had, on the one hand, to make arrangements for collecting information with regard to plague in general, both by the examination of witnesses and by personal inspection first in Bombay and afterwards in other parts of India which were being visited or which had in the past been visited by plague. We had, on the other hand, to make arrangements for undertaking certain scientific investigations. These had reference (1) to the determination of the character and effect of Mr. Haffkine's prophylactic fluid; (2) to the determination of the character and effect of Yersin's and Lustig's sera; and (3) to the elucidation (so far as might be possible) of several unsolved problems connected with the propagation of plague, and with regard to the question as to what are the most appropriate measures for combating it.

The steps which we took for carrying out these two entirely distinct tasks may be separately considered.

II.—Proceedings of the Commission.

Therapeutic effect of Yersin's serum tested, and other bacteriological investigations carried out.

6. We may begin with the description of the arrangements which we made for carrying out the scientific investigations which have just been referred to. In the first place, through the courtesy of the Municipal Commissioner for Bombay, we obtained the use of the small Public Health Laboratory in the Municipal Buildings, which had previously been placed at the disposal of the German Plague Commission. This done we made an application to the Government of India for the services of four selected Medical Officers who had specially distinguished themselves during their course of study at Netley by their clinical and bacteriological knowledge and by their aptitude for scientific research. These officers, whose services were immediately placed at our disposal by the Government of India, were employed upon a variety of scientific tasks.

To Lieut. (now Capt.) H. J. Walton, I.M.S., and to Lieut. Stewart Douglas, I.M.S., was committed in addition to other work the task of testing the therapeutic effect of Yersin's serum upon plague-infected patients. The investigation in question was undertaken first at Bangalore, where an epidemic of plague was at that particular time raging. Afterwards when the epidemic at Bangalore was declining the investigation was continued in Bombay under our direct supervision. To Capt. C. G. Spencer, R.A.M.C., and Lieut. W. G. Liston, I.M.S., were committed a variety of bacteriological investigations.

The details of all the investigations which were carried out under our direct personal supervision can be gathered from the evidence of the four Medical Officers who have just been mentioned.

III. 329–346.

Before bringing to a close this portion of the narrative of our proceedings, we desire gratefully to acknowledge the readiness with which the Government of India met our wishes in the matter of placing the services of these selected officers at our disposal. We desire to express our sense of the great zeal and ability with which each of these officers carried out the scientific task which was entrusted to him. We have also to acknowledge the courtesy of the Government of the Mysore State in placing the services of Dr. Shrinivasa Rau, Chemical Examiner and Bacteriologist to the Government, at the disposal of the President for the purpose of conducting certain bacteriological researches.

Arrival in India; local inquiry in Bombay City and Presidency, Mysore, Madras, Hyderabad (Deccan), Central Provinces, Bengal North-West Provinces, Punjab, Cutch State, Baroda State, and Daman, and personal supervision by members of Commission of scientific investigations in Bombay.

7. We may now pass on to the narration of the steps which we took for carrying out that larger part of our programme which consisted in the collection of information about plague generally.

We arrived at Bombay, as has been said above, on the 26th of November 1898. We commenced to take evidence there on 29th November.

During this our first visit to Bombay, we determined to confine ourselves to the examination of Mr. Haffkine, Dr. Galeotti (the representative of Professor Lustig), and certain selected witnesses who were very familiar with the course of the plague in the City.

On 5th December we left Bombay for Dharwar and Hubli. We here inquired more particularly into the results which had been obtained by Mr. Haffkine's plague inoculations.

From Hubli we proceeded to Bangalore. The evidence there laid before us dealt in large part with the epidemic of plague, which had at that time somewhat abated in the native City, though it was still very acute in the Civil and Military Station. Further, it dealt with the measures which were being taken to protect the Madras Presidency against infection.

From Bangalore we proceeded to Guntakal to inquire into the circumstances of an epidemic of plague which had occurred in the neighbouring villages. We made an inspection of two of these villages which were being evacuated, and took note of the condition of the evcauated people and of the disinfecting operations which were in progress.

From Guntakal we proceeded to Hyderabad, where we examined witnesses as to the course of plague in the Hyderabad State. From Hyderabad we went to Wardha in the Central Provinces, where evidence was received as to the small outbreak at Hinganghat and Wardha. From Wardha we proceeded to Calcutta, where we arrived on 24th December. We were engaged in taking evidence at Calcutta till 5th January 1899. We proceeded from Calcutta to Northern India. Evidence about the outbreaks at Hardwar and the neighbourhood, the Sirohi State, and Khandraoni in the Gwalior State was recorded by us at Agra, and we completed our inquiry into outbreaks in Northern India at Lahore, where we received evidence about the epidemic in the villages in certain tracts of the Jullundur and Hoshiarpur districts in the Punjab. At this stage one of us proceeded to Bombay to take personal charge of the bacteriological investigations which had already been initiated. We left Lahore on 21st January, and at Karachi on 23rd January opened an inquiry into the outbreaks in Sind.

From Karachi we went to Cutch Mandvi, Ahmedabad, Baroda, Surat, Daman, and Bombay, where we arrived on 9th February. We took evidence in Bombay from 11th February to 22nd February. During this time two of us were continuously engaged in the conduct of bacteriological investigations. We then proceeded to Poona. After completing our inquiry at Poona two of us returned to Bombay. The remainder of us proceeded to Satara, Belgaum, Bijapur, Sholapur, Ahmednagar, and Nasik, and returned on 8th March to Bombay. During this time, and up to our departure from Bombay, two of us were, as before, engaged in bacteriological investigations at Bombay. We again took evidence in Bombay from 13th March to 22nd March.

Number of sitting and conclusion of proceedings in India; arrangements for completion of scientific investigations in progress, and for special inquiry regarding plague in Kumaun and Garhwal.

8. We left Bombay on 25th March 1899. During our inquiry in India we held 70 sittings for taking evidence and examined 260 witnesses, of whom 31 were recalled once and 6 were under examination three times.

Before leaving India we made arrangements for the completion, by Lieutenant Liston, I.M.S., of certain of the bacteriological investigations which were in progress. The results of these further investigations have been incorporated into Lieutenant Liston's evidence.

We also, before leaving India, requested the Government of India to depute Lieutenant (now Captain) H. J. Walton, I.M.S., and Lieutenant Stewart Douglas, I.M.S.,

two of the Medical Officers who had been attached to us on special duty, to make a special investigation into the question of the presence of endemic plague in the hill country of Kumaun and Garhwal. The nature of that investigation will be gathered from the instructions which we issued for the guidance of these officers and from the report of their proceedings. These will be found in Appendix XXV. (1) in Vol. II. of the minutes of evidence.

II. 365–375.

Return to England, record of evidence in London, and consideration of Report.

9. It had at one time been our intention to visit Egypt on our way to England with the object of examining the members of the Egyptian Plague Commission which visited Bombay in 1896–97. As, however, we had obtained in India sufficient evidence upon the points upon which we had proposed to examine Sir John Rogers and Dr. Bitter, we abandoned this intention. One of us (Dr. Ruffer) landed in Egypt, and, on the outbreak of plague at Alexandria a short time afterwards, was detained there on official business until the end of July. The remainder of us proceeded to England. We held two sittings in London for the reception of evidence and then proceeded to the consideration and preparation of our Report. The work in connexion with this latter has been greatly interfered with in consequence of the return of three members of the Commission to their duties abroad.

We desire, in conclusion, to acknowledge in the fullest manner the valuable assistance received by us in the preparation of our Report from our able and energetic Secretary, Mr. Hallifax.

CHAPTER II.

ACCOUNT OF PLAGUE IN INDIA FROM 1896.

I.—Discussion of the Origin of the Outbreak of Plague at Bombay in 1896.

Plague in India before and after 1896.

10. Before we endeavour to answer the first two questions referred to us, viz., the origin of the different outbreaks of plague in India in and after 1896, and the manner in which the disease is communicated, it is necessary to give some account of the spread of plague more particularly subsequent to the period covered by the historical account given by Mr. Nathan in Vol. I., pp. 71 to 94 of the collection of official reports entitled "The Plague in India, 1896–97." We have here carried the history of plague in India down to September 1899, the date of the last reports which reached us before two of us had to return to duty in India and Egypt respectively, and were no longer able to take an active part in the Proceedings of the Commission. The prevalence of plague in each half-yearly period from 1st October 1896, to 30th September 1899, is exhibited in the six maps of India, which are printed as an Appendix to our Proceedings.

Vol. IV., pp. 155–160.

Introduction of plague into Bombay City: infection from the Himalayas improbable.

11. The manner in which plague was introduced into Bombay City is very obscure and defies elucidation. Even the exact date of the introduction cannot be ascertained. Some witnesses go so far as to say there was no recent importation, and that plague was already present in Bombay before 1896. This theory, however, needs no refutation, inasmuch as, if plague had been occurring in Bombay before 1896, it would certainly have come under the notice of the medical profession in the City. There is no reason for thinking that, except when the districts in the immediate neighbourhood of Kumaun were infected in 1853–4, plague had been present in the plains of India since the outbreak of 1836, until it appeared in Bombay during the rainy season of 1896. Lieut.-Col. Weir, I.M.S., and Mr. Vincent, late Commissioner of Police in Bombay, suggest that the disease was imported from Kumaun, and the grounds that are adduced in support of this theory are that between May and August 1896 numerous pilgrims from Kumaun came down to Nasik and Bombay, and that sudden deaths are reported to have occurred among them. But there is nothing to show that these pilgrims had plague among them, or that they came from any place in the Hills where there was plague. On the contrary, it appears certain that there was no considerable outbreak of plague in the Hills for some time before the Bombay outbreak occurred. Even when there has been plague in the Hills, the experience of the past teaches us that it has shown little tendency to spread even to the neighbouring districts of the plains. In case the disease was carried to Bombay from the Hills, it would naturally make its appearance first in the neighbourhood of the Hills, and it is very improbable that infective material could have been carried so far from the Hills as Bombay without causing outbreaks in places through which it was carried on its way, for it takes 10 days to reach Bombay from Kumaun, even if the journey is not broken, and pilgrims are in the habit of travelling slowly and of stopping at many places *en route*. Moreover, the plague in Bombay City, when first brought under the notice of the authorities, was

901; 18,276.

6181.

977, 988, 1027, 1545 *seq.*

8834–6.

6184–6.

26,691–700.

not found in the quarter of the City in which its appearance might have been expected if it had been introduced into their lodgings by Kumaun pilgrims. 1023.

12. When first detected, plague was found to have obtained a hold upon the Mandvi quarter, and it remained confined to that quarter for some time. This quarter is that of the docks and of the warehouses in which imported merchandise, and grain for export are stored. The fact that plague first broke out here manifestly favours the theory that the infection was imported by sea. No definite facts, however, have come to light to show that it was so imported. On the one hand, there is no record of the occurrence of plague among men or rats on board ships arriving at Bombay before the outbreak of plague in the City; on the other hand, there are no clearly established facts pointing to importation in clothes or merchandise. The only attempt to propound any definite theory regarding the first outbreak of plague in Bombay was that made by Mr. Ismail Jan Mahomad and the Honourable Mr. Bhalchandra Krishna. About the 15th of August 1896, the first-named witness saw in one house two fatal cases of disease characterised by fever and pneumonic symptoms which he afterwards concluded to be cases of plague. The patients in question had been at Nasik for some days before they took ill, but Mr. Ismail Jan Mahomad thinks that the disease was in both cases contracted at Bombay, because "there was at the time no plague at Nasik," while in Bombay "there was already a scare of the men of the firm" to which the patients belonged, on account of sickness that had broken out among them. From subsequent inquiries, it appeared that, in the neighbourhood in which the cases were observed by Mr. Ismail Jan Mahomad, there were warehouses of Chinese goods—tea, sugar, and silks—and in Argyle Street, where plague was first detected and where it was very severe, there were three warehouses containing Chinese crackers imported from Hong Kong. In one of the sugar warehouses and in a granary at the back of that warehouse, dead rats had been found before the middle of August, and Mr. Ismail Jan Mahomad's two patients, to whom have just been referred, lived in a house adjoining the sugar warehouse where rats died. Plague cases also occurred in the house above the granary where rats died. Crackers, it is said, are imported in the early part of July and in the beginning of August for use at festivals that take place about that time, and it is suggested that the infection was imported in them from Hong Kong and was communicated to rats and then by rats to men. The above shows that the evidence which connects the Chinese crackers in question with the origin of plague in Bombay City is of the slenderest. What appears to be fairly well established—and beyond this everything would appear to be doubtful—is that plague cases occurred first in the Mandvi quarter, and that they were, as appears to be shown by the evidence of Dr. Underwood and Mr. Engineer, occurring there as early as the month of July 1896. The first cases officially recognised were, however, the cases reported by Mr. Viegas on the 23rd of September 1896.

Theory of the introduction of plague into Bombay City from Hong Kong. 911. 776, 1191. 17,760 *seq.* 19,016 *seq.* 19,133, 18,295. 579–80.

13. The plague must, in our opinion, have reached Bombay by sea, but, as we have just seen, we found it impossible to procure any evidence to show from what place it was brought. Hong Kong was, indeed, infected for some time before plague appeared in Bombay, and the possibility of the importation of infection directly from Hong Kong is established by the fact that a case of plague was actually landed at Bombay in the course of the third epidemic, in March 1899, from the steamship *Bormida*, which came from Hong Kong. There is, however, nothing definite to connect the existence of plague in Hong Kong with the outbreak in Bombay, and it is at least possible that plague may have come to Bombay from some endemic focus in the Persian Gulf or Arabia.

Importation of plague into Bombay City by sea probable. 25,506. *Vide infra.* *Bormida.*

14. In attempting to give a historical account of the spread of plague in each season it will be convenient to refer in detail, first to Bombay City, then to the Presidency of Bombay and Sind, to which plague has been, with a few exceptions, confined hitherto, and lastly to the comparatively few places which have been infected in other parts of India. Before referring to the statistics of the outbreaks in particular places, we will give a brief general outline of the course of plague during each of the three periods of its increase and decline between September 1896 and June 1899.

Outline of the treatment of the history of plague in this Report

II.—Sketch of the Course, to the end of June 1899, of the present Epidemic of Plague in India.

15. The Presidency of Bombay is an irregular strip of land stretching along the eastern coast of the Arabian Sea as far as the valley of the Indus. The continuous coast line is broken on the north only by the Gulf of Cambay and the Gulf of Cutch, between which Kathiawar, a tract of country containing 187 small Native States, projects. Along the long coast line there are numerous ports. In other directions the

The geographical limits and description of the Bombay Presidency and Sind.

Bombay Presidency is bounded by the Mysore State (on the south), the Madras Presidency (on the south-east), the Hyderabad State, the Hyderabad Assigned Districts, and the Central Provinces (on the east), the Native States of Central India, and the Native States of Rajputana (on the north). To the north of the River Narbada lie almost the whole of the Province of Gujarat (containing the British districts of Ahmedabad, Kaira, the Panch Mahals, Broach, and Surat), the Baroda State, a number of Native States, and the Province of Sind. Sind is bounded by the sea and the Rann of Cutch (on the south), the States of Rajputana (on the east), the Punjab (on the north-east), and Baluchistan (on the north and west). To the south of the Narbada there are three tracts, the Konkan (including Bombay City and Island, and the Thana, Kolaba, and Ratnagiri, districts), the Deccan (including the Nasik, Ahmednagar, Poona, Sholapur, and Satara, districts), and the Southern Mahratta tract (including the Belgaum, Dharwar, and Bijapur, districts, and the Native States included in the Kolhapur country). The two districts of Khandesh and Kanara do not fall strictly within any of these three divisions, but adjoin the Deccan and the Konkan respectively.

Extent of infection in the Presidency since 1896.

16. Bombay City has remained continuously infected since the discovery of plague there in September 1896. From time to time plague has occurred in every one of the districts in the Bombay Presidency proper, and in many of the Native States adjoining the British districts. In some districts the infection has been very severe, in others very mild. In Sind there have been three epidmics at Karachi, and several other places in Sind have been attacked.

Course of the plague up to June 30th, 1897.

17. Between the outbreak of plague in Bombay in the autumn of 1896 and 30th June 1897, the town of Karachi was attacked and the outbreak in Sind occurred. In the Bombay Presidency proper, the main seat of the infection was during this period the Konkan coast, from Surat to Ratnagiri, which includes Bombay, Surat, Thana, Kolaba, part of the Baroda State, and the small Portuguese possession of Daman. Another tract affected at this time was the west and centre of the Deccan districts, including the districts of Nasik, Poona, Satara, and Ahmednagar. The Eastern Deccan districts and the British districts in the Southern Mahratta country escaped any serious infection at this time, but the Kolhapur country was infected. There were scattered indigenous* cases of plague in the Gujarat districts north of the Surat district, but the only outbreak of any consequence was at Palanpur, the capital of the Palanpur State. There was a virulent epidemic at Cutch Mandvi, the capital of the Cutch State.

Up to 30th June 1897, the only places outside the boundaries of the territory controlled by the Bombay Government from which indigenous plague had been reported were the small village of Khandraoni, in the Gwalior State, and Hardwar, in the North-Western Provinces.

Course of the plague from July 1st, 1897, to June 30th, 1898.

18. In the next period, from 1st July 1897, to 30th June 1898, the Konkan coast was not generally so badly infected as in the previous period. There were bad epidemics at Karachi, Surat, Cutch Mandvi, Baroda, and Palanpur, on the north. The principal centre of infection, outside Bombay City, moved to the east and south. The districts of Poona, Satara, Sholapur, and Ahmednagar suffered, and at each of the chief towns in these districts there was an epidemic. The Khandesh district was slightly infected. The infection spread in the Kolhapur State and to the adjoining district of Belgaum.

During this period there were four new seats of infection outside the Presidency of Bombay and Sind. The villages of the Hyderabad State adjoining Sholapur and other Bombay districts became infected in December 1897. Calcutta was infected in April 1898. The villages in parts of the Julluudur and Hoshiarpur districts in the Punjab were infected in October 1897. A few villages in the Sirohi State, in Rajputana, were infected in November 1897. The infection spread from Hardwar, in the North-Western Provinces, to other places in its immediate neighbourhood.

Course of the plague from July 1st, 1898, to June 30th, 1899.

19. In the third period from 1st July 1898 to 30th June 1899, the chief seats of plague were the Dharwar, Belgaum, and Satara, districts and the Kolhapur country. The rural areas in this part of the country suffered severely, as well as the towns of Belgaum, Dharwar, and Hubli. The outbreaks in the tracts in question reached their highest point in October and November, and then declined to small proportions in May, with a slight increase again in June. In the Sholapur, Poona, and Bijapur districts there were outbreaks which culminated in October, November, and January respectively. In Kathiawar an outbreak occurred which reached its climax in September. In Thana there was an outbreak in which the number of cases reached

* The use of the term "indigenous" in this sense is not strictly accurate, but it is adopted in this Report as it has been generally used in India.

a high point in August, and then, after declining till November, increased again till March, when another decline commenced. In Nasik there was a considerable amount of plague in October, and in this district as well as in Surat and Broach, where there was plague, the disease, after declining at the end of 1898, increased in the first months of 1899, and decreased after February. In Kaira, the Panch Mahals, and Khandesh also there were outbreaks which were at their worst in January and February 1899.

In Bombay City the number of cases was highest in March 1899, and then began to decline. Cutch Mandvi and Baroda were again attacked in the beginning of 1899, and another severe outbreak commenced in February in Poona City, while Karachi City was severely affected from February to May.

To the south of the Bombay Presidency a very malignant outbreak took place in the Mysore State. Some districts of the Madras Presidency also became infected. An outbreak occurred in the Central Provinces, but it was confined within narrow limits. Calcutta, certain parts of the Hyderabad State, and some villages in the Punjab also suffered in this period.

III.—Detailed Account of the Course, to the end of June 1899, of the Present Epidemic in India.

20. In the more detailed account of the spread of plague which follows, we have endeavoured to give a short outline of the course of the outbreaks in the larger towns.

Remarks as to the statistics regarding attacks and death from plague.

As the figures which we quote may not in all cases be found to coincide exactly with those given in some of the official reports, we would explain that we have found difficulty in reconciling exactly some of the different returns of plague cases and plague mortality which we have received. The differences have, however, in no case been of any material importance, and have often been due to the returns not having embraced exactly the same periods.

For the Bombay Presidency and the States connected with it, the figures we have adopted have generally been taken from the official weekly returns of the Bombay Government, which have been supplied to us. These give a more uniform basis of comparison between different towns and districts than the figures separately supplied by witnesses, all of which are not prepared on one system. Moreover, the official weekly returns of the Bombay Government enable us, in carrying on the history of the disease in Bombay beyond the date on which we ceased to take evidence, to use figures that can be compared with those relating to the period up to the date on which we ceased to take evidence.

In preparing the appended maps, consideration has been given only to fatal indigenous cases, and in the case of towns—except Bombay, Poona, and Karachi—the official weekly returns give us figures for indigenous cases only. Where for this or any other reason, such as the necessity of quoting figures for a period not coinciding with any given in the returns, we have used figures not taken from the returns, the source whence our figures are derived is quoted in the margin.

Vol. IV., pp. 155–160.

We have made especial use of the following reports regarding the outbreak in Bombay City, viz.:—

(1.) Report on the outbreak of Bubonic Plague in Bombay, 1896–97, by P. C. H. Snow, Esq., I.C.S., Municipal Commissioner for the City of Bombay.

(2.) Report on the Bubonic Plague in Bombay, by Brigadier-General W. F. Gatacre, C.B., D.S.O., Chairman, Plague Committee, Bombay, 1896–7.

(3.) Report of the Bombay Plague Committee on the Plague in Bombay, for the period extending from the 1st of July 1897 to the 30th April 1898, under the Chairmanship of Sir James MacNabb Campbell, I.C.S., K.C.I.E.

For places outside the Bombay Presidency we have adopted figures supplied to us in periodical reports with which we have been favoured by the local authorities, and have cited the authorities from which we have obtained other figures quoted in this history.

We have, as a rule, given the population of the towns as it was ascertained in the census of 1891. The population is, in most instances, believed to be considerably higher now, but the increase is more or less a matter of guess work. In calculating the incidence of plague attacks on the census population, there is a considerable margin of error, since it has generally happened in large towns that part of the population fled during the progress of an epidemic. Generally speaking, however, a calculation of the rate of attack on any assumed population would also be erroneous. We therefore think it best to calculate the incidence on the census population, and to point out that the incidence was nearly always higher, and very often much higher, than this calculation would show.

III.—(*a.*) *History of the Plague, to the end of June* 1899, *in Bombay City.*

First outbreak in Bombay City. 1018, 1175. 892.

21. Plague was officially recognised in Bombay City on 23rd September 1896. Suspicious cases had occurred before that date, and it is clear that a number of them were true cases of plague The mortality in the City, which had been 2,299 in July 1896, was 2,669 in August, and 2,616 in September. The exact date, however, of the first occurrence of a case of plague has not been, and cannot now be, ascertained.

Bombay City. Measures adopted at first under the management of the Municipal authorities. Mr. Snow's Report Para. 7 and pp. 72–3, 908–916.

22. The responsibility of dealing with the plague was, in the first instance, left with the Municipal authorities. To assist them in suppressing the outbreak special powers for dealing with it were, on 6th October 1896, conferred on the Municipal Commissioner, under Section 434 of the City of Bombay Municipal Act of 1888. On that date the Commissioner issued a notification, firstly, requiring any person having the control or charge of a building to open it on the demand of any authorised Municipal officer, and to permit it to be cleansed and disinfected, and any articles in it to be removed for disinfection or destruction; secondly, rendering any person, certified by the Executive Health Officer of the Municipality or by any duly qualified medical practitioner to be suffering from bubonic fever, liable to be removed to a hospital for the treatment of that disease; and, thirdly, providing for the isolation of any house in which a case might occur.

Bombay City. Effect of measures first adopted, and their modification.

Mr. Snow's Report. Para. 9.

23. The provisions of this notification, and particularly the one relating to the removal of sick persons to hospital, caused great alarm and excitement. The great majority of cases were concealed, and the most extraordinary rumours were circulated as to the intentions of Government in wishing the people to go to hospital. In consequence it became necessary (on 30th October) for the Municipal Commissioner to issue two proclamations—one addressed to the public and the other to the mill-hands—explaining and modifying the orders issued about segregation on the 6th. These proclamations contained a promise that no sufferer from plague should be removed to hospital from premises in which proper segregation and treatment could be carried out, and that where removal to hospital was necessary no action should be taken except upon the certificate of a qualified medical practitioner employed under the Municipal Health Department.

The practice actually followed at this time was that, when a case of plague had been detected, the patient was removed to the room in his house which had the best light and ventilation, and endeavours were made to prevent all but the necessary attendants from frequenting the portion of the house occupied by him. Where suitable arrangements could not be made in the house, or where the patient was a pauper or friendless, he was taken to the Municipal Hospital.

There was not at this time any general evacuation of infected houses or segregation of the healthy residents from them. But in particular instances, where the disease appeared with concentrated virulence in a house, the whole of the residents were removed. This measure did not meet with the same objection as the removal of the sick to hospital, but it was not carried out on any very large scale, and the camps which the Municipality provided for healthy persons were not freely used.

Bombay City. Disinfection of infected localities in first outbreak. Mr. Snow's Report. Para. 13.

24. At the time when the outbreak occurred the Municipal Commissioner had not at his disposal either the trained staff or the supply of articles necessary for thorough disinfection on a large scale. Great efforts were, however, made to disinfect infected localities. The steps which it was thought advisable to adopt were the following:—

(1.) A thorough cleansing and disinfection of every room in which a case was reported to have occurred, and the overhauling and disinfection of all traps and household connexions;

(2.) The sprinkling of dry chloride of lime or carbolic acid powder over the floors and passages both of the affected and of the neighbouring houses;

(3.) The thorough flushing of the affected building, the opening of the roof, and the removal of all obstructions to light and air;

(4.) The burning of all rags, old clothes, and rubbish, found in the affected building, and the disinfection of all valuable articles liable to infection;

(5.) The limewashing of the whole of each affected house before re-occupation;

(6.) The closing for 20 days of every warehouse for the storage of grain in which plague was shown to have taken place, the exposure of the grain and other merchandise to the sun, and the flushing and disinfection of the warehouse;

(7.) The closing, limewashing, and complete fumigation for three days, of every shop in which a case occurred, and the exposure to the sun of all goods in it for at least 24 hours.

These operations were energetically undertaken in infected localities and were accompanied by other measures taken in areas not yet infected. Wherever the approach of plague was feared, disinfectants were distributed to the people with instructions how to use them, instructions which, apparently, they seldom followed. The sewers in certain parts of the town were specially flushed and disinfected with perchloride of mercury and phenyle in November, and limewashing was carried out on a large scale throughout the greater part of the City and suburbs in December. Powers were taken in February 1897 under the Epidemic Diseases Act to prohibit the use of insanitary houses, or to demolish them, compensation being given to the owners. Many temporary erections were burnt, many insanitary houses were condemned and demolished, and many were altered and improved.

25. The outbreak of plague in Bombay City was followed by an exodus of the population, which, though ultimately great, was not of large proportions in the early days of the epidemic. The population of the City was returned in the census of 1891 at 821,764, but it was estimated to have increased to 846,000 in October 1896. In October 1896 about 20,000 left the town. It is estimated that during the last two months of the year the population was reduced by 171,500, and in January 1897, by 187,400 more.

By the end of November there had been 887 attacks and 643 deaths from plague. In December the number of reported cases of plague rose to 1,655 with 1,160 deaths, in January 1897 to 2,374 with 1,835 deaths, and in the first 26 days of February to 2,958 attacks with 2,873 deaths. In the middle of February the number of attacks and deaths began to abate, and from the end of that month, by which time the population had fallen to 450,000 approximately, native opinion began to be reassured and the people to return to the City. Outsiders also began to flow into Bombay till the end of June, numbers of them unfortunately being indigent and under-fed people who were feeling the effects of the famine year.

Plague attacks and deaths, and movement of population, Bombay City from September 1896 to the end of February 1897. Mr. Snow's Report. Para. 25

26. In the early part of March 1897 the Government of Bombay determined to relieve the Municipal authorities of the control of operations against the plague in the City of Bombay, and to appoint a Committee, consisting of Brigadier-General Gatacre, C.B., D.S.O., Chairman; P. C. H. Snow, Esq., Municipal Commissioner; Surgeon-Major Dimmock; and C. C. James, Esq., Municipal Engineer; with Surgeon-Lieut.-Colonel Weir, the Health Officer of the Municipality, as Chief Medical Officer of the Committee, to take charge of plague operations with effect from 1st April. The appointment of this Committee was resolved on by the Bombay Government owing to its view that it was evident that the continued existence of plague in the City, in spite of the thorough cleansing operations undertaken by the executive of the Municipality, showed that it could not be stamped out by such measures.

Bombay City. Plague Committee under General Gatacre assumed control from 1st April 1897.

27. The total mortality in the City from 23rd September 1896, the date on which plague was recognised, up to 31st March 1897, when the Municipal authorities ceased to be responsible for the working of measures against the plague, was 33,161. The normal mortality for this period might, had the town been full, be taken at 13,318. The difference between these two figures is 19,843. The recorded plague mortality was, however, only 9,142. Thus it would appear that considerably more than half the cases remained undiscovered during this period.

Plague mortality in Bombay City from commencement of outbreak until the appointment of Plague Committee. 25,719–20–21.

28. The measures to be taken by the Committee over which General Gatacre was appointed to preside were intended to comprise an organization for—

(1) the discovery of all cases of plague;
(2) the treatment of all cases in hospitals, which were to be provided for persons of different castes at convenient places; and
(3) the gradual segregation of those living in the same room with, or in close attendance on, a person found to have the plague.

Bombay City. Measures adopted by General Gatacre's Committee. General Gatacre' Report, p. 5.

29. The Committee divided the town into ten districts, each in charge of a responsible Medical Officer. The duties of these Medical Officers were—

(i) to superintend and exercise sanitary supervision over the whole district in their respective charges, reporting daily to the Plague Committee and the Health Officer;
(ii) to supervise the working of the district hospitals and to attend the same;
(iii) to supervise the work of the search parties in conjunction with the Justices of the Peace;

Bombay City. Duties of officers put in charge of divisions by General Gatacre's Committee. General Gatacre's Report, pp 7 and 8.

(iv) to control the disinfection of the infected areas and buildings;
(v) to inspect all segregation camps in the districts;
(vi) to call the attention at once of the officer appointed by Government for the condemnation of insanitary buildings to any such premises in their districts;
(vii) to report on the registration of burials and cremations in their districts and to supervise the sanitary condition of cemeteries;
(viii) to send daily reports to the Plague Committee and the Health Officer of what had happened in their districts in the previous 24 hours together with suggestions.

Each district was divided into sub-divisions and each sub-division into sections.

Bombay City. Search parties in first outbreak. General Gatacre's Report, pp. 177–183.

General Gatacre's Report, p. 18.

30. The attempt was made to discover the cases of plague through the Justices of the Peace, who volunteered for this duty, assisted by search parties of soldiers. The proclamation issued to the public on the subject announced that it was necessary to remove all persons suffering from plague from their houses for a short time, and to treat them in hospitals public and private. The presence of the Justices of the Peace was intended to reassure the people that nothing contrary to their religion or needlessly offensive to their prejudices was intended. The number of soldiers employed was 26 British non-commissioned officers and men with three officers, and 566 native non-commissioned officers and men with four British and 11 native officers.

Work was commenced at 7 A.M. each day, and endeavour was made by the Justice of each sub-division to visit as many houses as possible within it each day. Whenever a Justice found a sick person he made a note of the place and left a sepoy on guard. On the conclusion of his work the Justice gave his report to the sub-divisional Medical Officer, who then proceeded to arrange for the despatch of ambulances. Every sick person had to be seen by the sub-divisional Medical Officer or one of his assistants before being removed to hospital, or, if a female, by a lady doctor whenever this was possible.

Bombay City. Plague hospitals in first outbreak. General Gatacre's Report. Chapter II.

31. An important item of the Plague Committee's work was the establishment of temporary hospitals for the reception of persons suffering from plague. The Committee aimed at procuring such situations for the hospitals that the removal of patients to them from any part of a district should not involve movement over too great a distance, and so add the fatigue of transport to the dangers of the disease. In some of the larger districts it was found necessary to erect a hospital in each sub-division. Private hospitals were only sanctioned when they were open to all the members of any particular caste or sect, and all applications for hospitals for private individuals or groups of individuals were refused. Conditions insisted on before a private hospital could be established were that all arrangements must be carried out by a hospital board, and that the medical attendants and staff must be under the supervision and control of the Medical Officer of the district in which the hospital was located. There were fifteen hospitals maintained by Government, and twenty-eight private hospitals.

General Gatacre's Report, pp. 12–14.

Bombay City. Disinfection in first outbreak. General Gatacre's Report, pp. 183–5.

32. Immediately after the removal of a patient to hospital disinfection commenced. It appears that, as a general rule, only the quarters in which the infected person had lived were disinfected, although sometimes the whole house was done. All rags, bedding and clothes belonging to the patient were removed and burnt, and the infected place was treated with a solution of perchloride of mercury. The solution was, in April 1897, fixed at a strength not weaker than 1 in 1,000, and the additional precaution was taken of digging up the floor, if of earth, to a depth of four inches. The poor received compensation for articles burnt and valuable property, and the contents of infected shops and warehouses were exposed to the sun and air in the road under a guard for three days. As soon as the disinfected place dried up, the walls, floor, and ceiling, were limewashed with quicklime, laid on hot. In addition to the infected room the privies, traps, and water spouts, were disinfected, and the roofs opened so as to admit light and air thoroughly. The re-occupation of a disinfected place was not permitted except with the previous sanction of the District Medical Officer.

Besides the actual disinfection and limewashing of the inside of houses affected with plague, steam flushing engines were used to thoroughly wash down the outside of the premises, and the gullies, drains, and traps, were flushed with disinfectants. When four or more cases had occurred in one house the place was vacated, and was not re-occupied until the District Medical Officer had been satisfied that it was free from infection. When the house was in such an unsatisfactory sanitary condition as to be unfit for human habitation it was vacated, condemned by a Sanitary Board, and marked with the letters U. H. H. The Municipality provided huts for people thus turned out of their houses.

33. General Gatacre's work on the Plague Committee ceased on 30th June 1897. The total mortality during the three months while he was at the head of the Committee was 8,379, or only 1,341 above what might have been expected as the normal mortality had the full population of the City been present in it. The number of reported plague deaths was 1,743. The population present in the City during June 1897 was estimated at 750,000. The mortality in this month was 2,326, or about 600 more than the normal mortality of a population of this size at the rate of 28 per thousand per annum. The plague had declined to a very small amount, but there were a number of destitute immigrants in the City, and labour was scarce and prices high. Many of the houses declared unfit for habitation had been unavoidably re-occupied. There were thus distinct elements of danger favouring a revival of the plague.

Plague mortality in Bombay City, from appointment of Plague Committee till General Gatacre's retirement from it on 30th June 1897.
25,719-21.
25,722.
Sir James Campbell's Report, p. 1.

34. From 1st July 1897 Sir James Campbell became Chairman of the Plague Committee. There was at that time considerable difficulty in obtaining the medical staff required. Officers of the medical services in India were required for famine relief, and subsequently for the operations on the North-West frontier, and the medical men sent to India from England on temporary engagements did not arrive till September. During July there were only 42 registered plague deaths. In August an increase occurred in the number of plague deaths registered, and a considerable increase in the total mortality due mainly to a serious outbreak of cholera. People began to arrive in the middle of August by sea from Cutch Mandvi, and, to guard against the spread of plague in the City by means of imported cases, camps were opened in which arrivals from Cutch and Karachi were detained for ten days. People arriving from the Konkan by boat were also detained. The Wari Bandar Hospital was converted into an observation camp, and enlarged so as to accommodate about 1,500 inmates. At the beginning of September the death-rate was fifty per cent. above the normal, and it rose during the month. The cholera outbreak of July and August was over, but there was a severe outbreak of relapsing fever. The number of plague cases reported in the four weeks ending on the 1st of October was 209, and of deaths from plague 159. Large numbers arrived in the City from the infected districts of Satara and Poona by boats from ports in the Ratnagiri district. A fresh observation camp, to accommodate 2,500 inmates, was opened at Modikhana. During the first half of the month of October the total death-rate and the recorded plague deaths both decreased. Plague cases increased in the second half of October, and from the 2nd to the 29th of the month 262 cases and 165 deaths were reported. The Plague Committee, owing to the cases and deaths outside the City of Bombay being nearly twice as numerous in October as in September, proposed to subject to quarantine persons arriving in Bombay from infected districts by land, but, owing to the objections of the Chamber of Commerce, quarantine was not introduced as a general measure during October. Special steps were, however, taken to prevent persons of the lower classes from bringing infection from the badly-infected cities of Sholapur and Surat. Third class passengers from Sholapur were removed from the train at Sion, and sent to the segregation camp, and orders were issued to stop the booking of third class railway passengers from Surat. These measures were followed by the enforcement of quarantine from the 19th November.

Bombay City.
Sir James Campbell succeeds General Gatacre as Chairman of Plague Committee on 1st July 1897.
25,721.
Sir James Campbell's Report, pp. 3-10.

35. There was no very great increase in the number of reported plague cases in November, 430 having been reported in the five weeks from the 30th October to the 3rd December. The first part of December passed without a serious rise. In the week ending 18th December, however, plague began to increase generally, and especially in the east and west of the town. The cases reported from the 4th December to the end of the month increased to 769, and the deaths to 605. Two measures came under consideration in his month, viz. :—

(i.) The segregation of "contacts," meaning thereby the patient's family and all who have been intimately associated with the patient;

(ii.) The vacation of infected or unwholesome houses, combined with the removal of the inmates to health camps.

Second outbreak in Bombay City. December 1897.

Sir James Campbell's Report, pp. 12-14.

Hitherto the segregation of contacts had been limited to encouraging or requiring one or at most two members of the family to accompany and attend the patient in hospital. The inmates of emptied houses were generally allowed to find fresh lodging where they liked. The rules now made regarding contacts provided that in large chawls (tenement-houses which often contain many hundreds of people), unless several attacks had taken place, it was not usually necessary to segregate the inmates of more than two or three rooms, and contacts in large private houses were permitted to be segregated either in another part of the house or in an outbuilding. Isolation was

Bombay City. Segregation of contacts in second outbreak.
Sir James Campbell's Report, p 12

allowed in private segregation camps which had been certified fit for the purpose. The inmates of public segregation camps were supplied with rations, and the bread-winner (male or female) was allowed to go to work provided the night was spent in the camp. All the clothes of contacts were disinfected on their admission into the camps.

Bombay City. Health camps in second outbreak. Sir James Campbell's Report, p. 13.

The second measure was the emptying of infected and unwholesome houses, and the placing of the inmates in health camps. The Committee considered the question of constructing large health camps in the Harbour Islands or Salsette. The Harbour Islands were found unsuitable from want of water, but convenient sites were found to be available in Salsette. These sites, however, being distant from Bombay, were suitable for the well-to-do classes only. The working people could not face either the going so far from their houses, or the time required for the daily journey. Arrangements were accordingly made to construct camps on the Dadar Flats, and at other places on the Island of Bombay, to accommodate 40,000 persons.

Bombay City. January and February, 1898. Measures adopted in the beginning of 1898, and the movements of population. Sir James Campbell's Report, pp. 16-21.

36. In January 1898 the epidemic became much more serious. In the first 28 days of the month the plague attacks rose to 2,532, and the deaths to 2,515. Bombay was practically isolated. No person, unless he was a resident in an uninfected quarter who had been for seven days previously under the supervision of the local Medical Plague Officer, was given a pass to leave the town without spending in the Modikhana camp the seven days previous to his departure. Ten days' detention at Kalyan and Bandra was imposed on persons leaving by railway. Native craft were prohibited from taking passengers from Bombay to any port between Karachi on the north and Bhatkal on the south. The detention of persons arriving in Bombay was stopped on 25th January. The Marwari and Gujarati traders left in large numbers during the month, and the excess of departures over arrivals was, in January, about 57,000, and in February, about 36,000. The total excess of departures over arrivals between July 1897 and April 1898 inclusive,—the people beginning to return in the latter month—was about 155,000.

During January a beginning was made in the formation of military search parties with a force of 50 men, under Major Ross, who were employed only in two quarters of the town occupied by Parsees and lower class Hindus. The staff of soldiers was somewhat increased in February. In the four weeks ending February 25th there were 4,591 reported cases and 4,464 deaths.

Bombay City. Corpse inspection. Sir James Campbell's Report, pp. 23-27

37. Early in March, the Committee had discussed the question of corpse inspection. They determined that it was not in agreement with the customs and feelings of any class in Bombay, that any attempt to enforce it was likely to give rise to active ill-feeling, and that the possible advantages from the practice were outweighed by the certainty of discontent. The rumour was circulated that corpse inspection was to be introduced, and on 9th March 1898 a riot occurred in connection with the taking of the daughter of a Muhammadan weaver to hospital. The riot was followed by a strike among cartmen and dockyard workers on 11th March. On 15th March the Governor announced that medical search parties would be replaced by the formation of a large number of volunteer committees. This change took effect from the 17th March, but was not in full working order till the middle of April. The number of recorded plague cases during the period from the 26th February to the 1st April was 5,449, and deaths 5,512. The climax was reached on 18th March, when the epidemic began to decline. When the turn had been taken it diminished rapidly. In April, from the 2nd to the 29th, the number of reported plague attacks declined to 2,438, and the deaths to 2,052. The total mortality, which had been 8,289 in the previous four weeks, was 4,871 in the four weeks ending 29th April. The decline was continued in the two following months. In the four weeks ending 27th May the total mortality was 2,700, and the reported plague mortality 514. Between 28th May and 1st July the figures were 2,496 and 200, respectively.

Bombay City. March to June 1898.

Bombay City. Municipal authorities resume charge of plague administration on 6th June 1898.

38. On the 6th June, the control of the plague operations was resumed by the Municipal authorities and the Plague Committee ceased to exist. The system pursued by the Municipal authorities provided for the division of the City into nine divisions, each under a District Officer, selected from the officers of the Indian Staff Corps and others on plague duty, who had a staff of doctors under him and was assisted by volunteer committees of native gentlemen. A case of plague having been reported, the Medical Officer went to the house and examined the patient. If he was satisfied that the case was one of plague, he wrote a certificate and sent the patient to the nearest hospital, except when the patient was, in his opinion, in a moribund state. This concession has lately been extended so that a certificate from a medical

25,555-25,561.

attendant, that a patient being attended by him cannot be removed without risk, exempts the patient from being taken to hospital. As soon as a case had been reported, the Contact Inspector visited the house, received a list of contacts from the medical officer, and sent them to a contact camp. They were required to keep in camp for ten nights, but were permitted to occupy themselves in their business during the daytime. The house was disinfected during their absence. A house in which a series of cases had occurred was evacuated. Any residents who had not been in contact with a plague case were sent to a health camp. There were a number of camps of this kind.

Mr. Du Boulay's evidence, Vol. III, pp. 305-310.

39. The mortality in Bombay City remained low in July 1898. In the four weeks ending 29th July the total mortality was 2,184, and the plague mortality 237. Then came a slight rise. The figures for the four weeks ending on 26th August were 2,858 and 444. The outbreak gained strength in September and the early part of October, and then began to decline again. From 27th August to 28th October (a period of nine weeks) there were 7,576 deaths of which 1,478 were reported to have been due to plague. In the following period of nine weeks, from 29th October to 30th December, the total mortality was 6,130 and the plague deaths 695. But the mortality had begun to increase seriously in the latter part of December, and with the beginning of the new year the outbreak rapidly became more serious. The following figures show the general mortality and the mortality from plague in the three succeeding periods of four weeks :—

Third outbreak in Bombay City, Statistics from July 1898 to June 1899.

Date.	Average Mortality.	Total Mortality.	Reported Plague Mortality.
31 December 1898 to 27 January 1899 -	2,025	4,975	1,298
28 January to 24 February 1899 - -	2,190	7,250	2,813
25 February to 24 March 1899 - - -	2,160	9,394	4,252

In the last week in March 1899 the mortality began to decline, and the decline was maintained, as the following figures show, till at the end of June there were very few cases of plague :—

1899.	Average Mortality.	Total Mortality.	Reported Plague Mortality.
25 March to 21 April - - -	2,327	7,154	2,788
22 April to 19 May - - - -	2,421	4,333	1,249
19 May to 16 June - - - -	2,109	2,623	359
17 June to 30 June (fortnight) - -	957	1,073	88

40. Taking the figures of total mortality and recorded plague mortality in Bombay City for a period of two-and-a-half years, from the beginning of January 1897 to the 30th June 1899, we find the following results. The total mortality in the whole period has been :—

Mortality recorded in Bombay City from plague and from all causes from January 1897 to June 1899.

January to December 1897 - - - -	49,128
January to December 1898 - - - - -	53,409
January to June 1899 - - - -	36,802
Total - - - -	139,339

The recorded plague mortality has been 41,194. The normal mortality in this period would be just about 69,000. The actual mortality has exceeded the normal mortality by about 70,400. It appears practically certain that at least 30,000 deaths, and probably more since the population was at different times far below the normal number, due to plague have been attributed to other causes. Assuming that the excess mortality in the two-and-a-half years had been due to plague, the mortality from plague in a population of 850,000 would be at the rate of 8·3 per cent. in the whole period, or about 3·4 per cent. per annum. Even if reasonable allowance be made for the extent to which the population has been below the normal number at different periods during the epidemics, the mortality must be considered remarkably low in a city which has suffered without interruption for so long.

25,719. In the three periods of six months, from January to June (which includes the period in which the epidemic was at its worst in each season), the normal mortality, the actual mortality, and the plague mortality, contrast as follows:—

Date.	Normal Mortality. (*a.*)	Total Mortality. (*b.*)	Plague Mortality. (*c.*)	Difference between (*a.*) + (*c.*) and (*b.*)
1st January to 30th June, 1897	13,700	28,350	8,950	5,700
" " 1898	13,700	34,700	15,250	5,750
" " 1899	13,700	36,800	12,850	10,250

III.—(*b.*) *History of the Plague, to the end of June* 1899, *in the Bombay Presidency, outside Bombay City.*

III.—(*b.*) (1.) *History of the Outbreaks in Sind in* 1896-7.

Plague in the Bombay Presidency, outside Bombay City, 1896-7.

41. In October and November 1896, isolated imported cases were detected from time to time at different places in the Bombay Presidency, but no important local infection was discovered during these two months.

First outbreak in Karachi City 1896-7. 11,093.

The first place outside Bombay City to be seriously attacked was Karachi City (population 105,200), which stands on the shore of a bay of the Indian Ocean at the extreme northern end of the Indian delta. It is in constant communication with Bombay. Plague was discovered here on the 10th December 1896. The mortality in the City during October November and December in 1896, taken either daily, weekly, or monthly, showed no indication of the presence of plague. The imported cases usually discovered before a local outbreak are wanting in the history of the introduction of plague into Karachi.

Manner of introduction of plague into Karachi not traced. 11,093; 11,846.

The first case of plague that was detected in the City came to the notice of the authorities on the 10th of December 1896, the patient having apparently been taken ill about the 6th of December. It is doubtful whether this is the first case that actually occurred in Karachi, for on the 11th of December another case of plague was found in the immediate neighbourhood, and no connexion could be traced between the two cases. Subsequently others were discovered in gradually increasing numbers. The man whose illness was discovered on the 10th of December was a Hindu cook, who was a resident of Karachi and does not appear to have come into any personal communication with people from Bombay. He, however, lived in Rampart Road, which was "inhabited by merchants who have a great deal to do with Bombay," and it was the general idea that the source of his infection came from Bombay, which was in constant communication by sea with Karachi. As precautions were taken to prevent the importation of infection by means of infected persons or clothes, rats and cargo have been suggested as the probable means by which the infection was carried. In the absence of any facts, however, it is unwise to adopt this or any other theory as to the mode by which the infection was conveyed to Karachi.

Course of first epidemic at Karachi, and measures adopted.

The epidemic reached its height in the second week of February 1897; it began to decline in the middle of March, but it did not stop till the end of July. The recorded plague mortality during the outbreak was:—

December 1896	59
January 1897	743
February (1st to 26th) 1897	939
27th February to 2nd April 1897	979
3rd to 30th April 1897	485
1st to 28th May "	150
29th May to 2nd July 1897	38
3rd to 30th July 1897	9

The recorded mortality of 3,402 falls at the rate of 3·2 per cent. of the census population. In the early stages of the outbreak, the sick were not compelled to go to hospital. The people were invited to send their sick to hospital, but few did so. It was only in cases in which the sick were moved from affected to non-affected areas that removal to hospital was insisted on. The disease spread very slowly. In January, people were encouraged to leave Karachi, and about 25,000 did so. About six weeks after the discovery of the first case, a group of about 2,000 people from the part of the

11,105. 11,133.

town where plague was worst were removed into camp across the Lyari, a river which flows to the west of the main portion of the town and has, for the greater part of the year, practically a dry bed. They took their sick with them, and were permitted to go daily to their business in the City. A good many cases occurred among them. They remained in camp until July. Following their example, a number of the other inhabitants of the town went out into camps. By the middle of February it had become the practice to remove the healthy people from infected houses to regular segregation camps; and, in March, this practice was extended to the inhabitants of neighbouring houses. On the 20th of March the operations were placed in charge of a Plague Committee. House to house searches were resorted to, stricter measures were taken for guarding people in the segregation camps, and the clothes of all entering or leaving the segregation camps were disinfected. 11,142. 11,149.

When the first epidemic died out at Karachi, strict precautions were taken to protect the City from re-infection by sea. Persons arriving by sea from infected places were detained under observation for 10 days, the crews of ships from infected ports were not allowed to land, and measures were taken to prevent native boats from infected places going alongside the Native Jetty, where they usually load and unload, or from communicating with the shore until they had undergone 10 days' quarantine. 12,151; 12,317–9

42. As the outbreak in Karachi City led directly to outbreaks in the three districts of Karachi, Hyderabad, and Shikarpur, it will be convenient to refer to them here before proceeding to other outbreaks in the Bombay Presidency proper. In the Karachi district itself there were only 234 cases (60 indigenous) and 174 deaths (45 indigenous). But the disease became more prevalent at places at a greater distance from Karachi. Sind outside Karachi City.

The town of Hyderabad (population 58,000 at the time of the Census, and now estimated at about 65,000) became infected from Karachi at the end of February. Thirty-five imported cases were discovered before the disease became indigenous. There was not a large exodus from the place. House to house inspection was resorted to in order to discover cases. About three-quarters of the population were removed from their houses. They were allowed to re-enter the town daily for business purposes. The sick were taken to hospital, and the healthy inmates of infected houses segregated. The spread of plague was slow till the beginning of April when there were 148 indigenous cases in the week ending on the 16th, immediately before and after which more than 80 cases were recorded each week. It then declined and the epidemic had died out by the beginning of June. The reported cases were 536, and the deaths 405. The attacks were at the rate of a little over ·8 per cent. of the census population. In the district of Hyderabad, outside the town, there were only 21 cases and 18 deaths. Outbreak at Hyderabad in Sind 1897. 18,115. 18,087.

The Shikarpur district is in the extreme north of Sind. The towns of Sukkur (population 29,300) and Rohri (population 9,000) are in this district, and are separated from each other by the Indus at a point where the railway crosses it. The first indigenous case of plague occurred in Sukkur on 12th February 1897. There is no record of any imported case having preceded it. There was no great amount of plague till towards the end of March, but in the five weeks ending 16th April, 423 cases were reported. The disease then declined and the last case was on 2nd June. The number of attacks was 537, and the deaths 392. The attacks were at the rate of 1·8 per cent. of the census population. About 10,000 people left the town. The worst infected quarter of the town was evacuated. Outbreak at Sukkur, 1897. 3,723. Account of Plague Administration in Bombay Presidency from September 1896 till May 1897, by Mr. Couchman, I.C.S., pp. 172–3

Rohri was infected in the second half of April. Twenty-two cases were imported into Rohri from Sukkur, the first at the end of February. Plague in an indigenous form was reported on 17th April. Half the population had then left the town. As cases occurred the blocks of houses in the neighbourhood were evacuated, the people taken for ten days out of their houses, and the houses disinfected both at the time when they were emptied and when they were re-occupied. The largest number of people taken out at any one time was 150. The infection was not serious. The epidemic declined from the middle of May and disappeared before the end of June. There were 151 cases and 92 deaths. The attacks were at the rate of 1·6 of the census population of the town. Outbreak at Rohri 1897. 12,024. 12,039. 12,043–4.

The disease spread to two villages on the north of Rohri, close to the frontier of the Punjab. The people were removed from their houses, and the outbreak ceased without the infection being carried across the border. 12,083–7.

During the outbreak in Karachi and the rest of Sind up to June 1897, 6,016 cases and 4,746 deaths were reported. During an epidemic of cholera in 1892, the three districts which were infected with plague in 1897 lost 14,659 persons by cholera.

III.—(*b.*) (2.) *Course of the Plague in the Bombay Presidency in December* 1896 *and January* 1897.

Bombay Presidency, December 1896 and January 1897.

43. Some places of the Bombay Presidency proper had become infected soon after Karachi. Towards the end of December 1896, plague moved into the Thana district, which adjoins the town and island of Bombay. Ninety-seven cases were reported from the district in that month, and 359 in January; the towns of Bandra (population 18,300) and Bhiwandi (population 14,400) were attacked in December, the towns of Kurla (population 11,500) and Thana (population 17,500) being attacked in January. Seventy-four cases, chiefly imported, occurred in the Ratnagiri district in January. Cases also occurred in Kolaba and Satara, and a few in other districts.

First outbreak at Poona City and Cantonments, 1896–7.

A number of imported cases had also been discovered in December, 1896, at Poona, the chief town of the Poona district, between which and Bombay there is very close communication. At the end of the month two local cases occurred in the town. The population of the town proper, ascertained at the census of 1891, was 118,790, that of the suburban area, 7,506, and that of the Cantonment, 35,094. The area on which this population (161,390 in all) is collected, extends over about 13 square miles. By the end of January, 1897, plague had taken a firm hold on the City, and it became much worse in February. It was determined to employ the agency of troops in dealing with the outbreak. At the beginning of March a Plague Committee was appointed to control the measures for its suppression. These measures, which provided for the detection of plague cases by means of search parties, the compulsory removal of plague patients to hospital, the removal to segregation camps of healthy persons who had been exposed to plague infection, and the disinfection of houses and of movable property which had been exposed to infection, were of a more stringent character than had been attempted elsewhere up to this time. At one time the number of troops employed on plague duty was 1,112.

Page 34 and App. 53 of Mr. Rand's Report attached to the account of Plague Administration in the Bombay Presidency from September 1896 to May 1897, prepared by M. E. Couchman, I.C.S.

The measures undertaken met with great opposition. On 22nd June Mr. Rand, the Magistrate and Chairman of the Plague Committee, and Lieutenant Ayerst, who was mistaken for another officer employed on plague duty, were murdered.

The outbreak was at its worst in the middle of March and the mortality remained high till the middle of April, when it declined rapidly. By the end of May it was practically over and only isolated cases occurred in the two following months. The recorded attacks and deaths from plague, the total mortality, and the normal mortality, are compared, for the period up to 20th May, in the following table compiled from statements printed in Mr. Rand's Report, the figures being the totals for the City, the suburbs, and the Cantonments taken together:—

Date.	Plague Attacks.	Plague Deaths.	Normal Mortality	Total Mortality.
December 1896	49	37	364	388
January 1897	193	150	358	657
February 1897	271	211	304	1,114
March 1897	1,198	816	339	1,842
April 1897	718	497	363	853
May to 20th	76	58	237	200
Total	2,505	1,769	1,965	5,054

Towards the close of the epidemic the difference between the actual and the normal mortality is more than accounted for by the mortality recorded from plague. But this was not the case in the early stages of the outbreak, and while the epidemic was at its height. Considering that the population was below the normal strength, it is reasonable to infer that the difference between the actual and normal mortality during the epidemic, viz., 3,089, more nearly represents the actual mortality from plague than does the recorded plague mortality of 1,769. In the City alone the normal mortality should have been 1,435 and the actual number of deaths exceeded this figure by 2,787, which represents a death-rate of 2·3 per cent. on the census population of the City proper; the number of recorded deaths in the City was 1,481, which is 1·2 per cent. of its census population.

III.—(*b.*) (3.) *Course of the Plague in the Bombay Presidency in February* 1897.

Bombay Presidency, February 1897.

44. In February, up to the 26th, there were 979 cases in the Thana district (in which the infection spread to the town of Bassein, of which the population amounts to about

11,300), 97 in Ratnagiri, and 53 in Satara. The Surat (117 cases), and Kolaba districts (82 cases), Kolhapur (8 cases), and parts of Kathiawar (51 cases), became infected.

Some indigenous cases occurred in the town of Surat (population 108,700) at the end of the month. There appear to have been more than one source of infection, but one, at all events, seems to have come from Rander, a neighbouring town. Thirty-three imported cases were detected before any indigenous case was reported. The infection of the town on this occasion was very slight, and when plague ceased on 11th May, there had only been 49 imported and 65 indigenous cases. The sick were sent to hospital and their relatives segregated. The infected houses were emptied, limewashed, and their tiles taken off. First outbreak Surat, 1897. 15,856. 15,562. 15,626.

The town of Bulsar in the Surat district (population 13,000) was attacked with indigenous plague in February, after six cases had been imported from Bombay. An epidemic then broke out which lasted till the end of June 1897. The number of reported indigenous cases was 902 (6·9 per cent. of the population). First outbreak Bulsar, 1897.

III.—(*b.*) (4.) *Course of the Plague in the Bombay Presidency in March* 1897.

45. From the 27th of February to the 2nd of April, 1,859 cases were reported from the Thana district, where the outbreak reached its height at the end of the month, 940 from the Surat district, 131 from the Poona district excluding the City, 186 from the Kolaba district, 98 from Ratnagiri, and 53 from Kathiawar. Fresh centres of infection were discovered in the Naosari division of the Baroda State (103 cases), in Palanpur, the capital of the Palanpur State (85 cases), and in the Cutch State (83 cases). Bombay Presidency, March 1897.

The town of Palanpur (population 21,000) was infected in February 1897 by a trader who had been to Bombay. The epidemic was not serious, but the reported cases (141 between February and 15th May 1897 when the outbreak ceased) understate its actual extent. First outbreak Palanpur, 1897. 13,054. 13,159.

Indigenous plague occurred in Cutch Mandvi, a walled town of 38,000 inhabitants, which is the chief port of the Cutch State, and is situate between Karachi and Bombay. It is in frequent communication with both of these places. It appears that the actual source of infection was Karachi. Cases were not reported to the Political Agent till the middle of April 1897, when the town was already badly infected. In the early part of the outbreak there were no arrangements for dealing with the disease. By the end of April hospitals had been started and measures taken to segregate contacts. The outbreak was a virulent one, particularly at the end of April and in the beginning of May. After the climax had been reached in the first week of May the disease gradually declined, and was practically over early in July. Plague lingered on, however, till 13th August. The number of attacks is given by the State authorities as 4,298, and the number of deaths as 4,224, but neither of these figures has any claim to be accurate. Calculated upon them the rate of mortality per cent. of the census population was over 11, but it was, undoubtedly, much higher. All deaths ascertained at the burning or burial grounds were treated as due to plague, and the people were removed from the infected house and allowed to camp out where they liked. About 20,000 persons left the city and went into camp. First outbreak Cutch Mandvi, 1897. 13,360, *sqq.* 13,384–5. 13,374–7. 13,378–9.

From Cutch Mandvi plague spread to the interior of the Cutch State in which twenty-four places were infected.

About the beginning of March 1897, plague began also in Daman, a small Portuguese settlement situated on the coast between Bombay and Surat. The town is divided by a stream into Upper Daman (population 5,000) and Lower Daman (population 7,000). During this outbreak plague was confined to Lower Daman. There was no record of mortality, but it is estimated that 2,000 persons (16·6 per cent. of the population of the whole of the town) perished between the outbreak of the plague and its cessation at the beginning of July. Practically no measures were taken to arrest the progress of the disease. Outbreak at Daman, 1897. 16,343, *sqq.* 16,385. 16,438, 16,486. 27,214. 27,229.

III.—(*b.*) (5.) *Course of the Plague in the Bombay Presidency in April* 1897.

46. From the 3rd of April to the end of the month there was a slight improvement in the Thana district, but 1,201 cases were reported from it. From the Surat district 784 cases were reported. The outbreak in this district reached its highest point in the middle of the month and then began to decline. In the Kolaba district there were 568 cases. In the Cutch State, including the town of Cutch Mandvi, there were 601 cases, and in the Baroda State 245. From the Palanpur State 54 cases were Bombay Presidency, April 1897.

returned. The outbreak there was over by the middle of the month. Seventy cases were returned from the Poona district excluding the City, 51 from the Ratnagiri district, and 64 from Kathiawar.

III.—(*b.*) (6.) *Course of the Plague in the Bombay Presidency in May* 1897.

Bombay Presidency, May 1897.

47. In May there was a general improvement, except in the Cutch State. The returns from that State for the period from the 1st May to the 4th June showed 3,482 cases altogether, of which 3,403 were indigenous cases in the town of Cutch Mandvi, and 23 indigenous cases in other parts of the State, the rest of the cases being imported. The cases reported from the Thana and Surat districts fell to 416 and 138 respectively. In the Thana district the disease died out in Bandra and Bhiwandi in the beginning and middle of the month, after the total number of indigenous deaths from plague in these towns had reached 711 in the case of the former, and 432 in the case of the latter. The towns of Thana and Kurla became free of plague about the end of the month, the total number of indigenous deaths reported having been 90 in Thana, and 366 in Kurla. Three hundred and fifty-six cases were reported from the Kolaba district, and 272 from the neighbouring State of Janjira, of which almost all occurred in the town of Murud which had become infected towards the end of March and reported 38 cases in April. One hundred and thirty-two cases were reported from the Baroda State, and 68 from Kathiawar. Plague was present in a few other districts, but to a very unimportant extent.

III.—(*b.*) (7.) *Course of the Plague in the Bombay Presidency in June* 1897.

Bombay Presidency, June 1897.

48. In June matters still further improved. From the 5th of June to the 2nd of July 912 cases were reported in the Bombay Presidency and connected Native States, including 157 which occurred in Bombay City. In this period of five weeks 566 cases occurred in the Cutch State, of which 324 were indigenous cases in the town of Cutch Mandvi and 232 indigenous cases in other parts of the State, where Gundiali and Maska were chiefly infected. Of the 189 cases returned from the rest of the Presidency, 35 occurred in the Kolaba, and 36 in the Thana district, where the outbreak in Bassein came to an end after 351 indigenous deaths had occurred in the town.

III.—(*b.*) (8.) *Course of the Plague in the Bombay Presidency in July* 1897.

Bombay Presidency, July 1897.

49. Though only 644 cases were reported during July 1897 from the 3rd to the 30th of the month, including 52 in Bombay City, the increase in the number of cases as the month advanced pointed to the likelihood of a fresh outbreak. One hundred and twelve cases were reported from the villages in the Poona district.

Outbreak in Poona and Kirki Cantonments, 1897.

21,193.

21,208.

21,247.

21,222.

21,251.

21,259.

21,221.

21,314, 21,316.

The Cantonment of Poona also had become infected on 27th June 1897. Plague first broke out among the followers in the Native cavalry lines. The lines were vacated on 6th July, and ultimately, as cases continued, the regiment went into a camp in the neighbourhood but was moved into camp across the river on 15th September. On 12th July four cases were found in the neighbouring Cantonment of Kirki (population 11,000). Dropping cases continued till 31st July when the disease became epidemic. On the 7th August, cases appeared in the Royal Artillery followers' lines contiguous to the Kirki Bazar. The occupants were moved out and the lines disinfected. On 9th August, a case imported from Kirki was discovered in the Poona Cantonment, and thereafter other cases occurred. From 25th August, a rigorous system of search parties was introduced. The sick, on discovery, were sent to hospital, the whole of the inhabitants of the house segregated, and the house immediately disinfected. The people were allowed to return to their houses after ten days. Plague ceased to be epidemic in the Cantonment of Kirki in the middle of September, and was practically over by the end of the month. At about the same time the epidemic became virulent in the Cantonment of Poona. In September and October it attacked the native troops and their followers. A Plague Committee, consisting of the late Major-General Duncan, Lieut.-Colonel Fawcett, R.A.M.C., and Major Ross was appointed in November. The whole Cantonment was then searched daily. Plague ceased to be indigenous at the end of December, but it did not stop finally till 11th March 1898. The population, nominally about 35,000, dwindled during the epidemic to about 12,000. The number of cases reported in the Poona Cantonment was 422, and the deaths 298. In Kirki, the outbreak, though it lasted a shorter time, was more severe. There were

690 cases and 561 deaths in Kirki. The rate of attacks was 6·3 per cent. of the census population.

The number of cases reported during July in the Surat district was 60. Plague broke out again in the town of Surat in that month. One case occurred on 4th July, but until 30th July no further cases were reported. The mortality did not rise above the average until August. From 15th August till the beginning of the following May, cases occurred continuously. Between 28th July 1897, and 3rd May 1898, the mortality exceeded the average mortality by 4,154. The reported plague cases were 2,551, and the deaths 1,737. The outbreak was at its worst in October and November. In this outbreak, people suffering from plague were taken to hospital, each patient with one attendant. All the inmates of a plague-infected house were removed to the segregation camp, and those of the neighbouring houses to health camps. As soon as the rains permitted, this course—the general evacuation of infected localities—was enforced. At one time nearly 17,000 persons were in camps. Under the system adopted the whole of a locality where more than one case occurred within a short time, or where plague was found to be persistent, was evacuated. All uncertificated deaths were treated as plague deaths, and the inmates of houses in which an uncertificated death occurred were segregated. It would appear, however, that many cases due to plague must have been certified as due to other causes. The infected houses were disinfected with perchloride of mercury, and afterwards lime-washed, and light and air was, if necessary, admitted. The incidence of reported cases per cent. of the census population was 2·3, but the number of cases that occurred must have been far beyond the number reported.

Second outbreak at Surat, 1897. 15,728. 15,737. 15,729. 15,715. 15,734.

One hundred and fifty-one cases were returned from the Satara district in July 1897. The seat of infection here was the town of Karad (population 12,000). Many people from this part of the country are employed in Bombay as porters or on similar work. Plague was not notified at Karad till 7th July, when it had clearly been present for at least two months. Plague was discovered when the rains were very heavy, and it was impossible to evacuate the town. The sick, and those in contact with them, were isolated, and the infected houses and their contents disinfected. These measures were begun at the end of July, and improved in September. Plague continued till the beginning of December. The number of cases reported during the outbreak was 1,517, and the deaths 1,153. The rate of attack per cent. of the population was 12·5.

Outbreak at Karad, 1897. 21,060. 21,090.

A few cases were reported during July in the Kolaba district (23), and the Cutch (129) and Baroda (53) States. Karachi City was very slightly infected, but the rest of Sind was free.

III.—(*b.*) (9.) *Course of the Plague in the Bombay Presidency in August* 1897.

50. In August, in the five weeks beginning on the 31st July and ending on the 3rd September, there was a sharp increase in the returns from the Satara (1,421), and Poona (934), districts. The infection in the Satara district was still centred in the town of Karad. Two hundred and eighty cases were reported from the Cutch State.

Bombay Presidency, August 1897.

The City of Poona was re-infected in August, it is supposed from the neighbouring Cantonment of Kirki. The second epidemic in the City became very severe in October and November, and reached its climax in the second week in December. After that there was a continuous fall until the City became free at the end of February 1898. The measures taken during the second were less stringent than those taken in the first epidemic. Endeavours were made to work with the co-operation of the people by means of a system of employing volunteers, and this resulted in better information being obtained. The system of segregation was made less strict. The number of cases reported in this epidemic was 4,644, and of deaths 3,633. The population in the City during this outbreak was much reduced. It is estimated that it fell to not more than 80,000. Calculated on the normal population of the City proper, the ratio per cent. of attacks is 3·9, and on the population actually assumed to be present 5·8 per cent.

Second outbreak in Poona City, 1897–8. 21,681. 21,684. 21,694. 21,685. 21,782.

The Nasik district also became infected in August at Igatpuri. This is a town of 7,500 inhabitants, which has grown up on the Western Ghats, whence the Great Indian Peninsula Railway descends into the Konkan. It consists almost entirely of people in the employment of the railway company. It was infected from either Bombay or Poona. The place has a very high rainfall, and, on account of heavy rains, it was impossible to evacuate it. All that could be done was to put the sick into hospital, to clean up and disinfect houses that had been vacated by their owners and to place in them the people from infected houses, to disinfect infected houses and allow the inmates to reoccupy them at once, and eventually to evacuate infected localities by accommodating the

Outbreak at Igatpuri, 1897. 16,884. 23,196. 19,493.

people in waggons supplied by the railway company. The epidemic here lasted 13 weeks, during which time there were 512 cases and 411 deaths. The incidence of attacks amounted to 6·8 per cent. of the population. From Igatpuri plague was taken to other places in the Nasik district.

During August a few cases were returned from the Thana (56, chiefly from Kalyan, which has a population of about 12,500), Kolaba (13), and Surat (48), districts, the Palanpur State (61), and the Baroda State (15).

III.—(*b.*) (10.) *Course of the Plague in the Bombay Presidency in September* 1897.

Bombay Presidency September 1897

51. In September the outbreak became very serious in the Poona (759 cases), and Satara (1,960 cases), districts. The figures here given for this month refer to the period from the 4th of September to the 1st of October. In the City of Poona reported cases rose to 354. The returns from the Cutch State showed 501 cases, the Nasik district had 304 cases (chiefly at Igatpuri), the Surat district 182, the Thana district 86 (Kalyan remaining the chief centre of infection), and the Kolaba district 83. There were a few cases in the Baroda State (35), and in the districts of Ahmednagar (37), Sholapur, and Ratnagiri.

Second outbreak at Palanpur, 1897.

13,059–60.

Two hundred and fifty-four cases were reported from the Palanpur State. The first outbreak in Palanpur City had ceased in May 1897. In the beginning of August 1897 plague reappeared in the quarter of the town which had been previously infected. The first case was again that of a trader who had returned from Bombay three days before he was attacked. About one-third of the people left the town. Search parties were utilized to find cases. Patients were sent to hospital, and their relatives and those living in adjacent houses segregated. The disease began to decline towards the end of September, but many of the people who had left the town came back then for a festival. An immediate increase in the number of cases resulted. The inhabitants of the town (then 13,000) were on 2nd November turned out of it. The whole town was then disinfected. The people were allowed to return on 15th December. Seven or eight imported cases occurred afterwards, but no indigenous cases. The number of cases reported during the epidemic was 674. The ratio of attacks per cent. of the normal population is 3·2, but the population present during the epidemic was never nearly as high as the ordinary population.

13,080

13,159.

III.—(*b.*) (11.) *Course of the Plague in the Bombay Presidency in October* 1897.

Bombay Presidency, October 1897.

52. In October, from the 2nd to the 29th, the outbreak gathered force in the Satara district (3,506 cases), especially in the villages in the Karad taluka; an outbreak also began in the town of Tasgaon (population 11,300) in the Tasgaon taluka, where one or two other places also were severely attacked. Plague spread to the Aundh State in the neighbourhood of the Satara district in August, and up to the 29th October about 120 cases occurred in the State, of which about 50 took place in the town of Aundh. In the villages in the Poona district there was a substantial decline, the cases returned amounting to 517, compared with 759 in previous five weeks. On the other hand the outbreak became very severe in Poona City (1,380 cases). The number of cases reported from the Cutch State (471) and the Thana (62) and Kolaba districts (41) was slightly less than in the previous month. In the Surat district 369 cases were reported in October, mainly in the town of Surat. In the Palanpur State (244 cases) the outbreak was stationary, and there was very little plague in the Baroda State (37 cases). The plague had died out in Ratnagiri. The cases returned from Nasik district fell to 191, but Nasik town became infected from Igatpuri during the month.

First outbreak at Nasik, 1897–8.

16,918–46.

Nasik is a town of 24,500 inhabitants, in which several imported cases had been discovered between March 1897 and 12th October 1897, when the first indigenous case was found. At the beginning of the outbreak only the occupants of infected houses were removed, and the houses disinfected. The next step was to remove the residents of neighbouring houses. In the middle of November the town was divided into wards, and the wards gradually evacuated as they became infected. Eventually (by the beginning of January 1898) the whole town on the south of the Godavari river was emptied. Plague did not stop till 25th March 1898, nearly five-and-a-half months after it was discovered. There were 450 cases during the outbreak (1·8 per cent. of the population).

16,937.

Outbreak at Sholapur, 1897–8.

23,888 *sqq*

20,230 *sqq*.

In the Sholapur district (from which 170 cases were reported) the epidemic was confined to the town of Sholapur (population 62,000). The infection was apparently brought from Poona; the outbreak began in a suburb half a mile outside the town in September. A cordon was drawn round this suburb, but this endeavour

to isolate the people was a complete failure, and in October the town itself was attacked. On the 15th of November the infected suburb was evacuated, and plague ceased among the inhabitants of it in a fortnight. In the town a beginning was made by evacuating the worst quarters, and by the end of November four quarters containing 400 houses had been evacuated. The plague still spread, and at the beginning of December it was determined to evacuate the whole town. After the town had been emptied the camps, which were occupied by 25,000 to 30,000 persons, enjoyed almost complete immunity from plague. The sick were taken to hospital, and people exposed to infection to the contact camps. The number of cases reported, during the epidemic, which ceased early in February, was 2,628. The incidence of reported attacks per cent. of the population was 4·2. 20,244, 23,903. 20,233, 23,932.

In the Ahmednagar district 138 cases were reported from the Ahmednagar Cantonment and one other place. The first case detected in the Cantonment was reported on 27th September 1897. The infection apparently came from Poona. Different parts of the Cantonment were evacuated, the first on 8th October 1897, and the last in the beginning of December. The people were allowed to return on 19th March 1898, after the houses had been disinfected and whitewashed, and, except in one quarter, also unroofed. Outbreak in Ahmednagar Cantonment, 1897–8. 24,131–3.

The infection reached the City of Ahmednagar (population 36,000), apparently from the Cantonment, at the end of October, and the last case occurred on 21st March 1898. The usual steps were taken to send patients to hospital. The residents of the infected house, and of a certain number of houses on each side, were taken to the contact camp, and the houses disinfected. As these measures were ineffective, it was decided to evacuate whole wards at a time, and ultimately the whole City was evacuated by the middle of March, about 16,000 people having left the place and about 20,000 being in the camps outside. The number of reported indigenous cases in the outbreak was 407, of which 315 were fatal. The rate of attack per cent. of the population was 1·1. Outbreak in Ahmednagar Town, 1897–8. 24,227. 24,237–8 24,358–9.

III.—(*b.*) (12.) *Course of the Plague in the Bombay Presidency in November* 1897.

53. In November the returns from the Satara district were worse than ever and from the 30th of October to the 3rd of December, 4,788 cases were reported. Plague had appeared in 84 places in this district. There were 251 indigenous cases in the Aundh State. Bombay Presidency, November 1897.

A few cases occurred in Satara City (population 25,700) in October and November, but the outbreak did not become serious till the middle of December. The usual measures were taken with the sick and contacts, but there was no general evacuation of the town. The outbreak lasted till April 1st. There were 873 cases and 727 deaths. The rate of attack was at about 3 per cent. of the census population. Outbreak in Satara Town 1897–8. 22,258.

The cases returned for the Poona district, in which 40 places were infected, had risen to 855. In addition to this, 2,805 cases were returned from the City of Poona. Owing to the large and rapid rise in Sholapur City, 1,357 cases were reported from the Sholapur district. From the Surat district 1,017 cases were reported, 370 from the Cutch State, and 168 from the Ahmednagar district.

Two hundred and ninety cases were reported from the Nasik district. The town of Malegaon (population 18,000) in this district became infected in November from Bombay. Blocks of the town were at first evacuated, disinfected, and whitewashed. Plague mortality, however, remained high, and in January 1898 the whole population was removed from the town. It took a fortnight to effect this evacuation, and within a month of its completion plague had ceased. There were 395 cases in all, with 314 deaths, a little under 2·2 per cent. of the census population having been attacked. Outbreak at Malegaon, 1897–8. 13,774.

Two hundred and twenty-eight cases were returned from the Belgaum district, mainly from the Cantonment at Belgaum. Plague was imported into the Cantonment in September 1897 from Sirur, in the Poona district. It began in the City at the end of November. In this outbreak infected and neighbouring houses were evacuated, the sick sent to hospital, and those connected with them segregated. The houses were not allowed to be re-occupied till they had been disinfected, and were rarely re-occupied till a fortnight after they had been vacated. The outbreak was at its worst in January 1898, and continued till the second week in May. During this epidemic 762 indigenous cases were reported in the town and Cantonment. The population of the town and Cantonment is 41,400, but that of the town alone is about 30,700, and, as 432 indigenous cases were reported there, the rate of attack in the town, calculated on the normal population, was 1·4 per cent. Outbreak at Belgaum, 1897–8. 23,028. 23,122.

In November there were 166 cases in Palanpur, 63 in Kolaba, 51 in Kolhapur, 38 in Thana, and 30 in the Baroda State.

The number of cases reported during October and November from the Karachi district was 19 and 57. An outbreak took place in the town of Kotri on the 28th October, and continued till the beginning of February 1898.

III.—(*b.*) (13.) *Course of the Plague in the Bombay Presidency in December* 1897.

Bombay Presidency, December 1897.

54. There was an improvement in the general position in December, particularly during the second half of the month. From the 3rd day of the month to the 31st, the outbreaks declined in the Satara district (1,604, cases) the Aundh State (194 cases), Poona City (1,403 cases), the Poona district (390 cases), and the Cutch State (132). In the Surat district also (654 cases), and especially in Surat town, there was a material decline in the number of attacks. As many as 1,238 cases were reported from the Sholapur district, almost entirely from the town, but by the close of the month the number of cases was rapidly falling there. On the other hand, matters became worse in the Nasik district (344 cases), in which Nasik and Malegaon were the places most infected; the Palanpur State (400), where the increase occurred among the villages; and in the Ahmednagar district (273), chiefly in Ahmednagar itself. The town and cantonment of Belgaum were mainly responsible for the 308 cases returned from the Belgaum district. The cases returned from the neighbouring State of Kolhapur rose to 72, and 37 cases were reported in the adjoining district of Dharwar also. The small town of Jalgaon (population 14,000), in the Khandesh district, adjoining the Central Provinces and the Hyderabad Assigned Districts, became infected, and 69 cases were reported from the district. The number of cases in the Thana district (67) increased owing to the infection of the town of Bassein; the town of Thana was also infected. Forty-six cases were reported from the Akalkot State, 39 from the Janjira State, 19 from the Kolaba district, and 16 from Kotri.

First outbreak at Baroda, 1897–8.

14,582, 15,584.

14,596.

The number of cases reported from the State of Baroda rose from 30 in November to 100 in December, owing to the increase of cases in Baroda City. This city, the capital of the Baroda State, has a population of 112,500. A few indigenous cases of plague occurred in it in April 1897, but there was no outbreak. During the rainy season many persons came from Surat and other infected places, and the town became infected in October 1897. The isolation of the sick and the segregation of contacts were undertaken from the beginning, and subsequently whole streets were evacuated. In spite of these measures, and the disinfection of infected houses, plague spread, and ultimately the whole town was evacuated in March 1898. Eleven thousand persons were lodged in the camps maintained by the State, and about 40,000 persons went into the fields. Plague ceased in April 1898, and by the end of May the town was nearly full again. Fifteen hundred and five cases were reported between the beginning of the outbreak and the end of May. The general mortality was very high, and doubtless many deaths due to plague were attributed to other causes. The rate of attack, calculated on the reported cases, and the normal population, which was, however, much reduced, is 1·3 per cent., but the figure is quite unreliable.

14,614.
14,620, 14,677.
14,628.
14,637.
14,983.

III.—(*b.*) (14.) *Course of the Plague in the Bombay Presidency in January* 1898.

Bombay Presidency, January 1898.

55. The general improvement observed in December was maintained in January 1898. From the 1st January to the 4th February the number of cases in the Satara, Sholapur, and Poona, districts declined to 360, 605 and 154 respectively. In the middle of the month the outbreak at Tasgaon in the Satara district ceased, after causing 453 deaths. Four hundred and eighty-three cases were reported from Poona City. The Bhor State, in the neighbourhood of Poona, in which about 30 cases had occurred up to the end of 1897, reported 24 cases in January 1898. The cases returned from the Baroda State numbered 624, chiefly from Baroda City. The number reported from Nasik was 461 (mainly from Nasik and Malegaon), from the Surat district 807, and from the Belgaum district 427, chiefly from the City. Plague rose slightly in the Cutch State (264 cases), the Kolhapur State (245 cases), and the Kolaba district (95). The number of cases reported from the Thana district rose to 151, the infection spreading to Bandra during the month. Six hundred and fifty-one indigenous deaths took place in Bandra before the outbreak there came to an end in the latter part of May. Seventy-three cases were reported in Khandesh, where the infection was slight, and plague declined a little in the Palanpur State (258), and the Ahmednagar district (252 cases). The only cases reported from Sind were eight cases at Kotri.

III.—(*b.*) (15.) *Course of the Plague in the Bombay Presidency in February* 1898.

56. Further improvement was reported in February. The figures given below cover the four weeks from the 5th February to the 4th March. The cases reported from the Satara district increased to 525; but in Poona City they declined to 113 cases, and in the rest of the Poona district to 86 cases. On the other hand, in the Surat district (878 cases) there was no improvement. The number of cases reported from the Baroda State was 1,036, mainly from Baroda City. An increase occurred in the Kolaba district to 158 cases. In the Sholapur district 424 cases were reported, almost entirely from the villages. In Kolhapur (chiefly in the Sangli State) there were 329 cases; in the Belgaum district 275 cases; in the Thana district 177 cases; in the Nasik district 203 cases; in the Ahmednagar district 140 cases; in the Palanpur State 103 cases; in the Cutch State 78 cases; in the Khandesh district 79 cases; in the Aundh State 50 cases; and in the Akalkot State 40 cases. No cases were returned from Sind. Bombay Presidency, February 1898.

III.—(*b.*) (16.) *Course of the Plague in the Bombay Presidency in March* 1898.

57. Between 4th March and 1st April there was again a substantial improvement. The number of cases reported from some places was large during this period, but in the majority of instances, and noticeably in the Surat district and the Baroda State, the outbreak was declining towards its close. Two hundred and eighty-eight cases occurred in the Surat district. These figures and those given for the district for previous months include figures for the Sachin State, in which 192 cases had occurred up to the beginning of April, 95 indigenous cases having occurred in March and 44 in February. From the Satara district, 324 cases were reported; from the Poona district, 84 cases (28 in the City); from the Baroda State, 844 cases; from the Thana district, 308 cases, the worst infected place in the district being Bassein; from the Kolaba district, 235 cases; from the Belgaum district, 221 cases; from Kolhapur, 326 cases; from the Khandesh district, 88 cases; from the Nasik district, 180 cases; from the Sholapur district, 96 cases; from the Ahmednagar district, 34 cases; from the Palanpur State, 157 cases; from the Cutch State, 45 cases; and from Kathiawar, 66 cases. One hundred and fifty-one cases occurred in Ratnagiri and a few cases in the Kaira, Dharwar, and Bijapur, districts. Bombay Presidency, March 1898

Nineteen cases were reported from Karachi City, which had been free from plague since July 1897. In investigating the cause of the second epidemic of plague in Karachi, and inquiring into the possibility of a re-importation of infection, the following circumstances must be considered. It is not impossible that the infection remained in the City without altogether dying out between the first epidemic and that which is now in question, for certain suspicious cases, that may have been cases of mild plague, were observed in the interval between the first and second epidemics. In September 1897, one month after the close of the first epidemic, cases of enlarged cervical glands were observed, and at least one of these proved fatal. Later, and simultaneously with the outbreak of the second epidemic, a suspicious case of enlargement of the femoral glands was detected in a boy. Mr. Giles, the Commissioner in Sind, however, was confident from the result of the inquiries which were instituted in the above cases, that Karachi was quite free from plague between the end of the first and the beginning of the second epidemic. This latter was ushered in by the discovery towards the end of March 1898 of two cases of plague, which apparently originated independently in the City. The first case was that of a Sikh carpenter, a resident of Karachi, who lived in the Sarai quarter. The case, which was seen on the 22nd of March by Colonel McCloghry, I.M.S., occurred in a part of the town that had escaped infection in the first epidemic. The patient had apparently not visited any infected quarter, and the source of infection could not be traced. In view of the fact that the carpenter worked at the Native Jetty, it is surmised that he may have been infected by cargo landed there from Bombay. This inference, however, is not conclusive, for in the first place it is not certain that the carpenter's case was the first case of the second epidemic; again, dead rats appear to have been found before the case occurred; and lastly, a severe outbreak, which was apparently quite unconnected with the case of the carpenter, broke out immediately after in the Joria Bazar, about half a mile from the Native Jetty. It is in connexion with the outbreak in the Joria Bazar —an outbreak which marks the commencement of the second epidemic—that the second case spoken of above was discovered. No light has been thrown on the source of infection from which this epidemic originated. All that is certain with regard to

Cause of second outbreak at Karachi not ascertained.

11,599–612.

11,183.

11,512–6.

11,195.

11,189.

it is that it was preceded by great mortality among rats, and that infection first established itself in the bazar, which is occupied by retail grain sellers, who, though they have little personal communication with the harbour or the Native Jetty, deal in goods imported from Bombay. In view of this circumstance, the theory was mooted that the second epidemic of plague in Karachi was due to the importation of the infection in merchandise from Bombay. Nothing, however, has come to light which would warrant either the acceptance or the rejection of this theory.

11,197; 11,338–44 11,357–64. 817.

Course of the second and third outbreaks at Karachi City. 1898–9. 11,260.

The second epidemic spread far more rapidly than the previous one. A special feature in the measures taken to deal with this outbreak was the establishment of the two health camps in the desert area across the Lyari river, which accommodated nearly 25,000 persons between them. The people were, after the middle of September 1898, allowed to leave the City with their sick for those camps, and were not detained in the ordinary segregation camps, provided that all cases of sickness were notified. 586 cases and 412 deaths were reported at Karachi in April up to the 29th. From the 30th April to the 3rd June the returns were 2,093 cases and 1,704 deaths. Towards the end of May matters improved, and in June, from the 3rd June to the 1st July, the cases fell to 294, and the deaths to 225. Cases, however, continued to occur, and by the end of December 1898 there had been 3,263 cases and 2,540 deaths. 27 attacks were recorded in the first five weeks of the year 1899, and cases began to get more numerous in February. In March the outbreak became very serious again, the number of cases reported from the 3rd to the 31st of the month being 767, and the deaths 513. The outbreak increased in severity in the early part of April, but began to decline during the second half of the month. There were 1,421 cases and 1,028 deaths between the 1st and 28th April, and in the next four weeks 646 cases and 501 deaths. In the last fortnight of June there were only three fresh cases and three deaths. From the date on which plague re-appeared, in March 1898, to the end of June 1899, the number of cases reported was 6,301, and the deaths 4,731. The attacks fall at the rate of nearly 6 per cent. of the census population.

III.—(*b.*) (17.) *Course of the Plague in the Bombay Presidency in April* 1898.

Bombay Presidency, April 1898.

58. Except at Karachi, where 69 imported cases occurred in the district in addition to the cases in the City, the outlook materially improved in April, and particularly in the second half of the month. Between the 2nd and the 29th of April, plague declined in the Satara (128 cases), Poona (13 cases), Surat (188 cases), Sholapur (30 cases), Nasik (33 cases), Khandesh (27 cases), Kolaba (104 cases), and Belgaum (147 cases), districts, and in the Baroda State (335 cases), the Palanpur State (41 cases), and the Cutch State (16 cases). In two districts, viz., Thana (381 cases), and Ratnagiri (197 cases), and in the Kolhapur State (357 cases), plague grew worse.

Second outbreak at Bulsar, 1898. 16,159, 16,160.

At the end of the month a second outbreak began in the town of Bulsar, which lasted till the following September. The infection was brought from Billimora in the Baroda State. The number of indigenous cases reported in this outbreak was 527. The attacks were at the rate of 4 per cent. of the census population.

III.—(*b.*) (18.) *Course of the Plague in the Bombay Presidency in May* 1898.

Bombay Presidency, May 1898.

59. From the 30th April to the 3rd June—five weeks—there were, outside the Cities of Bombay and Karachi, 1,872 cases, compared with 2,186 in four weeks of April.

Outbreak at Bhiwandi, 1898. 22,108.

The greatest number of cases (323) was returned from the Thana district. The town of Bhiwandi (population 14,400) in this district was re-infected in the middle of April. A severe epidemic began in it in June and lasted till the end of September. The population present in the town dwindled to 7,800. Nearly all the patients were

22,119.

treated in their own houses, a small minority of 153 consenting to go to hospital. All infected houses were disinfected when the sick persons resident in them had either died or

14,482.

recovered. The number of cases reported during this outbreak was 1,788. The reported attacks were at the rate of 12 per cent. on the normal population, and must have been at a far higher rate on the population actually present at different times during the epidemic.

The only other places in which there was plague to any appreciable extent were the districts of Surat (106 cases), Satara (141 cases), Belgaum (179 cases), Kolaba (40 cases), and the Baroda State (54 cases), Kolhapur (250 cases), the State of Porbandar in Kathiawar (47 cases), and the Cutch State (279 cases). In the Karachi district outside the City 318, for the most part imported, cases were reported.

The town of Cutch Mandvi had again become infected, and all the indigenous cases but six returned during the month from the Cutch State occurred there. It is not clear how the place became reinfected. There were about 12 indigenous cases in January, February, and March, and 26 infected persons who arrived from Bombay were detected and detained in the observation camps between the middle of January and the beginning of April. Between the 2nd and 29th of April there were 10 indigenous cases. In the next five weeks, up to the 3rd June, there were 279 cases and 195 deaths. Between 30,000 and 35,000 of the population were induced to go outside the walls of the town, leaving only 6,000 persons within them. There was a considerable decline in the number of cases in June, but the outbreak continued till November, when it became very mild. Up to the middle of January 1899, 922 cases and 748 deaths were reported. In February plague began to get worse. From the 4th to the 31st of March there were 326 cases and 236 deaths, and in the five weeks ending on 5th May 1,269 cases and 935 deaths. Between 5th and 26th May 378 cases were reported, and then the outbreak declined till it died out entirely during the latter half of June. Between the beginning of March 1898 and the end of June 1899 there were 2,678 cases and 2,011 deaths. The reported cases fall at the rate of 7 per cent. on the population.

Second and third outbreaks at Cutch Mandvi, 1898–9. 13,411. 13,424–5. 13,414.

III.—(*b.*) (19.) *Course of the Plague in the Bombay Presidency in June* 1898.

60. In June there was, generally speaking, an abatement of the disease during the earlier part of the month and an increase towards its close. Of 1,344 persons attacked during the month, from the 4th June to the 1st July, outside Bombay and Karachi Cities, 523 were attacked between the 5th and 17th, and 821 in the last fortnight of the month. The places chiefly affected were the Thana (232 cases) and Belgaum (264 cases) districts, Kolhapur (167 cases), the town of Hubli in Dharwar (102 cases), the Surat (112 cases) and Satara (98 cases) districts, the Cutch State (176 cases), and the Kathiawar State (87 cases); 32 cases occurred in the Karachi district.

Bombay Presidency, June 1898.

III.—(*b.*) (20.) *Course of the Plague in the Bombay Presidency in July* 1898.

61. The number of deaths from plague in the first week of July outside Bombay City was 457. The places most infected were the towns of Karachi and Cutch Mandvi, the Surat district to the north-west of Bombay, the Thana district (particularly the town of Bhiwandi) in the immediate neighbourhood of Bombay, the Satara district, the Kolhapur State, the Belgaum district, and the town of Hubli in the Dharwar district. As the month progressed plague became more virulent, particularly in the Kolhapur country, the Cantonment and district of Belgaum, and the town of Hubli.

Bombay Presidency, July 1898.

The population of Hubli was 52,000 when the census was taken in 1891, but it has probably increased by about 5,000. Plague first appeared in the town in indigenous form in October 1897. Before that, in spite of the measures adopted to prevent the entry of the disease, at least one imported case came to the knowledge of the authorities. This was the case of a woman who, on her return from Bombay in February 1897, developed plague at Hubli in the chawls near the premises which are occupied by railway employés. This case terminated in recovery, and the infection did not spread. No further case occurred till October 1897. On the 15th of that month a suspicious case came to light in the railway chawls. This case was diagnosed as plague on the 17th. The chawls were then cordoned with a view to keeping the town free from the disease, and measures were taken to suppress the outbreak in the chawls. These were ultimately successful, but in the meantime the infection had been carried into the town. There would appear to be no doubt that the cordon did not act as an effective bar to inter-communication. In particular, we are told that the relative of one of the police guards went to and slept in one of the chawls and then returned to the town carrying the infection with him. The first case in the town, which was detected about the 4th of November, was, as we were informed by Dr. Cardoz, that of a man who was employed in the chawls. A similar history of a connexion with the railway chawls was elicited in connexion with others of the earlier cases in the town. It is noteworthy that only occasional cases occurred in the early months of 1898, and that at the beginning of May 1898 the town was almost free from plague. In spite of the measures adopted to check the spread of the infection—which included the burning down of an infected street—plague, with the advent of the rains, broke out in a very severe form. As in the case of the outbreak in October, which was variously imputed to an importation from Sholapur on the one

Outbreak at Hubli. 1898. 1570 1582. 2396. 2399. 2436-9; 20,151. 1586-7. 2398. 20,141-2.

hand and from Poona on the other, nothing has been elicited to account for the outbreak of a severe epidemic in May. It was supposed by some observers that it was due to a re-importation of the infection—possibly to an importation in merchandise from Bombay. In favour of this view it was urged that the places which were attacked were nearest to the goods shed and that the cases occurred among people who worked on the railway premises. On the other hand, it was pointed out that, even if it was
20,170. necessary to assume a re-infection of the town (and plainly there is no necessity for making such an assumption), the re-infection of the town might quite well have been brought about by the arrival of infected persons from infected places in the neighbourhood.

The outbreak in Hubli which had become formidable in the middle of May, when heavy rain fell and interfered with the work of those employed in finding cases and with the other plague measures, gained strength till the end of August, when it began to decline. It ceased in the middle of November. The experiment of inoculating the population with Mr. Haffkine's vaccine on a large scale was made at Hubli, where 35,959 persons
25,151. had been twice inoculated and 5,157 once inoculated, by the 2nd December. The number of reported cases in Hubli was 3,388 with 2,946 deaths. The percentage of
1615. attacks on the normal population was just over 7. The attempt was made to keep a census of the average population present during the epidemic. Calculated on this, the
1598, 25,151. percentage of attacks was 8·6, and that of deaths 7·07.

Outbreak at Ankleshwar, 1898.
14,206–8.
14,214.

Plague broke out in an epidemic form in July at Ankleshwar (population 10,700) in the Broach district. Infection was imported from Chandod in Rewa Kantha in April 1898, but only eight cases were reported in Ankleshwar before July. The outbreak became severe at the end of that month, and was at its worst in August. The town was emptied by the 23rd of August. Plague continued till the beginning of November. Three hundred and thirty-one attacks and 245 deaths were reported. The attacks were at the rate of 3 per cent. on the census population, but it is to be noted that during the course of the epidemic many people left the town.

A few cases occurred in July in the Sholapur district, the Baroda State, and Kathiawar. None of these tracts was, however, seriously affected during the month. The Sachin State returned 147 cases; in the previous month there had been 54 cases, but the number fell to 17 in the succeeding month, and in all the State had 452 cases.

III.—(*b.*) (21.) *Course of the Plague in the Bombay Presidency in August* 1898.

Bombay Presidency, August 1898.

62. In August plague was very severe in the towns of Bhiwandi and Hubli. It remained severe in Surat and increased in Satara, and became virulent in the villages of the districts of Belgaum (2,565 cases from the 29th July to the 2nd September) and Dharwar (1,091 cases, excluding indigenous cases at Hubli). It reappeared in the Nasik district, particularly at Vinchur, and in the Akalkot State. It became worse in a few places in the Sholapur district, at Ankleshwar in the Broach district, in the Baroda, Cutch, and Kolhapur, States, and in the Bhor State near Poona.

Outbreak at Bantwa, 1898.
17,549 *sqq.*

An outbreak had begun at Bantwa, a small town of 8,200 in a native State in Kathiawar, at the end of July. The rains interfered with the measures taken. About 2,000 persons were removed from their houses in August. A storm took place which drove them all back to their houses. The population generally did not move out till between the 25th and 30th of September. Plague did not stop till the beginning of
17,552–5. November. The cases reported during the outbreak amounted to 944 and the deaths to 602. The incidence of attacks was a little over 11 per cent. of the population.

III.—(*b.*) (22.) *Course of the Plague in the Bombay Presidency in September* 1898.

Bombay Presidency, September 1898.

63. The outbreaks in the Belgaum and Dharwar districts grew worse in September. The number of cases reported from the former district from the 3rd to the 30th of the month was 3,605. In addition to the indigenous cases returned from Hubli and Dharwar towns, 5,913 cases were returned in September from the Dharwar district, the villages of which were badly infected by refugees from these two towns.

Second outbreak at Belgaum City, 1898–9.
23,127.
23,128.

Plague had begun to occur again in Belgaum City in July, and 120 cases were reported in August. The sick were treated at home during this epidemic. The people were not segregated, except for the time necessary to disinfect their houses. In September the number of cases rose to 830. In October there were 1,304 cases, and from the middle of the month the number of cases declined. Plague continued during November and December, but in January 1899 there were only 14 cases. From the

beginning of July to the end of January, there were 2,721 cases and 2,025 deaths. The attacks were at the rate of a little over 9 per cent. of the population. 23,127-8.

The town of Dharwar (population 26,600) began to be badly infected in the latter part of August, and 301 deaths among indigenous cases were reported in September. Here also, large numbers of the population were inoculated. The town was evacuated when the rains ceased. The epidemic ceased at the end of November, but a few cases continued for some time afterwards. Up to the beginning of December, 1,316 cases and 965 deaths had been reported. The percentage of attacks on the normal population was about four. Here, as in Hubli, special efforts were made to keep a census of the population which remained present in the town. The incidence of attacks on the average population during the epidemic is calculated at 6, and that of deaths at 4·4 per cent. Outbreak at Dharwar, 1898. 1,608. 25,151.

There was an increase in September in the number of cases reported from Kolhapur (1,854), and the Satara district (1,107), where Tasgaon was reinfected, though the outbreak that occurred was not serious, and, up to the middle of November when it ceased, was the cause of only 27 deaths. Six hundred and three cases were returned in September from the Nasik district, 611 from the Baroda State, and 270 from the Cutch State. The town of Umreth, in the Kaira district, was attacked; the infection apparently came from Savli in the Baroda State. The district of Poona became slightly infected, there were 237 cases in the Bhor State, and plague remained in a few places in the Sholapur district (89 cases). There was an improvement in the Surat (133 cases) and Thana (376 cases) districts. 155 cases occurred in the Broach district, chiefly at Ankleshwar. 14,014, 14,020.

III.—(*b.*) (23.) *Course of the Plague in the Bombay Presidency in October* 1898.

64. In October, plague continued very severe in the Southern Mahratta country. Six thousand four hundred and seventy-one deaths were returned from the Dharwar district and 4,862 from the Belgaum district in the four weeks ending 28th October. There was a great improvement at Hubli town, where the number of cases reported was 128, and of deaths 124. In Dharwar town (657 cases), however, and Belgaum town (1,304 cases), plague continued severe. The mortality was also high in the Kolhapur country (2,243), and the Satara district (1,826). Out of 17,815 deaths reported outside the City of Bombay during the month, 15,403 were reported from the three British districts of Satara, Belgaum, and Dharwar, and from the Native States in the Kolhapur country. Bombay Presidency, October 1898.

The returns from the Poona district became worse (354 cases). The small towns of Ilkal (population 11,200), in the Bijapur district, and Kalol (population 4,500), in the Panch Mahals district, were attacked. A case or two occurred in the town of Ahmednagar. There was great improvement in the Surat and Thana districts and in Kathiawar. In the Thana district the epidemics in Bhiwandi and Bassein ceased, 274 indigenous deaths having occurred in Bassein since its infection in December 1897. The cases in Karachi dwindled to 18, and the deaths to 12. There were 566 cases in the Nasik district, 501 in the Baroda State, 250 in the Bhor State, 215 in Sholapur, 159 in the Cutch State, and 122 in the Aundh State.

The outbreak ceased in Ankleshwar, but the infection had been carried from it to Broach (population 40,000), by some butchers who came to reside with their friends at the latter place. The first case was found on 14th September, 1898. The quarter in which the butchers resided was evacuated and disinfected. Cases, however, spread in other parts of the town, and the mortality was very high in December. The practice was to send plague patients to hospital and the contacts to segregation camp, and to remove the neighbours to health camps. Special arrangements were made by the millowners for camps for those employed in the mills. About 18,000 persons left the town and either camped near the town or went to villages in the neighbourhood. There were altogether 36 indigenous cases with 24 deaths up to the 2nd December, the number of cases in October and November being nine and five respectively. In December there were 36 cases, and the outbreak gathered force. The epidemic was severe in January and February, but diminished in March and ceased at the end of April. During February 213 indigenous cases were reported, and such cases exceeded 100 in January and March, while the total number of cases reported was 624 and the deaths 515. The rate of attack per cent. of the census population was 1·5. Outbreak at Broach, 1898-9. 15,484. 15,484, 15,502.

III.—(*b.*) (24.) *Course of the Plague in the Bombay Presidency in November* 1898.

65. The mortality outside Bombay City, between the middle of October and the middle of November, was higher than it had ever been before. In the four weeks ending on Bombay Presidency, November 1898.

11th November 24,219 cases and 18,770 deaths were reported outside Bombay. The rural areas in the Satara, Belgaum, and Dharwar, districts, and in the Kolhapur country continued to be most severely affected. Over 16,500 deaths were reported from these tracts in the four weeks referred to above. The condition of the towns in this part of the country improved. Plague practically ceased in Hubli, and between the 28th October and the 25th November the cases reported in the towns of Dharwar and Belgaum fell to 474 and 142 respectively. From 29th October to 2nd December 408 cases occurred in the Poona district outside the City, and in this period of five weeks there were 292 cases in the Baroda State, and 237 in the State of Aundh. A few cases occurred at Karachi and Ahmednagar. There was a certain amount of plague in the Poona district, the Baroda State, Nasik, Bijapur, and Sholapur, but it was generally less than it had been. The infection was reduced to very slight dimensions in Surat, Broach, Kathiawar, the Cutch State, and the Thana district, the town of Thana becoming free of the disease which had existed in it since January 1898 and had caused 177 indigenous deaths in this outbreak.

An improvement took place after the middle of November. From the 5th to the 18th of that month the deaths from plague reported outside Bombay City were 8,740; in the next fortnight they were 6,787.

III.—(*b.*) (25.) *Course of the Plague in the Bombay Presidency in December* 1898.

Bombay Presidency, December 1898.

66. The improvement continued in December. The chief seat of the infection was still the rural tracts of Satara and the Southern Mahratta country, but the number of deaths reported there in the four weeks ending on 30th December fell to 7,629, of which 4,125 were reported from the Dharwar district. The situation in the other three tracts improved very materially, and in the Belgaum district the deaths reported did not exceed half the number reported in the previous month. The outbreaks in Belgaum and Dharwar towns practically ended during the month. Outside the Southern Mahratta country, the districts of Khandesh and Kanara became slightly infected. Plague got worse in the Kaira district, the Panch Mahals, and the Bijapur district (609 cases). It improved in the Poona district, Nasik, and the Baroda State. There was a little in the Ahmednagar district (only one case in the City) and the Sholapur district. In Kathiawar and the Cutch State the infection practically ceased, but 343 cases were reported from the Aundh State. In Karachi City only 16 cases were reported.

Second Outbreak at Baroda City, 1898–9.

In the City of Baroda there had been a few cases in November. These increased in December, and the epidemic was severe in January, February, and March, when the number of cases reported were 220, 300, and 379, respectively. It declined in April, and had ceased by the end of May. There were 1,096 indigenous cases and 886 deaths reported up to the end of May. The attacks in this outbreak were at the rate of 1 per cent. of the population.

III.—(*b.*) (26.) *Course of the Plague in the Bombay Presidency in January* 1899.

Bombay Presidency, January 1899.

67. There was still further improvement outside the City of Bombay in January 1899, and early in this month the capital of the Presidency became the most seriously infected place within it. In the five weeks ending 3rd February the cases reported outside Bombay itself numbered 10,227 and the deaths 8,152. The deaths reported from the Satara, Belgaum, and Dharwar, districts, and the Kolhapur territory, fell to about half what they had been in the previous period of four weeks. There was a little more plague in the Kaira district, the Panch Mahals (where there was a sharp outbreak at Godhra, population 14,700, which had become first infected towards the end of November), and the Thana district in which there was an outbreak of some virulence at Kalyan. The outbreaks at Broach, Baroda, and Aundh continued. In Karachi City there were 27 attacks. In Nasik and the Poona district the outlook became more promising. Plague continued in the Bijapur, Sholapur, Kanara, and Khandesh, districts, but not to any serious extent.

III.—(*b.*) (27.) *Course of the Plague in the Bombay Presidency in February* 1899.

Bombay Presidency, February 1899.

68. There was, on the whole, some improvement outside Bombay City during February, especially in the Southern Mahratta country. The cases reported outside Bombay between 4th February and 3rd March numbered 7,864, and the deaths 6,068. There were a number of deaths from plague in several towns in the Thana district,

notably Bassein and Bandra. Plague got worse in Baroda City the number of cases reported from the State being 563; and at Godhra (302 cases); but at the latter place the outbreak was declining rapidly before the end of the month. Two hundred and fifty-nine cases were reported from Broach, and 106 from Nasik town. Igatpuri, in the Nasik district, was slightly infected. Plague continued without being serious in the Poona, Ahmednagar, Kolaba, Sholapur, Khandesh, Bijapur, and Kaira, districts. More cases began to be reported in the Surat district. In Karachi City the number of deaths rose to 70, the prelude of another outbreak already referred to.

Towards the close of the month Poona City, which had remained free for a long time, was again attacked. This outbreak at Poona has been extremely bad. In March the prospects looked very gloomy, but they improved in April and May. Plague grew a little worse in June and by 30th June 1,641 cases and 1,308 deaths had been reported since the week ending on the 27th January when the first case was reported. Third outbreak in Poona City, 1899.

III.—(*b.*) (28.) *Course of the Plague in the Bombay Presidency in March* 1899.

69. The outbreak at Bombay City was at its height in March, and between 4th March and 31st March 6,882 cases, and 5,574 deaths, were reported from other places. From the Southern Mahratta country 1,372 deaths were reported, a number which, if large, is still much smaller than had been reported for some months. The most serious features of the visitation during this month were the outbreaks in some large cities. From Karachi City 767 cases were reported, from Poona City 393 cases, from the Baroda State 793 cases, from the Nasik district 281 of which 161 were indigenous cases in the town of Nasik, and from the Cutch State 326 cases, the town of Cutch Mandvi, with 300 indigenous cases, being chiefly affected. There were 119 cases in the town of Broach, but the outbreak was rapidly declining by the end of the month. A few cases were reported from the towns of Ahmednagar and Ratnagiri. Bombay Presidency, March 1899.

There was more plague than in the previous month in the Thana (953 cases) district, but at the end of the month cases ceased to occur in Kalyan after 195 indigenous deaths had been reported in the outbreak which had begun in that town in the beginning of December 1898.

Plague lingered, without being serious, in the Poona, Surat, Kolaba, Ahmednagar, Sholapur, Khandesh, Bijapur, Kaira, Kanara, and Rewa Kantha, districts. The outbreak at Godhra in the Panch Mahals ceased. In the last week of the month there were 1,639 cases outside Bombay City.

III.—(*b.*) (29.) *Course of the Plague in the Bombay Presidency in April* 1899.

70. In April there was a substantial improvement, both in the City of Bombay and elsewhere. The number of cases reported outside Bombay from the 1st of April to the 5th of May was 6,903, and the deaths 5,309. In the weeks ending on 28th April and 5th May there were 895 and 828 deaths outside Bombay. The places which suffered most during the month were Karachi City (1,698 cases), Cutch Mandvi (1,071 cases), and Poona City (413 cases). Almost one half of the deaths reported outside Bombay City occurred in these three places. The Baroda State had 399 cases, and the Nasik district had 204; the outbreaks in Baroda City (131 cases) and Nasik town (145) began to decline materially before the close of the month, and that at Broach ceased. There were 94 cases at Ratnagiri town, but the outbreak was not serious. In the last week of the month 16 cases were reported from Belgaum town. Outside the places named there was more plague in the Thana district (776 cases) than elsewhere. Plague died out in the Panch Mahals. There were very few cases in the Poona and Surat districts, and but few in the Satara (258), Kolaba (304), Nasik, Dharwar, Belgaum, Khandesh, Bijapur, and Kaira, districts, and in the Kolhapur country. Bombay Presidency, April 1899.

Thirty-nine cases were reported from the town of Hyderabad in Sind, in which plague commenced again towards the end of March.

III.—(*b.*) (30.) *Course of the Plague in the Bombay Presidency in May* 1899.

71. In the four weeks between 6th May and 26th May 2,297 cases and 1,797 deaths were reported outside Bombay City. Karachi City (369 cases), Cutch Mandvi (254), and Poona City (224), continued to be the places most infected. During the latter half of the month the epidemic at the two former places declined rapidly. Plague had not died out at Nasik (35 cases), or Baroda City (18 cases), but the mortality in both Bombay Presidency, May 1899.

these places was comparatively small. There were 40 cases at Ratnagiri, 40 cases in Belgaum City, and 25 cases at Hyderabad in Sind. The Khandesh district became free. The rural areas with most infection were in the Belgaum (193 cases), Satara (140), and Kolaba (130), districts, and the Kolhapur country (136).

Two hundred and twenty-four cases were reported from the Thana district where the outbreaks in Bandra and Bassein ceased about the end of the month. The outbreak in Bandra had commenced in the beginning of November 1898, and 333 indigenous deaths were reported in the town during its continuance. In Bassein the outbreak commenced in the beginning of December 1898, and caused 263 indigenous deaths.

In the week ending 26th May there were only 455 deaths outside Bombay City.

III.—(*b.*) (31.) *Course of the Plague in the Bombay Presidency in June* 1899.

Bombay Presidency, June 1899.

72. The returns from the week ending 2nd June to 30th June show 2,945 cases and 2,150 deaths outside Bombay City. In the last of the five weeks included in this period the figures are higher than in any of the previous weeks. The places most seriously affected during June were Poona City (482 cases), and Belgaum City (193 cases). At both these places the neighbouring Cantonment was also seriously affected, and the Belgaum district altogether had 714 cases The outbreak at Nasik (107 cases) showed signs of reviving. The outbreaks at Baroda City and Cutch Mandvi had completely stopped, while at Karachi (72 cases) and Ratnagiri (33 cases) they had been reduced to very small dimensions. No more cases had occurred at Hyderabad in Sind. At the end of June there was no plague in the Baroda State, or the Sholapur and Kolaba districts, and practically none in the Ratnagiri, Ahmednagar, and Kaira, districts. Although it had not reached any serious proportions, it had at the close of the month begun to rise in the Poona and Satara (203 cases) districts and in the Kolhapur country (566 cases). In the Thana district 173 cases occurred; in the beginning of the month cases ceased in the town of Kurla, where they had been reported continuously since the beginning of December 1898, 203 indigenous deaths taking place in the town during this outbreak; by the end of the month 53 indigenous deaths had occurred in Bhiwandi, which was reinfected in the beginning of March, and the outbreak continued in the town after June; the town of Thana was reinfected during the month. There was a little plague in the Surat, Dharwar, and Bijapur, districts, and the aggregate number of deaths in these three districts in the week ending 30th June was only 71.

III.—(*c.*) *History of Plague, to the end of June* 1899, *in India outside the Bombay Presidency.*

Outbreaks in places other than the Bombay Presidency, Sind, and the adjoining Native States.

73. Of the outbreaks which have occurred outside the Bombay Presidency and Sind and the adjoining Native States, the most serious, though not the first in point of time, is that which began in the Mysore State during the rainy season of 1898. In Southern India there have been epidemics also in parts of the Hyderabad State in 1898 and 1899, and in certain districts of the Madras Presidency in the winter of 1898–99.

The Central Provinces, adjoining the north-eastern boundary of the Bombay Presidency, were not infected till nearly the end of 1898, and the infection was not then widespread, and did not last long. Of places more distant from Bombay, Calcutta, where cases had possibly occurred in 1896, became definitely infected in 1898, and had a second epidemic in 1899. During the latter epidemic the infection was carried to a number of other places in Bengal. The villages of two districts in the Punjab were infected in October 1897. Plague has been present there ever since. In the North-Western Provinces and Oudh, the only seat of infection has been Hardwar and a few small towns and villages in its immediate neighbourhood. Plague began there in April 1897 and ceased entirely in the beginning of 1898. A few villages were attacked in the Sirohi State in Rajputana in November 1897; the infection died out in April 1898, and has not reappeared. One village in the Gwalior State was infected in the beginning of 1897; the outbreak ceased in March, and there has been no recurrence of plague. This enumeration exhausts the list of outbreaks outside the Bombay Presidency and Sind and the Native States within or around them.

III.—(*c.*) (1.) *History of the Plague, to the end of June* 1899, *in Bangalore and the Mysore State.*

Outbreak in the Mysore State. 2534–6.

74 The Civil and Military Station of Bangalore and the City of Bangalore are practically one place, since the division between them is a purely artificial and

administrative one. The population of the Civil and Military Station is about 100,000 (of whom 13,000 are troops and followers), and that of the City about 80,000 to 85,000.

Introduction of the infection into Bangalore City and Civil and Military Station. 2538-44.

During the time that Hubli, which is closely connected by railway with Bangalore, was severely affected by plague the danger of the spread of the infection along the railway from Hubli to Bangalore was apprehended, and precautionary measures were adopted, both in the Civil and Military Station and in the City, to guard against the importation of the disease. In spite of these measures infection was imported from Hubli. In particular, on the 12th of August 1898, a case of plague was detected in the Civil and Military Station in a servant who had just returned to Bangalore from a visit to Hubli which had lasted four days. Owing to the promptness with which this case was detected, it would appear that it did not give origin to the subsequent outbreak in the Civil and Military Station. Two days later, on the 14th of August, 3230.
it was ascertained that the City was infected and that the disease had probably existed there for some time before. The exact manner in which the infection was introduced has never been elicited, but the details regarding the discovery of the first cases are as follows. Inquiries instituted on the 12th of August in consequence of 3448.
the occurrence of two burials from one house in the Gowdenpet quarter of the City—*i.e.*, the quarter which adjoins the railway goods yard—brought to light the facts that one of the persons buried had a bubo and that cases of fever had occurred in the house. Some of the inmates had already run away and died elsewhere. The discovery of other cases of plague in the neighbourhood followed, the employés of the railway goods sheds and workshops being the persons who were at first principally affected.

In view of the fact that the transhipment coolies and the railway workshop hands were 3222-3.
the first to suffer severely, it is suggested that the infection may have been introduced by the agency of rats, or in green hides or other goods, or possibly by infected railway waggons, from Hubli. No attempt was made to substantiate any of these suggestions except in the case of the raw hides. Some of these imported from Hubli are said to have been stolen by one Mustan, with the result that plague subsequently appeared in his house. Inquiries, however, elicited the fact that plague had already appeared in the house opposite Mustan's before any member of the latter's family was attacked. There is thus no reason to assume that the plague in Mustan's house was imputable to the stolen hides. Another focus of infection, which was by the local authorities assumed to have originated from a separate importation of the disease, was later discovered in the Akkipet quarter in the heart of the City. The infection is said to have been introduced here at the same time as, or a little later than, it was introduced
into the Gowdenpet quarter, but the first case did not come to notice before the 22nd 3474.
of August, when a railway guard was found to be suffering from plague. It was said that this man had already been ill eight days. It appears that the patient had not for a considerable period before his attack been to Hubli or to any other infected place, and it is therefore suggested that he was infected either by coolies who carried his baggage for him to and from the railway station, or by people from infected places who might have been smuggled through to his house, without undergoing the prescribed inspection on arrival at the railway station. There is no direct
evidence in support of either of these suggestions. It is even, as Mr. Madhava Rao 3223.
told us, uncertain "whether he was first attacked or his relatives or friends." The assumption that the guard contracted the disease from an importation of infection separate from that which gave rise to the plague in Gowdenpet, seems to us, therefore, to be a perfectly gratuitous one.

It was not till 15th September that cases of plague imported from the City were 2548.
discovered in the Civil and Military Station. Indigenous cases in the Civil and Military Station began on 30th September.

City and Civil and Military Station of Bangalore: August 1898 to March 1899.

75. The number of cases reported in the City in the month of August was 22, and the deaths 15. In the next four weeks there were 205 cases and 129 deaths. At the beginning of October matters got much worse, and the returns for the four weeks ending 27th October showed 1,211 cases and 939 deaths. The outbreak at this time became bad in the Civil and Military Station, from which in these four weeks 224 cases and 160 deaths were reported. In the four weeks ending 24th November 1,361 cases and 1,129 deaths were reported from the City, and 1,261 cases with 1,036 deaths from the Civil and Military Station. During the second half of November the number of cases had begun to decline in the City. There was a further substantial improvement in the four weeks ending 23rd December. In that period there were 447 cases and 350 deaths in the City. The outbreak in the Civil and Military Station

was, however, getting worse at this time. The number of cases reported there was 2,077, and the deaths 1,720. After the middle of the month an improvement took place. In the following four weeks ending 20th January the number of cases reported in the City fell to 58, and the deaths to 71, while in the Civil and Military Station they were 358 and 311 respectively. The decline in the number of attacks at both places then became very rapid. Between 20th January and 3rd March 27 cases and 19 deaths were reported in the City, and 85 cases and 76 deaths in the Civil and Military Station. Between 3rd March and 31st March there were only four cases in the City, and then plague ceased there entirely. In the Civil and Military Station it lingered a little longer. Nineteen cases and 12 deaths were reported in March, and five cases, of which two were imported, and five deaths in April. There was one indigenous case in the last week of May, and one indigenous case in the last week of June, there being also two imported cases in May and two in June.

Comparison between outbreaks in the City and Civil and Military Station, Bangalore.

76. The two outbreaks thus ran practically the same course. Beginning in the Civil and Military Station a month after it had begun in the City, plague ceased in the latter place about a month later than in the former. The reported mortality from plague in Bangalore City from the beginning of the epidemic (12th August) up to 31st March was 2,655, and in the Civil and Military Station, from the end of September to the same date, 3,315. The total mortality during the same period was 7,237 in the City, and 6,272 in the Civil and Military Station. At the normal rate the deaths should have been in the City 867, and in the Civil and Military Station 1,041. The excess of mortality during the epidemic was thus 6,370 in the City, and 5,231 in the Civil and Military Station. Allowance has to be made, on the one hand, for the normal mortality having been pitched too low owing to defective registration in the past, and, on the other, for the fact that the mortality during the epidemic occurred among a population very reduced in numbers. It is estimated that at one time only 45,000 persons remained in the City, and 59,000 in the Civil and Military Station. The real plague mortality was probably not less than the difference between the actual and the normal mortality. Assuming that this was the actual plague mortality, its incidence on the normal population would be 7·5 in the City, and 5·4 in the Civil and Military Station.

3504–5
2639.

Measures adopted in the City and Civil and Military Station of Bangalore.
2536, 2553, 3273–6.
2572.
3279.

77. The measures adopted in the Civil and Military Station and in the City for dealing with the epidemic were, practically, the same. Both places were divided into circles, and the circles into blocks, and at first a voluntary agency was used to detect cases. In both places this agency broke down completely, and an official agency had to be substituted for it. House to house searching, with the help of soldiers, was adopted in the Civil and Military Station, but not in the City. The segregation of the sick and contacts was undertaken. At first the only exception to this rule was that, when a whole household had been inoculated, the patient and the family were allowed to remain on condition that the house was disinfected on the death or recovery of the patient. Segregation was much resented, and it was a complete failure in both the City and the Civil and Military Station. Many cases escaped detection, and those which were discovered were not, as a rule, found till after death, when the other inmates of the house had usually disappeared. In a certain number of cases dead bodies were found under circumstances which rendered it impossible to discover anything about them. Concealment of cases prevented the arrangements for disinfection from being at all complete. Bodies were removed from the houses in which death occurred, and many houses in which deaths took place were never known to have been infected. At first only houses in which cases were known to have occurred were disinfected, but at a later period of the epidemic the systematic disinfection of all houses in infected quarters was undertaken. The fear of the measures caused numbers of people, as has already been noticed, to desert both the City and the Civil and Military Station.

2684, 3348.

Mysore State, November 1898 to January 1899. Infection carried to parts of Mysore near Bangalore.
3302.
3304–5.

78. As the result of the exodus of the people from Bangalore, the neighbouring country became badly infected. The Mysore State is divided into eight districts. The infection spread into four of these districts, viz., the Bangalore, Mysore, Tumkur, and Kolar districts. Up to 7th December, excluding Mysore City, 51 villages had been found to be infected, and the number of cases and deaths returned from them had been 814 and 664 respectively. No difficulty was experienced in inducing the village population to vacate their houses on the occurrence of plague. In the week ending 17th December 532 cases and 446 deaths were returned from the villages, and in the following week 650 cases and 626 deaths, mainly in the Bangalore district.

79. Plague broke out in the City of Mysore in November 1898. Eight cases had been imported from Bangalore in October, but the first indigenous case was not reported till 26th November. The population of the town was, in 1891, 74,000, and is now estimated at 80,000. Twenty-one thousand of the inhabitants had been inoculated when the outbreak began, and this number increased to 28,750 by the end of January. Outbreak in Mysore City, November 1898. 3302. 3481. 25,034.

Seventeen indigenous cases occurred in Mysore City up to 10th December. Indigenous cases began to increase towards the end of December, 33 cases and 20 deaths being reported in the last fortnight of the month. In the first four weeks of January, 637 cases and 474 deaths were reported from Mysore City. A different system to that followed at Bangalore was adopted here. There was no compulsory removal to hospital or segregation of contacts. Patients were encouraged to go to hospital, but few went. They were permitted to be treated in their own houses, on condition that an infected house had to be disinfected on the death or recovery of the patient. In practice, houses were generally disinfected on the discovery of a case, except the room in which the patient was living. The result of this change of policy is stated to have been that the people were not disturbed, and plague spread more slowly: the neighbouring country was not so widely affected: cases were more strictly reported: business was not interfered with: the cost of the operations was much less. Mysore City and State, December 1898 and January 1899. 24,956.

At the end of January there were 75 villages infected in the State, 47 in the Bangalore district, 10 in Kolar, where the gold-fields are situated, 13 in Tumkur, and only 5 in the Mysore district.

80. In the four weeks ending 24th February there were 1,125 cases and 904 deaths in Mysore City. Seringapatam, in the neighbourhood, had a few cases. The number of villages infected at the end of the month was 79, of which 10 only were in the Mysore district. Twenty-one were infected in the Kolar district, and a considerable number of cases occurred in the goldfields. Mysore State, February and March 1899.

81. The condition of Mysore City began to improve in the latter half of February, and there was a distinct improvement in March. In that month the cases reported in the City numbered 586, and the deaths 530, and in the last week there were only 69 cases and 59 deaths. A census was taken on 25th March, and an estimate was made from the return then obtained that 13,000 had left the City at that time. Mysore State, March 1899.

The number of villages affected at the end of the month had fallen to 36, 10 of which were in the Bangalore, and 18 in the Kolar, district. There were 280 cases from the 4th to the 31st of March in the goldfields.

82. During March, revised rules for the evacuation of villages, and the disinfection of houses in villages and towns other than Bangalore and Mysore, were issued. By these, disinfection by chemicals was abandoned as being of uncertain value, disliked by the people, and costly, and because the use of chemicals, unless they were mixed and used with care, was not unattended with risk. In place of chemical disinfection letting in of light and air, burning of floors, whitewashing, and general cleaning up of the houses and backyards, were prescribed. Disinfection by chemicals abandoned in Mysore State.

83. In the first four weeks in April 149 cases and 131 deaths were reported in Mysore City. In the same time there were 216 cases in the goldfields. The number of villages attacked was, at the end of the month, 20, 18 of which were in the Kolar district and one each in the Bangalore and Mysore districts. Between 29th April and 2nd June there were 77 cases in Mysore City and 66 deaths. The cases at the gold fields in this period were 187. Between 3rd June and 30th June the cases in Mysore City were 52 and the deaths 38, and at the gold fields 51 and 34 respectively. Thus, though plague had not died out at either centre, the epidemic had become comparatively mild. Twenty villages in the State were infected, nine in the Kolar district, five in the Bangalore district, and six in the Mysore district. Mysore State, April, May, and June 1899.

84. Excluding the three big towns infected, viz., Bangalore City, the Civil and Military Station, and Mysore City, the number of cases reported in the Mysore State up to 30th June was 8,816, and the number of deaths 7,437. Statistics up to 30th June 1899, for Mysore State, excluding Bangalore City and Civil and Military Station.

Though the outbreak in Mysore City had become comparatively mild, it was not over. It had lasted for five-and-a-half months, during which time the reported cases of plague numbered 2,667 and the deaths 2,171. The difference between the actual and the normal mortality from the beginning of January to the end of June was 2,866 (3680–814). The recorded plague mortality in this period was 2,143. The greater part of the difference between the recorded and actual plague mortality occurred

in the first six weeks of the epidemic. Calculated on the difference between the actual and the normal mortality, the incidence of plague mortality was, up to the end of June, 3·5 per cent. on the population of the City of Mysore.

III.—(c.) (2.) *History of the Plague, to the end of June* 1899, *in the Hyderabad State.*

Outbreak in the Hyderabad State, December 1897. Progress of Plague to December 1898. 5153.

85. The first indigenous cases of plague in the territories of His Highness the Nizam were reported in December 1897. Up to the middle of December 1898, 67 villages had been attacked, the number of attacks reported up to that time having been 3,971, with 3,440 deaths. The outbreak commenced in the Naldurg district, on the roads leading from Sholapur to Bidar and Hyderabad. Plague had been raging in Sholapur City at the end of 1897, and many refugees from it crossed over into Hyderabad territory. The original infection from Sholapur at the end of 1897 was shortly followed by the introduction of the disease into another tract from the Ahmednagar district. It would appear that in the first three months of 1898 and up to the middle of April the infection was worst in the Naldurg and Ganjoti divisions of the Naldurg district, in the latter of which the town of Ganjoti had 243 cases and Umarghi had 194; the town of Palaskheda also returned 89 cases in this period, and parts of the Gulburga district were also among the places seriously affected. During April and May it was thought that the plague had died down in the Naldurg area, but the disease continued its hold in His Highness the Nizam's dominions. Later on the Lingsagur district, particularly the Kopbal subdivision, was infected from Dharwar and Bijapur, and in September numerous cases were reported in the Kopbal and Yelburga subdivisions of the district. In the former 539 cases occurred in Kowloor and 92 at Gudlanur up to the middle of December, and in the former, up to the same time, there were 428 cases at Bannikop and 219 at Sidnikop. In October the village of Sassora, in the Tuljapur subdivision of Naldurg, was infected and returned 135 cases up to the 12th December. In November the most serious spread of infection appears to have been in the Lingsagur district: up to the 12th December Sirguppi and Dotihal in the Kushtagi subdivision of the district had 145 and 109 cases respectively; Seyjulgooda and Nagarhal, in the Mudgal subdivision, had 82 and 65 cases up to that date; and in the Yelburga subdivision, Binhal, up to the 8th December, had 104 cases, and Erihanchinal had 71: by the end of the month the number of cases reported in these last two villages was 122 and 189 respectively.

5422.

Hyderabad State. Progress of plague, January to June 1899.

86. For the month of January 1899 and succeeding months we have received regular statements of plague in Hyderabad, from which we have prepared the following note of the progress of the disease in the State. In January 487 cases were reported from 43 places, of which 18 were newly infected. 175 cases occurred in the Naldurg district, and all the rest, except a few in Yelburga, in the Kopbal and other subdivisions of Lingsagur. Kopbal town, with the neighbouring towns of Katurgi and Kuknur, with a total population of 18,000, became infected during the month. The infection occurred among the Marwari traders of the place. The people resorted voluntarily to evacuation, which was commenced on the 25th January and completed by the last day of the month. In Kuknur only five cases were reported, and these all occurred in January, but in the two other towns the outbreak continued till April. In January, Kopbal had six cases, and Katurgi 47; in February, the numbers reported were 21 and 10 cases respectively; Kopbal had 22 cases in March, with five in April; while the reports show in Katurgi the occurrence of seven cases in March and one in April.

In February 628 cases occurred in 52 places in the State, of which 28 had not previously returned any cases. The outbreak at Mashal (143 cases all fatal) in the Gulburga district was the worst, but except for this the state of affairs in Gulburga improved, and Naldurg and Lingsagur (particularly the subdivisions of Kushtagi (169 cases), Gulburga, and Kopbal) remained the most affected districts.

In March the outbreak in Mashal continued severe (402 cases), and for this reason the Gulburga district showed the most infection. Though the total number of cases in the State rose to 675, there was a general improvement, except in Mashal, the largest number of cases which occurred in one place, next to Mashal, being 24 at Kinhal in Kopbal. Plague was reported from 46 places in all, and of these, 10 were newly infected during the month.

In April the cases reported for the whole State fell to 99. A village, Bhoria, newly infected in the Naldurg district, had 15 cases, and two villages in the Kopbal subdivision each had 14 cases, but in all other places the number of reported cases

was less. The Kopbal subdivision was worst infected, with five out of the 20 infected villages of the State; 8 villages in all were newly infected during the month.

In May, cases occurred in villages in the Lingsagur and Naldurg districts only. The largest number, 26, occurred in Tallihal, a place newly infected in Lingsagur. 33 cases were reported in all from the five villages in which alone cases occurred throughout the State during the month.

In June the Lingsagur district was free, except for 17 cases in two villages, of which 15 occurred in Tallihal. Two villages were also affected in Naldurg, of which one, Murli, returned 33 cases, having had none previously. The total number of cases for the month was 57, and the number reported from the beginning of January to the end of June amounted to 1,979.

Measures adopted in the Hyderabad State. 5155. 5455 5465. 5466.

87. The measures adopted in the rural areas of the Hyderabad State were the evacuation of the infected villages, the disinfection of useless clothing and bedding by burning, and of bedding and clothing generally by boiling, and the disinfection of the infected houses by burning the surfaces of the floors and walls. Here, as elsewhere, the villagers after a time came to evacuate their villages at once on the occurrence of plague. Sepoys were posted in every village to prevent people from going from the infected to the uninfected area, and a census was maintained with the object of checking departures. The clothing and bedding of people leaving their houses was boiled twice, firstly, when they moved into camp, and, secondly, when they returned to their villages.

III.—(c.) (3.) *History of the Plague, to the end of June* 1899, *in the Madras Presidency.*

Outbreak in the Madras Presidency. 3882–93. 4104–19. 4397.

88. Plague occurred in August 1898 at Guntakal Junction in the Anantapur district of the Madras Presidency. It is not clear whether the local infection came from Bangalore or Hubli. The infected place, which is only a small railway centre, was cleared out and only six cases occurred. But a man took plague from Guntakal Junction to Timmencherla, a village of 2,000 people a mile off, and a woman afterwards took it from Timmencherla to Guntakal village (population 5,000). The policy followed in these villages was to make the people evacuate the village site and camp in the fields. The disinfection of the village was carried out often by detachments of native troops while the villagers were in camp. This system was followed in the agricultural areas which became infected in the Madras Presidency. The infection entered the Bellary district from Guntakal. Cases also began to occur in the town of Hosur in the Salem district, 25 miles by road from Bangalore, at the beginning of December, but indigenous plague did not occur there till about the end of January 1899.

Madras Presidency, December 1898 and January 1899.

89. In the first fortnight of December 1898, 128 seizures and 98 deaths were reported from the Anantapur, Bellary, and Salem, districts, most of the cases being indigenous. Indigenous cases were also reported from the Kurnool district in a village within 10 miles of infected villages in Guntakal and Bellary from which infection was probably imported. 149 cases in all were reported in the first fortnight of December, and 190 in the second fortnight, during which no fresh districts were indigenously affected. The number of cases in the Presidency in January 1899 was 230 in the first two weeks and 210 in the last two weeks, with 363 deaths in all. The North Arcot district for the first time had indigenous cases during the month. People with plague had come to the district from Bangalore as early as September, and eight imported cases occurred in December, but there was no local outbreak till plague appeared in an indigenous form in the town of Kuppam and the larger town of Vellore (population 45,000), following, in both cases, importation from Mysore.

Madras Presidency. Measures adopted in towns.

The infected quarter at Hospet (population 12,900) in the Bellary district, in which indigenous cases occurred, was evacuated, but plague did not stop, and it became necessary to eventually evacuate the whole of the town. The infection appeared at first in only one part of Kuppam, but when this quarter was recovering the other side of the town became suddenly infected, rats being first observed to suffer. Part of the town was evacuated at once, but, notwithstanding this, the outbreak spread. Contacts were segregated with difficulty. Only one ward of Vellore was infected at first. Nearly all the surrounding country is occupied by rocks, tanks, and wet cultivation. Evacuation on a large scale was in these circumstances impossible, and the people strongly objected to the transfer of contacts to the segregation camp. In most instances, in the early stage of the outbreak, home segregation was allowed. The result of this concession was, as appears from a statement made in the periodical reports relating to plague in the Madras Presidency, that the people were found more ready to help the authorities by giving information of sickness and accepting disinfection. Towards

the end of January plague appeared in the town of Arcot (population 11,000). Cases occurred in all parts and the whole town seemed to be infected. Here also general evacuation was found to be impossible and home segregation was permitted. The people refused to go to the Government camp. People were not found so ready to help the authorities as they had been at Vellore. Cases began to occur in a different quarter in the latter town at the beginning of February. Wallajahnagar (population 10,500) immediately opposite Arcot, on the northern bank of the Palan, also became infected. The more severely infected part of the town was to a large extent evacuated. In this town home segregation was permitted. At the end of February Ranipettai, a town close to Wallajahnagar and Arcot, was infected. Here also the people were opposed to evacuation and refused to go to camps. So that in the four towns infected in the North Arcot district, viz., Arcot, Vellore, Wallajahnagar, and Ranipettai, the sick and contacts were segregated under guards in their own houses. While this was the course adopted in the above-mentioned towns, the general evacuation of the towns of Kuppam and Hosur was being carried out.

Madras residency, February to June 1899.

90. Eight hundred and one cases were reported from the 29th January to the 25th February. Cases were thus more numerous in February than in January, but an improvement took place in the latter half of the month in the agricultural tracts. The condition of the towns was still bad. From this time, however, the general outlook began to improve. In the fortnight ending on the 11th March there were 260 cases and 228 deaths, and in the next fortnight 169 cases and 123 deaths. 96 of the cases and 69 of the deaths in the latter fortnight were in the towns of Vellore, Kuppam, Arcot, Wallajahnagar, and Ranipettai, while 24 cases and 21 deaths occurred in Hosur. In April there was a further improvement. The number of cases reported in the four weeks ending 22nd April was 186, with 139 deaths. The decline continued in May. In the fortnight ending 6th May, Arcot, Hospet, and Kuppam, returned no cases of plague; there was only one each in Hosur and Wallajahnagar, eight in Vellore, and ten in Ranipettai. A change had come over the attitude of the people at Vellore, where the objection to segregation had diminished, and the majority of contacts were taken to the camp. By the beginning of June, plague had practically died out in all the towns except Ranipettai. In the two fortnights ending on the 3rd and 17th of June the number of cases reported in the Presidency was 30 and 15 respectively, while in the last fortnight of June only 10 cases and 5 deaths were reported from four places in the Salem district and from Ranipettai.

Total cases and deaths in Madras Presidency up to the end of June 1898.

91. The number of cases and deaths reported during the outbreaks up to the 1st July 1899 in the Madras Presidency, was 2,705 and 2,064 respectively. In the towns the number were as follows :—

District	Town.	Cases.	Deaths.
Bellary	Hospet	83	71
North Arcot	Arcot	173	132
	Kuppam	108	78
	Ranipettai	93	36
	Vellore	214	181
Salem	Hosur	115	88

The largest number of cases reported up to the date mentioned from other places was 136 from Guntakal, and 129 from Molagavalli.

The town of Madras has not been infected so far, though a number of imported cases have occurred in it.

III.—(*c.*) (4.) *History of the Plague, to the end of June* 1899, *in the Central Provinces.*

The Central Provinces, outbreak at Hinganghat.

92. Plague did not become indigenous anywhere in the Central Provinces until the autumn of 1898, when it broke out in the town of Hinganghat, situated in the Wardha district. The population of the town is about 11,000, many of the inhabitants being mill hands, and there is a certain amount of communication between the place and Bombay.

Little information is available regarding the manner in which plague was imported into Hinganghat. In spite of the precautionary measures taken by the authorities to prevent its importation and to secure, in the event of their preventive measures failing, the early notification of the disease, the plague was imported and it

remained undetected till the 19th of November 1898, by which date it had probably been in the place for some time, though the death rate was low and gave no indication of its presence. It appears that the Hospital Assistant in charge of the local dispensary had as early as the 11th of November treated a patient suffering from fever and glandular swellings, and he said he had observed cases of the same nature 15 days before that date. Dead rats had been observed as early as the 7th of November. It was subsequently discovered that in all 19 cases had occurred up to the 20th of November, most of them among the mill hands and their relatives. Under these circumstances it proved impossible to fix the first case or to determine the date of the introduction of the disease. It was, however, suggested that two mill hands, who arrived on the 4th of November from Bombay and who themselves remained healthy, introduced the infection. This suggestion was not substantiated, and there were many other arrivals from infected places. 5977; 6200. 6395-9. 6240-7; 6293.

The different quarters of the town were evacuated one after another, until the whole town was practically evacuated by the 20th of December. A large number of people were located in private camps. The outbreak had two separate periods of virulence. Up to the 25th of November 38 seizures were registered, six of them having been discovered on the 25th by a house search. Only one seizure occurred between the 25th of November and the 2nd of December, and the disease seemed to be dying out. The people were very anxious to go back to their evacuated houses to get supplies and to look after their goods and their grain. Disinfection had not been effectually completed, but under the circumstances the authorities thought it safe to allow the people back into the town, and on the 28th of November some of them were permitted to visit their houses. The visits were made on the 29th November, and were promptly followed by a revival of the epidemic. The fresh cases were discovered on 2nd December, by house searching. This discovery led to the abandonment of the system of house inquiry on which reliance had at first been placed, and thereafter house-searching was insisted on. The outbreak subsided from the middle of January. Only two seizures and two deaths were recorded in the week ending on the 3rd of February, and the same numbers from the 3rd to the 8th of February when the last case occurred. The town was declared free of plague in March. In all there were 148 attacks and 124 deaths. The incidence of attacks was 1·3, and that of deaths 1·1 per cent. of the population. 6001. 6007, 6024, 6069

93. Hinganghat is distant 21 miles by rail from Wardha, the headquarters of the district, a town of about 8,500 inhabitants. A case appears to have come into Wardha from Hinganghat about the 23rd of November 1898, and to have escaped detection till the patient was brought to the Plague Hospital on 28th November. The death rate in Wardha, as in Hinganghat, had been for some time lower than usual. Two indigenous cases were detected on 5th December in different parts of the town, and it was soon discovered that plague was not confined to any particular part of it. By the end of December the whole town was practically evacuated. Plague continued in the town for about 14 weeks, the last case—that of a police-constable who had recently come to Wardha from Nagpur—having occurred in the week ending on the 3rd of March. The outbreak was never virulent, and there were only 76 reported cases, with 54 deaths. Of the 76 attacks, 38 were reported before 6th January. The incidence of attacks was ·8 and that of deaths ·6 per cent. of the population. The Central Provinces. Outbreak at Wardha. 6212-4. 6218.

94. Nagpur, the headquarters of the Central Provinces, a town with a population, excluding the Cantonment, of about 93,000, is 48 miles from Wardha, being, like it, on the Great Indian Peninsula Railway. There was no plague in Nagpur before the end of December 1898, and the actual mortality of the town during November and December 1898 and January 1899, viz., 927, was considerably below the normal (1,015). It seems likely, however, that the population within the town had decreased owing to departures. Two imported cases of plague, both from Wardha, were reported in the week ending on the 30th of December, 1898. The first that occurred was that of a middle class Hindu who was in the habit of going to Wardha. After returning from that place on the 21st of December, he was found on the 24th to be ill, and died in the hospital on the same day. The interval between the death of the only arrivals at Nagpur who were known to have suffered from plague and the first indigenous case was long. After 24th December there were no more imported cases, and no indigenous case was found till 16th February. Five cases were seen on the 17th, and enquiry then showed that from the 6th to the 16th February there had been 12 deaths, which had in all probability resulted from plague. Six days The Central Provinces. Outbreak at Nagpur.

after the outbreak was discovered dead rats were found in its neighbourhood. For a time after plague had been discovered there were very few cases. Ten were found between 16th February and 23rd February, and then none till 11th March. On the 11th and 12th March there were two cases, one each day, and then no more till 20th March, and cases then continued to occur up to the 12th of May generally, but not always, in the vicinity of the part of the town which was first infected. The disease, however, never assumed the character of an epidemic, and in nearly all cases infection was traced to the locality originally infected in the town. The total number of detached cases was 36, of which 29 were fatal. With the exception of one person, who was treated in his own house, all the sick who did not appear to be moribund were taken to hospital, and the contacts to segregation camp. The neighbours were all sent to the observation camp for 10 days.

Plague in villages of the Central Provinces.

There was very little spread of infection to the villages in the neighbourhood from the outbreaks in the towns of the Central Provinces. Only two places (including Nagpur) outside the Wardha district were infected from Hinganghat or Wardha, and only four places were infected from Hinganghat, and eight from Wardha. In the villages, except at Takli, where there were 19 cases reported, all fatal, the disease was quickly controlled and suppressed.

III.—(c.) (5.) *History of the Plague, to the end of June* 1899, *in Bengal.*

The existence of plague in Calcutta suspected in 1896.

95. In the opinion of Dr. Simpson, formerly Medical Officer of Health for the Municipality of Calcutta, the existence of plague in Calcutta must be referred back to 1896, when a certain number of cases of bubonic disease were discovered in the city. The grounds upon which the diagnosis of plague was based were in some cases of the slenderest description, consisting as they did on the one hand in a clinical history of indolent bubo combined with a certain amount of transient constitutional disturbance, and on the other hand in the discovery, in the preparations made from the blood of patients, of a few isolated diplococcal forms, regarded as plague bacilli. In certain other cases, notably in the cases of Giga, Gulab, and Bepin, more serious grounds for suspicion existed. Two of these were cases of bubonic disease which ended fatally (one of them apparently in less than 24 hours), and the third, also a bubonic case, was associated with the occurrence of very suspicious illness among rats.

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Para. 156 of this Report.

7101–16.

The source of infection could not be traced in any of these cases. In the case of Giga a suggestion was, indeed, thrown out that infection may have been imported in piece goods from Bombay, but the suspicion was quite unconfirmed.

Calcutta. Outbreak of plague in 1898; source of infection not traced. 6462; 6466; 6471–3.

We pass now to 1898, when there were undoubted cases of plague in Calcutta. The first case recognised as plague was that of a man who was found dead on the 17th of April 1898; he had not left the City for some time, and the source of his infection could not be traced. Anterior to the finding of this case it is probable that a certain number of cases had passed unrecognised. In particular it was afterwards found that one such case had been admitted into the Medical College Hospital as early as the 31st of March.

6608.

7178.

6701.

7481; 7488–97.

As to the source of the infection, Dr. Cook, the Medical Officer of Health for the City, said that there was no evidence of importation by land by a plague patient or by a person incubating the disease. Dr. Clemow went further, thinking that importation by rail was impossible. Major Evans held a similar opinion. But we may note that Major Dyson, though he was unable to obtain exact evidence regarding the case, had heard of a man who was said to have died of plague in Calcutta after arrival from Bombay, and Mr. Kailas Chandra Bose told us that he had known of people from infected places evading the railway measures and getting into Calcutta without the disinfection either of their persons or of their clothes.

7027; 7055.

Lieut.-Col. Sanders, I.M.S., thought that the plague had been introduced by a man who came by sea from Bombay in 1897 and landed ill with plague; the witness had seen this patient professionally two months after his arrival. Dr. Clemow, however, told us that no case of plague could be traced on any ship that had arrived at Calcutta.

7178.

7118–23.

Major Charles, I.M.S., and Dr. Cook seem to have been of opinion that the disease was introduced by rats conveyed in ships from plague-infected places to Calcutta. The former witness told us that he had heard of the death of rats on board ships which came to Calcutta, and he was of opinion that diseased rats escaped from ships and infected the rats in the neighbourhood of the wharves, and in particular in Fairlie Place, one of the first centres of plague in Calcutta. The facts of the case, so far as we have been able to gather them from Dr. Cook's Report on the plague in

Calcutta, are that dead rats were found about the 15th of April, in the East Indian Railways Printing House in Fairlie Place and in the Steam Navigation Company's godowns in which goods were stored. We have, however, seen that the disease had been in Calcutta for some time before these dead rats were found. 1. 464.

In view of the careful system of railway inspection which was in force and of the difficulties placed in the way of the importation of infection by sea-passengers, owing to the fact that the voyage from Bombay rarely lasts less than three weeks, Major Evans was of opinion that the infection could not have been brought by people, but must have been introduced in infected articles. 6882.

From the above summary of opinions, it will be seen that there is no accurate information available as to how the plague was introduced into Calcutta.

Measures adopted at Calcutta in April 1898, and exodus of population. 6540–1. 6520–39.

96. The announcement that plague was in Calcutta was followed by a large exodus of the population. It is estimated that 150,000 to 200,000 of the population of 700,000 left, mainly owing to panic due to fears regarding the measures which might be taken. Unscrupulous persons did their best to foment panic and to deter people from being inoculated. The sick were, except in a few instances, taken to hospital. After the first case or two, none of the contacts were placed in segregation camp. They were required to vacate the room in which a case occurred, but were allowed to go where they liked. The infected room was thoroughly disinfected. Houses adjoining infected houses were not evacuated.

Calcutta. Statistics, April to September, 1898. P. 473 Vol. I. 6757. 6575, 6815.

97. Plague was never severe in Calcutta during this outbreak. Up to 31st July, 1898, there were 190 cases and 155 deaths, and up to the end of September 230 cases and 192 deaths. The average number of cases was 39 in a month, and the highest number (77) occurred in June. These cases occurred in a period of about five months, and the incidence of plague cases was only ·03 per cent. of the census population. Of the 230 cases found, 188 were males and 42 females, or four males to every female. Cases were generally found after death, or just before it, and the disproportion of male and female attacks points to some concealment of cases. It could not, however, have been large, since the mortality from all causes between April and September 1898 was low, even if allowance be made for the number of persons who had left the town.

Second outbreak at Calcutta, February 1899. 6756. 6760–93. App. No. B. in Vol. II.

98. Calcutta was declared free from plague on 10th October, 1898. But there were shortly afterwards a few cases, and it would appear that plague was never absolutely extinct, though it was reduced to an extremely small amount. In February 1899 the town was again declared to be infected. We have been favoured by the Government of Bengal with an account of this outbreak up to 27th May, in the letter from the Secretary to that Government, dated 14th June, 1899. On this occasion it was determined to leave the sick, if they preferred it, to be treated in their own houses. Prompt disinfection of infected premises was mainly relied on to prevent the spread of the disease. The treatment of plague patients in their rooms to some extent interfered with the complete disinfection of the premises, but, on the other hand, the absence of compulsory segregation encouraged the people to report plague, so that many places were disinfected which would otherwise have escaped. Very few cases came to light in which there was a recurrence of the disease after disinfection, and "from the time when" in consequence of prompt reports of plague, "disinfection became immediate the epidemic began to abate." It is impossible to assert that the abatement was the result of the proceedings for disinfection. Vol. II., p. 534, para. 8.

Calcutta. Mortality, February to June 1899. Para. 5 of Bengal Government letter of 14th June 1899 App. B., Vol. II.

99. There appear to have been 59 attacks and 44 deaths from plague from 8th January, 1899, up to the week ending 4th March: of these deaths 33 took place after 24th February. In the four weeks ending on 1st April 492 cases were reported, and 439 deaths. The figures for the four weeks ending 29th April were 502 cases and 459 deaths. In the middle of April the outbreak began to decline. In the four weeks ending 27th May there were 189 cases and 175 deaths. There were 81 deaths in the fortnight ending 31st May and 49 in the remaining four weeks before 1st July.

Since the difference between the actual and the average mortality from the beginning of March to the end of May is considerably less than the number of reported deaths from plague, it would appear probable that there was not any general concealment of plague during this period. The ratio of mortality from plague to the population of the town from the commencement of the second outbreak until the end of June is only ·17 per cent.

Outbreaks in Bengal, outside Calcutta. 7331–75. Para. 10, Bengal Government letter above quoted.

100. The infection did not spread from Calcutta much into the interior of Bengal. In the earlier period of the epidemic an outbreak took place in the Backergunge district, but it was quickly suppressed. In the spring and summer of 1899 the plague had spread into 15 districts of the province; but in eight of them the total number of cases was only 21. In the remaining seven there were up to the middle of June 509 cases and 413 deaths. By that time plague had disappeared from every place outside Calcutta, except the town of Howrah which is only separated from Calcutta by the river.

III.—(*c.*) (6.) *History of the Plague, to the end of June* 1899, *in the Punjab.*

Outbreak in the Punjab. Procs. of H.H. the Lieut.-Gov. of the Punjab, Home (Med. and San.) Dep., No. 1479. L.P., dated 17.12.98.

101. A considerable amount of plague has occurred in the Jullundur and Hoshiarpur districts in the Punjab. The outbreak practically was confined to two revenue sub-divisions of the Jullundur district and one of the Hoshiarpur district. Up to the end of September 1898, 86 villages, with an aggregate population of 91,892, were attacked. The number of seizures reported was 3,409 (3·7 per cent. of the population) with 2,103 deaths. During this period only two towns, viz., Banga (population 5,010) and Garhshankar (population 6,000) were attacked.

Punjab. Source of infection of Khatkar Kalan not certain. Spread of plague, October to December 1897. 9917–30.

102. Plague was detected within this area in a small village called Khatkar Kalan in the Jullundur district on 21st October 1897. The suggestion has been made that the infection of this village was due to one Ram Saran, who returned to it from Hardwar at the end of April 1897. It is not certain that Ram Saran, who died the day after his return to Khatkar Kalan, actually died of plague, though it appears probable that he did so. In the interval between his death and the discovery of plague in the village (a period of nearly six months) there were a few suspicious deaths among people connected with him. There is, however, nothing in the nature of proof to connect the outbreak in Khatkar Kalan in October 1897 with the visit of Ram Saran to Hardwar in April of that year. No facts were available to suggest any other means by which the infection reached the Punjab. Many residents of the Punjab are employed on steam vessels which sail from Bombay, and others go there in search of other employment. Though there is nothing to show that this was the case, it seems possible that the infection may have been taken to the Punjab by some one who had been to Bombay for employment.

9933. App. XXXIV.

On the discovery of plague in Khatkar Kalan, special arrangements were made to search the neighbouring villages. During November plague was discovered in two more villages in the Jullundur district, and in one in the Hoshiarpur district. In December one fresh village was found to be infected in the Jullundur district.

Measures adopted in the Punjab. 9977. 10,235. 9980. 10,459.

103. Energetic measures were taken to prevent the spread of plague. When plague was discovered in a village it was evacuated. Partial evacuation was tried in some larger villages, and especially in Banga town, but it failed in most cases, and total evacuation had to be resorted to. The largest places evacuated in the Jullundur district were Rahon, with a population of 10,000, and Banga, and in the Hoshiarpur district Garhshankar. Considerable opposition in some places was experienced towards the measures adopted, and this culminated in an attack on the police at Garhshankar in April 1898. The police were obliged to fire on the mob, and the town was forcibly evacuated, after which no more resistance was experienced, either in the Hoshiarpur or the Jullundur district. The people were allowed to take as much property as they could with them, but their houses were not stripped till later on when disinfection took place. The people and their effects were disinfected on entry into the camp and were required to put out their property in the sun every day for 10 days after arrival in camp. In camp the sick and contacts were kept apart from the rest. While in camp the people were placed between two cordons, one round the village site to prevent people from entering it from the camps, and the other outside the land in the immediate vicinity of the village to prevent communication between the infected village and the outside world. After the people had been in camp for 10 days the houses were disinfected. There were four processes in the work of disinfection, viz., firstly, disinfecting with antiseptic solutions; secondly, cleansing; thirdly, ventilating; and fourthly, whitewashing. The people were kept in camp until they had been free from disease for three weeks, and were permitted to return only after they had taken a phenyle bath and all their property had been dipped in phenyle.

9984. 9993. 10,048. 10,057. 9996. 10,013–42. 10,054. 10,049.

Punjab. Course of

104. Up to the end of the year 1897, only five villages had been found to be infected. No fresh centres were found in January 1898, though it would appear that

seven had been attacked. The two villages originally infected were declared free in this month. In the month of February 15 and in March 27 fresh villages were discovered to be infected. The outbreak was very severe in April, when 33 fresh villages were found to be infected. The number of attacks fell rapidly in the end of May. Plague was detected in only eight new villages in that month and in one in June. A few cases occurred in June and July; at the beginning of June one fresh village was infected, and towards the end of July one case which occurred was found in a village which had before been infected and declared free, while four cases of a mild type were discovered in a village which had previously had only one case, and that not followed by reports of other cases. In August some cases were discovered in two villages which had been previously infected, and in a third which had not. At the end of September cases were discovered in two other villages. In the middle of November it was discovered that the town of Rahon (population, 10,667) was infected. There had evidently been at least one death from plague previously, which had been reported as due to asthma. The cases first discovered were in separate quarters of the town. It is uncertain how Rahon became infected. Plague had been present in the village of Kariam since September, and the last case reported there was on 2nd November, a few days before the first case was reported at Rahon. An easy explanation is that the infection came from Kariam, but against it is the fact that the people attacked in Rahon were of a different race to those attacked in Kariam. It appears possible that infected clothes may have been passed from Kariam to Rahon in bags of fodder, which are known to have been secretly passed across the cordon. Two quarters of the town were evacuated and the other usual measures were adopted. The residents were, in due course, allowed to return, and plague then started in another quarter, a resident of which had been in the habit of visiting the man whose disciple he was while in segregation camp. The whole town was then placed in camp. The evacuation began on 25th December, and was completed by 12th January. Only 12 cases and six deaths occurred in all; the date of the last case was 22nd February 1899.

the plague January 1898 to June 1899. App. XXXIV.

10,541, 10,551.

Procs. of H.H. the Lieut. Gov. quoted above.

10,459, 10,553.

10,534

10,546.

10,555.

10,562.

The infection in 1899 spread to 27 villages, of which nine had been infected in 1898. It was discovered in two villages in February 1899, in two in March, in 15 in April, in six in May, and in two in June. The outbreak was not severe. By the middle of June 361 cases and 202 deaths had been reported outside the town of Rahon, and by the end of June the outbreak ceased. Seven cases of plague, with 2 deaths, occurred in a small village of the Gujar Khan subdivision of the Rawalpindi district, from the 20th to the 23rd June. There were no further cases, and the source of infection was not traced.

III.—(*c.*) (7.) *History of the Plague in* 1897–8 *in the North-Western Provinces.*

105. Plague was discovered in Hardwar, a town of 5,000 inhabitants, in the extreme north of the North Western Provinces, in the first week in April 1897. Hardwar is situated where the Ganges flows into the plains. Kankhal is one mile from Hardwar, on the same side of the river, and Jawalapur is about three miles off on the opposite side of the Ganges Canal, the head of which is half a mile below Hardwar. At the time when plague was introduced into Hardwar, the disease was epidemic only in localities that are situated at a great distance from that town. The existence of plague in the town was first ascertained on the 8th of April 1897. On that date information reached the authorities of the death of a woman who had been living in a lodging house frequented by Sindis from Karachi. A suspicion—subsequently confirmed by bacteriological examination—that the woman had died of plague, led to the inspection of the lodging house in which she had lived, and on the 8th of April the woman who kept the house was found to be recovering from an attack of plague, having been ill for about a month. Mr. Winter, the magistrate of the district in which Hardwar is situated, who was in charge of the plague operations, said:—"It was not possible to ascertain with any degree of certainty how the disease was imported, but there appears to be little doubt that it first appeared in the lodging house above referred to, and it seems probable that it was imported by one of the pilgrims from Sind resorting to the lodging house before special measures were taken to segregate pilgrims from infected areas." Captain Chaytor White, I.M.S., on the other hand, was of opinion that the introduction of the infection probably took place through the agency of the post, and that it was conveyed from Karachi in bags containing the bones or ashes of Hindus who had died of the plague. This theory was suggested (*a*) by the circumstance that the ashes of Hindus who die in places far distant from the Ganges are forwarded in bags through the post to Hardwar, with a view to their being thrown into the sacred

Outbreak in the North Western Provinces. Hardwar.

9003, 9016, 9212

9025.

9028.

9456-9.

river; and (*b*) by the circumstance that some of the first cases of plague which came to light in Hardwar occurred near the Post Office. When it is noted that no case of plague occurred among the postal officials, it will be obvious that the theory rests on a very slender basis of fact. The outbreak at Hardwar was confined to a large block of about five or six acres, which it was easy to cut off from the rest of the town. This block was evacuated, and the whole town disinfected. The number of persons actually left in the town was between 300 and 500. There were 18 cases of plague at Hardwar, the last on 8th June 1898.

9029-48. App. XXVIII. 9064.

North Western Provinces. Kankhal. 9066–9. 9073. 9101–2.

106. The town of Kankhal is of about the same size as Hardwar. A man from Hardwar died in Kankhal from plague on 16th May, 1897, and in the first week of June rats began to die in Kankhal. It was not, however, till the beginning of August that suspicious deaths began to occur in the town, and plague was not recognised there till 16th September. The infected block was then evacuated, but other parts of the town began to be infected. Block after block was evacuated on ten or eleven different occasions, till the inhabitants of the town were all in camp by the end of October. The houses were disinfected by the middle of November. After evacuation a few cases occurred in the camp, and the epidemic came to an end in December. There were 61 cases of plague during the outbreak at Kankhal, 51 before the end of October, 5 in November, 4 in December, and 1 at the beginning of January 1898. The incidence of attacks on the population was 1·2 per cent. A special incident of this outbreak was the considerable mortality among the monkeys which infest Kankhal, and some of them undoubtedly died of plague.

9113. 9132. 9119–23.

North Western Provinces. Jamalpur, Jagjitpur, and neighbouring villages. 9133–4. 9137–48.

107. An outbreak of plague occurred in two villages in the neighbourhood of Kankhal, and sporadic cases in six other villages. In the villages where sporadic cases occurred a large area round the infected house was evacuated, and the actually infected house was, as a rule, burnt. The two villages in which an outbreak took place were Jagjitpur and Jamalpur. The former, Jagjitpur, is a village of 1,200 inhabitants, in which plague was detected on 29th December 1897. The infection seems to have come from Kankhal. The people were evacuated by 8th January 1898. Ten cases occurred before evacuation, and 13 afterwards; all the latter, with one or two exceptions, within a few days of the evacuation of the village. Jamalpur (population 1,400) was attacked on 12th February 1898. The father of the first person attacked had been to Jawalapur a few days before. The second case occurred a week after. A portion of the village was evacuated immediately. Then two disinfection coolies were attacked, and then four more cases occurred in the village at the beginning of March. The village was evacuated by 10th March. Twenty-six more cases occurred, the last on 18th April. The disease, especially during the later stage of the outbreak, was observed to run through families, and not to spread to those not related to persons previously affected.

9150–68.

North Western Provinces. Jawalapur. 9173–86.

108. The most severe outbreak in the neighbourhood took place at Jawalapur, a town of about 16,000 inhabitants. About 13,000 persons were present in the town when plague began and the number fell to 12,000 in February. The first case was discovered on 9th January, 1898, but it was impossible to trace the source of infection. A block was evacuated at once and disinfection started. An attempt was made to disinfect the whole town without evacuation, but this was found not to be sufficient, and cases occurred very rapidly. On the occurrence of each case a fresh area was evacuated, and eventually there was a voluntary evacuation of the whole town except a quarter occupied by 600 persons. Plague was practically stopped at the end of April, and the people returned during the last three weeks of that month. There were three cases in May, one being the case of a man engaged in work connected with disinfection, the second that of a person who was attacked and died in the town after returning to her house, and the third that of a relation of the second, who died in the segregation camp. Of the 116 cases 31 occurred in the town, 24 in the segregation or contact camps, 30 in the evacuation camps, and 29 (an unusual number) among persons "connected more or less with the work of disinfection." The incidence of attacks is at the rate of ·07 per cent. on the normal and ·09 on the assumed population when the outbreak took place (12,000).

9059–226.

A sporadic case or two occurred both in Hardwar and Kankhal while Jawalapur was infected, but these two places were not re-infected, and plague has not recurred in the neighbourhood.

Measures adopted successful in suppressing plague in the North Western Provinces.

109. The operations at Hardwar and the neighbourhood were completely successful in preventing the spread of plague from this centre. The country was a favourable one for these operations, and the establishments employed were very strong and very

expensive. In addition to the civil and medical establishments employed there was at one period of the outbreak a force of police and village watchmen numbering nearly 450 employed on them. 9212. 9230.

III.—(*c.*) (8.) *History of the Epidemic of Plague in the Sirohi State in* 1897–8.

110. Plague was taken to Teuri in the Sirohi State by a shopkeeper from Poona, who died the day after his arrival in the village. At the funeral feast his clothes were distributed. The infection was not discovered till 22nd November 1897. Seventeen deaths had then taken place in four villages within two miles of one another. The people were at once taken out of the villages into camps and kept there for some time. Nine cases occurred after their removal. The houses were disinfected. The people who were not infected were on account of the cold weather allowed to return to their homes early. All the 26 persons attacked in this outbreak belonged to the Banniah (shopkeeper) caste. Outbreak in the Sirohi State, November 1897. 9702. 9709–91. 9770. 9764.

111. At the beginning of January fresh cases of plague were discovered in three of the villages that had been infected in November, and in Kalindri, a village of 4,000 inhabitants, which had escaped at that time. The people were again moved out of their villages, and those from two villages were in camp on 11th January, those from the third on 21st January, and those from Kalindri on 20th January. A force of police and soldiers (20 in number when the villages were being evacuated, and 140 directly afterwards) was employed to prevent the people from going to their own or neighbouring villages. Plague ceased four days after evacuation in the village evacuated on 21st January (population 400). There were five cases before the people evacuated the village, and three afterwards. In Teuri (population 1,800) there were six cases before the people went into camp, and 20 afterwards. Plague continued for 20 days after evacuation. In Sheudial (population 1200) there were nine cases before the people went into camp, and 13 afterwards. The last case was 22 days after evacuation. The outbreak was more obstinate in Kalindri. Eighteen cases occurred before the place was evacuated, 20 between 20th January and the end of the month, 32 in February, and eight in March. Three weeks after other cases had ceased there were six isolated cases in April among people who were about half a mile away from the village in a place in or near which there had before been no plague. Sirohi State. Second outbreak in January 1898. 9810. 9823. 9879. 9825–9. 9873–7. 9862–6. 9830–8 9859–61.

Of the 140 persons attacked in this outbreak, 117 died. The houses were more thoroughly disinfected than on the first occasion, and were kept unroofed for a considerable time. There has been no return of plague to this district. 9886.

III.—(*c.*) (9.) *History of the Epidemic of Plague at Khandraoni in the Gwalior State in* 1897–8.

112. The village of Khandraoni is situated in the State of His Highness the Maharaja of Gwalior, and has a population of about 450. It was infected early in January 1897 by two brothers, natives of the village, who had been living in Bombay. One of them apparently travelled, when sick, from Bombay, and died a few days after his arrival in the village. His brother, who travelled with him, fell ill two days after his death, and died three days later. Others were attacked in about a week, and then the outbreak took place. It did not come to the notice of the authorities till the middle of March, when 59 persons had been attacked, of whom 47 had died. At this stage of the outbreak the village was evacuated, the sick and contacts segregated, and the village cut off from neighbouring villages by a cordon of soldiers. The soldiers were also employed to cut the crops. Four hundred and fifty men were engaged on these duties. Only four cases occurred after the village was evacuated, the last in the hospital camp on the 12th day after evacuation. The village was disinfected, and a part of it burnt down. The infection did not spread to any neighbouring place. Outbreak in the Gwalior State. 9643. 9637. 9694. 9650–3. 9662.

IV.—History of the Plague in India in July, August, and September, 1899.

IV.—(1.) *Course of the Plague in Bombay City in July, August, and September*, 1899.

113. The above account carries the history of the present outbreak of plague in India down to the end of June 1899. We may supplement it with a short account of what occurred between the end of June and the end of September 1899. Bombay City July, August, and September 1899.

During July, August and September, plague was not severe in Bombay City. The decline in the number of cases which had set in in March continued till July, but, as in 1898, the number of cases began to increase again after July. The increase, however, in 1899, was less than it had been in the previous year. From the 1st to the 28th July 1899, only 280 cases were reported to have occurred; 484 cases were reported in the five weeks ending on the 1st of September 1899, and in the next four weeks, ending on the 29th September 1899, the number reported was 403. The figures for the corresponding periods in 1898 were 296 cases in four weeks in July, 709 cases in the next five weeks (August), and 844 cases in four weeks in September. The number of plague deaths reported to have occurred from the 1st July to the 29th September 1899 was 980; during this period the actual mortality from all causes (8,179 deaths) exceeded the average number of deaths from all causes for the period (6,994) by 1,185.

IV.—(2.) *Course of the Plague in the Bombay Presidency in July, August, and September*, 1899.

Bombay Presidency, excluding Bombay City. July, August, and September 1899.

114. Excluding the City of Bombay, there occurred in the Bombay Presidency, from the 2nd to the 30th June 1899, 2,582 plague cases with 1,754 deaths. The numbers rose in the period from the 1st to the 28th July to 9,033 cases and 6,782 deaths—more than three-and-a-half times as many as in the previous four weeks. In August, again, there was a very great rise in the figures; 23,397 cases and 17,947 deaths took place from the 28th July to the 1st September. In September there was a further rise, but it was not so large, the numbers of cases and deaths that occurred from the 2nd to the 29th September being 23,776 and 17,951 respectively.

Poona, Nasik, Ahmednagar, and the Southern Mahratta country and neighbouring districts July, August, and September 1899.

115. In the Bombay Presidency, the most serious outbreak of plague during July August and September 1899 was that in the City and district of Poona. In the City, the number of cases was large in July, attained its maximum in August, and began to decline in September; during the three months the total numbers of indigenous cases in the City, in the Cantonments, and within the suburban limits of the Municipality, were 10,158, 1,686, and 244 respectively. In the Poona district, where there was little plague in June, the disease spread very widely during the months now in question. The Cantonment of Kirki was again attacked in the middle of July and had 150 indigenous cases up to the end of September. During these three months the districts also of Belgaum, Nasik, Satara, Dharwar, and Ahmednagar, and the Kolhapur State, were badly infected. The number of cases that occurred in July was much greater than the number that had occurred in June, and the increase of cases in August and September was very considerable. Taking together all the places and districts named above, 2,264 cases occurred in them in June, and the number rose to 8,035 in July, and to 20,801 in September. The state of affairs in these districts and places is shown in the following table:—

1899.	26th May to 30th June.		1st July to 28th July.		29th July to 1st Sept.		2nd Sept. to 29th Sept.	
	Cases.	Villages infected at end of Period.	Cases.	Villages infected at end of Period.	Cases.	Villages infected at end of Period.	Cases.	Villages infected at end of Period.
Poona City	482	—	2,519	—	6,884	—	2,820	—
„ district	25	5	189	11	1,293	30	3,106	70
Belgaum district	714	18	1,930	44	3,511	66	3,105	76
Kolhapur State	566	14	1,576	26	3,033	40	3,028	61
Nasik district	129	2	714	12	2,708	29	3,545	54
Satara district	203	7	774	18	1,433	37	2,315	53
Dharwar district	131	9	282	13	845	19	1,573	26
Ahmednagar district	14	1	51	4	489	9	1,309	13
Totals	2,264		8,035		20,196		20,801	

In the Belgaum district the most important places which were attacked were the City and Cantonments of Belgaum, and the town of Nipani (population 11,800). Plague continued in these places during the whole of July, August, and September, and the number of indigenous cases that occurred was 1,641 in the City, 304 in the Cantonments, and 474 in Nipani. The principal places attacked in the Nasik district

were Nasik town, Sinnar, and Igatpuri, which had 1,684, 623, and 272 indigenous cases respectively; the two former towns had plague throughout the three months, and Igatpuri was re-infected in the first week of July. Satara town was not again infected, and the largest places which had plague in the Satara district were Karad, re-infected in the third week of July (586 indigenous cases), and Tasgaon, where cases had continued to occur since the middle of February 1899, the number of deaths reported between the time of re-infection and the end of September being 332, of which 128 occurred in the three months now under consideration. Dharwar town was infected during the whole period now under consideration, while Hubli and Gadag, in the Dharwar district, became re-infected in the second week of July. Up to the 29th of September there had been 745 indigenous cases in Dharwar town, 720 in Hubli, and 37 in Gadag. Ahmednagar town became infected towards the end of July, and by the end of September had had 1,006 indigenous cases.

In the neighbourhood of the districts named above there was plague in the Bijapur and Sholapur districts, and in the States of Akalkot, Savanur, Bhor, and Aundh. In Bijapur the number of cases increased from 49 and 90 in June and July to 507 and 549 in August and September, the number of places infected in the district at the end of September being 21. Sholapur, which was practically free in the first two months, had 140 and 179 cases in the last two months, and by the end of September 10 places in the district were attacked. Plague re-appeared in Akalkot in September, when there were 20 cases. In Savanur the number of cases rose from three in July to 118 in September. The outbreak in Aundh also became more severe during the period under consideration, there being 41 cases in July and 192 in September. The Bhor State was re-infected in August, when it had 35 cases; there were 97 cases in the State during September.

Places near and to the south of Bombay. July, August, and September, 1899.

116. Places in the neighbourhood, and to the south, of Bombay continued infected. The outbreaks, however, declined in the Kanara district, and were slight in the Janjira State, the Kolaba district, and the Ratnagiri district, the most seriously infected of these being the district of Ratnagiri, which had 91 cases in September. In the Thana district the infection was worse, and 277, 535, and 244 cases respectively occurred there in July, August, and September. The towns of Thana and Bhiwandi had plague throughout these months, the number of indigenous cases that occurred in them being 115 and 660. Two other considerable towns in the district, Bassein and Kurla, were re-infected. The re-appearance of plague in Bassein occurred in the third week of July, and 52 indigenous cases took place before the end of September. Kurla was not re-infected till late in August and had only a few cases.

The Central and Northern part of Bombay Presidency, excluding Sind. July, August, and September 1899.

117. In the Central and Northern part of the Bombay Presidency, excluding Sind, there was plague during July, August, and September, 1899 in Khandesh, Surat, and Broach, Rewa Kantha, the State of Baroda, the Kaira and Ahmedabad districts and the Cutch State, and Kutiana in Kathiawar. There was practically no plague in these places in June, except in Surat and the Cutch State, and the number of cases reported in July, August, and September was not large. Khandesh, Ahmedabad, and Kathiawar had no cases in July, a very few cases in August (when plague reappeared in these places), and a few cases in September. In the other places named the outbreaks generally were more severe in August than in July, with a slight decrease of cases in September. The largest number of attacks was that reported from the Surat district, which had 153 cases in July, 387 in August, and 346 in September, the number of places infected in the district at the end of September being 8. The towns of Surat and Bulsar remained infected throughout the three months, and in that period had 414 and 129 indigenous cases respectively. Kaira had 56, 288, and 181 cases respectively in July, August and September, and at the end of the period there were 10 places infected in the district. The number of cases that occurred in the other places named exceeded 100 in August in Broach, in the Cutch and Baroda States, and in Rewa Kantha, and in September in the Cutch and Baroda States only. The town of Cutch Mandvi was free from plague except for a few days, and Baroda City, though it was re-infected in the beginning of August, had only 11 indigenous cases between the date of its re-infection and the end of September. In Rewa Kantha the town of Balasinor was infected in the beginning of June, and had 198 indigenous cases in the period now under consideration.

Sind. July, August, and September 1899.

118. In Sind, the plague in the City and district of Karachi, where there had been 72 and 17 cases respectively in June 1899, declined in July. In that month 13 cases only were reported in Sind. All these occurred in Karachi City, in which cases

continued to occur during the next two months, there being 11 in the five weeks ending on the 1st September, and 22 in the four weeks ending on the 29th September. The Karachi district remained free in August, but in September it had six cases, which were not indigenous. The town of Hyderabad was re-infected in the middle of August, and had 151 cases between the date of re-infection and the 1st September, with 258 cases in the next four weeks.

IV.—(3.) *Course of the Plague in Bangalore and the Mysore State in July, August, and September*, 1899.

Mysore State. July, August, and September 1899.

119. Outside the Bombay Presidency there was plague in July, August, and September, 1899, in Mysore, the Hyderabad State, the Madras Presidency, the Central Provinces, Bengal, and the Punjab.

Plague had ceased in Bangalore City in the end of March 1899, but after the middle of June the infection was again imported into the City. The infection was imported from an infected part of the Mysore State, and in the week ending on the 23rd June there were four indigenous cases, followed by five more indigenous cases in the last week of June. From the 1st to the 28th July 20 cases, all indigenous, occurred in the City. In the next four weeks the number of cases rose to 28, of which all but one were indigenous. In the next five weeks, ending on the 29th September 1899, there was a further rise, to 47 cases—of which only two were imported.

In each of the months of May and June 1899, only three cases of plague had occurred in the Civil and Military Station of Bangalore, two of the cases in each month being imported, and one being indigenous. Two cases, both indigenous, occurred in July, in the last two weeks of the month. In August such cases began to occur more frequently. In the week ending on the 25th August there were four such cases, in the next week there were eight, and indigenous plague cases then continued to be reported every week. From the 1st to the 29th September there were 25 cases in the Civil and Military Station (all but seven being indigenous), and in the last week of this period the indigenous cases amounted to nine.

The number of cases reported from Mysore City was 61 in July 1899—a slight increase on the number reported in the previous month. In August the number of cases fell to 46, and in the four weeks ending on the 29th September there was a still further fall to 25 cases.

The state of affairs in the districts of the Mysore State during the three months now in question is shown in the following table:—

Period.	Bangalore.		Mysore.		Kolar.		Tumkur.		Chitaldrug.	
	Villages infected at end of Period.	Cases during Period.	Villages infected at end of Period.	Cases during Period.	Villages infected at end of Period.	Cases during Period.	Villages infected at end of Period.	Cases during Period.	Villages infected at end of Period.	Cases during Period.
1–28 July 1899 - - -	13	242	12	160	4	70	1	4	—	—
29 July—1 September 1899 -	25	276	16	336	5	145	5	82	1	1
2–29 September 1899 - -	23	272	13	205	7	106	1	16	3	145

Throughout the whole of the period for which figures are given above, the state of affairs in the Bangalore and Mysore districts was worse than it had been in June 1899; the returns for September however, showed signs of improvement. In Kolar, including the gold fields, plague declined, and in Tumkur and Chitaldrug the number of places attacked was small. The other districts of the State had no plague.

IV.—(4.) *Course of the Plague in the Hyderabad State in July, August, and September*, 1899.

Hyderabad State. July, August, and September 1899.

120 In H.H. the Nizam's dominions there was a large increase of plague in July 1899. In the previous month only four villages remained infected, and 57 cases had

occurred. In July there were 813 cases in 25 villages. The Naldurg district with 362 cases in 10 villages, and the Gulburga district with 308 cases in 6 villages, were worst infected, 123 cases being reported in the Lingsagur district and 19 cases in Bidar.

In August plague was reported from 38 villages in the State, and the number of cases rose to 985. 247 cases occurred in Gangawathi, a village in the Kushtagi sub-division of Lingsagur, and eight other villages in that district had 82 cases. 482 cases occurred in the Naldurg district, in which the worst affected tract was that of Ganjoti, containing, among other infected places, the town of Umerga, which reported 79 cases in August, and 86 in the previous month.

In September the total number of infected villages rose to 48, and the number of plague cases to 1,334. The Naldurg district continued to be the worst infected. In it 844 cases were reported from 24 villages; the number of cases in Umerga fell to 13, but Muram had 249 cases, and Alur 119. The Lingsagur and Gulburga districts had 8 and 11 infected places with 273 and 180 cases respectively, the places with most plague being Gangawathi (139 cases) and Lingsagur (63 cases) in the Lingsagur district, and Jawargi (110 cases) in Gulburga.

IV.—(5.) *Course of the Plague in the Madras Presidency in July, August, and September*, 1899.

Madras Presidency. July, August, and September 1899.

121. In the Madras Presidency, outside the Salem district where the outbreak was not severe, there was practically no plague during July, August, and September 1899. Anantapur had four cases in two places in July and August; North Arcot reported one case in July and another in the fortnight covering the end of September and the beginning of October, and Bellary also had one case in the period last mentioned. In Salem, from the 1st to the 29th July, six places reported 34 cases; in the next four weeks 11 places reported 22 cases; and in the six weeks ending on the 8th October there were 13 places infected, in which 88 cases were reported.

IV.—(6.) *Course of the Plague in the Central Provinces in July, August, and September*, 1899.

Central Provinces. August and September 1899.

122. In August 1899 plague re-appeared in the town of Nagpur, in the Central Provinces. Up to the 30th August 22 cases occurred, while between the 30th August and the 1st October the number of cases in the town rose to 154. The outbreak continued after the end of September. A village near the town was infected in the beginning of September by a contact who had got out of Nagpur, but prompt measures suppressed the outbreak in the village, after two fatal cases had occurred. At the end of September two other villages became infected.

IV.—(7.) *Course of the Plague in Bengal in July, August, and September*, 1899.

Bengal. July, August, and September, 1899.

123. In Bengal plague occurred during July, August, and September, 1899, only in Calcutta and Howrah. The number of cases and deaths reported in Calcutta gradually increased from 10 cases, all fatal, in the first fortnight of July to 113 cases, with 110 deaths, in the last fortnight of August. In the first and second fortnights of September respectively there were reported 103 cases, all fatal, and 100 cases, of which 99 were fatal. The total for the three months was 460 cases and 454 deaths. It is to be noticed that on the average of the mortality returns for past years, on the assumption that there was no exodus at this time, 2,406 deaths from all causes might have been expected to occur from the 16th August to the 30th September, and that in 1899 in this period the number of deaths that actually did occur was 2,684. In Howrah there were during July, August, and September 1899, 16 cases of plague with 15 deaths; nine of these cases with eight deaths occurred in the first fortnight of August, and there was only one case and death in September. In the last fortnight of September a few cases suspected to be plague occurred in the Saran district.

IV.—(8.) *Course of the Plague in the Punjab in July, August, and September*, 1899.

Punjab. July 1899.

124. In the Punjab there were five cases on the 16th July 1899, in a village of the Murree revenue subdivision of the Rawalpindi district. These cases all ended in recovery, and they were not followed by other cases. A few cases occurred in a village which had been infected in June in the Hoshiarpur district, but the disease practically disappeared from the Province by the end of July 1899.

V.—General Remarks on the Mortality due to Plague in India.

General remarks on the mortality from plague.

125. It will be observed from the details given above regarding the outbreaks at the larger centres at which plague has occurred, that the differences in the proportion of the population which has been attacked and has died at different places have been very striking. In some places the percentage of attacks and deaths has been very high. Up to the 29th September 1899, counting indigenous cases only, and stating populations according to the returns of the Census of 1891, there had been in the different outbreaks which have attacked Poona City and Cantonments and Kirki 23,262 reported cases with 18,063 deaths out of a population of 172,341; in Karachi City, 10,498 cases with 8,156 deaths out of a population of 105,199; in the town of Bulsar, 1,625 cases with 1,191 deaths out of a population of 12,999; at Bhiwandi, 3,004 cases with 2,197 deaths out of a population of 14,387; at Karad, 2,171 cases with 1,714 deaths out of a population of 12,374; in Belgaum City and Cantonment, 6,826 cases with 5,298 deaths out of a population of 41,413; at Sangli in the Sangli State in Kolhapur, 3,010 cases with 2,682 deaths out of a population of 14,798; at Shahapur in the same State, 2,202 cases with 1,924 deaths out of a population of 12,046; at Cutch Mandvi, 6,999 cases with 5,832 deaths out of a population of 38,155; at Bantwa, 944 cases with 602 deaths out of a population of 8,200.

In some small places the mortality was still more appalling. It is estimated that in the Portuguese Settlement of Lower Daman 2,000 out of a population of 7,000 died between the end of February and the beginning of June 1897. In the Dharwar district some villages suffered fearfully during the autumn of 1898. At Ibrahimpur (population 1,716) the attacks came to 50, and the deaths to 35, per cent. of the population; at Ingalhalli (population 2,203) the corresponding figures were 48 and 37; at Datnal (population 1,280), 45 and 36; at Shelwadi (population 4,222), 32 and 28; at Lingdhal (population 1,469), 30 and 25; and at Bayahatti (population 3,589), 28 and 22. We have quoted these instances to show how severe an outbreak of plague may be. Although this enumeration does not exhaust the list of very severe outbreaks, cases in which the mortality has even approached these figures have been very rare. The number of outbreaks in which even 10 per cent. of the population has been attacked is comparatively small.

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Total recorded plague mortality in India from September 1896 to September 1899.

126. The deaths reported up to the 29th September 1899 as due to plague in the Bombay Presidency, including the Native States connected with that Presidency, number 250,677. The plague deaths reported up to the end of September 1899 from other Provinces of British India and from Native States outside Bombay number about 36,300. There is, however, no doubt that the reported deaths do not represent the real mortality from plague, and in order to obtain an adequate estimate of the mortality due to the present epidemic of the disease in India, we may, without undue exaggeration, assume that even as many as 35 per cent. of deaths really due to plague have been wrongly assigned to other causes. On this assumption the mortality from plague in the 36 months from September 1896 to September 1899 amounts to over 376,000 in the Bombay Presidency, and to about 54,450 in the rest of India. This computation gives a total of about 430,500 deaths for the whole of India.

If we restrict ourselves to the figures for the Bombay Presidency and the connected Native States, within which plague has been principally confined up to the end of 1899, we find that, in a population which in 1891 amounted to about 19,400,000, the computed number of deaths from plague in three years represents 1·9 per cent. of the population, and, if the mortality were spread equally over the three years, this would give an annual death rate of ·6 per cent. The effect of plague, however, on the mortality of the whole of India has been much less than this. The population of the whole of India may, without taking account of any increase that may have occurred since the census of 1891, be taken as being over 287 millions, and a calculation on this of the computed number of deaths up to September 1899 shows that only ·15 per cent. of that population has during three years died of plague, giving a death rate that is equal to an average mortality of ·05 per cent. per annum.

Total plague mortality recorded in India not high in comparison with total mortality from "fevers" and cholera in India.

127. A comparison of the above figures with the vital statistics compiled for India during recent years shows that the mortality from plague has not as yet attained to any considerable proportion of the total mortality of the country, and that plague has not in three years been able to extend and take a hold of the country in such a way as seriously to affect the ordinary death rate. In British India the average annual number of deaths from all causes during the six years from 1893 to 1898 has been about 6½ millions, in a population which may be reckoned at 221 millions on the

census of 1891 and without making allowances for any increase that may have occurred since the census. The figure, 6½ millions, may be taken as fairly representing the average mortality of an ordinary year. The diseases which contribute most to this total are "fevers" and cholera. The incidence of mortality from "fevers" and cholera varies considerably in different parts of India; the mortality from cholera varies considerably from year to year; the mortality from "fevers," which is very inaccurately classified but in the main represents deaths which are the direct or indirect results of malaria, is fairly constant from year to year over the whole of British India. Calculated on the returns for the years from 1893 to 1898, the average number of deaths from cholera in British India was about 370,000 per annum, or about ·16 of the population, the cholera mortality in the Bombay Presidency, excluding the connected Native States, being about ·14 per cent. of the population; while the deaths from "fevers" amounted annually in British India to more than 4⅓ millions, or about 2 per cent. of the population. Taking with these figures the figures which we have given above for plague, we find that, in the three years following the appearance of the present epidemic of plague in Bombay, although the mortality from plague in the Bombay Presidency has been far larger than the average mortality from cholera in the same area, the plague mortality has not, over the whole of India, approached the average mortality from cholera, while over the whole of India the plague mortality of three years amounts to only a little more than 9·5 per cent. of the mortality attributed in British India to "fevers" in a single year, and has equalled about 6·6 only of the total mortality in British India in an ordinary year. Thus, although the figures of plague mortality when taken by themselves are high, it is evident that plague has not as yet been able to make itself felt as one of the most important factors that influence the total mortality of India.

CHAPTER III.

CHARACTER OF PLAGUE, AND THE MEANS BY WHICH THE DISEASE IS COMMUNICATED AND SPREAD FROM PLACE TO PLACE.

I.—Scope of Chapter.

Scope of the Chapter.

128. We have embodied in the historical survey which forms the subject-matter of the last Chapter, the results of our special inquiries into the origin of plague epidemics in some of the more important centres of population which have been visited by the disease in India. It will have been seen in the course of that survey that in none of the centres in question was definite information available as to the manner in which the plague was introduced. In this Chapter we proceed to describe the character of plague and to study in detail the means by which the disease is communicated and spread from place to place. With a view to throwing light upon these questions, we propose on the one hand to draw upon the clinical and bacteriological facts which have been obtained from a scientific study of the disease and its specific infective agent, the plague bacillus, and on the other hand to draw upon the epidemiological observations which have been furnished to us by Medical Officers and others engaged in plague operations in India.

Order in which the subject-matters are discussed in this Chapter.

129. It will be well to preface our consideration of the wide ranging subject-matter which will come under review by setting forth in brief synopsis the plan adopted in this Chapter.

Main features of plague.

We commence by setting forth so much of the main features of the disease as is essential to the proper appreciation of the facts which come under consideration in connexion with the spread of plague.

Bacteriology.

This done, we consider the plague bacillus, dealing first with its morphological and cultural characteristics, the conditions which favour its growth, and its powers of resistance outside the organism. Following upon this, we discuss the means which are

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available for differentiating the specific infective agent of plague from other bacteria, by microscopical examination, cultivation on artificial media, and inoculation into animals.

Mode of infection. We then pass to the discussion of the mode in which the infective agent may effect an entrance into the organism.

Incubation period. The duration of the incubation period is next discussed.

Clinical features. The detailed description of the clinical symptoms and pathological anatomy of plague and the consideration of the statistical data available with regard to case mortality and the relative frequency of various clinical forms of the disease, and also certain facts with regard to the recurrence of the disease in the same person, would, at this point, quite naturally have come up for discussion. But, inasmuch as these questions have only an indirect relation to the particular subject-matter in hand and lie somewhat outside the scope of our Reference, we have decided that they can best be dealt with in Appendices. In Appendix I., for which we are jointly responsible, will be found the purely statistical matter which we have collected, and in Appendix II. the clinical features of plague have been dealt with by the President.

Infectivity of persons, rats, houses, and other objects. We therefore pass on directly from the discussion of the incubation period to the discussion of the infectivity of plague. Beginning the consideration of this question by treating of the infectivity of plague patients generally, we then consider, under a series of separate headings, the infectivity of the various clinical forms of plague. After disposing of the question of the infectivity of the human patient, we consider the question of the infectivity of rats infected with, or dead of, the disease. The infectivity of dwelling-houses, of clothes and household effects, and of merchandise, which may have come in contact with plague patients or plague rats, next comes under consideration.

Manner in which the disease spreads. The discussion of the questions dealt with above will then naturally lead up to the consideration of the manner in which plague spreads from infected to uninfected towns and villages, and then from house to house within an infected town or village; incidentally, and with a view to making our discussion of the question of spread complete, we deal also with the conveyance of plague over sea.

Influence of age, &c. Another aspect of the matter then presents itself, this being the question as to how far age, sex, race, occupation, and habits, affect the liability of the individual to plague infection.

Epidemiological problems. Lastly, we endeavour to complete our survey of the question of the spread of plague by considering a series of epidemiological problems which arise in connexion with the disease. Such problems present themselves (*a*) in connexion with the question of the occurrence of a prolonged interval between the importation of infection and the epidemic outbreak of the disease; (*b*) in connexion with the occurrence of atypical forms of the disease at the outset of an epidemic; (*c*) in connexion with the fatality of plague at different phases of an epidemic; (*d*) in connexion with the influence of meteorological conditions and seasonal variations in the severity of plague epidemics; (*e*) in connexion with the influence of insanitary surroundings on the severity of plague epidemics; (*f*) in connexion with the existence of an endemic focus of plague in the Himalayas, and the general question of the possibility of a recrudescence of the disease, apart from re-importation of the infection; and (*g*) in connexion with the theory that the plague bacillus requires to be acclimatised to the particular conditions obtaining in each locality before it can give origin to a plague epidemic.

II.—General Survey of the Character of Plague.

Brief review of former appearances of plague in India and elsewhere. 130. In conformity with our scheme, we begin with a general survey of the character of the disease.

Ever since the dawn of history we have records of epidemics of pestilential disease, associated with buboes and accompanied by a very high mortality. The pestilential disease characterised by "emerods in the secret parts," which is noted in the biblical records as striking down small and great among the Philistines in the time of the Judges; the pestilence which devastated Athens during the course of the Peloponesian War; the "Black Death" of the Middle Ages; the "Great Plague" of London; the fatal pestilential disease which was widespread in the Deccan and Western India, during

the latter part of the 17th Century; and the plague at Marseilles in the 18th Century; were probably all true bubonic plague. Coming to more recent times there is no doubt that, on the one hand, the "Oriental Plague" of Turkey, Egypt, and the Levant, in the early part of the 19th Century, and, on the other hand, the plague which occurred in Western India at various times from 1812 to 1837, were genuine bubonic plague; and, lastly, it seems, as we shall see, to be beyond question that the "Mahamari" which, from 1823 onwards, has been reported from the Himalayas, is to be identified with genuine plague.

Difficulties experienced in former epidemics in connexion with the identification of atypical forms of plague in man.

131. The difficulty of discriminating plague from other diseases in pre-bacteriological days is well brought out by the fact that, both in the case of the plague in Western India in the early part of the present century, and in the case of the mahamari of the Himalayas, some observers were of opinion that the epidemics with which they were dealing were epidemics, not of true bubonic plague, but of typhus, a disease which, both in its epidemiology, and in certain of its clinical characters, has many points of similarity with plague. Again, the difficulties, in the absence of bacteriological investigation, of diagnosing certain forms of plague, are well illustrated by the uncertainty which has constantly prevailed as to whether cases of pneumonic affections and cases of high fever ending fatally, where such came under observation in association with cases of typical bubonic plague, were to be regarded as aberrant forms of the disease. We may refer here, for instance, to the fact that in the case of the plague, which raged in Kathiawar and the adjoining parts of Western India in the second decade of the 19th Century, there came under observation epidemics, which must, from their clinical description, have been epidemics of pneumonic plague, and which were, even then, surmised to be such.*

Early surmises with regard to the occurrence of plague in animals.

132. The uncertainty that prevailed in pre-bacteriological days, as to what forms of disease should be brought under the category of plague, extended also to diseases occurring among animals concurrently with epidemics of plague among men. Murrain among cattle, and mortality among rats, are frequently recorded as having occurred in association with plague epidemics, though it is remarkable that mortality among rats has not figured so prominently in the descriptions of past epidemics as might in accordance with recent experience have been expected. Perhaps the earliest passage which has been unearthed with reference to mortality among rats, is a passage from the *Bhagavati Purana*, quoted by Mr. Hankin in a memorandum on plague, enjoining upon the people that they should abandon their houses when rats "fall from the roof above, jump about, and die." Noteworthy also in this connexion is a passage cited from the biblical writings, where it is recorded that the Philistines, with a view to staying the plague which was devastating them, made golden images of their emerods and of "the mice that marred the land," and placed them with the Ark when they sent this back into the land of Judah. In the case of India, the occurrence of mortality among rats in the Himalayan villages, in association with the outbreak of plague among men, has been recorded from 1830 onwards. The Pali plague also was, it would seem, accompanied by rat mortality.

Classification of the different types of plague in man in the light of recent clinical study and bacteriological investigation.

133. Since 1894, when the plague bacillus was discovered, it has been possible to confirm, or as the case might be to correct, the inferences arrived at in pre-bacteriological days with regard to the human and animal diseases which were previously surmised to stand in association with plague. It has now been placed beyond doubt, by accurate clinical study and bacteriological investigation, that the plague may in man assume any one of several very different clinical forms. It will conduce to the intelligibility of what will be said in the sequel with regard to the spread of plague if we here very briefly characterise these different manifestations of plague.

(1.) Bubonic type.

(1.) Plague, in its most typical variety, takes the form of bubonic plague. In this form of plague the bacteria are carried by the lymph stream into the lymphatic glands, in particular into the lymphatic glands of the groin and the axilla—less frequently into those of the neck. The bacteria multiply there, and excite a considerable amount of swelling and inflammation, which manifest themselves in the form of a bubo. The development of this bubo is often associated with severe pain. Following closely upon—more rarely preceding—the development of the bubo,

* *See* Report on a disease which prevailed in Kattyawar in 1819–20, by Thomas Whyte, Esq., Bombay Medical and Physical Society, Vol. I., 1838, p. 167; *and* Report on a disease which prevailed in the Zillah of Ahmedabad, in the years 1817–9, by J. Gilder, Bombay Medical and Physical Society, Vol. I., 1838, p. 190.

constitutional symptoms set in. They consist in malaise, headache, and fever, frequently accompanied by collapse and vomiting. When the bacteria have grown through the meshes of the lymphatic filter, they are carried on by the lymph stream into the blood. The disease then becomes septicæmic.

(2.) Septicæmic type.

(2.) Distinguishable clinically, though, from the point of view of the pathologist, not sharply marked off from the secondary plague septicæmias just described, are the cases of plague commonly spoken of as septicæmic in contradistinction to bubonic cases. These are the cases where, owing to the more rapid passage of bacteria through the lymphatic filter, and possibly to a greater production of bacterial poisons, the constitutional symptoms precede and overshadow the local symptoms, the disease being in most cases rapidly fatal.

(3.) Pneumonic type.

(3.) In a third form of plague, first bacteriologically established by Captain Childe, I.M.S., plague bacilli invade the lungs and give rise to pneumonia, death occurring in most cases within a few days after the patient has been attacked. Owing to the absence of buboes, the pneumonic form is often classed, along with the septicæmic form, in the category of non-bubonic plague.

(4.) Pestis minor or ambulans.

(4.) In addition to the three main types of plague which have been described above, an abortive form of bubonic plague comes under observation. This is technically known as pestis minor, or pestis ambulans. It cannot be doubted that in these abortive bubonic cases the bacteria are, as in the case of ordinary bubonic plague, carried to the lymphatic glands, but they are held back there, the disease stopping short of the septicæmic stage. In correspondence with this the constitutional symptoms are very light. Indeed in certain cases, not only the constitutional, but also the local, symptoms may be so slight as to be, except for their pathological interest, almost undeserving of attention. Such cases appear to be extremely common among persons who have been much exposed to the infection of plague, and are characterised by sensations of numbness and tingling, or by neuralgic pains, which in many cases are associated with the development of shotty glands in the armpits and the groins. We may, however, remark here that the whole question of pestis minor urgently requires to be more fully elucidated.

Susceptibility to plague infection of various classes of animals, and the spontaneous occurrence of epidemics among them.

134. Passing from the consideration of the various forms in which plague may manifest itself in man to the question of the occurrence of the disease in animals, it is to be noted that this question also has been considerably elucidated by recent bacteriological research. It has been established beyond doubt by inoculation experiments that rats, mice, guinea pigs, and squirrels, can readily be infected with plague. Among the most susceptible animals may be enumerated also certain species of monkeys. Rabbits are less readily infected by plague, while cats are even less readily infected. Inoculation experiments, and the negative experience which has accumulated from countless places in India, would appear to show definitely that cattle, and the larger animals generally, are insusceptible to the disease. Birds appear to be almost incapable of contracting the infection.

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Finally, we may note that it has been established, in many cases by bacteriological examinations, which are more particularly referred to hereafter, that epidemics which have occurred among animals in association with plague among men have been epidemics of plague. Such epidemics have been shown to occur in India, not only among rats, but also among mice, among squirrels, and among monkeys. In the particular case of rats, the symptoms associated with the outbreak of plague among them are very striking. The rats come out of their holes in the broad daylight, the hair of their coat stands on end, they stagger about in a dazed condition, so much so that they can be easily killed, and when the disease terminates fatally they are seen to fall over on their side, death being preceded by general paralysis, or more commonly by a series of violent tetanic spasms. In isolated cases cats also appear, so far as can be judged from the clinical symptoms, to have been found suffering from the disease.

These preliminary points having been dealt with, we may pass on to the consideration of the infective agent of plague.

III.—Bacteriology of Plague.

III.(*a*).—*Morphology of the Plague Bacillus.*

Shape and dimensions of the plague bacillus.

135. The bacillus of plague (or cocco-bacillus, as it ought in accordance with stricter nomenclature to be called) is, as seen in its typical form, a small ovoid micro-organism,

varying between 1 and 2 micro-millimetres in length and between 0·4 and 0·8 micro millimetres in breadth.

Staining reactions, and disposition of the plague bacillus as seen in material derived from the infected organism.

136. The plague bacillus can readily be stained by basic aniline dyes. It does not retain the stain when treated by Gram's process, unless when weakened (50 per cent.) spirit is employed in lieu of absolute alcohol in the decolourization process. As seen in the blood and infected tissues, the micro-organisms are for the most part found either disposed singly or aggregated together in twos, forming the so-called 'diplo-coccal forms.' Occasionally, however, and in particular in the peritoneal effusion of guinea pigs and rats, the bacteria may be aggregated together into 'strepto-coccal forms.' These strepto-coccal forms may also under exceptional circumstances be found in the buboes of plague-infected animals. Plague bacteria of most irregular shapes, resembling the involution forms presently to be described in connexion with artificial cultures, are occasionally to be met with in the infected organism, more particularly in the tissues of animals which have acquired, or which possess by nature, a certain amount of resistance to plague. Degenerated plague bacilli are found also in blood taken some time after death from men or animals that have succumbed to plague; under such circumstances the bacilli are found to have lost their sharp outlines, and to have swollen up into spherical forms which, to a large extent, have lost their property of staining with basic aniline dyes. At a later stage the micro-organisms are found to have become distinctly vacuolated, this last change being probably a preliminary to a dissolution of the bacilli in the body fluids.

Details of morphology of the bacillus as seen in stained preparations.

137. In preparations stained with any of the ordinary basic dyes, and in particular in preparations which have been treated first with a dilute solution of acetic acid and afterwards with a solution of methylene blue or carbol fuchsin, the ends of the bacillus are found to have taken the dye while the central portion generally remains unstained. The unstained portion or vacuole, instead of being centrally situated, may, in some cases, be situated either laterally or at one extremity of the bacillus. Though almost invariably to be seen in the case of plague bacilli obtained directly from the animal organism (being particularly well seen in the case of bacilli derived from buboes at a late stage of their development), the central vacuole is only exceptionally to be seen in bacilli taken directly from artificial media after having been cultivated thereon for a series of successive generations.

The bacillus furnished with a capsule in the form of a glutinous envelope.

138. In preparations which have been treated with ordinary basic dyes, the bacillus is occasionally seen surrounded by a clear unstained halo. In silver stained specimens, and, indeed, in specimens stained by many of the other methods used for demonstrating flagella, a definite, finely granular, faintly stained, envelope can without difficulty be seen round the bacillus. The appearances just described are probably to be interpreted as signifying that the bacillus is surrounded by a gelatinous covering. Whether we dignify this by the designation of a capsule or not, it at any rate appears certain that the individual bacilli in the organs and tissues of the infected organism—and the same thing holds true, as we shall see, of bacilli grown on artificial nutrient media—are embedded in and bound together by a kind of glutinous matrix.

Question as to whether the plague bacillus is motile and provided with flagella.

139. The plague bacillus does not appear to possess any power of spontaneous motility. We have not been able to detect anything of the movement which Gordon, on the one hand, and Kasenski, on the other, claim to have occasionally seen in individual bacilli derived from young cultures. Neither have we been able, either by Van Ermengen's method, or by any other method of staining, to detect the presence of flagella, described by Gordon as occurring in cultures which have been cultivated for 24 hours on agar at the temperature of 30° C. (86° F.).

Question as to the development of spores.

140. No trustworthy observer has yet detected anything in the nature of a spore formation in the plague bacillus, either within or without the organism. Moreover, old plague cultures are not more resistant to heat and antiseptics than young plague cultures. It may, therefore, we think, be assumed with confidence that the plague bacillus does not form spores. It is true that it has been asserted that a production of spores takes place in broth cultures of plague to which small quantities of phenol have been added. But the observations in question seem to us quite unconvincing. We think it probable that the the spore-like involution forms, which may, as we shall see, develop in such media, may have been confused with genuine spores.

III.(*b*).—*Conditions affecting the Development of the Plague Bacillus outside the Organism, and Specific Character of Plague in Artificial Cultures.*

Conditions affecting development of plague bacillus in artificial cultures.

141. Before passing on to describe the appearances of plague bacilli when growing on artificial media, it may be well to summarise a few general points with regard to the conditions which are respectively favourable and unfavourable to the growth and survival of the plague bacillus in artificial cultures.

Nature and reaction of nutrient medium.

The plague bacillus grows well on ordinary agar, gelatine, and bouillon. It grows less well in glycerinized media. Luxuriant growth is obtained only on neutral or slightly alkaline media.

Temperature.

Unlike most pathogenic micro-organisms, the plague bacillus appears to grow best at temperatures below blood heat. The optimum temperature is probably to be found between 30° and 36° C. (circa 86° to 97° F.). Cultures of the plague bacillus, however, develop satisfactorily at temperatures of 20° to 25° C. (68° to 77° F.). According to the Austrian Plague Commission, who have carefully investigated this point, satisfactory growth is obtained even at temperatures as high as 42° C. (circa 107° F.). At the lower end of the scale, a certain amount of growth occurs even at temperatures as low as those which ordinarily obtain in an ice-safe. In the case of two series of experiments conducted by the Austrian Plague Commission, distinct, though slow, growth was observed in cultures kept in the ice-safe at average temperatures of 11°·8 C. (53° F.) and 6·9° C. (44° · 5 F.) respectively. The same phenomenon has been observed by one of us.

Report of Austrian Plague Commission, Part II. C., pp. 625–627.

Aeration.

In artificial cultures the plague bacillus grows luxuriantly only when it is provided in abundance with oxygen. A certain amount of growth does, however, take place where the supply of oxygen is as limited as it is in the depth of gelatine or agar tubes. And even when oxygen has, by a stream of hydrogen gas, been all but completely removed from a culture medium, the growth of the plague bacillus would appear not to be entirely suspended.

Destructive and injurious influences which may come into operation on the plague bacillus outside the human organism. Section III.(4) in Chapter VI. of this Report.

142. The discussion of the destructive and injurious influences which may come into operation on the plague bacillus outside the human organism may conveniently be treated of under two headings. Under one of these headings would fall the discussion of the destructive influences exerted by chemical disinfectants, extreme heat, desiccation, and direct sunlight. We shall deal with these in a subsequent chapter where the general question of disinfection will come up for consideration. Under another heading would come naturally a discussion of the influence exerted on the plague bacillus; (*a*) by exclusive cultivation on artificial media and cultivation under special conditions; (*b*) by exposure of the cultures to extremes of cold; and (*c*) by the passage of the bacillus through a series of animals. The discussion of the influences grouped under this latter heading may appropriately be taken up here. It will be found that they have an important bearing on some of the problems in connexion with the spread of plague which we shall presently have to consider.

Length of survival on artificial media.

Report of Austrian Plague Commission, Part II. C., p. 630.

143. Unlike many other pathogenic micro-organisms, the plague bacillus retains its vitality, and, apparently, in many cases also its virulence, for considerable periods when grown on artificial media. The Austrian Plague Commission, who devoted special attention to this matter, give it as their experience that the plague bacillus generally remains alive on agar for several months. In exceptional cases it may remain alive much longer, and in one particular instance, detailed by the above mentioned observers, the bacillus was found alive on agar 470 days after the tube had been originally inoculated. All the observations in question apply to cases where the cultures were to some extent protected from desiccation by being kept at a temperature not exceeding 20° or 21° C. (68° or 70° F.), and by placing rubber caps on the tube before the culture medium had dried up. Where these precautions were not observed the plague bacillus died out in shorter periods. It, for instance, generally died out on agar when exposed for seven to eight days to a temperature of blood heat.

Effect on virulence of exclusive cultivation on artificial media.

Report of Austrian Plague Commission, Part II. C., p. 729, *seq.*

144. With regard to the maintenance of virulence in plague bacilli cultivated for prolonged periods exclusively on artificial media, the observations of the Austrian Plague Commission give us valuable information. The Commission tested the virulence of six out of a series of 27 cultures which had been cultivated exclusively on artificial media for a period of 14 months. Three of these were found to have retained their full virulence—one of the cultures which was found virulent being the culture spoken of above as having been found alive 470 days after it had been inoculated

upon agar. Of the three remaining plague cultures tested, two were found to be definitely attenuated, and the results with regard to the third culture were somewhat ambiguous.

Effect on virulence of cultivation in specially modified conditions.

Report of Austrian Plague Commission, Part II. C., p. 747, *seq.*

145. The further question as to whether an alteration in the virulence can be effected by special modifications of the method of cultivation was carefully investigated by the Austrian Plague Commission. For the purposes of this investigation the plague bacillus was grown on agar for a series of generations, on the one hand, at very low temperatures (5° to 10° C., corresponding to from 41° to 50° F.) and, on the other hand, at a temperature of 36° or 37° C. (blood heat). Further it was grown at a temperature of 20° C. (68° F.), not only on neutral agar, but also on agar which had received additions of grape sugar (up to 5 per cent.), glycerine (5 per cent.), caustic soda (2 per cent.), and hydrochloric acid (0·7 per cent.). No alteration of virulence was achieved from any of these chemical additions. The cultivation at low temperatures was also quite without effect. On the other hand, a striking influence was exerted by growing the cultures at a temperature corresponding to blood heat Here, after cultivation had proceeded for a period of 43 days in a series of sub-cultures, the virulence was notably diminished.

Effect of exposure of cultures to extreme degrees of cold.

Centralblatt für Bact., XXIV., 1898, p. 588.

Centralblatt für Bact., XXV., 1899, p. 182.

146. The plague bacillus appears effectively to resist exposure to the very extremest cold. Gladin reports that it survives exposure to a temperature of — 20° C. (— 4° F.). Similarly Kasenski reports that the plague bacillus was found alive in some of a series of cultures which were directly exposed to the cold of a Russian winter, when the thermometer registered 33° C. of frost (— 27° F.). We are told that the virulence of the bacteria in question was found to be reduced.

Effect on virulence of bacillus of passage through a series of animals.

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Report of Austrian Plague Commission, Part II. C., p. 767, Table 20.

147. The question as to whether the virulence of the plague bacillus is affected, either in the direction of an exaltation or of an attenuation, by a series of passages through animals, has an obvious importance in connexion with the spread of plague. Mr. Hankin gave it as his experience that an exaltation of the virulence of the plague bacillus could be achieved by a series of passages through mice. The reverse of this held good of rats; in the case of these, Mr. Hankin found that the bacillus became rapidly attenuated, so much so that when he attempted to make a series of passages from rat to rat, without intercalating between the passages a series of cultivations on artificial media, his plague virus died out. Our experience with regard to this confirmed that of Mr. Hankin. The Austrian Plague Commission, who have investigated so many points in connexion with the variations in the virulence of the plague bacillus, do not appear to have paid special attention to this particular point. We note, however, on going through their experiments that a series of passages through rats, which was undertaken by them, was twice interrupted by the survival of the animals, these interruptions occurring in spite of the fact that cultures on artificial media were intercalated between the successive rat passages. It is just possible that the difficulty experienced in carrying through a series of passages in the case of rats may depend upon the very considerable resistance to infection which is manifested (quite independently of previous exposure to infection) by certain individual rats. On the other hand it is possible (though this would be somewhat difficult to reconcile with the fact that severe plague epidemics have occurred among rats, independently or almost independently of concurrent plague among men) that the bacillus of plague may undergo a certain amount of attenuation during its passage through each and every rat.

Naked eye appearances of plague in artificial cultures.

148. We now pass to consider the naked eye and microscopical characters of plague as seen in artificial cultures.

Appearance of plague on gelatine plates.

In gelatine plates, at a temperature of from 20° to 25° C. (68° to 77° F.), colonies of the plague bacillus become sufficiently large to be visible to the naked eye after the expiration of from 48 to 72 hours. The gelatine is never liquefied. The colonies appear first on the surface in the form of fine transparent protuberances; these, when isolated, closely resemble drops of dew. When very numerous, they give to the plate an appearance which has been compared to that of a plate powdered over with glass dust. The surface colonies, when they are sufficiently isolated from each other to allow of their developing in an unrestrained manner, develop irregularly crenated margins. The individual colonies generally differ from each other in their rate of growth. Examined under a low power of the microscope, the colonies have a yellowish appearance in the centre where they are opaque, the border being transparent and glass-like. The colonies

in the depth of the gelatine develop later than on the surface. They form round, somewhat granular, clearly outlined, highly refracting, spherical, masses, becoming slightly darker as they grow older.

Appearance of gelatine stab-cultivations.

In gelatine stab cultures the bacillus grows uniformly along the whole track of the needle, forming a fine whitish streak which never increases very much in breadth. On the surface, however, the culture may spread, forming a superficial film over the gelatine. After some weeks, horizontal thready out-growths make their appearance in the upper half of the gelatine. In conformity with the fact that the plague bacillus grows more luxuriantly when it is freely in contact with the oxygen of the air, the outgrowths are always more numerous in the upper, than in the lower, part of the stab.

Special microscopical characters of bacilli derived from gelatine cultures.

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Bacilli, when taken from young gelatine cultures, may show bipolar staining, and are found either singly, or united in twos as diplo-coccal forms. Occasionally the micro-organisms assume a filamentous form, such filaments being for the most part unprovided with transverse septa. Sometimes, however, septa make their appearance. Certain of the colonies which develop on gelatine are said by Dr. Klein to be composed exclusively of filamentous forms. Considerable weight is laid by this observer on the development of a certain number of filamentous colonies, as an aid in discriminating plague from other micro-organisms.

Appearance of plague cultures on agar.

149. The plague bacillus grows luxuriantly on the surface of agar,—a pasty, irregular, streak, consisting of confluent colonies, developing in the course of 48 hours along the whole track of the needle. When the colonies are sufficiently separated out from each other, the margins are seen to be crenated; also, certain of the colonies are seen to develop more quickly, and to grow to a larger size, than their fellows. The cultures, when touched with the needle, are found to be distinctly sticky, it being possible to

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draw out the growth in the form of a glutinous thread. The stickiness of the plague cultures is said by some observers, in particular by Mr. Hankin, to be more marked in the case of virulent bacilli, freshly obtained from the animal organism, than in the case of bacilli which have been cultivated for prolonged periods on

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artificial media. In addition to the characters described above, emphasis has been laid, in particular by Mr. Haffkine and his fellow workers, on a dull, specular, appearance seen in colonies grown upon dry agar, when these are viewed from behind, by reflected light through the nutrient medium. The appearance in question appears to us to

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depend merely upon the collection of moisture in and under the growing colony. Mr. Hankin informed us that he had, on the one hand, found plague cultures which did not give this characteristic appearance, and that he had, on the other hand, met with cultures of other micro-organisms which, when inoculated upon agar, gave a ground glass appearance indistinguishable from that produced by typical plague cultures. For these reasons, we are not disposed to attribute very much importance to this particular character as an aid to the discrimination of cultures of plague.

Special microscopic appearance of plague bacilli derived from agar cultures.

When agar cultures are examined microscopically, they are found for the most part to consist of isolated bacteria, interspersed with a certain number of diplo-coccal forms. When recently obtained from the animal organism, the individual bacteria for the most part show very distinct bipolar staining. In cultures of bacteria which have been grown upon artificial media for a number of generations, the central vacuoles are much less marked, if not entirely absent. In the case of cultures which have been incubated for several days there are often found, interspersed with the isolated bacteria and diplo-coccal forms, the degenerated bacteria spoken of as involution forms.

Involution forms as seen on agar.

150. Certain of these involution forms, apparently derived from isolated bacteria, assume appearances which remind the observer of yeast cells or amœbæ. Angular involution forms are also met with. Filamentous involution forms, however, are those most commonly found. These are three or four times as long as the undegenerated filaments seen in ordinary cultures and often terminate in swollen club-shaped extremities. The staining properties of all these various involution forms are extremely variable. In some cases they stain deeply with basic aniline dyes; in other cases staining takes place in an extremely irregular manner. In exceptional cases involution forms may remain quite unstained, even after long exposure to the action of basic aniline dyes. Characteristic involution forms are much more commonly found in the case of cultures recently derived from the animal organism than in the case of cultures which have been grown for a number of generations upon artificial media. These last may altogether fail to give characteristic involution forms.

Exaggerated productions of involution forms on salted agar. 8717.

151. Mr. Hankin made the important observation that when the plague bacillus is cultivated on an agar which contains from $2\frac{1}{2}$ to $3\frac{1}{2}$ per cent. of common salt, involution forms make their appearance earlier and in a more exaggerated manner than on ordinary agar. On salted agar, kept at a temperature of 37° C. (98°·4 F.), they often make their appearance within 24 hours; at a lower temperature, *e.g.*, at a temperature of 30° C. (86° F.), they make their appearance only after two or more days. Differences are observed in the cultures according as more or less salt has been added to the agar. In the case of plague bacilli inoculated on a medium containing a percentage of salt greater than the optimum (and this optimum seems to vary with the particular variety of medium employed), the bacilli do not proliferate and undergo a profound alteration in their morphological structure. The culture, when examined after 24 hours, shows exaggerated involution forms, consisting, for the most part, of large spheres and irregular pear-shaped bodies. When percentages of salt somewhat below the optimum are employed, there is much less inhibition of growth, and the extreme involution forms, which have been spoken of above, are comparatively rare. The most characteristic feature of such cultures is to be found in a uniform increase in size of the bacteria, so much so that, when a culture which has been grown on the medium in question is microscopically examined side by side with a culture grown on ordinary agar, the impression produced resembles that which would be associated with the substitution of a higher for a lower power of the microscope.

Ztschft. f. Hygiene, 1900, p. 495 *et seq.*

Important confirmation of the value of Mr. Hankin's test considered as a means for discriminating the plague bacillus from all other micro-organisms is afforded in a recent paper by Teisi Matzuschita. This observer, working under Gaffky, found that none of the many varieties of bacteria which he cultivated on salted agar gave anything like the exaggerated involution forms which are obtained in the case of plague.

Appearance of bouillon cultures of plague.

152. When the plague bacillus is inoculated into bouillon, the first appearances of growth are generally observable after a period of 24 hours. In exceptional cases, the culture medium becomes evenly turbid; much more commonly, growth appears in the form of delicate floccular masses on the surface of the culture medium. These for a time adhere to the sides of the containing vessel, but they sooner or later become detached, though, if the tube is left quite undisturbed, a delicate pellicle may in some cases spread over the surface. However, on the least agitation of the tube, portions of this pellicle become detached and fall to the bottom, leaving a trail behind them as they sink.

Special microscopical appearances of plague bacilli derived from bouillon cultures.

On microscopical examination of bouillon cultures, it is not uncommon to find, in addition to the isolated bacilli and diplo-coccal forms found in cultivations on solid media, long chains of bacilli which differ from strepto-cocci only in the fact that the bacterial chains themselves are less convoluted and that the individual elements are, as seen under a high power, ovoid rather than spherical. Such chains, though they may occasionally extend right across the field of the microscope, consist for the most part of not more than half-a-dozen elements. As in the case of agar cultures, so also in bouillon cultures, bacilli, both when isolated and when forming elements in diplo-coccal and strepto-coccal forms, can by suitable methods of preparation be seen to be surrounded by a mucilaginous capsular substance. Involution forms do not make their appearance in ordinary bouillon cultures.

Stalactitic form of growth. 27.

153. Mr. Haffkine has shown that if oil is introduced into the culture medium, the plague bacillus attaches itself to the floating drops of oil, and grows downwards from these in the form of tapering stalactites. These stalactites, like the pellicle spoken of above, become detached and fall to the bottom on the least agitation of the vessel. The growth in the form of tapering stalactites is apparently conditioned, firstly, by the circumstance that the plague bacilli as they grow adhere together by their glutinous capsules; secondly, by the circumstance that the new growth is, owing to the comparatively high specific gravity of the bacteria, necessarily carried downwards; and, thirdly, by the circumstance that the bacteria grow more rapidly and more luxuriantly on the surface, where they have freer access to oxygen, than in the depth of the culture medium, where there is less oxygen. These considerations have an important bearing on the question as to whether or not the stalactitic form of growth is absolutely characteristic of, and specific to, the plague bacillus. In connexion with this question we may note that Mr. Hankin informed us that he had met with a bacillus which formed stalactites which could not, by mere inspection of the culture, be distinguished from those formed by the plague bacillus. "The stalactites 8663

obtained," said Mr. Hankin, "differed from those produced by the plague bacillus only in the fact that they were more resistant and stronger." In accordance with this, it would appear to be their length, and the readiness with which they are detached, which constitute the characteristic features of plague stalactites. Both these characteristics were seen in a beautiful manner in the cultures exhibited to us by Mr. Haffkine. Now, the development of these special characteristics must obviously depend, on the one hand, upon the strength of the glutinous matrix which holds together the masses of bacteria, and, on the other hand, upon the relation in which the specific gravity of the bacteria stands to the specific gravity of the bouillon in which they are suspended. Variation in both these respects is, of course, to be expected. Bearing directly upon
8662. this, is the circumstance that while plague bacilli which were isolated by Mr. Hankin from human beings, monkeys, and rats, at the commencement of the plague epidemic at Jawalapur gave typical stalactites, the stalactitic form of growth was no longer obtained with the plague bacilli isolated towards the end of the epidemic. The plague
8662. bacilli then obtained were found to sink rapidly to the bottom of the flask. Mr. Hankin imputed this difference in behaviour to alterations in the specific gravity of the plague bacillus. But it may have depended simply upon the employment in the second series of cultivations of a bouillon of a specific gravity less than that of the bouillon used in the first series. Or, again, as consideration will show, it may have
8669. depended upon a smaller development of the glutinous matrix (and it is interesting in this connexion to note that the cultures obtained by Mr. Hankin at the end of the Jawalapur epidemic were observed to be less sticky than the more virulent cultures obtained in Bombay and in the Hardwar epidemic). Whatever may have been the cause of the difference in the character of the cultures noticed by Mr. Hankin, his observations seem to to bring out clearly the fact that the successful development of typical plague stalactites is dependent on a somewhat delicate adjustment of the specific gravity of the medium to the specific gravity of the particular plague bacillus which is being cultivated.

III.(*c*).—*The Bacteriological Diagnosis of Plague by Microscopical Examination of Suspected Materials.*

Conditions under which a diagnosis of plague may be established by simple microscopical examination.

154. We may begin our consideration of the bacteriological methods by which the diagnosis of plague can be established by considering the question as to how far conclusions as to the presence or absence of the specific micro-organism of plague can be drawn from a simple microscopical examination of stained or unstained preparations of suspected material. In appraising the value attaching to positive and negative conclusions arrived at in this manner, we must bear in mind that plague bacilli may be present in the material examined even though they may be too few to allow of their detection by simple microscopical examination. For this reason no great weight, and indeed in some cases no weight at all, attaches to negative conclusions based on the non-discovery of the plague bacillus. In the case of a positive conclusion, based on a simple microscopical examination, the value attaching to it will be governed by the consideration that, while the morphological characters and staining reactions of the plague bacillus are, when they are developed in a typical manner, sufficiently distinctive to allow of this bacillus being discriminated from all other pathogenic bacteria found in the mammalian organism, they are not sufficiently distinctive to allow of the plague bacillus being differentiated from all the other species of bacteria which are found in external nature. Thus quite different values will attach to the result of a microscopical examination, according as the material which is examined consists of (*a*) blood or pus or exudation fluid taken directly from the organism of a man or animal showing suspicious symptoms, or (*b*) excretions derived from sick men or animals, or (*c*) material obtained elsewhere than from the animal organism.

Inferences that can be drawn from microscopical examination of lymph or pus from buboes and contents of blisters.

155. In the case of material obtained directly from a bubo, or from a blister or other lesion of the skin, the presence of micro-organisms showing the morphological characteristics of the plague bacillus will, especially if the characteristic micro-organisms are present in large numbers, amount to strong presumptive proof that the case from which the material under examination has been derived is one of plague. On the other hand, the non-discovery of bacteria possessing the morphological characters of the plague bacillus will not necessarily negative the diagnosis of plague. Especially will it not negative such a diagnosis in cases where the material has been obtained by the puncture of a bubo in the very early stages of its development. In such circumstances the actual gland may have been missed, and the surrounding tissues may not yet have been invaded by plague bacilli. Again, in

the later stages in the development of the bubo—and the same applies also to the case of the blister—contaminating micro-organisms may have effected an entrance and may have proliferated to such an extent as to supplant the plague bacillus. Cases of this sort will, however, be exceptional, and in most cases a positive or negative diagnosis of plague can be based with practical certainty upon the discovery or non-discovery of characteristic forms of micro-organisms by a simple microscopical observation of the material obtained by puncturing the inflamed lymphatic gland.

Inferences that can be drawn from microscopical examination of blood.

156. In the case of the blood derived from a patient who is suspected of suffering from plague, the detection of bacteria possessing the morphological characters of the plague bacillus will (especially if these are present in large numbers, and when it is determined that these become decolourized by Gram's procees) be conclusive evidence that the patient is suffering from plague. The mere finding of a few isolated bacteria arranged together as diplo-coccal forms cannot, especially when Gram's test has not been applied, be accepted as establishing the diagnosis of plague. We have in view here in particular certain suspected cases which occurred in Calcutta in 1896, in which it was claimed by Professor Simpson that the diagnosis of plague was confirmed by his bacteriological examinations. We would note with regard to these, first, that in some of the suspected patients only a few isolated diplo-coccal forms were found after long searching through a number of films prepared from the blood. Again, in view of some of the figures reproduced in the record of Dr. Simpson's evidence, particularly figures I., VI., C. and F., it is important to note that it does not seem to have been determined whether the bacteria became decolourized when treated by Gram's process, and, lastly, the fact must not be lost sight of that saprophytic diplo-cocci of various kinds are widely distributed in nature, and that there is always a possibility of microscopical preparations containing as contaminations a small number of such diplo-coccal forms. Whilst the finding in blood of bacteria showing typical morphological and staining characters (always supposing a sufficiency of such bacteria have been found) will be conclusive as to the presence of plague, the non-discovery of such bacteria will not in any way rebut a diagnosis of plague based upon clinical symptoms. This will be manifest when it is considered that, unless in a period immediately antecedent to death, plague bacilli will rarely be present in large numbers in the blood.

III. 369–76.
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Inferences that can be drawn from simple microscopical observation of the excretions.

157. In dealing with the question of the microscopical examination of the excretions, we may confine our attention to the sputum, which is the only excretory material in which plague bacilli are likely to be found in sufficient numbers to allow of their detection by microscopical observation. In the case of typical primary plague pneumonia, there will rarely be any difficulty in establishing the diagnosis by the microscopical examination of the sputum; the presence in large quantities of isolated bacteria or diplo-coccal forms showing a central vacuole is in most cases quite sufficient to place the diagnosis beyond question. A certain amount of difficulty may, however, arise on the one hand in cases where the plague bacilli have undergone degenerative changes, and on the other hand in cases where the sputum contains many pneumo-cocci. In this last case it will be difficult to arrive at a certainty that the preparations contain, in addition to the pneumo-cocci which retain the stain when treated by Gram's process, also the diplo-cocci of plague which become decolourized.

Inferences that can be drawn from microscopic examination of material grossly contaminated with saprophytic bacteria.

158. In dealing with material derived elsewhere than from the human or animal organism, it will be impossible to arrive at a diagnosis of plague by a mere microscopical examination. It is true that Lieut.-Colonel Lawrie, I.M.S., and Mr. Stevens, the Deputy Plague Commissioner for the Hyderabad State, have claimed that the plague bacillus can be readily detected in the floor materials of infected houses by the aid of the microscope alone. Indeed, Lieut.-Col. Lawrie and Mr. Stevens went so far as to assert "that the infection of plague could be tracked about by means of the microscope with as much certainty as if it were visible to the naked eye," and that the infectivity or otherwise of a house could by these means be determined. It was contended that the results of simple microscopical observation were in some cases confirmed in the Hyderabad State Laboratory by subsequent cultivation and by inoculation tests. Doubt, however, is cast on the value of the confirmation in question by a study of the technique employed, and by a consideration of the negative results which have, as will be seen below, invariably been obtained by other observers who have endeavoured to detect the plague bacillus in soil. Even if in some particular cases the bacteria isolated in the Hyderabad State Laboratory really were plague bacteria, the method of simple microscopical examination would, none the less, be

I. 447.
I. 189–94.
I. 202–11.
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quite fallacious as a method for general use, seeing that, as has already been incidentally mentioned, there are to be found in ordinary earth many different diplococcal forms, some varieties of which show a central vacuole. We have found such forms to be numerous in earth obtained from places in India which were, so far as was known, free from plague infection. We have found them also in street dust and in mud collected from the sea-shore in Egypt.

III.(*d*).—*The Bacteriological Diagnosis of Plague by Cultivation Methods.*

Diagnosis of plague by means of cultivations ; advantages and disadvantages of the method generally.

159. The above ends our consideration of the microscopical diagnosis of plague. The employment of cultivation methods as aids to the diagnosis of plague comes under consideration, firstly, in connexion with the detection of the plague bacillus where it is present in material uncontaminated by other bacteria, and, secondly, in connexion with the isolation of the plague bacillus when it is intermixed with contaminating micro-organisms. About the first of these points, it will not be necessary to say more than that, where microscopical examination is not conclusive in the case of a material which contains only very few plague bacilli, we can, by inoculating on to a nutrient medium, cause the contained bacilli to develop into colonies which shall be macroscopically visible, with the result that their detection becomes a perfectly simple matter. It might be supposed that we should gain a similar advantage by inoculation on to a nutrient medium in cases where the material under examination contained plague bacilli in admixture with contaminating micro-organisms. Generally, however, it is the very reverse of this which obtains. For many varieties of contaminating micro-organisms exert on the plague bacillus an influence in restraint of growth. This influence, which operates in such a manner as to prevent, or almost prevent, the development of plague colonies, makes itself felt even when, as in plate cultivations, the contaminating bacteria have been comparatively widely spaced out. Among the pathogenic micro-organisms found in the human organism, the strepto-coccus pyogenes, the pneumo-coccus of Frænkel, and no doubt others, exert this influence on the plague bacillus, colonies of plague rarely making their appearance in plates where there is an admixture of these micro-organisms. A similar inhibitory action on the growth of the plague bacillus is exerted also by many of the non-pathogenic micro-organisms which are distributed universally in nature. The bacillus subtilis, the bacillus mesentericus, the micro-coccus prodigiosus,—all of which are practically ubiquitous—exert such an inhibitory influence. An even more marked inhibitory influence is exerted by the bacillus coli, which is constantly found both in material derived from the human organism and in material elsewhere derived. Much remains to be elicited with regard to the nature of this inhibitory influence. In the case of the bacillus coli the German Plague Commission, in view of the result of their experiments, are inclined to attribute the influence exerted by this bacillus to the circumstance this it grows faster than the plague bacillus, and uses up all the nutrient material in the culture medium. The Austrian Plague Commission also adopt this view. The retarding influence of the bacillus coli would, however, appear to be at least in part due to the fact that some of the products of the metabolism of that bacillus (possibly the acid products) are injurious to the plague bacillus. Contrary to what was found by the Austrian Plague Commission, we found that the growth of the plague bacillus was markedly retarded when it was inoculated on to agar which had been flushed with a filtrate obtained from a bouillon culture of the bacillus coli. Whatever be the explanation of the inhibitory influence which is exerted by this and so many other micro-organisms on the growth of the plague bacillus, it is, at any rate, certain that we have here a very formidable obstacle to the isolation of the plague bacillus from material containing a large admixture of other micro-organisms.

Report of German Plague Commission, p. 269.

Report of Austrian Plague Commission, Part II. C., 639–40 26,150

Advantages and fallacies of cultivation methods as applied to the examination of— (*a*) lymph or pus from blisters or pustules;

160. Passing now from the consideration of general principles to the consideration of the conclusions which can be drawn from the positive and negative results of cultivations made from suspected materials we may deal first with cultivations instituted with material derived from phlyctenules or other skin lesions. While cultivation methods have here an advantage over methods of simple microscopical examination in the fact that the plague bacteria may be increased in number, and while they have a further incidental advantage in the fact that the rate of growth and the appearance of the cultivation supply valuable data for judging of the nature of the micro-organisms, it must be borne in mind that contaminating micro-organisms, supposing such to be present, may completely inhibit the growth of the plague bacilli.

In conformity with these considerations, a positive result obtained by the employment of cultivation methods will be more conclusive than a positive result obtained by simple microscopical examination; on the contrary, a negative result, where contaminating micro-organisms make their appearance in the culture, will, considered as evidence of the absence of the plague bacillus, be even less conclusive than a negative result obtained by simple microscopical examination.

(*b*) material derived from buboes

Exactly the same statement holds true with regard to material derived from buboes. In other words, while the advantages which attach to the employment of cultivation methods will be conspicuous in cases where contaminations are not present, fallacious conclusions may be drawn in cases where a growth consisting only of contaminating micro-organisms is obtained.

(*c*) blood;

In the case of the blood the advantage of the cultivation method over the method of simple microscopical examination comes out most clearly, a positive result being frequently obtained in cases where the plague bacteria would, owing to their small numbers, have escaped detection on simple microscopical examination. The utility of this method of diagnosis is, however, limited by the consideration that positive results can be obtained only after that stage of the disease has been reached at which the bacteria invade the blood stream. Even then success will in many cases be secured only when the quantities of blood employed for the cultivation are, relatively speaking, considerable. The chances of securing colonies of plague bacilli in the culture are manifestly much better when 5 or 10 c.c. of blood are employed than when merely one or two drops of blood from a prick in the finger are inoculated on to the nutrient medium. Even when sufficient blood has been taken for cultivation purposes from a plague case which has reached the septicæmic stage, fallacious negative results may be obtained, if contaminating bacteria, such as the pneumo-coccus and streptococcus, are present in the blood and inhibit the growth of the plague bacillus. Another point which deserves mention is that the number of plague colonies which develop in the culture are often much fewer than the number of plague bacteria seen on microscopical examination of the blood.

(*d*) excreta and material only indirectly derived from the animal organism.

In the application of methods of cultivation to the detection of the plague bacillus in excreta and material derived elsewhere than from the animal organism, it is to be borne in mind that, owing to the large number of contaminating micro-organisms which have to be dealt with, the difficulties of isolating the plague bacillus are enormous. The magnitude of the difficulties which have to be confronted will appear clearly when we consider the conditions under which the plague bacillus has to be sought for in grossly contaminated substances. Taking, for instance, the case where the plague bacillus is sought for in material derived from the cowdung and earth floors of native houses, we may take note of the fact, brought out in Dr. Marsh's experiments, that one gramme of floor material taken from inhabited houses contains, 18,805.
on the average, something like three millions, and, in exceptional cases, up to eight millions, of saprophytic micro-organisms. The isolation of an individual plague bacillus from a gramme's weight of such material would thus involve (1) the mechanical separation of this bacillus from, say, three millions of contaminating bacteria, *i.e.*, the spacing out of the plague bacillus to such a distance from the contaminating bacteria as would take it beyond the sphere of influence of those which would inhibit its growth; (2) the scrutiny of each of the three millions of colonies; and (3) the microscopical examination and further testing of all such colonies as might present any of the characteristics of plague colonies. It will be obvious that the ordinary methods of isolation by means of plate-cultivation would here prove quite impracticable. In spite of these considerations, we did examine, by 27,137.
the method of plate-cultivation, seven specimens of earth taken from plague-infected houses. From these specimens we isolated four different kinds of bacilli, whose microscopical appearance resembled that of plague, but each of these, when further investigated, presented points of difference from the plague bacillus.

Experiments made to test a method of isolation proposed by Mr. Hankin. Vol. II., p. 8. 8,594.

161. Finding the discovery of the plague bacillus in earth by ordinary methods impracticable, we addressed ourselves to the task of testing a method of isolation which had been suggested by Mr. Hankin, and for which was claimed by Dr. Shrinavasa Rau that it enabled the plague bacillus to be isolated from fæces and other grossly contaminated material. The method in question consists in inoculating the material to be tested into a series of tubes of bouillon, which have received graduated additions

of 1 to 8 drops of a reducing mixture, consisting of ferrous sulphate, 10 parts; tartaric acid, 10 parts; citric acid, 1 part; water, 100 parts; the whole made alkaline with ammonia. Before instituting experiments to determine whether the plague bacillus could be recovered by this method from suspected earth, or from earth which had been artificially contaminated with plague, we made preliminary experiments to determine whether the plague bacillus, when added in pure culture to bouillon tubes containing the reducing fluid in the quantities prescribed by Mr. Hankin, could afterwards be recovered by cultivation. We were uniformly unsuccessful in our attempts to recover the plague bacillus from tubes of bouillon which had received the prescribed additions of Mr. Hankin's re-agent. In view, however, of the fact that Mr. Hankin had succeeded in this, we undertook a further series of experiments to see whether the plague bacillus could be isolated by this method from the floors of infected houses. Ten specimens of material from earth and cowdung floors, and one specimen of dust from a cement floor, all taken from rooms from which plague cases had just been removed, were examined by mixing the material into sterile broth and inoculating from this mixture into a series of tubes of bouillon containing from 1 to 5 drops of Mr. Hankin's solution. From four of the 11 samples of floor material examined, bacilli possessing some of the morphological characters of the plague bacillus were recovered, but the bacteria recovered, when tested by inoculation into animals, proved to be altogether devoid of pathogenic power. Interpreting this result in the light of our failure to recover the plague bacillus when added in pure cultivation to Mr. Hankin's medium, and bearing in mind the wide distribution of vacuolated diplo-coccal forms in earth, it seems to us that the isolation method which is here in question is one upon which no reliance can be placed.

26,129.

26,139.

Attempt to isolate the plague bacillus by cultivation in an atmosphere of carbonic acid gas.

162. In view of our want of success with this method, we determined to try the effect of cultivating a mixture of plague bacilli and contaminating micro-organisms in an atmosphere of carbonic acid gas. We were induced to do this by the consideration that Dr. Marsh had in certain experiments found the plague bacillus to develop in the form of luxuriant strepto-coccal chains in cultures which had been buried in the soil, and also in cultures which were grown in an atmosphere charged with carbonic acid. The time at our disposal did not allow of our testing this method in any adequate way, but we think it noteworthy that we succeeded in recovering the plague bacillus by this method in two out of three specimens of unsterilised garden soil which had been artificially infected with a culture of the plague bacillus. The method employed was to introduce a small quantity of the infected unsterilised garden earth into a tube of sterile broth; the tube was then drawn out in the flame, and a stream of carbonic acid was passed through; this done, the tube was sealed; it was then incubated for from four to seven days. On making zigzag strokes on agar from tubes treated in this manner, in the two instances referred to colonies of plague bacteria were obtained. These, when inoculated into animals, caused death with typical symptoms, the plague bacillus being recovered from the blood. In another series of experiments in which some infected earth was inoculated into six broth tubes in the manner indicated, certain of the colonies obtained consisted of a bacillus which morphologically resembled plague. The bacillus in question, however, had no pathogenic properties, producing no symptoms even when inoculated in considerable doses in the peritoneal cavity of mice. It could, therefore, not be identified with the plague bacillus. Again, in another series of experiments, when seven specimens of earth derived from plague-infected houses were examined by the method described, from two specimens bacilli morphologically resembling the plague bacillus were obtained. Owing to our departure from Bombay, the nature of these bacilli was not further investigated.

26,144–5.

26,434*b*.

26,141.

General conclusions as to methods of cultivation employed for detection of plague bacillus in contaminated material.

163. It will be seen from the above that there is as yet no cultural method which can be depended upon to reveal the presence of plague bacilli when mixed with large numbers of contaminating micro-organisms. Where, therefore, negative results as to the presence of plague are obtained from the examination of fæcal, or other grossly contaminated, material, it will not be legitimate, from these negative results, to infer that the plague bacillus is really absent from the material examined.

Special supplementary methods of cultivation applicable for the

164. We have to deal now with the employment of certain special supplementary methods of cultivation for the further identification of such cultures as may have been obtained in the preliminary cultivations, which we have up to this time been considering.

identification of the plague bacillus after it has been obtained in pure culture.

To verify the nature of the culture obtained, inoculations may be made into ordinary agar, gelatine, and bouillon, with a view of seeing whether the growth develops the features described above as associated with the growth of the plague bacillus in these several media. There is, as we have seen, nothing absolutely characteristic in these features, although valuable indications may be obtained from the rate of growth, the irregularity of the contour of the isolated colonies, the varying size of these, and the stickiness of the culture. The floccular growth in broth, and, in microscopic preparations, the bipolar staining, the isolated or diplo-coccal form of growth, the size of the bacillus, and, further, the presence of certain thread-like and other involution forms, constitute, when taken together, a sufficiency of characters to establish the diagnosis with all but absolute certainty. In addition to planting out the culture on ordinary media, it may with advantage be planted out also on salt agar and on bouillon to which an addition of oil has been made with a view to obtaining the characteristic involution and stalactitic forms described above. For the purpose of rapid diagnosis the former of these two characters is, however, the more valuable, the stalactitic growth requiring an interval of at least a certain number of days for its characteristic development.

III.(*e*).—*The Bacteriological Diagnosis of Plague by the Aid of Inoculation Experiments.*

On the inoculation of animals considered as an aid to the discovery of the plague bacillus.

165. The inoculation of highly susceptible animals is a method which may be resorted to, on the one hand, with a view to isolating the plague bacillus from a mixture of contaminating micro-organisms, and, on the other hand, with a view to determining whether a micro-organism which has been isolated by cultivation methods from suspected material is, or is not, a plague bacillus.

Subcutaneous inoculation considered as a method for the isolation of the plague bacillus from material grossly contaminated with saprophytic bacteria.

166. We may conveniently deal, first, with the employment of inoculation for the former purpose. In view of the difficulties which are, as we have seen, in the present state of our knowledge, associated with all attempts to isolate the plague bacillus by cultivation methods from material grossly contaminated with saprophytic bacteria, many observers have sought to achieve the isolation of the bacillus by inoculating the suspected material into the subcutaneous tissue of highly susceptible animals, such as rats, mice, and guinea pigs. This method has been employed by Mr. Hankin in an extensive series of investigations instituted with a view to collecting information with regard to the distribution of the plague bacillus in external nature. 8588–9.
After a wide experience, Mr. Hankin is impressed by the great uncertainty of the results obtained by subcutaneous inoculation. He found that, where cultivation methods have failed to disclose plague, the method of inoculation will occasionally give positive results; on the other hand he found that the method of inoculation may 8522–4.
give negative results, even when plague bacilli are present in quantities which would 8586.
be sufficient to kill the animal if separately inoculated.

Employment of the method by Mr. Hankin with the view to the isolation of the plague bacillus from materials derived from an infected house.

167. A detailed description like the following, which we have extracted from an official report by Mr. Hankin, of an actual experiment on material obtained from a plague-infected room, brings out the untrustworthiness of the method of subcutaneous inoculation in a very striking manner. The material in question was obtained from a house in Cross Lane, Currey Road, Bombay. "Professor Muller," says Mr. Hankin, "found this house while carrying out his plague inspections. He kindly telephoned for me, and I arrived about an hour after he had left. On opening the door of the single room, about ten feet square, that was inhabited by the family, he found three persons, the mother and two daughters, lying on the floor suffering from plague. The mother and one daughter were at once taken away to hospital. The other daughter, a girl of about eight years old, died while they were removing her, and the body was left in the corner of the room. The father, Dattu Koli, a mill-hand, had died on the preceding day. Another child had died four days earlier. Thus the whole family had been carried off. On entering the ill-ventilated room I found it in a great state of disorder. Clothes, food, and filth of all sorts covered the floor. In one corner was the cooking place and numerous brass vessels. Another corner was occupied by the nanee or drain, in which were some human dejecta. Another corner contained an altar with a brass image of Ganesha, the God of Wisdom. The body of the little girl was in the other corner. The room was pervaded by an overpowering smell that produced

nausea. I spent about an hour collecting specimens in the room, and also helped the Sanitary Inspector to burn the clothes and property. I arrived at my laboratory within an hour of leaving the place, and immediately proceeded to inject extracts of the specimens I had collected into mice. The specimens of earth of the mud and cowdung-floor were all taken from the surface and at places in which traces of dejecta were visible.

"The following is a list of injections made into mice :—

Number of Mouse.	Substance Injected.	Result.
1	Earth from floor	Death after 24 hours.
2	„ „	„ 96 „
3	Another specimen of earth	„ 24 „
4	„ „	„ 24 „
5	„ „	Survived.
6	„ „	„
7	„ „	Death after 48 hours.
8	„ „	Survived.
9	„ „	Death after 48 hours
10	„ „	„ 8 days.
11	Stain of dried saliva found on a stool	Survived.
12	„ „ „	Death after 18 days.
13	Water in brass lota on floor	„ 72 hours.
14	„ „	„ 5 days.
15	„ in large degchie	Survived.
16	„ „	„
17	Dejecta from corner of drain	Death after 24 hours.
18	„ „	„ „
19	Millet found between grindstones	„ „
20	„ „	Survived.
21	Stain on clothing (? vomit)	Death after 25 days.
22	„ „	Survived.
23	Drain outside house	Death in 48 hours.
24	„ „	„ after 5 days.
25	Dust from bamboo lamp-frame over shrine	Survived.
26	„ „ „	„
27	Stain of saliva mixed with betel on edge of drain	Death in 24 hours.
28	„ „ „	Survived.
29	Dust from a shelf	„
30	„ „	Death in 24 hours.
31	Canvas sacking used as bedding	„ „
32	„ „	„ after 12 days.
33	Salt found in an earthen pot	Survived.
34	„ „	„
35	Partly eaten pudding made of rice on floor by corpse (*sic*)	„
36	„ „ „	„
37	Washings of three cigarettes found on the floor	„
38	„ „ „	„

"Of the 20 mice that died mentioned in the above table, 17 were subjected to further examination by inoculation into a second series of mice. In each case the spleen was ground up in bouillon, and two or three drops inoculated into mice. Of the 17 mice of this second series, only four died. From each of these four mice other mice were inoculated, some with quantities of the spleen, others with only traces. Altogether in this experiment 94 mice were inoculated. Numerous agaragar cultures were inoculated from the spleens and other organs of every mouse that died.

"The plague microbe was not detected in a single case, although there is every probability that the room was badly infected. The microscopic appearances in mice, Nos. 10 and 31 were somewhat suspicious of plague, but no plague microbes could be isolated. With regard to mouse No. 24, that had been inoculated from dirt in the drain outside the house, passages were made from its spleen through three mice. From the last of these mice a microbe was isolated that gave involution forms somewhat similar to those of plague, but it grew on agaragar more vigorously than plague, and did not respond to Pfeiffer's serum test."

168. We find it difficult, in view of facts like these, and other similar facts, with regard to both mice and rats, placed on record by Mr. Hankin, to adopt Mr. Haffkine's view that the subcutaneous inoculation of contaminated plague-infected material into rats is "an exceedingly sensitive method" for the purpose of isolating the plague bacillus from infected earth. Indeed, the consistently negative results of the soil-examinations which were instituted by Dr. Gibson in Mr. Haffkine's laboratory would seem of themselves to afford proof of the untrustworthiness of the method of subcutaneous inoculation, considered as a method for the isolation of the plague bacillus from a mixture of contaminating micro-organisms.

Conclusions arrived at with regard to the method of subcutaneous inoculation considered as a method for the isolation of the plague bacillus.

153.

619–20.

169. With a view to avoiding the fallacies inseparable from inoculations which involve, as those which we have been considering do, the introduction into the subcutaneous tissue of large numbers of contaminating bacteria, many of which may have pathogenic properties, various other methods of inoculation which are less open to objection on this score have been resorted to. The German Plague Commission resorted in many instances to inoculation on to the conjunctiva. Yersin, Roux, and others, have proposed inoculation on to the mucous membrane of the nose. The Austrian Plague Commission speak very favourably of inoculation on to a shaved and scarified skin-surface. These various methods of inoculation will come up for consideration again in connexion with the discussion of the channels by which plague bacilli may obtain access to the body. In the present connexion it will suffice to note that, except in the case of inoculation on to a scarified skin surface, which appears in the hands of the Austrian Plague Commission to have been very successful, very little experience is yet to hand as to the value of the methods in question, considered as methods for isolating the plague bacillus from a mixture of contaminating micro-organisms.

Consideration of other methods of inoculation designed to achieve the isolation of the plague bacillus.

Report of German Plague Commission, p. 284.

Report of Austrian Plague Commission, Part II. C., *passim*.

170. The question of the employment of inoculation into animals as a means of confirming a diagnosis of plague arrived at by microscopical examination, or by this last in combination with cultivation methods, can be disposed of in a few words. The following points would appear deserving of special note. The death of the inoculated animal is not, unless in cases where plague bacilli are found in its blood or spleen, to be regarded as confirming a diagnosis of plague. Mice and guinea pigs would appear to be the most suitable animals to employ for inoculation experiments. Rats, as already indicated above, would appear to be less suitable, inasmuch as they, in certain instances at least, show a considerable amount of resistance to plague.

Employment of inoculation for the purpose of confirming a diagnosis arrived a by other methods.

III.(*f*).—*The Serum Diagnosis of Plague.*

171. When once Widal had shown that typhoid fever could be discriminated from other specific fevers by the method of serum-diagnosis, which Gruber and Durham had previously exploited in the case of inoculated animals, the question of the possibility of employing the same method of diagnosis in the case of plague immediately presented itself.

Question as to the application of the method of serum-diagnosis to the discrimination of plague from other diseases.

The German Plague Commission, in particular, undertook the investigation of this question. From the investigations undertaken by them it appears to result that agglutinating substances are in point of fact produced in the system of animals and men who have suffered from plague, and also of animals who have been repeatedly inoculated with dead plague cultures. On the question as to whether the presence of these agglutinating substances in the blood is of assistance in the diagnosis of doubtful cases, the German Plague Commission express themselves somewhat guardedly. They would seem to recognise that the practical utility of this method of diagnosis is considerably limited, firstly, by the fact that definitely negative results are not infrequently obtained in the case of persons convalescing from plague; and secondly, by the fact that the bacteria in a plague culture are, even in the absence of a specifically active serum, so massed together into clumps as to make it a matter of difficulty to determine by microscopical examination whether agglutination has, or has not, been produced by the addition of serum. With a view to overcoming this last difficulty the German Plague Commission recommend that the sedimentation, as distinguished from the agglutination, of the bacteria under the influence of the serum should be taken as the criterion of a positive reaction.

Investigation of the question by the German Plague Commission.

Report of German Plague Commission, p. 321, *seq.*

In connexion with the employment of the method as an aid in the differentiation of plague bacteria from other bacteria, we note that Mr. Hankin appears in two or

Employment of the method by Mr. Hankin and

Captain Leumann respectively. 8587; 8672. 3196, *seq.*

three instances to have had recourse to the serum-test for this purpose. We are not informed as to whether the results he obtained were unambiguous.

The statements of Captain Leumann, I.M.S., with regard to the presence of agglutinins in the blood of persons inoculated twice, and the absence of these substances in the blood of persons inoculated once, with Haffkine's prophylactic, must, we think, be discounted in view of serious defects in the technique employed by the witness in the experiments on which he based his conclusions.

Difficulties and fallacies of serum-diagnosis a applied to plague.

We think that the difficulties and fallacies of the serum-diagnosis of plague, though they are recognised by the German Plague Commission, have not been sufficiently emphasized by them. The serum-diagnosis of plague is rendered uncertain, not only by the fact that plague bacilli are normally agglomerated together into masses by the intermediary of their glutinous capsules, but also by the fact that these agglomerated masses, when they become detached, normally sink to the bottom of the containing vessel. It is thus, unless in cases of specially rapid sedimentation, practically impossible to be sure that a specific action is being exerted.

Our personal experience of the method.

Our personal experiences in the matter of the practical exploitation of the serum-diagnosis of plague were extremely disappointing. We were quite unable to satisfy ourselves of the presence of specific agglutinating and sedimenting powers in the blood, either of plague convalescents, or of persons who had been inoculated with Haffkine's plague vaccine. More than this, we were unable to satisfy ourselves as to the presence of any agglutinins in either of the two consignments of antiplague sera which were kindly furnished by the authorities of the Pasteur Institute in Paris. In conformity with this we are of opinion that no practical value attaches to the method of serum-diagnosis in the case of plague.

IV.—Mode in which the Plague Bacillus effects an Entrance into the Human and Animal Organism.

IV.(1.).—*Mode in which the Plague Bacillus effects an Entrance into the Human Organism in the Bubonic and Septicæmic Forms of Plague*

Mode in which the plague bacillus effects an entrance into the human and animal organism.

172. We may commence our consideration of the question as to how the infective material of plague makes its way into the organism by dealing with the human organism. We may consider, in the first place, how the bacillus of plague makes its way into the system in cases of bubonic and septicæmic plague.

Proof of entrance of infective material through breach of skin surface afforded by cases of plague resulting from accidental introduction of virus at post-mortems into wounds. Para. 191 of this Report.

Conclusive evidence of the possibility of the infective material entering by a breach of the surface of the skin is afforded by the fact that the disease has in many cases been contracted by the direct inoculation of the virus in the course of post-mortem examinations on plague patients. We have collected particulars of no less than 13 cases (11 of these being cases occurring in India) in which the disease was contracted in this manner. A brief summary of the history of each of these cases will be found in a subsequent section of this Chapter. As is shown there, in some of the cases in question infection took place through abrasions of the finger, or through partially healed scars; in other instances, again, infective material was inoculated by a prick with the point of a knife, or by a scratch inflicted by a projecting spicule of bone. In some cases the scratch in question was, it may be remarked, so slight as to pass quite unnoticed at the time at which it was inflicted. The cases in question afford conclusive proof that the infection of plague is very readily contracted when the virus comes in contact either with a broken surface of skin or when it is subcutaneously inoculated.

The occurrence of local reactions noticed in only two of the above cases.

173. It is important, in connexion with the question of the occurrence of a local reaction after the inoculation of plague virus into the skin, to note that in 11 of the 13 cases referred to, most of which came directly under the observation of medical men, it is either specifically stated that there was no reaction at the site of inoculation, or the record is silent on the matter. In the two remaining cases a small phlyctenule developed at the site of inoculation. The details with regard to these two cases in which phlyctenules developed are important.

Case of Dr. Sticker. Snow's Report on Plague in Bombay, 1896–7, p. 127.

In the first case, that of Dr. Sticker, a member of the German Plague Commission, the infective material appears to have obtained access to the system by an unnoticed scratch inflicted on the hand when conducting a post-mortem examination. The first definite indication of infection was a certain amount of general malaise. Following upon this a small vesicle developed in the hand. A burning and pricking sensation made itself felt in the region of the vesicle. Shortly afterwards definite lymphangitis, associated with lancinating pains in the arm, set in. Plague bacilli were detected in the contents of the vesicle.

In the second case, that of Captain Leumann, I.M.S., the infective material appears to have effected an entrance through a scratch contracted in the course of a post-mortem made on a plague patient. The first symptom of the disease was a kind of mental aberration; the next was a painful swelling of the glands of the axilla; subsequently to this a bleb developed at the site of the scratch. The contents of the bleb were examined for plague bacilli with negative results.

Case of Captain Leumann, I.M.S. 4542, *seq.*

174. Captain Childe, I.M.S., communicated to us the four following interesting cases:—

Details of cases in which similar local reactions occurred, apart from a history of local introduction of infective material. I. 374.

"B., Hindu man, age 40, came to hospital on 5th October, 1896. Three days before he had been seized with fever, and shortly afterwards found pain and swelling in the left axilla. On admission he looked very ill, temperature 103, and there was a large bubo in the left axilla. On the outer side of the left forearm near the wrist there was a small papule the size of a pea, with hair growing through it; it was slightly red at the base, contained a little serum at the apex and was partly covered by a scab. It was not painful, and the patient knew nothing about it; there were no inflamed lymphatics leading from it. Cultivations were made from the serum squeezed from the papule and a typical growth of plague was obtained. Part of the culture was injected into a rat, and the rat died of plague after 60 hours. On the same date, 5th October, cultures were made from the finger blood, but all remained sterile. This man died on 8th October.

"N. S., Hindu man, aged 28, came to hospital December 1st, 1896. There was a large bubo in the vertical set of the right inguinal glands. He said he had been ill three days. There were many old cracks and fissures about the feet and toes, the skin being thick and horny, and some scratch marks on both legs. On the outer surface of the right calf there was a slightly raised œdematous portion of skin about one inch long, and on it there was a small bleb, the size of a large pin's head. From the bleb some thin serum was squeezed out. Cultures were made from the fluid in the bleb and from the finger blood. A pure growth of the plague bacillus was ultimately obtained from the bleb, whereas the cultures of the blood remained sterile. The patient could give no history of the œdematous patch on the leg, and there was no pain in it

"N., Hindu man, aged 30 years, was admitted on March 3rd, 1897, with a bubo at the back part of the right axilla. He had been ill two days. On the back, near the angle of the right scapula, there was a papule, the size of a pea, on a slightly reddened base, and partly scabbed over; it looked like a pimple which had been scratched. Cultures were made from the serum squeezed from this papule and from the finger blood; pure growth of the plague bacillus was ultimately obtained from the papule, but the cultures of the finger-blood remained sterile.

"P., Hindu boy, aged 16, admitted on October 26th, 1896. On either side several of the oblique inguinal glands were enlarged to the size of walnuts, and painful. There was a small unopened papule on the glans penis exactly in the dorsal middle line; its apex contained a little pus and serum. It did not look at all like a chancre, and the boy said he knew nothing about it. Cultures were made from the papule and from the finger blood. From the former a pure growth of plague was ultimately obtained. This was injected into a rat, and the rat died of plague. Cultures of the finger-blood remained sterile. This patient recovered."

The four cases just cited, even if they stood alone, would suffice to establish that infection may, under natural conditions, be contracted by the channel of the skin, and this, so far as can be discovered, independently of the existence of any previous wound or breach of continuity of the skin. Vesicles such as those which came under observation in these cases are not very uncommon in cases of bubonic plague. Dr. Simond gives a list of 61 cases of plague in which vesicles were observed at the presumptive site of inoculation. In each of these cases the plague bacillus is said to have been found in the contents of the vesicle. The German Plague Commission give particulars of 11 cases where primary vesicles were observed. Plague bacilli were detected in only three of these cases.

Annales de l'Institut Pasteur, October 1898.

Report of German Plague Commission, p. 265.

175. Coming now to the question as to whether any estimate can be formed of the frequency of infection from the skin, it would, at first sight, seem that inferences of value might possibly be arrived at by inquiring in what percentage of cases a local reaction, either in the form of a phlyctenule, or in the form of a pustule or carbuncle originating in a phlyctenule, has been recorded at the site of inoculation. Consideration will, however, show that an estimate based on data of this nature must necessarily be fallacious. In the first place, as the histories of the cases infected while making post-mortem examinations indicate, phlyctenules develop at the site of inoculation only in

No inference as to frequency of infection through skin surface can be deduced from hospital statistics with regard to occurrence of local inflammation of skin.

very small percentage of cases. In the second place, as will appear from the four cases cited above from Captain Childe, the local reaction at the site of inoculation may, when present, be so insignificant as to be very readily overlooked. In the third place, reflection will show that the development of a phlyctenule (that is, the development of a serous effusion between the upper and lower layers of the epidermis) could only be expected in cases where the virus is deposited in that particular situation. Now this can manifestly occur only in a very small minority of cases. Ordinarily the virus will be inoculated directly into the subcutaneous tissue. In such a case no phlyctenule will be developed, for such serous effusion as takes place will take place below the epidermis.

Frequency with which buboes develop in connexion with lymphatics from skin surfaces establishes frequency of infection through skin.

App. I. to this Report.

If we want to learn in what percentage of cases infection has been contracted through the skin, we must inquire what percentage of primary buboes develop in connexion with lymphatics which stand in anatomical relation to external, as distinguished from internal, surfaces. Now the salient fact in connexion with the distribution of the buboes which are found in plague is, as will be seen on reference to our Statistical Appendix, that primary buboes practically always develop in connexion with lymphatics originating in the skin, and that it is only in the very rarest cases that primary buboes develop in connexion with lymphatics originating in the mucous membranes of internal surfaces. The broad inference which we can draw from this fact is, plainly, that the system is invaded, for the most part, not from internal surfaces, but from the skin.

This inference confirmed by relative frequency of buboes in different regions of the body.

This inference is confirmed when we realise that primary buboes are found with greater or less frequency in the various regions of the body according as larger or smaller areas of skin surface drain into the glands of the regions in question. In this connexion the following figures, extracted from Vierordt's tables, may be considered:—

Area of Skin covering different Regions of the Body.

Head	803 sq. cms.
Neck	456 „ „
Trunk	2,491 „ „
Upper extremity	1,998 „ „
Lower extremity, including the pelvis	5,016 „ „

With a view to obtaining from these figures some sort of a rough estimate of the area of skin surface which stands in connexion with the lymphatic glands in each separate region of the body, we may make the assumption that half of the lymphatics of the trunk drain into the glands of the arm-pit, and that the other half drain into the glands of the groin. Working on this assumption, we obtain the following figures for the areas of skin surface which stand respectively in relation with the cervical, axillary, and inguinal, glands:—

Glands of the neck receive lymphatics from an area of	1,259 sq. cms.
Glands of the arm-pit receive lymphatics from an area of	3,244 „
Glands of the groin receive lymphatics from an area of	6,261 „

It will be seen that the areas of skin surface which drain respectively into the glands of the neck, the axilla, and the groin, stand to each other approximately as the figures 1 : 1·8 : 5.

App. I. to this Report.

If now we consider the hospital statistics published in our Statistical Appendix, which set forth the numbers of primary buboes in the neck, axilla, and groin, observed in Bombay, Karachi, and the Punjab, respectively, we shall find that the numbers of cervical, axillary, and inguinal, buboes recorded in the three sets of hospital statistics stand to each other severally in the relations expressed by the figures 1 : 2 : 7, 1 : 1 : 5, and 1 : 1·3 : 5·5. Aggregating the figures for the three hospitals, we find that the relations in question are expressed by the figures 1 : 1·3 : 5·8. There is an extremely striking coincidence between these figures and those which express the relation between the superficial areas of the regions whose lymphatics drain into the respective groups of glands.

Inference as to channel of infection drawn from comparison between plague persons who wear shoes and those who go bare-foot.

176. Though this would appear to be quite unnecessary, further confirmation of the conclusion that the infective material enters through the skin has been sought also in other directions. In particular, comparisons have been instituted between the prevalence of plague in persons who are accustomed to wear shoes and in persons who are accustomed to go bare-foot. A comparison, instituted by Dr. Aoyama in Formosa, between the Chinese, who walked bare-foot, and the Japanese, who wore shoes, is said

to have shown that buboes in the groin were more frequent in the former. Material for instituting the comparison in question does not appear to exist in India. It is true that the lower class of natives walk bare-foot in the streets, while the higher class natives wear shoes; but a comparison based on this would be vitiated by the fact that it is a common practice among all classes of the native population to walk bare-foot in the house. And it is in the house, rather than in the street, that infection is likely to be contracted. Again, though it might be possible to institute a comparison such as is here contemplated between natives who walk bare-foot in the house and Europeans who do not adopt this practice, such a comparison would be of little value, not only because the statistical data would, in the case of Europeans, be drawn from too small a number of cases, but also because there are many points of difference between the European and the native, other than the wearing or discarding of shoes, which might exercise an influence on the distribution of buboes.

Possibility of infection through the channel of the nose and throat.

177. The question as to whether there are channels other than the skin through which the plague bacillus effects an entrance into the system may unhesitatingly be answered in the affirmative. In favour of the view that the infective material may in some cases obtain access to the system through the mucous membrane of the nose, mouth, or pharynx, are: first, the fact that the infection of plague can, in animals, be experimentally produced by the inoculation of the plague bacillus on the mucous membrane of the nose; secondly, the fact that in man buboes under the chin and about the angle of the jaw are not at all uncommon; thirdly, the fact that the plague bacillus has been found in the human patient in association with primary inflammatory lesions of the tonsils and of the mucous membrane of the nose and the pharynx; lastly, certain epidemiological facts appear to speak in favour of the possibility of the bacillus entering the system by means of the mucous membrane of the nose, such as the fact that, in certain places, epidemics which have been diagnosed as epidemics of mumps preceded and may possibly have stood in casual relation to epidemics of plague. Again, in one instance at least, there is, as we shall see hereafter, reason to suspect that an epidemic of severe coryza may have stood in causal association with a subsequent epidemic of plague.

Para. 317 of Report.

Possibility of infection through the channel of the conjunctiva.

178. Infective material may gain access to the human organism also by the conjunctiva. In this connexion we may refer to the fact that a case of plague occurred in an English nurse in the Parel Hospital, Bombay, as the result of accidental contamination of the conjunctiva by sputum coughed up by a patient suffering from pneumonic plague. The clinical symptoms of the case, in particular the fact that inflammation of the conjunctiva set in, and that this was followed by the appearance of an enlarged gland just below the ear on the affected side, make it clear that the disease was really contracted through the channel of the conjunctiva.

7215.

We have learned unofficially of a similar case of infection which occurred in Hong Kong, also in the person of a nurse.

Possibility of infection by the channel of the stomach and intestinal tract.

179. We have not been able to discover any facts which would support the suggestion that plague may effect an entrance into the human organism through the channel of the stomach or intestinal canal. If infection takes place at all through this channel, it can be only in the very rarest instances.

Possibility of infective material effecting an entrance into the organism through mucous membranes of the genital tract and anus.

180. We may pass to consider the possibility of infective material effecting an entrance into the organism through the mucous membranes of the genital tract and anus. The possibility that infection may be contracted through the former of these channels is suggested by cases where plague phlyctenules have made their appearance on the glans penis (one of these cases has been detailed above), and further by the fact that three patients are alleged to have contracted the infection in the Parel Hospital through the channel in question. The possibility of such a mode of inoculation has been confirmed by experiments on animals.

Para. 173 of this Report.

22,264.

Our attention was drawn by Lieut.-Colonel Henderson, I.M.S., to the possibility of the infection of plague being conveyed by means of the earth balls which fulfil in Bombay the daily office which is served by ablutions in other parts of India.

18,113.

25,778.

IV.(2.).—*Mode in which the Plague Bacillus effects an Entry into the Human Organism in Primary Plague Pneumonia.*

Possibility of infection by means of the lungs and

181. The question as to whether plague can be contracted by the inhalation of infective material into the respiratory tract comes prominently into consideration in connexion with the causation of primary plague pneumonia.

respiratory system generally.

In favour of the view that there is a direct inter-relation between the inhalation of infective material and the supervention of primary plague pneumonia, is, on the one hand, the localisation of the disease in the lungs, and on the other hand, the circumstance that plague pneumonia can be produced in animals by the conveyance of plague-infected material into the respiratory passages, either by intra-tracheal injections, or, though with less certainty, by the deposition of the infective material on the mucous membrane of the nose.

Difficulties in assumption that plague pneumonia caused simply by inhalation of infective material of ordinary plague.

182. On the other hand, it must be noted that there are difficulties in the way of assuming a simple inter-relation between the inhalation of plague bacteria into the lungs and the supervention of primary plague pneumonia

Doubt based on small proportion of pneumonic cases among disinfectors. 10,340.

The first difficulty that confronts us is the fact that primary plague pneumonia has apparently been of extremely rare occurrence among disinfectors, who are by the nature of their occupation very much exposed to the inhalation of dust. On the theory that plague pneumonia is due to the inhalation into the lungs of the infective material of ordinary plague, it would have been reasonable to expect to find a large number of cases of the pneumonic form of plague among the disinfecting staffs.

Further doubt suggested by fact that proportion of plague pneumonia to total plague cases varies greatly in different epidemics or different phases of the same epidemic.

A second difficulty presents itself when we consider the statistics which set forth the proportion in which cases of primary plague pneumonia have stood to the total of plague cases in different epidemics in India. For if the relation between the inhalation of plague bacteria into the lungs and the production of plague pneumonia was a simple and defined relation, like that which obtains between the introduction of plague-infected material into the legs or arms and the subsequent development of a bubo in the groin or arm-pit, the proportion of cases of primary plague pneumonia ought to stand to the total plague cases in a relation not less constant than that in which buboes in the groin or axilla stand to the total of bubonic cases.

Statistical evidence from hospitals. App. I. in this Vol.

But, while a study of the figures in the hospital statistics published in our Statistical Appendix, which set forth the proportion of buboes in the various regions of the body, shows that this proportion is wonderfully constant, a study of the figures which set forth the proportion of primary plague pneumonia to the total of plague cases indicates that this last proportion is subject to great variations.

Further doubt suggested by facts with regard to particular plague epidemics. Para. 203 of this Report. 11,163.

This indication is confirmed by such facts as have been supplied to us regarding the relative proportion of pneumonic cases to the total of plague cases in certain places. In Backergunge, the detailed history of which is given in a subsequent section, all the cases which occurred were, without exception, cases of primary pneumonia. In the case of Gharibabad, the plague cases are said to have been "generally of the type of pneumonia plague." Again, in other epidemics, or more accurately in certain phases of certain epidemics, there has been a very marked excess or absence of primary plague pneumonia. There appears to have been almost complete absence of pneumonic cases in Bangalore towards the latter end of the epidemic of 1898–9. This appears also to have been the case in Bombay in the early months of 1899. Here, although the epidemic was at its height during the period in question, we failed to find a single definite case of primary plague pneumonia. Dr. Choksy, speaking with an experience which extends over four epidemics in Bombay City, and which embraces a total of 6,000 plague cases, notes that plague pneumonia "exists variably in different epidemics; in no two does it appear in anything like a fixed ratio." We may instance Ankleshwar, Hyderabad (Sind), Kotri, Nasik, Porbandar, and Satara, as places where pneumonic cases were few.

Bombay Medical and Physical Society, Sept. 1900. 14,268, 18,093–4, 18,151–3, 12,700, 14,705–11, 13,724–7, 22,153.

Doubt arising from absence of history of origination of primary plague pneumonia from contact with bubonic case, and from frequency of successive cases of primary plague pneumonia.

Differences such as those mentioned above in the proportion of cases of primary plague pneumonia obviously do not conform with the theory that the introduction of plague bacilli into the lungs is the only factor which comes into play in the production of this type of plague. But perhaps the inadequacy of the view that there is a simple inter-relation between the introduction of plague infective material of ordinary plague into the lungs and the supervention of plague pneumonia is most clearly established by the fact that we were unable, though we made special inquiries into this matter, to discover any definite instance of primary plague pneumonia having originated from contact with a case of ordinary bubonic plague. In the large majority of cases where a history of infection was obtainable, that history has pointed to an origination of primary plague pneumonia from a previous case of pneumonic plague. Noticeable examples of this are detailed in the history of the epidemic in Backergunge, and also in connexion with certain other minor epidemics of plague pneumonia to which attention is drawn in a subsequent section of this Chapter.

Paras. 202–3 of this Report.

183. We, therefore, must endeavour to substitute for the theory that there is a simple inter-relation between the introduction of plague bacteria into the lungs and the supervention of primary plague pneumonia a more adequate theory.

Adequate theory of origin of plague pneumonia must take into consideration:

We must take into consideration the possibility that there may be something, either in the form, or in the manner, in which the infective material escapes from the body, which favours the conveyance of the infection into the lungs of persons in attendance on cases of plague pneumonia. We have incidentally seen that opportunity for the direct transference of infective material to attendants is afforded by the fits of coughing to which pneumonic patients are subject. Opportunities for indirect transference are afforded by the circumstance that sputum may readily become desiccated and pulverised, and that it may in this condition readily be inhaled into the lungs.

firstly, special conditions of form and manner in which infective material may be communicated from pneumonic patients;

A second possibility which must be kept in view is that there may be something specific in the infective material which conditions the supervention of plague pneumonia when the material is introduced into the lungs. This view would appear to win support, firstly, from the circumstance that instances are on record of the outbreak of pneumonic plague among persons who have attended the funerals of patients who had died of pneumonic plague, and, secondly, from the circumstance that in the Dharwar Hospital where "unfortunately a pneumonia case was left in the same ward with two other patients suffering from the bubonic form, these two, who at the time were doing well, developed pneumonic symptoms within 48 hours, and both died. From the above," says Dr. Hornabrook, who reports the case, "it will be seen that the pneumonic type of plague is dangerous even to a patient suffering from the bubonic fever."

secondly, that something specific in infective material of plague pneumonia causes pneumonic plague. Paras. 202–3 of this Report.

The questions opened up by the above observations would appear to be deserving of future investigation. With a view to indicating the paths along which that investigation might appropriately proceed, we may perhaps call attention to the following.

Necessity for further inquiry and suggestions regarding lines along which inquiry might proceed.

There can be no question of any specific difference between the plague bacillus which gives origin to cases of plague pneumonia and the bacillus which gives origin to cases of bubonic plague; for our inquiries have elicited that cases of bubonic plague have not infrequently originated from contact with cases of primary pneumonic plague.

1517, 12,997, 13,177, 15,182, 15,280, 16,048, 17,990, 18,214, 4567, 9566, 7218.

It is, on the other hand, possible that there may be a difference in point of virulence between the plague bacillus which causes pneumonic plague and the plague bacillus which causes bubonic plague. Against this, however, may be urged the fact that the third epidemic in Bombay was at its maximum of virulence at a time when plague pneumonia was absent.

Again, it is possible that the plague bacillus may, in the case of infective material derived from a pneumonic case, be associated with some other bacillus which favours its growth and contributes to the production of plague pneumonia; in favour of this would appear to be the fact that, in many cases of plague pneumonia, pneumo-cocci, strepto-cocci, and other pathogenic bacilli, have been found associated with the plague bacillus; against this may be urged, though this is not conclusive, that plague bacilli have in some instances, on microscopical examination, appeared to have been present in pure cultivation in sputum. The great difference in the character of the symptoms in different cases appears also to suggest that primary plague pneumonia may be a mixed infection, in which the secondary rôle may be played at one time by one micro-organism, at another time by another. Lastly, we may note that it has been suggested, from a survey of the pathological changes in the lungs, that pneumo-cocci may in many cases be agents in preparing the way for a subsequent development of the plague bacilli in the lungs.

Report of German Plague Commission, p. 74.

IV.(3.).—*Mode in which the Plague Bacillus effects an Entrance into the Organism of Rats.*

184. We pass now to the discussion of the mode in which the plague bacillus effects its entrance into the organism of rats, this question being of great importance in view of the fact that rats play a great part in the dissemination of the infection of plague.

Mode by which the plague bacillus effects an entrance into the organism of rats.

In the case of rats, as in the case of men, we have first to consider the possibility of the infective material effecting an entrance into the organism through small wounds in the skin. That the bacillus can invade the rat organism in this way emerges clearly from the results of inoculation experiments. The only questions which arise

Possibility of the infection being contracted by rats through the channel of the skin.

for discussion, therefore, in this connexion are, first, as to what quantity of the infective material is required for communicating the disease, and, secondly, the question as to whether rats infect themselves in a state of nature through the channel of the skin.

Report of German Plague Commission. p. 282.

With regard to the first of these questions, the German Plague Commission state that in their experience a mere prick with a plague-infected needle suffices to bring about the death of an ordinary rat. The Austrian Plague Commission, while noting that the inoculation of small quantities of very virulent cultures is almost always followed by fatal results, record that in isolated instances rats showed no reaction to the inoculation of very considerable quantities of fully virulent cultures. We ourselves (and we were dealing with cultures freshly isolated from fatal cases of plague) found that it was in all cases necessary to inoculate a very sensible quantity of plague culture in order to ensure the death of the animal, and in some cases we found evidence of great resisting powers on the part of the rats with which we had to deal in Bombay.

Report of Austrian Plague Commission, Part II., C., p 684.

26,311.

The data which are available with regard to the post-mortem appearance of rats do not permit of a final judgment being arrived at as to whether the system is generally invaded in a state of nature through the channel of the skin. In a certain proportion of cases buboes are found which seem to indicate cutaneous infection. It has been suggested that rats communicate the infection to each other by means of bites, but the circumstance that the bodies of rats are enveloped in a close covering of hair would seem to afford, at any rate, a large measure of protection against cutaneous wounds. It has been suggested also that the infection may be inoculated into rats by fleas. This question, involving as it does the general question of conveyance of plague by insects, will be treated of in a subsequent section of this Chapter.

Section IV. (4) of this Chapter.

Possibility of rats being infected through the channel of the alimentary canal.

185. The results of experiments which relate to the possibility of infection through the channel of the alimentary canal are of a very conflicting nature.

Negative results were obtained by Dr. Gibson, Mr. Hankin, and Dr. Simond, who have all experimented on this subject.

The details of Dr. Gibson's series of experiments are of great interest. He prepared the 10 rats which he used for the experiments by keeping them without food for 24 hours. He then fed them on small bundles of corn, wrapped in pieces of gunny bags, and then soaked in a broth cultivation of plague which was found to be sufficiently virulent to kill in 36 hours when inoculated hypodermically into rats in a dose of 1 c.c. The bundles of corn which were supplied to the rats that formed the subject of the feeding experiments were given to them in a moist condition. The supply of infected food was renewed as occasion required for about a fortnight. At the end of that time the rats, though each had consumed from 100 to 150 c.c. of the original plague culture, all remained well. Subsequently, with a view to testing their resistance, they each received hypodermically 1 c.c. of the plague culture in question, with the result that they all succumbed to plague in 36 hours.

619; 635–44.

Mr. Hankin made experiments with similar negative results. He is of opinion that rats contract plague from the intestinal canal only when large quantities of very virulent cultures are introduced into their food.

8691–2.

Annales de l'Institut Pasteur, Oct. 1898.

Dr. Simond's experiments go even further in this direction. In one experiment he caused a rat to drink a watery suspension of plague bacteria which he made from a virulent agar culture of plague. In another experiment he fed a rat for three consecutive days on virulent agar cultures, one of these being administered daily. In certain further experiments he fed rats upon the organs and dead bodies of animals which had died of plague. In other experiments, again, rats were fed with pneumonic sputum, and with food that was soiled with infected blood. In each and all of these experiments the rats remained healthy.

Report of the German Plague Commission, p. 284.

The German Plague Commission, on the contrary, found that plague could be conveyed to rats by feeding them on plague cultures or the bodies of rats dead of plague. They infer from this, that in a state of nature rats generally become infected in this manner. They suggest, however, that in such cases the infective material may penetrate into the system through the mucous membrane of the throat or nose, and not through the mucous membrane of the alimentary canal.

Report of Austrian Plague Commission,

The Austrian Plague Commission found that rats could easily be infected by feeding them on plague-infected material. The pathological changes found in the body after death pointed in most cases to the penetration of the bacillus through the mouth, and

only in rare cases to the penetration through the intestinal wall. They found, however, that not all rats could be infected with plague by the mouth. Out of seven rats which were repeatedly fed in Bombay on plague-infected material, three remained alive. Part II., C., p. 684.

In view of the above, we think that it is quite possible that rats may occasionally, under natural conditions, contract plague by feeding either on the dead bodies of their companions, or on grain or other food contaminated with plague excretions, or, as has been suggested by gnawing the dead bodies of plague patients. We think, however, the results recorded by Dr. Gibson, Mr. Hankin, and Dr. Simond, justify the conclusion that the infection of rats through the alimentary canal cannot be very common. 7185.

186. The results of experiments, in which plague bacilli have been placed in contact with the uninjured mucous membrane of the nose, place it beyond doubt that plague bacteria may effect an entrance into the rat organism by this channel. There is, however, a difference of opinion among observers as to whether rats are more easily infected in this manner than by hypodermic inoculation. While some observers are of opinion that rats are more susceptible to infection through the mucous membrane of the nose, other observers are of the contrary opinion. Among these is Mr. Hankin, who is disposed to think that rats are not particularly susceptible to infection by the mucous membrane of the nose. Our own experience, which is a very restricted one, is in conformity with that of Mr. Hankin. In two cases where we filled up the nostrils of rats with a broth culture of virulent plague, the rat remained perfectly healthy. We think, however, we were not justified in inferring from these cases that infection may not in nature often be contracted through this channel. Of all channels of infection this seems to us the most likely in such cases, for instance, as those where an outbreak of plague among rats appeared to have been traceable to an importation of plague-infected clothing. Possibility of the infection being contracted by the channel of the nose and the respiratory tract generally. 8703. 26,311. Para. 271 of this Report.

IV.(4.).—*Question as to the Transference of Plague Infection by the Agency of Suctorial Insects.*

187. It has, in the case of plague, as also in the case of many other septicæmic diseases, both of animals and of men, been suggested that infection may be transferred from the sick to the healthy by the agency of suctorial insects. In particular, the suggestion that insects may be instrumental in the spread of the plague has been mooted as furnishing a possible explanation of the extraordinary number of attacks which, as we shall see, are recorded among the Jains, a Hindu sect with whom it is an article of religion not to take any form of life, even that of insect parasites. The suggestion that insects take an active part in the dissemination of plague has, however, been most prominently put forward in a paper on the spread of plague, which was published by Dr. Simond. Question of the transference of plague by the agency of suctorial insects from rat to rat and from rat to man. Para. 300 of this Report. Annales de l'Institut Pasteur, Oct. 1898.

188. Dr. Simond conceives that fleas which have been parasitic on plague-infected rats migrate on the death of their hosts, and convey infection to healthy men and rats. With a view to establishing the conveyance of the infection by the agency of insects, and the importance of this agency, Dr. Simond endeavours to establish the following:— Inductive process by which Simond endeavours to prove such transference.

Firstly, that plague rats are eminently infective when infested with fleas, and that they cease to be infective when they have been deserted by their parasites;

Secondly, that living plague bacilli are found in association with fleas which are taken from plague-infected rats;

Thirdly, that plague can pass from infected rats to other animals which have not come directly in contact with them or with their infected excretions; and

Fourthly, that the fleas which infest rats will transfer themselves as parasites to men.

With a view to proving that plague rats are very infectious immediately after death, Dr. Simond recites certain occurrences which point to infection having been contracted by handling rats which were recently dead of plague. It will not be necessary to detail these occurrences here; we shall come upon them in a subsequent section of this Chapter, which deals with the general question of the infectivity of plague-infected rats. We may here so far anticipate as to say that, on a survey of the whole of the facts, the particular point which Dr. Simond endeavours to prove does not appear to be clearly made out. Pre-eminent infectivity of rats immediately after death not clearly established. Section VI.(5)(*a*) of this Chapter.

No proof that decline of infectivity follows on desertion of rats by insect parasites.

To establish the proposition that rats cease to be infectious shortly after death, Dr. Simond cites the fact that, in an isolated experiment made by him, a batch of seven rats remained healthy after they had been placed for 24 hours in contact with the corpse of "a plague rat found dead in the city of Karachi and deserted by its parasites." It will be seen that the pre-eminent infectivity of rats which have recently died of plague, as compared with rats which have been dead somewhat longer, is far from being established by an isolated observation like this. Even if this particular point were hereafter to be established, there would be no necessity for assuming that the decline of infectivity was due to the abandonment of the rats by their insect parasites. A simpler explanation would be furnished by the gradual desiccation of the infective material on the external surface of the corpses, and the gradual dying out of the plague bacilli in the interior of the organism.

Presence of plague bacteria in fleas does not prove possibility of further transfer of the bacteria by fleas to other animals.

With a view to establishing that living plague bacteria are found in association with fleas which have been parasitic on plague-infected rats, Dr. Simond adduces three observations. In each of the experiments in question an extract, made by triturating fleas taken from plague-infected rats, was injected into a mouse. The three mice which were thus inoculated succumbed, but only in one case was proof obtained that the death was due to plague. The proposition that living plague bacteria are found in association with fleas taken from plague-infected animals rests, then, so far as Dr. Simond is concerned, upon a single positive result. But even given that the evidence of this single positive result were conclusive, it would, as consideration will show, establish only that the plague bacillus may be taken up by the flea from the plague-infected rat, and it would not suffice to establish that the plague bacillus may be further transferred from the flea to another animal.

Dr. Simond put forward the view that the further transference of the plague bacillus by the flea to its new host is probably effected by the inoculation of the flea's dejecta into puncture wounds produced by the flea's bites. To establish the possibility of this, it would be necessary to show that living plague bacilli are found in the flea's dejecta. No experiments on this question are to hand, but we would note that the careful experiments of Dr. Nuttall, in the parallel case of bugs, have demonstrated that plague bacilli, when taken into the body of these insects, are in process of time destroyed there.

Johns Hopkins Hospital Reports, VIII., Oct. 1899, p. 18.

Inconclusive nature of experiments made to prove transfer of plague by fleas from rats to other animals.

Passing now to the consideration of the experiments which Dr. Simond adduces in proof of his proposition that plague can be transferred by the agency of fleas, we have to deal with a series of four experiments. In each of these experiments a healthy animal (in three cases a rat, and in the fourth case a mouse) was placed in a cage which was introduced into a glass vessel in which a rat was dying of plague. With the design of ensuring that opportunity should be afforded for the transference of plague from the dying to the healthy animal by the agency of parasites, a certain number of fleas had previously been placed in the glass vessel. Of the animals experimented upon, one rat and one mouse contracted plague; the other two rats remained healthy. We may note here, first, that fleas obtained from cats were selected as agents for the transfer of the infection, without apparently any inquiry as to whether the fleas which are parasitic on cats will transfer themselves as parasites to rats or mice. It is thus not certain that the infection can, in the cases where the animals experimented upon died, have been conveyed by the agency of the insect parasites in question. We may further note that we do not gather from the description of the arrangements that precautions were adopted to prevent infected dejecta being projected by the struggles of the dying animal through the bars into the cage containing the healthy animals. It is thus not impossible that the infection may have been conveyed directly by ordinary mechanical agencies from the sick to the healthy animals.

Uncertainty whether rat fleas transfer themselves to men.

Centralblatt. Bact. 6.1.1900 and 28.12.1900.

Under the fourth of the headings set forth above no experiments are adduced by Dr. Simond, the observer contenting himself with a statement that he had satisfied himself that rat fleas will transfer themselves to men. The Hon. Lionel Rothschild, to whom, as having a special knowledge of insect parasites, we, by the advice of Prof. E. Ray Lankester, referred the question for an opinion, thought it possible that such a transference might in exceptional circumstances occur. Professor Galli Vallerio, on the other hand, was unsuccessful in his attempt to induce rat fleas to transfer themselves as parasites to men. There would thus appear to be some uncertainty as to whether rat fleas will, as Dr. Simond suggests, freely transfer themselves to men.

It will have become manifest that the process of induction by which Dr. Simond endeavours to establish the proposition that suctorial insects play an important part in the transference of plague from sick to healthy animals is so weak as to be hardly deserving of consideration.

Conclusion regarding evidence adduced by Dr. Simond.

We are thus left to consider the suggestion in the light of other experimental evidence.

189. The evidence dealing with the general question of the transference of infection by the agency of insects has been very ably summarised by Dr. Nuttall in a critical and historical study already cited above. It appears from the data which are collected together in that publication, and in particular from the careful and extensive series of experiments with anthrax, plague, chicken-cholera, and mouse septicæmia, that were performed by Dr. Nuttall himself, that no experimental evidence can be adduced in support of the theory that suctorial insects play an important part in the transference of bacterial diseases from one animal to another. It will perhaps be appropriate to cite here the following results obtained by Dr. Nuttall in his experiments on plague. "Twenty-two bugs which had sucked the blood of a mouse dying of plague (the blood contained many bacilli) were immediately placed on four mice. None of the mice sickened after being bitten by the infected bugs." In the case of anthrax—a disease in which the bacteria are, as in plague, present in large numbers in the blood—Dr. Nuttall's experiments were again entirely negative. His experiments were carried out on mice, which are extremely susceptible to anthrax. Eight mice bitten by 124 infected bugs all remained healthy. Uniformly negative results were obtained by Dr. Nuttall also in his experiments with chicken-cholera and mouse-septicæmia.

General considerations regard transfer of bacterial diseases by suctorial insects.

Johns Hopkins Hospital Reports, VIII., Oct. 1899.

The experience of Plague Hospitals in India, and in particular the experience obtained in the Arthur Road Hospital at Bombay, seems to indicate very clearly that suctorial insects do not come under consideration in connexion with the spread of plague. Dr. Choksy in particular drew attention to the fact that the staff and attendants in the Arthur Road Hospital were continually bitten by insects, especially mosquitoes, and that none the less no cases of the transfer of infection from the sick to the healthy came under observation.

20,640–1.
20,999–21,004.

V.—The Incubation Period of Plague.

190. In accordance with the scheme laid down at the outset of this Chapter, we now pass on to consider the question of the length of the incubation period of plague, a question the determination of which has an importance not only theoretically in connexion with the epidemiology of plague, but also practically in connexion with the measures adopted for combating the disease.

Length of incubation period of plague.

Information which makes it possible to fix the incubation period of plague can be drawn from various sources.

The most precise information available is derived from the histories of certain cases in which the disease has been contracted by persons who were either making, or assisting, at post-mortems on plague patients. Not less precise is the information available from the consideration of a case, already referred to, in which plague supervened upon the inoculation of infective material upon the conjunctiva, and of a case reported by Captain Leumann, I.M.S., where plague followed the bite of a delirious pneumonic patient. We have enumerated below in tabular form under the heading of Class I., Group A. the particulars of the cases referred to. In each of these cases not only is the exact time when the virus was inoculated fixed by the history, but in many of the cases there is, in the fact that a bubo subsequently developed in relation with the site of the inoculation, evidence that the virus inoculated on the occasion in question was the real cause of the disease.

Class I.A. Precise evidence available in historise of infection contracted at post-mortems.

In Class I., but under the separate heading of Group B., we have collected cases of persons who, having been previously unexposed to infection (or practically so), came specially in contact with infection on a particular specified occasion and not afterwards. Under this heading are included cases where a plague attack has occurred subsequent to a visit to an infected house, or subsequent to contact with plague-infected persons or rats. The information available in these cases is less valuable than the information derived from the consideration of the cases of accidental inoculation, inasmuch as it does not supply actual proof of the presence of infective material or of its coming in contact with some susceptible surface. None the less, as the historical

Class I.B. Important confirmatory evidence derived from histories of cases which came into contact with infection on a particular specified occasion.

presumption that the disease was contracted on the occasion specified is in most of the cases enumerated in Group B. a very strong one, these cases would appear to afford important confirmation of the inferences that can be deduced from the consideration of the histories of the cases enumerated under Group A.

Class II. Information as to extreme limit of incubation period available from histories of persons who were continuously exposed to infection from a particular date onwards.

Supplementary information as to the duration of the incubation period can be derived from the histories of persons who, having been previously quite unexposed to infection, or practically so, can be shown to have been from a particular date onward more or less continuously in the presence of infection. Cases such as are here in question are grouped together below in tabular form under the heading of Class II. We have under this heading enumerated among others (*a*) cases of persons arriving from uninfected places who have come into attendance upon plague-infected persons; (*b*) cases of persons who, living in previously uninfected localities, have, from a particular date onward, come in contact with imported infection; (*c*) cases of persons who, living in previously uninfected houses, have from a particular date onwards been exposed to infection introduced into those houses by plague-infected rats; and (*d*) cases of persons, who, while previously living in the presence of infection, have, from a particular date onwards, become more susceptible to infection by the fact that they have accidentally wounded themselves. Cases such as these, though they do not, like the cases enumerated in Class I., enable us to fix the beginning as well as the end of the incubation period, will manifestly assist in the determination of the duration of the incubation period by virtue of the fact that the interval which elapsed between the first exposure to infection and the supervention of the disease, cannot in these cases have exceeded in duration the incubation period and must in a large majority of cases have been less than the incubation period.

Class III. Indications which can be derived from histories of persons who developed plague after removal from infected surroundings.

Lastly, indications with regard to the duration of the incubation period can be derived from the consideration of the cases which we have grouped together under Class III. The cases referred to are those of persons who, having previously been continuously in contact with infected surroundings, have been removed from those surroundings on a particular date. Indications of the kind here in question are afforded by the records of segregation and evacuation camps, which set forth the number of plague attacks developed in camp and the dates of those attacks. The information obtained from consideration of the cases grouped together as Class III. is, as we shall see hereafter, of very little assistance to us in fixing the duration of the incubation period.

Class I., Group A.

191. We may now proceed to set forth, in the form of a brief synopsis under these various headings, the facts that we have collected with regard to the duration of the incubation period.

CLASS I.

Cases which allow of both the Beginning and the End of the Incubation Period being accurately fixed.

Group A.—Cases where there is a History of a Direct Inoculation of Infective Material.

Serial Number.	Authority.	Reference.	History of Case.	Length of Incubation as deduced from History.	Remarks.
1.	Evans -	6923–5, 6988–95	The patient in question, a Dome, assisted at the post-mortem examination of a plague patient on 27th and 29th April, and 1st May 1898. On the last date he got a scratch; fever was noticed in the evening of the 2nd, and the patient died of plague on the 3rd.	24–36 hours.	
2.	Elphick, also Chayter-White	9566, 9393	Patient, an Assistant Surgeon, on the 17th of January 1898, at the post-mortem on a pneumonic patient, scratched himself on the back of the right hand; plague manifested itself in 36 hours, fever setting in on January the 19th. The glands on the right side of the chest were enlarged.	36 hours.	

Serial Number.	Authority.	Reference.	History of Case.	Length of Incubation as deduced from History.	Remarks.
3	Cook - -	6462, 6493	Patient, a Dome, at 8 a.m. on 17th April 1898, scratched his hand at the post mortem of a plague case in Calcutta. At noon on 19th he was found to have fever, and plague declared itself.	2 days.	
4	Clemow -	7218	Patient, a Hospital Assistant, pricked a finger of his left hand at 3 p.m. on March the 14th, during the post mortem on a case of plague pneumonia. On the evening of the 16th he felt ill, and plague set in with enlargement of the left supratrochlear gland, and a bubo in the left axilla.	2 days.	
5	Clemow -	4562–9	Dr. Sticker, of the German Plague Commission, had at post mortems made during the second week in March 1898 several times cut or otherwise wounded his left hand without any evil consequences. On March 26th he performed three post mortems, and one at 3 p.m. on March 29th, without wounding himself. At 6 p.m. on the 29th, on drying his hands after a bath, he noticed over the metacarpo pharyngeal articulation of the right thumb a small slightly painful vesicle. This received a blow the same evening and the arm at once became painful and plague developed, the bacilli of plague being recovered afterwards from the vesicle. It is not quite clear when the patient inoculated himself, though it is probable that the incubation period was three days.	3 days (?)	
6	Leumann -	4542-55	On the 21st of December 1897, about 5 p.m., Captain Leumann, I.M.S., scratched himself at a post mortem. On the 24th December he had a feeling of malaise all day; in the evening he suffered from a sort of mental aberration; he got fever and an enlargement of glands in the armpit on the same side as the scratch and then went through a slight attack of plague.	3 days.	
7	Cook - -	6496, 6502	On Thursday, October 7th, 1897, Major Giffard, I.M.S., made at Poona a post mortem on an imported case. He had on his wrist a partly healed scar from a boil. He began to feel unwell on Sunday, October 10th, took to bed on Monday, developed an axillary bubo, and went through a typical attack of plague.	3 days.	
8	Cook -	6503–6	A Dome who assisted Major Giffard in the post mortem last mentioned died of plague, the bacillus being cultivated from material obtained from an axillary bubo from which he suffered. The patient was not medically attended before his death; his relatives said he was taken ill three days after the post mortem.	3 days.	
9	Evans -	6921 (*also* Abstract of Evidence not reprinted with the Proceedings of the Commission).	A Dome contracting plague 48 hours after assisting at a post mortem: an unprotected abrasion of one of his fingers may have been the point of entry of infection.	48 hours.	

Serial Number.	Authority.	Reference.	History of Case.	Length of Incubation as deduced from History.	Remarks.
10	Childe -	1411	A student in the Jamsetjee Hospital, Bombay, pricked himself in the left thumb while assisting in the post mortem of a case of plague. The prick was so slight that he did not notice it at the time. About three days afterwards he developed a bubo in the left axilla, and he subsequently died of plague.	3 days.	
11	Green -	6804 (7220 D. 28).	A man who assisted at a plague post mortem on 3rd June 1898, and who is said to have injured his hand, is reported to have developed plague on 6th. He died on the 10th, two days after he was seen by Major Green, I.M.S.	3 days.	
12	Hutchins -	"Lancet," December 1, 1900.	A warder at Brisbane assisted, on the 8th of October 1900, at two post-mortem examinations of patients subsequently proved to have died of plague. He sickened on the 11th of October, and on the 12th it was shown by clinical and bacteriological examination that the man was suffering from plague. He had abrasions of the hands and axillary buboes.	3 days.	
13	Pinching	Report on Plague in Egypt from May 1899 to June 1900, p. 45.	Patient, a post-mortem porter, in sewing up a plague corpse, on the 29th of May, contracted an abrasion on the little finger of his left hand from the friction of the string used as a ligature. On the 3rd of June he sickened, and developed plague with an axillary bubo.	3 days.	
14	Leumann -	4567	A Compounder at Hubli was bitten on the thumb by a pneumonic patient, and fell ill 3½ days or 4 days after; he developed a bubo in the axilla and had a mild attack of plague.	3½ to 4 days.	
15	Jennings, reported by Clemow.	7213–5	Sputum was accidentally coughed into the eye of a nurse in Parel Hospital, Bombay, on the 15th of February 1897. Her temperature rose on the 18th. On the 19th, plague symptoms were marked, a bubo developing under the ear on the side of the infected eye.	3 days.	

Class I., Group B. *Group B.—Cases where there is a History of the Patient having come specially into contact with Infection on a Particular Occasion.*

Serial Number.	Authority.	Reference.	History of Case.	Length of Incubation as deduced from History.	Remarks.
16	Silcock -	13,814	After the evacuated population of Trimbak had been perfectly free from plague for some time, two cases suddenly occurred in camp. The patients had the previous evening gone into an infected temple to get grain.	1 day.	
17	Steen -	22,732	Three cases of plague occurred at Andumbar among the evicted population within 48 hours of their having been allowed to revisit their houses in connexion with their disinfection.	48 hours or less.	

Serial Number.	Authority.	Reference.	History of Case.	Length of Incubation as deduced from History.	Remarks.
18	Hutcheson -	6006–11, 6024.	The evicted population of Hinganghat (among whom only one attack occurred during the six days following the 26th of November 1898) were, on the 29th of November, allowed to revisit their shops. In direct association with this, six new cases of plague occurred on 2nd December.	4 days.	
19	Winter -	2226	Two men developed plague three or four days after they had gone into the town of Jawalapur to see to the disinfection of their houses.	3 or 4 days.	
20	Katrak -	17,926	A woman took up a rat, found dead, and threw it away; she then scrubbed the floor with plain water. In six or eight hours she felt a sense of chilliness and plague developed.	8 hours.	
21	Bhalchandra Krishna.	19,023	A Parsee lady developed plague 24 hours after she had, while taking out rice, come in contact with a dead rat.	1 day.	
22	McCloghry	11,418	A fitter in Karachi developed plague three days after he handled a dead rat.	3 days.	
23	Dimmock -	20,418	A Goanese servant removed a dead rat from the floor and burnt it. Four days afterwards an attack of plague developed.	4 days.	
24	Da Cunha -	16,513	A woman in Daman, on re-entering her vacated house, trod on a dead rat and developed plague the same evening.	12 hours.	
25	Bhalchandra Krishna.	19,038	A man who assisted in carrying a plague corpse got plague four days after.	4 days.	
26	Justice -	6835–9, 7350, 7394.	Lakhi Some, who, on 1st September 1898, visited Tarak, who was suffering from pneumonic plague, contracted the disease on the 6th.	5 days	These three cases were cases of pneumonic plague which occurred during the epidemic at Backergunge, described in detail elsewhere in this report (para. 203 below).
27	„ -	„	Nukta, who visited Lakhi Some on the 8th September, fell ill with pneumonic plague on the 13th.	5 days	
28	„ -	„	Ramnidhi, who assisted at the cremation of Lakhi Some on 9th September, developed the disease on the 14th.	5 days	
29	McConaghy	12,883–8	A European newspaper editor, who lived in an uninfected part of Poona, and had not otherwise come in contact with plague, developed plague within 24 hours of his having gone round with a search party visiting plague-infected houses.	1 day.	
30	Wilkinson -	10,588–91	A Compounder, who was attacked with plague on May 4th, was probably infected on the previous evening by a visit to an infected house.	1 day	It was elicited that this man was engaged on disinfection work.
31	MacCartie quoted by Bombay Plague Research Committee.	I. 379	A fireman from the steamship *Hydaspes*, which arrived in Bombay on the 7th of January 1897, sickened with plague, with bubo in the groin, on the 9th of January. The ship had come from an uninfected part, and the patient had visited Bombay on the morning of the 8th, and possibly also on the evening of the 7th of January.	1 or 1½ days.	

Inferences derived from consideration of cases detailed in Class I.

It will be seen, on consideration of the histories enumerated in the first of the two tables given above, that in the 15 cases enumerated under Group A., the period of incubation was in only one case as long as five days, in another case it may have been as long as four days, while it was in three cases little, if at all, longer than 24 hours. The average duration of the incubation period of plague works out for the whole of the cases in Group A as under three days. A practically similar result is obtained from a consideration of the cases enumerated in Group B. It will be seen that here also there is in six or seven of the cases a history of plague supervening in as short a period as 24 hours after exposure to infection. The longest period of incubation recorded is a period of five days, and again the average duration of the incubation period works out as under three days.

Indications regarding incubation period derived from (1) history of Chak Kalal. (Serial number, 32.)
10,109–10; 10,603.
Para. 632 of this Report.
10,189.
10,110.

192. Perhaps more important than any of the data set forth under Group B. are the data relative to the outbreak of the epidemic in Chak Kalal, a village in the Punjab. The inhabitants of this village were, as will be more fully recorded elsewhere, evacuated as a measure of precaution before any plague had come among them. On a particular date they were driven in by a storm of rain, and they took temporary shelter in their houses, in which the bodies of numerous rats dead of plague were found. When after this the people moved out again into camp, 29 cases of plague occurred among them. The sequence of attacks was as follows :—

	Attacks.
On the first day subsequent to the day on which the people took shelter in their plague infected houses	0
On the second day	3
On the third day	7
On the fourth day	7
On the fifth day	1
On the sixth day	2
On the seventh day	2
On the eighth day	0
On the ninth day	3
On the tenth day	2
On the eleventh day	2

There were no further cases.

It will be seen that here the earliest cases occurred after an incubation period of 48 hours, and that the largest number of cases occurred on the third and fourth days. We thus have once more evidence that the incubation period of plague is rarely longer than four days, and in most cases is less than four days. The dropping cases which occurred from the fifth to the eleventh day, while they may possibly be instances of prolonged incubation, may also (as Captain James, who has placed these facts on record, recognises) quite well be due to infection contracted in camp.

10,191–3.

(2.) history of Mahlgahla. (Serial number, 33.)
10,348.

Similar to the case of Chak Kalal in all respects, except only in the fact that the period of exposure to infection here lasted for a full month, is the case of the village of Mahlgahla, also in the Punjab. After evacuation the disinfection of this village began on the 22nd of March 1898. Until the 28th of March work was confined to a particular part of the village, and no cases occurred among the disinfectors employed in this part of the village. On the 28th of March was commenced the disinfection of a part of the village where large numbers of dead rats were found. On the 2nd of April cases began to occur among the disinfectors, these not having come in contact with any plague patients. The disease spread to such an extent that work was stopped on the 22nd of April. On the 23rd of April there were 13 cases among the disinfectors; on the 24th, two cases; on the 25th, three cases; on the 26th, two cases; and on the 27th of April, one case. The epidemic then died out. This history brings out two points; first, that the disease developed five days after the disinfectors first came into contact with infection; and secondly, that the last case of the disease developed five days after exposure to infection ceased.

Class II.

193. We pass now to set forth and consider the cases which we have collected under Class II.

CLASS II.

Cases in which there is a History of the Patient having been in Contact with Infection on and after a Particular Day.

Serial Number.	Authority.	Reference.	History of Case.	Length of Incubation, as deduced from History, may have been less than, but cannot have exceeded	Remarks.
34	Illingworth	4481	A girl developed plague in a village near Guntakal 24 hours after the infection had been imported into that village by her father.	1 day.	
35	Hossack -	7576	A nurse at Poona contracted the disease in the pneumonic form when she had been "perhaps two or three days" in attendance on a case of pneumonic plague.	3 days.	
36	Bombay Plague Research Committee.	I. 379	On the 4th of January 1897, Nurse Joyce went to attend Major Manser, I.M.S., who was suffering from plague pneumonia. She nursed him from the 4th till the morning of the 6th. She was attacked by plague pneumonia on the evening of the 7th.	3 days.	
37	Anderson -	23,207	In Kersova a case was imported on 30th, proving fatal on the 31st of January 1897. The schoolmaster, who lived in the Talati's house, visited the patient, and on the 2nd of February the Talati's nephew sickened.	4 days.	
38	„ -	„	In Kelwa, five days after the arrival from Bombay of people who stayed three days, plague broke out among men, rats having died in the interval.	Under 5 days.	
39	Niblock -	12,446	Two servants, belonging to Banniahs in Sharafi and Reri, went to the Joria Bazar at Karachi on the 16th of May 1898, and returned on the 19th; one developed plague on the 21st in Reri; and the other on the 23rd in Sharafi; four of their masters, probably infected by them, developed plague on the 24th of May.	5 days. 7 days. 5 days.	
40	Dhurandhar	15,202	People from Bombay stopped three days in Ahmedabad; three days after they left, plague occurred among the people with whom they had stayed.	6 days.	
41	Justice -	6835–9, 7350–94.	Girija and Upendra, who had been exposed to the infection of pneumonic plague from the 27th to the 31st of August 1898, were attacked on the 1st of September.	6 days.	This and the subsequent cases reported by Dr. Justice are all cases of pneumonic plague which occurred in connexion with the epidemic at Backergunge, described in detail (para. 203 below).
42	„ -	„	Tarak, who had been similarly exposed, developed the disease on the 2nd of September.	7 days.	
43	„ -	„	Girija's wife, who nursed her husband, who had sickened on September 1st, developed the disease on the 9th.	9 days.	
44	„ -	„	Baikuntha, who nursed Lakhi Some from the 6th to the 9th of September, fell ill on the 14th.	8 days.	
45	„ -	„	Ramani, who also nursed Lakhi Some between the dates in question, developed the disease on the 13th of September.	7 days.	
46	„ -	„	Kamini, who nursed Baikuntha from the 13th to the 17th of September, fell ill on the 20th.	8 days.	
47	„ -	„	Harsundari nursed Ramnidhi from the 14th to the 16th of September, and fell ill with pneumonic plague on the 20th.	7 days.	

Serial Number.	Authority.	Reference.	History of Case.	Length of Incubation, as deduced from History, may have been less than, but cannot have exceeded	Remarks.
48	Dyson -	6716–20	The Compounder who attended Bishnadutt, who died of pneumonic plague at Chausa on 31st August, was taken ill three days after he saw the patient.	3 days	The history of this case is not altogether clear.
49	Hutcheson -	5958	A boy at Khandwa, who probably came in contact both with an imported case of plague on the 29th of October 1898, and with dead rats which were found in the house on the 30th and 31st of October, was attacked by plague on the 2nd of November.	5 days.	
50	Anderson -	23,207	A girl was attacked by plague in Jegapur three days after dead rats had been found in the house, and five or six days after her father returned from Dharwar, which was then infected with plague.	6 days.	
51	„ -	„	Plague broke out at Santa Cruz, three days after dead rats were found.	3 days.	
52	„ -	„	Plague broke out at New Kurla, four days after dead rats were found.	4 days.	
53	„ -	23,210	Plague broke out at Pali six or seven days after dead rats were found.	6 or 7 days.	
54	McCloghry	114,224	A woman in Karachi, living at a distance from any infected quarter, developed plague four or five days after dead mice were found in her house, no other source of infection being traceable.	5 days.	
55	Anderson -	23,210	Three days after the people of Joo returned to their houses, which were evacuated owing to dead rats being found, plague appeared among them.	3 days.	
56	Hay - -	22,223	In Areh dead rats were found on the 1st and 2nd of October 1898, in two houses; on the 3rd of October nine cases of plague occurred in both houses and in the adjoining houses.	2 days.	
57	Petigara -	14,363	On the fourth day after dead rats were found in the Karad Post Office, the Postmaster's daughter was attacked.	4 days.	
58	Hay - -	22,223	In Bhiwandi plague broke out in a house seven days after dead rats were found there.	7 days.	
59	Weir -	1180	On the 29th of November 1897, an old woman got plague in Bombay. Rats had been found in her house on the 19th of November and again on the 24th. On the former occasion the old woman is said to have thrown out the dead bodies.	10 days.	
60	Gcdinho -	17,996	Eight days after a listless rat was found on the 5th March 1898, a servant boy, who is said to have killed and burnt a rat, was attacked with plague.	8 days.	
61	Hyde-Cates	13,456	After the evacuated population of Bada had been free from plague for 17 days, they were, on the occurrence of rain, allowed into their houses for four days. Immediately after they came out again into camp four cases of plague occurred among them.	4 days.	
62	Steen -	22,737	After evacuation had stopped the plague in Pademal, people were, on the 29th of June, allowed to re-visit their houses for disinfection, and on the 4th of July cases began again.	6 days.	

Serial Number.	Authority.	Reference.	History of Case.	Length of Incubation, as deduced from History may have been less than, but cannot have exceeded	Remarks.
63	Giles -	11,271	A disinfected family that re-occupied a disinfected house had four cases of plague (including one death) within five days of re-occupation.	5 days.	
64	„ -	11,266	An infected room was left vacant till the 15th of April 1898. A man then went to live in it and got plague on the 18th; and later on, on the 14th of May, another man, who had used the place for cooking for two or three days only, got plague there.	3 days. 2 or 3 days.	
65	„ -	11,276	A Prostitute who, about the 23rd of April 1898, re-occupied a disinfected house, developed plague four days later.	4 days.	
66	„ -	„	On the 20th of April 1898, a disinfected and vacated house was re-occupied; plague occurred there again on 24th.	4 days.	
67	Niblock -	12,380	A Policeman died of pneumonic plague in the lines at Karachi on the 18th of April 1898. On the 26th of April, a second Policeman, who had wounded his right foot at noon, was admitted to hospital at night. He developed plague next morning, bubo developing on the right side. It was supposed that the infection was contracted through the wound on the foot.	24 hours.	
68	„ -	12,387	A phthisical patient, who had been in the Civil Hospital, Karachi, since the 12th of January, suddenly developed plague on the 6th of February. It was supposed that he had contracted the infection from a a case admitted on the 1st of February, that case having developed buboes and died on the 4th.	5 days.	
69	Cornwall -	4316–9, 4323–5.	A patient came to hospital and was there two days till he died. A friend came from an uninfected place and nursed him from the time of his admission. She and her clothes were disinfected when his death occurred. She fell ill of plague three days after the death.	5 days.	
70	—	—	Dr. Müller, a Member of the Austrian Plague Commission, who was in attendance from the 15th to the 18th October 1898, on a laboratory servant, Barisch, suffering from plague pneumonia, fell ill of plague pneumonia on the evening of the 20th of October. The case occurred at Vienna, where there was no other case of plague at the time, the first patient having contracted the disease in the laboratory.	5 days.	
71	—	—	Nurse Pecha, who attended the servant above mentioned, was also taken ill on the 20th October, after having been in contact with the patient for five days.	5 days.	

Inferences derived from consideration of cases in Class II.

194. We may most conveniently deal with the evidence collected together in the above table if we ask ourselves how far it supplies us with data for answering, first the question as to what is the shortest incubation period; secondly, as to what is the average length; and, thirdly, as to what is the extreme length of the incubation period in plague.

In connexion with the first question, it will not be necessary to do more than point out that three of the histories incorporated in the present table seem to afford confirmation of the fact, already established above, that plague may develop possibly within 24, and certainly within 48, hours after exposure to infection. Similar instances of incubation periods, not exceeding 24 hours, are, we may note, recorded in connexion with mahamari in Kumaun.

Para. 367 of this Report.

In connexion with the question of the average duration of the incubation period, a reference to the fifth column of the table will show that in a majority of cases, even counting in the time which must in many cases have intervened between first exposure to the infection and the contraction of the illness, the incubation period works out as shorter than five complete days. This, however, does not definitely resolve the question as to the average duration of the incubation period in plague, for manifestly cases of short incubation are more likely to arrest attention and therefore to be brought upon the record, to the exclusion of cases in which the period of incubation was longer. It is impossible to be sure that this source of fallacy has been entirely excluded, but we think that, even making allowance for it, it cannot have operated so as to falsify the record to any considerable degree. In confirmation of this opinion, we would point to the epidemic at Backergunge, which has supplied us with a considerable number of cases for fixing the duration of the incubation period. Here all the cases without exception have been brought upon the record, and there is among them no single instance of any long incubation period. This remark applies also to the cases which occurred among the disinfectors at Mahlgahla, subsequent to the suspension of the disinfection operations here, as we have seen, no case developed more than five days after exposure to infection ceased. And we would in particular, in this connexion, lay stress upon the circumstance that we constantly found that plague officers in India were unconsciously biased by the dictum of the Congress of Venice, which fixed the duration of the incubation period of plague for quarantine purposes at 10 days. We think that, under the influence of the unconscious bias just referred to, there would naturally be a tendency on the part of many plague officers, on the one hand, to notice and to record cases where the incubation period corresponded with the 10 day period of the Congress of Venice, and, on the other hand, to omit from recording, as in probability fallacious, cases which seemed to point to a shorter incubation period. We therefore think that the extreme limit of the period of incubation in an average case of plague probably lies well within the limit of five days, which is arrived at by a consideration of the cases collected in the table above.

Lastly, as to the question of the extreme limit to which the period of incubation may in exceptional cases be prolonged, it will be unnecessary at the present moment to say more than that data for determining this question cannot possibly be afforded by cases such as those now under consideration, where the patient has lived continuously in the presence of infection. Thus, for instance, it is manifest that the fact that periods of 8 and 10 days supervened in certain of the cases here recorded between the *first* exposure to infection and the invasion stage of the disease cannot be regarded as evidence that the incubation extended over the whole of the periods in question.

Class III.

195. We may pass from this to a consideration of the cases coming under Class III.

CLASS III.

Cases where Persons were removed from Infected Surroundings into Evacuation and Segregation Camps.

Reference.	Place.	Total Cases in Camp.	Number of Plague Attacks on successive Days after Arrival in Camp.										Remarks.
			1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	After 10.	
20,442	Bombay - - -	146	46	35	19	16	11	10	7	1	—	1	2	
17,367	(Wari Bandar. Mr. Shikari's Camp.)	166	52	35	20	21	12	14	7	1	—	1	3	Total in evidence, 167 wrong.
	Poona:—													
21,835	(1st and 2nd epidemic	146	21	20	17	22	22	15	7	6	8	2	6	
25,827	3rd epidemic to March)	14	2	3	3	4	1	1	—	—	—	—	—	
14,182	Anand - - -	78	32	1	17	9	7	1	6	2	1	1	1	
1,291														
25,030	Civil and Military Station, Bangalore.	75	26	16	5	5	8	1	3	1	7	3	—	
1,926	Dharwar - -	50	5	11	8	6	5	1	1	4	1	7	1	
23,693	Gadag - -	34	1	6	5	6	10	2	2	1	1	—	—	
1,291	Bhusawal - -	26	25	—	1	—	—	—	—	—	—	—	—	
14,215	Ankleshwar - - -	8	—	1	—	—	4	—	1	—	—	—	2	
13,722	Porbandar - - -	5	—	—	1	—	2	1	1	—	—	—	—	
- -	Total - -	753	210	128	96	89	82	46	35	15	18	15	15	

Inferences derived from a consideration of figures tabulated in Class III.

The question as to whether we can learn anything from cases in Class III., as to the shortest duration of the incubation period, can be immediately answered in the negative, when in the light of the information already obtained, we consider the fact that nearly half (338 out of the total of 753) cases recorded in the table occurred on the first two days after admission to camp. It is obvious from this fact that a large proportion of the total patients attacked cannot have been removed to camp until they were in a somewhat advanced stage of their incubation. All deductions as to the minimum duration stage of plague drawn from the figures above would thus be grossly fallacious.

On the question as to the average duration of the incubation period of plague, the figures in the table are of somewhat greater value. It would seem probable, from the fact that the number of attacks in camp drops from 82 to 46 on the sixth day in camp, that the influence of removal from plague infected dwellings to camp made itself felt on that day. It is suggestive that the period after which the marked decline in cases begins corresponds in an approximate manner with the period of incubation as fixed on the basis of the trustworthy evidence considered above.

Lastly, with regard to the question of the extreme limit of the incubation period, it would, indeed, on first approaching such a table as that given above, seem justifiable to hope that it would afford for the determination of this important question data even more valuable than those afforded by consideration of the cases we have collected under Class I. This hope is, however, not justified: for as we shall see in a subsequent section of this Report, where we consider the question of segregation camps, there was, in most of the segregation and evacuation camps, unavoidably some opportunity for a moderate spread of infection in camp. Thus the cases occurring on the later days in camp may quite well have originated from infection contracted after admission to camp, and they cannot be adduced as evidence of the occurrence of plague after a prolonged incubation period.

Sections III. (2) and IV. (3) of Chapter VI. of this Report.

Criticism of cases alleged to have had exceptionally long incubation periods.

196. The same canons of criticism apply to the isolated statements of witnesses to the effect that they had, in their experience of segregation camps, obtained evidence that the incubation period might be very prolonged. The statement of Captain Thompson, I.M.S., that he had met with a number of cases in segregation camp which pointed to an incubation period of over 10 days, may readily be explained by reference to the possibility of the infection of these cases having been contracted in camp. The possibility of such infection was, indeed, admitted by Captain Thompson himself. Mr. Cappel's statement, that he had seen contacts in camp develop plague 18 days after they had been received into camp, is probably capable of a similar explanation. The only other specific cases which have been reported to us with a view to proving the existence of an incubation period of more than 10 days, are the cases brought forward by Major Evans, I.M.S., by Mr. R. N. Jadhav, and by Colonel McConaghy,

22,414, 22,475.

1904.

6888. I.M.S. and Mr. Bharucha. The case reported by Major Evans was that of a medical student, who developed plague 14 days after he had assisted at the post-mortem of a plague case; it, however, is not certain that the infection was contracted at the post-mortem in question, as the patient had in the interval been engaged in bacteriological
15,187. work in a laboratory where investigations on plague were being conducted. In the particular case adduced by Mr. Jadhav, a native groom is recorded to have developed
12,889–92. plague five weeks after he had come from Bombay to Baroda. In the case reported
21,658–63. by Colonel McConaghy and Mr. Bharucha, a Parsee child is said to have developed plague 14 days after it had arrived in Poona from Bombay. It will be obvious that in the two last cases infection may have been carried from Bombay in personal effects, and in the particular case of the Parsee child there is the further possibility that infection may have been contracted in Poona, since suspicious cases were apparently occurring in Poona, in and about the time when the child contracted the disease.

Conclusion that no case on record where a longer incubation period than five days has been established; qualification with regard to cases of pestis minor if such develop into typical plague.

197. While thus no facts have been reported to us which establish that the incubation period may be prolonged beyond the period of five days, the limit fixed by the consideration of the numerous cases detailed above under Class I., we think that it is a matter of importance to point out that where the plague first takes the form of pestis minor, and where afterwards that pestis minor develops, owing to some subsequent loss of resisting power, into typical bubonic plague, there is a possibility of a considerable interval elapsing between the time at which the infection was contracted and the time at which the clinical symptoms become typical. Such cases, important as they are in the epidemiological study of the disease, as indicating one of the possible but quite exceptional ways in which plague may be carried about to distant countries over sea, are, it must be observed, of extremely rare occurrence. One such case has, however, come unofficially to our knowledge in Poona.

VI.—Epidemiological and Bacteriological Data regarding the Infectivity of Plague.

Infectivity of plague.

198. We propose now to deal with the question as to how far plague-infected persons and animals are directly infective, and as to how far the infection derived from infected persons and animals is harboured in the houses which have been inhabited or frequented by, and in objects which have been in contact with, such persons and animals.

VI.(1).—*Infectivity of Plague Patients taken generally.*

Data pointing to the slight infectivity of plague patients considered generally.

199. It will be convenient to postpone the consideration of differences in the infectivity of each of the different clinical types of plague taken separately until we have formed an estimate of the degree of danger incurred in coming into contact with plague patients taken generally. We are enabled to form such an estimate by availing ourselves of the data furnished to us with regard to the communication of the disease in hospitals to the staff and friends in attendance on the sick.

We are told, with regard to the principal Plague Hospital in Bombay—the Arthur Road Hospital—that of the large staff, not only of regular hospital attendants but also
20,989, 20,973. of relatives in attendance on the sick, only seven contracted plague. One of these was a relative in attendance. The remaining six were members of the hospital staff, who, according to Dr. Choksy, the physician in charge, lived in plague-infected houses outside the precincts of the hospital. If Dr. Choksy is correct in this, it would follow that in the case of this hospital, where nearly 4,000 plague cases were treated between September 1896 and February 1899, the disease could not in any single instance be brought home to infection from a patient.

We find from statements published in General Gatacre's Report on the Plague in Bombay that in the Grant Road Hospital, from the 4th March to 15th June 1897, there were no cases of plague among the 400 friends who either visited the patients or remained constantly at their bedsides. One attendant, however, contracted the disease. In the same hospital, it appears from statements published in Sir James Campbell's Report on plague in Bombay, there were in the last five months of 1897 and the first three of 1898, only two cases of plague among the relatives in attendance on 2,109 patients; among the staff of the hospital there were five cases.

Captain G. S. Thompson, I.M.S., summarized his experience gained in the Parel
22,489. Plague Hospital at Bombay in the following statement:—"In upwards of 240 instances the friends of the patients attended their sick, ... and in not a single

instance did the disease spread to the friends. Out of more than 140 attendants on the sick belonging to the hospital staff from time to time, only one sweeper was attacked, and he had been constantly helping in the post-mortem room, and had a very mild attack with small axillary bubo." Captain Thompson placed before us information which he had collected also with reference to five other smaller plague hospitals in Bombay. In these five hospitals there were in the aggregate 104 attendants, of whom only two contracted plague. In both these cases, it was believed that the disease was not contracted in hospital.

Major Deane, R.A.M.C., who had experience of Bombay hospitals, observed the absence of cases of infection among the staff and the attendants. 2789.

In Poona, at the General Plague Hospital, during the period covering the last six months of 1897 and the first three of 1898, no cases of infection came to notice among a total of 4,138 relatives in attendance on the sick; 26 cases occurred among the hospital staff, numbering 376. In the Hindu Hospital at Poona, during 2½ months beginning on the 15th March 1897, there were 2 cases among 104 persons who came to attend the sick: again, during six months in the second epidemic, 2 cases occurred among 533 friends who visited the sick, while 2 additional cases occurred among the members of the hospital staff, who numbered 57. 21,539, 12,876.

In the Hubli Hospital, only two cases of infection among the actual hospital attendants, who were on duty both night and day, came to the notice of Mr. Cappel. 1704.

In the Dharwar Hospital, where 740 patients had been treated up to the beginning of December 1898, there had at that date been six cases of plague among 31 hospital attendants, and 30 or 32 cases among relatives in attendance. These last numbered in all about 900. Many of these last may, it was suggested, have been infected before they came into the hospital. 1851. 1900, 1854. 1953. 1684-5. 1896-9.

In the Gadag Hospital, out of the relatives and friends in attendance on the sick, whose number has been estimated as approximately 250, three were attacked by plague. It is stated that these three patients probably contracted the disease outside. Of the hospital staff, who numbered 23 persons, none contracted plague. 23,685-8. 23,689-91. 23,757.

In Bangalore Civil and Military Station, we learn that of 61 attendants employed at different times in the North Camp Hospital, four, up to the beginning of December 1898, contracted plague. In the South Plague Camp, no member of the hospital establishment was attacked. 2737, 2922. 2923.

In Cutch Mandvi, Major Hyde-Cates told us that, in 1898, there were eight cases among the relatives of the patients, and that there were six cases among the hospital staff; it does not, however, appear whether in this last figure are included two cases of plague recorded by Captain Mason, R.A.M.C., as having occurred among the hospital staff in 1897. 13,533-5. I. 351.

In Surat, we were told by Lieut.-Col. Nariman, I.M.S., "In the General Plague Hospital, with the exception . . . . of a civil medical pupil, none of the hospital establishment was attacked with plague. Out of about 900 attendants who lived in the hospital to look after their relatives, only 12 were attacked with plague. Of these 8 were attacked almost within 24 hours of admission, 2 within 48 hours, and 1 each on the 10th day and 20th day respectively." The statements made as to the number of attacks among the staff and attendants and relatives in the Hindu Hospital at Surat are conflicting. 15,796. 15,824-5. 16,149.

In Porbandar there were two cases among the hospital staff and relatives in attendance on 465 cases of plague. 13,742. II. 484.

In the Sirohi State there were eight attacks of plague among persons who attended on 140 patients, each patient being allowed one attendant. 9906.

In none of the hospitals in Calcutta did any spread of infection in hospital come to notice. 7020; 7081.

Neither in Umreth nor in Broach was there any spread of infection in hospital. II. 233, 15,500.

Inferences derived from consideration of above statistics.

200. In spite of the fact that it is quite probable that all cases of plague occurring among the attendants on the sick in hospitals were not brought on to the record, the above figures show clearly that the danger of direct infection from plague patients is very small, under conditions such as those of hospitals in India, where attention is paid to the proper ventilation and cleanliness of the buildings, where the floors are, in a considerable number of the hospitals, periodically treated with disinfectants, and where precautions, such as the wearing of shoes are, as far as possible, insisted on. It must further be noted, as enforcing what has been said above with regard to the smallness of the risk that is incurred by direct contact with the sick, that in the hospitals under consideration personal disinfection was entirely neglected by the large majority of the staff, and that the

relatives attending on the sick rarely took even the most elementary precautions against infection. Not only did these last remain night and day with the patients, but they, in many cases, ate and drank out of the same vessels with them—not to speak of the fact that they, in some instances, received the sputum of the sick with their hands.

Confirmation of above inferences by experience of plague officers. 1439; 1516.

201. We may further, in connexion with the risk of direct infection from the plague patient, quote the conclusions arrived at by medical officers and others whose experience entitles them to speak on the question. Captain Childe, I.M.S., basing his opinion on the fact that the many unrecognised cases of plague, which had at the beginning of the outbreak in Bombay been admitted into the general hospitals, had not led to any spread of the infection, was of opinion that "in a well ventilated house or hospital the infectiousness is very slight indeed." On their experience in the Punjab that infection does not spread from cases treated in hospital, Captain James, I.M.S., and Captain Wilkinson, I.M.S., formed an opinion similar to that of Captain Childe. Lieut.-Col. Weir, I.M.S., Medical Officer of Health of Bombay City, expressed the opinion that plague is not very infectious from person to person. Lieut.-Colonel Henderson, I.M.S., thought that "the person to person infection is very rare indeed." Lieut.-Colonel McConaghy, I.M.S., thought that plague was not very infectious. Dr. Blaney's experience was that there is very little conveyance of infection from one person to another. This was also the experience of most of the witnesses who expressed an opinion on the subject—we may refer to the evidence of Dr. Clemow, Mr. Shepherd, Mr. Ghanekar, and Khan Bahadur Bomanji Biramji Patel.

10,374–5, 10,488.

1165. 18,146.

12,876. 17,722.

3243; 18,789; 22,554; 25,229.

VI.(2)(*a*).—*Epidemiological Facts bearing upon the Infectivity of Pneumonic Plague.*

Infectivity of clinical forms separately considered. Primary pneumonic plague—epidemiological and clinical evidence; very infectious, disease tending to reproduce itself in same clinical form. 15,182–5. 530, 13,366. 18,213.

202. In particular cases, however, a high degree of direct infectivity has been noticed; the cases in point appear in almost every instance to have been cases of primary pneumonic plague.

In the village of Gangaur, in the Baroda State, 37 fatal cases of plague originated from a case which was, from the history, inferred to be a case of pneumonic plague. Of these 37 cases, 13 occurred among the relatives of the man who was first attacked. Most of the other cases occurred among the friends who attended the funeral.

At Muska, in the Cutch State, a woman who, from the symptoms described, appears to have been suffering from pneumonic plague, communicated the disease to her sister, who attended upon her. This sister on her return to her village infected 10 other members of the family.

Mr. Shroff, a Deputy Health Officer of Bombay City, instanced the case of a lady who suffered from pneumonic plague and who communicated the same form of plague to two persons who came into contact with her. He also told us of a case of pneumonic plague in a Hospital Assistant which gave origin to 10 further cases of pneumonic plague, three or four of these subsequent cases occurring among persons who attended the funeral of the original patient.

18,209–12.

17,990–1.

Mr. Godinho, also a Deputy Health Officer of Bombay City, cited the case of pneumonic plague occurring in a Resident Surgeon in the Mahratta Hospital, Bombay, which gave origin to eight further fatal cases of plague.

Annales de l'Institut Pasteur, Oct. 1898, p. 681.

Dr. Simond cites the case of a sepoy at Karachi who died of plague pneumonia. Of 12 of his comrades who visited him when moribund and attended his funeral, seven developed plague within three days of the funeral.

12,997–8.

At Manora a case of pneumonic plague in a Muhammadan gave origin to four further cases among the co-inhabitants of the house. One of these cases was of the bubonic form and three were cases of pneumonic plague.

15,454–8.

At Baroda four cases of pneumonic plague were observed by Major Sarkies, I.M.S.; these all occurred quickly one after the other, and were due to infection conveyed from one to the other.

1891–2.

In the Dharwar Hospital four of the six attendants who were attacked suffered from pneumonic plague, and from the evidence of Dr. Hornabrook it would appear probable that they contracted the disease from patients who had the pneumonic form of plague.

9523–33.

A case of pneumonic plague, which occurred at Kankhal, gave rise to three further cases, all these occurring in the same family. These were the only cases of pneumonic plague which came under observation at Kankhal.

9533–5.

A case of pneumonic plague, which occurred at Jawalapur, was traced to infection contracted from a pneumonic patient.

18,509.

Mr. Britto cited a case of pneumonic plague from which two persons were infected, one of these being an attendant on the sick person.

Major Manser, I.M.S. contracted plague in the pneumonic form. The infection spread from him to the European nurse who was in attendance upon him. The nurse contracted the disease also in the pneumonic form. 1439.

Another example of the same sequence of events was furnished by the series of plague cases that occurred in Vienna. Here a laboratory attendant, Barisch, contracted the pneumonic form of plague. The infection then spread to his medical attendant and to a nurse. Dr. Müller and Nurse Pecha, the doctor and the nurse in question, both contracted the disease in its pneumonic form. 7220 (17), (22), and (58).

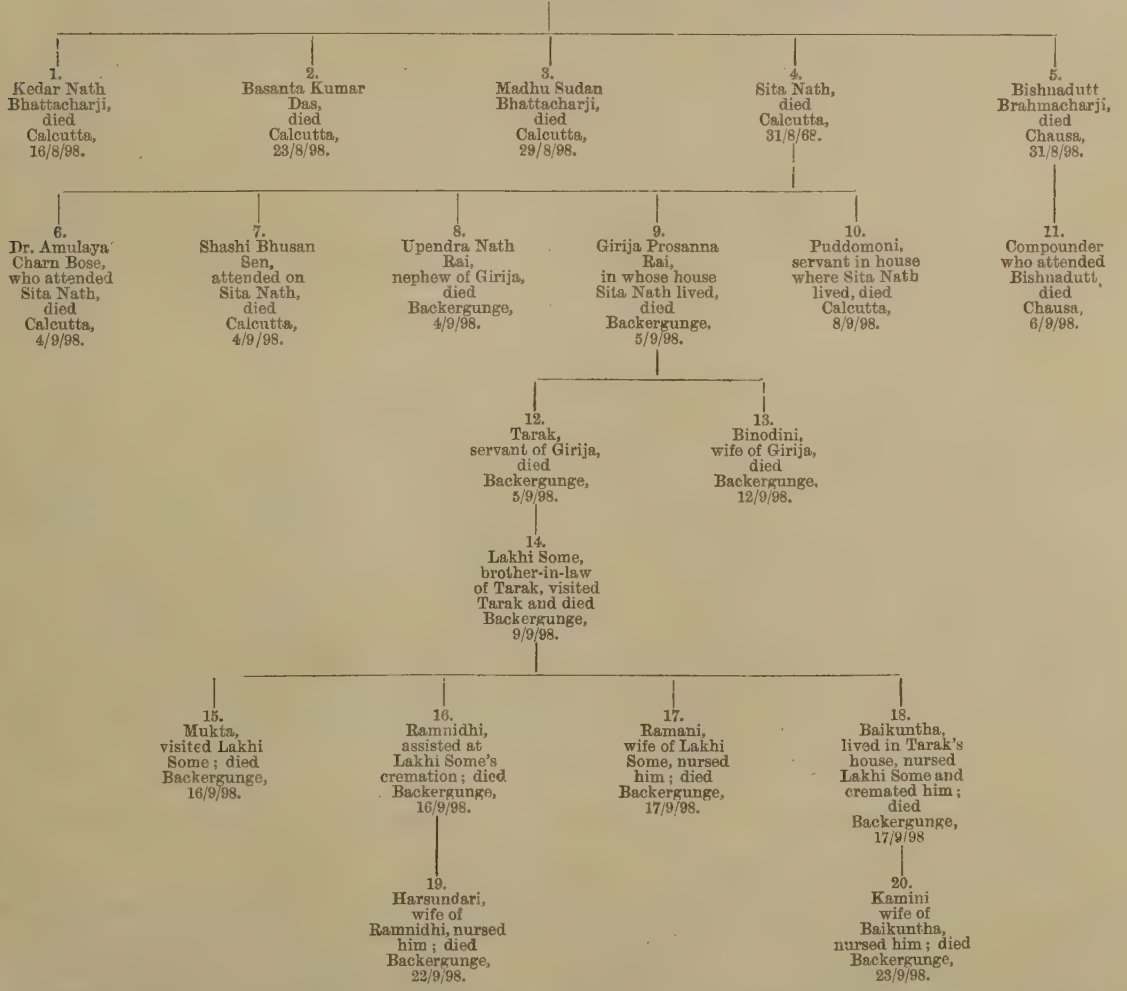
A nurse in Poona contracted plague in the pneumonic form two or three days after she had come into attendance on a patient suffering from this form of the disease. 7576.

In Calcutta, a case came under observation in which a person in attendance upon a patient suffering from plague pneumonia contracted the disease in the same form. 7515.

The above instances bring out with clearness the infective nature of pneumonic plague, and the risk that is run by those who are in attendance or contact with cases of this type. They also very strongly suggest that the infective material derived from a case of pneumonic plague tends to reproduce the disease in the same form. The facts detailed below with regard to an epidemic of plague pneumonia which occurred at Backergunge confirm these conclusions in a very striking manner.

203. The details of this epidemic, which originated in Calcutta in the house of Kaviraj Dwarka Nath, a lecturer on medicine, are given below in the form of a table which we have prepared from the data supplied to us by Major Green, I.M.S., and Dr. Justice :— History of the Backergunge epidemic of plague pneumonia.

ORIGINAL SOURCE OF INFECTION IN THE HOUSE OF KAVIRAJ DWARKA NATH IN CALCUTTA.


1. Kedar Nath Bhattacharji, died Calcutta, 16/8/98.
2. Basanta Kumar Das, died Calcutta, 23/8/98.
3. Madhu Sudan Bhattacharji, died Calcutta, 29/8/98.
4. Sita Nath, died Calcutta, 31/8/68.
5. Bishnadutt Brahmacharji, died Chausa, 31/8/98.
6. Dr. Amulaya Charn Bose, who attended Sita Nath, died Calcutta, 4/9/98.
7. Shashi Bhusan Sen, attended on Sita Nath, died Calcutta, 4/9/98.
8. Upendra Nath Rai, nephew of Girija, died Backergunge, 4/9/98.
9. Girija Prosanna Rai, in whose house Sita Nath lived, died Backergunge, 5/9/98.
10. Puddomoni, servant in house where Sita Nath lived, died Calcutta, 8/9/98.
11. Compounder who attended Bishnadutt, died Chausa, 6/9/98.
12. Tarak, servant of Girija, died Backergunge, 5/9/98.
13. Binodini, wife of Girija, died Backergunge, 12/9/98.
14. Lakhi Some, brother-in-law of Tarak, visited Tarak and died Backergunge, 9/9/98.
15. Mukta, visited Lakhi Some; died Backergunge, 16/9/98.
16. Ramnidhi, assisted at Lakhi Some's cremation; died Backergunge, 16/9/98.
17. Ramani, wife of Lakhi Some, nursed him; died Backergunge, 17/9/98.
18. Baikuntha, lived in Tarak's house, nursed Lakhi Some and cremated him; died Backergunge, 17/9/98
19. Harsundari, wife of Ramnidhi, nursed him; died Backergunge, 22/9/98.
20. Kamini wife of Baikuntha, nursed him; died Backergunge, 23/9/98.

The series of cases, it will be seen, originated in the house of Kaviraj Dwarka Nath, into which the infective material must by some channel or other, which has not been ascertained, have been introduced. Five persons in this house contracted pneumonic plague. Two only of these cases, those of Bishnadutt and Sita Nath, gave origin to further cases. We may deal first with the case of Bishnadutt. This man fled from Calcutta and was seized with symptoms of pneumonic plague at the railway detention camp at Chausa. He died there, after communicating fatal infection to the Com- 6836, *seq.* 7331, *seq.*

pounder who attended him. Sita Nath, whose name appears fourth in our table, was the other patient who was instrumental in the spread of the infection. He communicated the infection to five persons who came into contact with him, and all these persons died of pneumonic plague.

7067. One of these derivative cases (No. 6 in our table) was seen on four occasions by Lieut.-Col. R. C. Sanders, I.M.S., who formed the opinion that the case did not differ from any ordinary case of pneumonia. Lieut.-Col. Sanders based his opinion on the fact that the patient remained rational, and that he did not communicate the infection to any of the numerous persons who visited him. We think this is hardly sufficient ground for concluding that this was not a case of plague pneumonia, for consciousness is in cases of plague pneumonia often preserved to the last, and, fortunately, cases of plague pneumonia do not always give rise to further cases. Again, the patient seems to have been haunted throughout, even when his symptoms were comparatively slight, by a fixed premonition of death, such as has often been noticed in connexion with cases of plague pneumonia.

To return to our table. Only one of the cases derived from that of Sita Nath spread the infection. It will be seen that the man numbered 9 in the table, and placed fourth in the second generation of cases, contracted pneumonic plague and communicated the infection to his wife and to his servant, Tarak. Both of these died of pneumonic plague. The infection passed from Tarak through his brother-in-law, Lakhi Some, who also succumbed to the pneumonic form of the disease, to four persons, all of whom either visited the sick man or attended his cremation. These again all died of pneumonic plague, two of them handing on the infection to their wives, who, in their turn, contracted plague in a pneumonic form, and succumbed.

Thus we have, in all, twenty cases of plague, all of the pnemonic form, and all of which terminated fatally. This observation is, in our opinion, one of unique value. It derives its importance, first, from the fact that even in the absence of bacteriological information (the material which was designed for baeteriological examination accidentally miscarried), the diagnosis of plague is rendered almost certain by the extraordinary fatality of the attacks; secondly, from the fact that in view of the small amount of plague at that time in Calcutta, and of its entire absence from the neighbourhood of Backergunge, the genealogy of each case in the series seems to be securely established ; and, lastly, from the illustration which is here afforded of the tendency of pneumonic plague to reproduce pneumonic plague through a series of generations.

7473.

Opinions, embodying experience of witnesses, that pneumonic plague eminently infectious.
504.
1439.
9586.
10,201.
4370.
19,201–4.
14,717–9.
17,788.
11,163.

204. If anything more were required to confirm the highly infectious character of pneumonic plague, this would be supplied by the experience of witnesses who have been engaged in plague duties. Thus Lieut.-Col. Wilkins, I.M.S., said that he had many instances in which this type of plague had given rise to other cases. Captain Childe, I.M.S., said : "I think in many circumstances the pneumonic form is highly infectious." Captain Elphick, I.M.S., said : "I think the pneumonic cases are the infectious cases." Captain James, I.M.S., thought that the pneumonic form of plague was highly infectious. Lieut. Cornwall, I.M.S., expressed the opinion that the pneumonic form undoubtedly spreads the disease most. Dr. Freeman Underwood and Dr. Maynard thought pneumonic plague extremely infectious, and noted that pneumonic cases produced cases of the same type. Mr. Ismail Jan Mahomad also said that he had noticed that pneumonic cases gave rise to pneumonic cases. Mr. Giles referred to this type of plague as "known for its spread of infection."

VI.(2)(*b*).—*Bacteriological Facts bearing upon the Infectivity of Pneumonic Plague.*

Bacteriological data bearing on the infectivity of pneumonic plague.

205. The extreme infectivity of the primary pneumonic form of plague might have been concluded from the fact that the bacteria in this form of plague cultivate themselves in the lungs, and are thrown off in enormous quantities in the sputum. Another point with regard to the infectivity of pneumonic plague which has been brought out by bacteriological observation is that the bacteria continue to be thrown off in the sputum for prolonged periods. Dr. Cyril Cayley has furnished us with particulars with regard to this point relating to 17 out of 43 cases of plague pneumonia which he examined :—"As a rule, I found plague bacilli in the sputum for a fortnight after the lung symptoms developed, but there was one case which was 28 days, three days after the temperature was normal. . . . . In 13 cases the plague bacilli were found in periods varying from 10 to 14 days, in one case 15 days, in one case 19 days, and in another case 23 days." It would seem probable that many, at least, of the

25,334–5.
25,342.
25,346.

cases examined by Dr. Cayley were cases of secondary pneumonia. In cases of primary pneumonic plague (and we shall presently see that this holds true also in connexion with secondary pneumonic plague) plague bacilli may be found in the sputum long after convalescence has to all appearance been established. A case of primary plague pneumonia which illustrates this point has been published in detail by Dr. E. Gotschlich. In the case in question plague bacilli were, by means of inoculation experiments, shown to be present in the sputum for a period of 33 days after the temperature had returned to the normal. When they finally disappeared the patient had long been to all appearances perfectly well, and had been out of bed for 19 days.

Zeitschrift für Hygiene, 1899, p. 405.

VI.(3)(*a*).—*Epidemiological Facts bearing upon the Infectivity of Septicæmic and Bubonic Plague.*

206. With regard to the infectivity of septicæmic plague, no separate information is available; we may, however, refer to the opinions expressed by Lieut. Cornwall, I.M.S., and Lieut. Niblock, I.M.S., that this form of plague may be highly infectious, though death generally supervenes so rapidly that septicæmic cases do not contribute much to the spread of infection.

Infectivity of the septicæmic form of plague. 4371. 12,481.

The bacteriological data with regard to the infectivity of this type of plague will best come up for discussion when we deal with the septicæmic stage of bubonic plague.

207. We have not been furnished with any clear instance of infection having been contracted directly from a bubonic case,—the agency by which infection has spread being in the case of bubonic plague generally the soil of the patient's house or his clothes. Instructive in this connexion are the following opinions of witnesses who have had great experience of plague:—

Infectivity of the bubonic form of plague.

Lieut.-Col. Wilkins, I.M.S., said that he could not call to mind any bubonic case that had infected another case.—Lieut.-Col. Weir, I.M.S., thought that "the simple glandular form of the disease is practically not dangerous."—Major Dyson, I.M.S., said that the bubonic form of plague was not very infectious.—Captain Elphick, I.M.S., said, "I have never seen any other cases except pneumonic cases where anything approaching to infectivity appeared to exist."—Mr. Ismail Jan Mahomad thought that the bubonic type of plague did not convey infection by mere contact with a patient, and Lieut. Cornwall, I.M.S., noted that this type could be communicated only by the evacuations.

504–8. 925, 1165 19,964. 9586. 17,789–90. 4372–3.

VI.(3)(*b*).—*Bacteriological Considerations and Facts relating to the Escape of the Plague Bacillus from the Infected Organism in Septicæmic and Bubonic Cases.*

208. From the point of view of their early and continued infectivity, we may perhaps place in the same category as primary plague pneumonia those cases of bubonic plague where the symptoms point to a primary affection of the mucous membrane of the nose, mouth, and throat. In these cases not only is there a possibility of plague bacteria continuing to cultivate themselves on the surface of the mucous membranes where they first effect a lodgment, but there is also the possibility of the bacteria being re-conveyed from the infected glands to the mucous membrane by a back flow of lymph. The only bacteriological observations which are at our disposal in this matter are the following. The plague bacillus was detected in the saliva in each of a series of five cases of parotid and sub-maxillary buboes examined by Dr. Cayley. We ourselves succeeded in isolating the plague bacillus from a sanious discharge obtained from the nose of a patient who was suffering from a sub-mental bubo.

Bacteriological considerations regarding infectivity of bubonic plague with primary affections of throat and nose

25,355.

26,259.

209. Passing now to the consideration of ordinary bubonic cases of plague, where infective material invades the organism through the skin, it will be manifest that where cutaneous lesions, in the form of phlyctenules and carbuncles, are developed at the site of inoculation, there is a remote possibility that such cases may be infective from the very outset. Cases where a local reaction marks the point of entrance of the bacillus are, however, as we have seen, rare, and the number of plague bacteria which would come into consideration is so small, that the danger of infection from such cases may be practically neglected. In the large majority of cases of bubonic plague, where the infective material is, as we have seen, carried directly from the broken surface of skin to the lymphatic glands, the plague bacteria for a time cultivate themselves only in these glands, and are therefore completely shut off from the surface. Thus in ordinary cases of bubonic plague there will always be a period during which the patient is non-infective. The non-infectious period will extend up to the time when the bacteria make their entrance into the blood.

Bacteriological considerations with regard to non-infectivity of ordinary bubonic plague in early stages

Bacteriological considerations regarding infectivity of bacterial diseases after septicæmic stage has been reached.

210. As soon as a general invasion of the blood stream has taken place,—and this invasion would seem to occur only in serious cases, and to occur in these for the most part only shortly before death,—it will be possible for bacteria to make their escape from the body through a variety of different channels. We learn from a general study of infective disease what the possible channels are through which bacteria may escape from the organism when a disease has become septicæmic.

The bacteria may, in the first place, become lodged in the capillaries of the skin and of the mucous membranes, with the result that they may give rise there to an exanthematous eruption or to some other form of inflammatory reaction. In connexion with these inflammatory reactions opportunity may be afforded for a throwing off of the bacteria from the skin. In the second place, bacteria may lodge in the capillaries of the respiratory tract—in particular, the lungs—with the result that pneumonic symptoms may be set up, which would be associated with a discharge of bacteria from the organism in the sputum. In the third place, bacteria may be conveyed into the mucous membrane of the intestinal tract, and thence into the fæces. In the fourth place they may be conveyed into the kidneys, and may then escape into the external world by means of the urine. Lastly, bacteria may escape by means of hæmorrhages—in particular, such hæmorrhages may occur from the lungs, the nose, the intestines, and the uterus.

Application of above considerations to the case of the septicæmic stage of plague—(i.) Escape of bacteria from specific plague rashes.

16,794, 20,873, 20,883, 21,020.

Annales de l'Institut Pasteur, Dec. 1899, p. 884.

211. When we come to consider the application of these generalisations to the particular case of plague, the following are, perhaps, the most important points:—

(I.) Cases where specific exanthematous rashes occurred in plague, though exceptional, were observed in Bombay and other places in British India; they were of extremely common occurrence in the severe epidemic which occurred at Daman. Such rashes, when they occur in plague, take the form of multiple ecchymoses, or, as in varicella and in smallpox, of bullæ or pustules appearing all over the body. In connexion with such cases there will manifestly be opportunity for the escape of infective material from the surface of the skin. In illustration of this point we may refer to the two following cases, which have been bacteriologically examined. In the first of the cases, that described by Drs. Calmette and Salimbeni, the whole body was covered over with pustules, carbuncles (presumably resulting from the breaking down of pustules), and black ecchymoses, "the appearance of the patient closely resembling that seen in hæmorrhagic smallpox. The pustules contained a sero-sanguineous turbid fluid, containing a few cells and enormous numbers of plague microbes, the latter being free in the fluid." The second case referred to above is recorded as follows in the official Report, p. 51. "Report on Plague in Egypt, from May 1899 to July 1900":—"In another case we observed a general eruption of pustules, closely resembling those of varicella, and distributed chiefly over the face and arms. The case was that of a woman admitted to hospital with a left axillary bubo and two small pustules on the left breast (these probably indicating the point of entry of the virus). The general eruption of vesicles appeared two days after the patient's admission. The bacteriological examination of the contents of the pustules revealed the presence of considerable numbers of plague bacilli. The bacilli were not to be found by microscopical examination, but came up in large numbers when cultivated on agar."

(ii.) Escape of bacteria from membranes of the respiratory system.

(II.) In connexion with the conveyance of plague bacteria into the mucous membranes of the nose, mouth, and throat, the possibility must be kept in view that the bacteria may escape from the surface of these mucous membranes. Much more important, however, is the carrying of the bacteria into the lungs, and the setting up of an infectious secondary pneumonia. The development of such secondary pneumonia has been a very frequent feature in certain plague epidemics. In Bombay in the early months of 1899, about two-thirds of the cases of bubonic plague showed pulmonary complications, varying from slight bronchitis to pronounced consolidation. Even where lung symptoms were little marked, bacteria possessing the morphological and staining properties of plague bacteria were found in the sputum. We found them, for instance, in seven out of a series of 13 cases chosen among those in whom the lungs seemed to be least involved. It would thus seem probable that the sputum must, in a considerable proportion of bubonic cases, be infective. The degree, however, to which the lungs become involved would appear to be very inconstant.

26,256.

21,042.

(iii.) Escape of bacteria into fæces.

(III.) The next point to be considered is the conveyance of the bacteria into the capillaries of the intestinal wall, and thence into the fæces. Investigations have been

undertaken with a view to determining whether plague bacteria could be isolated from the fæces of plague patients. The German Plague Commission were uniformly unsuccessful in their attempts to isolate the plague bacillus from the fæces by inoculations and cultivation methods.—The Austrian Plague Commission were equally unsuccessful; but, in reconsidering their results, they expressed themselves as not altogether satisfied with the technique employed in their examination of the fæces of plague patients.—Dr. Cayley made four examinations with negative results. In a fifth case, where the fæces consisted of almost a pure mucus, he was able to detect the plague bacillus.—Dr. Gibson examined the fæces and obtained only negative results.—We ourselves examined the fæces in four cases, and obtained negative results.—Dr. Shrinavasa Rau, on the other hand, claims, by Mr. Hankin's method, to have isolated the plague bacillus in three out of four samples examined by him.

Report of German Plague Commission, p. 273.
Report of Austrian Plague Commission, Part II. B., p. 544; Part II. C., p. 633.
25,363–4.
17,271.
26,391
3564.

If we omit from consideration Dr. Shrinavasa Rau's results as results at present quite unconfirmed and obtained by a method which, as we have already said, seems to us to be untrustworthy, and if we further omit from consideration the case above referred to where the plague bacillus was isolated by Dr. Cayley from intestinal mucus, the results of the above series of examinations may be summarised in the statement that the plague bacillus has not as yet in any case been isolated from human fæces. Such a negative result is, however, of very little account in view of the fact that it is, as we have seen, a matter of extreme difficulty to isolate, by cultural methods, the plague bacillus from a material such as fæces, in which the bacillus coli and an infinity of other bacteria are to be found. The question of the infectivity of the fæces must therefore be adjudicated upon by reference to other considerations than those derived from the bacteriological examinations referred to above. In particular it is to be noted in this connexion that, in a large proportion of the human cases which have been examined, the plague bacillus has been detected in the bile and the gall bladder. Further, it is to be borne in mind that punctate hæmorrhages are not uncommonly found in the stomach and the intestinal canal in man. It is thus probable that the plague bacillus must very frequently be present in the intestinal contents.

Para. 161 of this Report.
Para. 163 of this Report.

(IV.) We now pass from this to consider the conveyance of the plague bacillus into the kidneys, and from thence into the urine. A considerable number of observers have made investigations on this question. The German Plague Commission place on record the fact that they found plague bacteria in the urine in two instances only (the number of their examinations is not specified).—The Austrian Plague Commission, owing to the contaminations which are nearly always present, entirely failed to find the plague bacillus in urine drawn off during life. They succeeded, however, in detecting the bacillus in 5 out of 17 samples of urine collected in the course of their post-mortem examinations.—Dr. Cayley examined the urine in 15 cases of plague. His results were uniformly negative.—Dr. Gibson examined the urine in five cases; he discovered the plague bacillus in only one of these cases.—Dr. Shrinavasa Rau claims to have detected the plague bacillus not only in two out of four cases where he himself collected the samples, but also in five out of eight samples of urine, sent to him for examination as having been derived from cases of suspected plague; there is no suggestion that any aseptic precautions were taken in collecting these last samples.—We ourselves examined 60 specimens of urine by cultivation methods. In only three cases were we able to isolate the plague bacillus by this means, contaminating bacteria making their appearance even in the cases where the urine was drawn off with all possible aseptic precautions. In the case of five further samples we sought for the bacillus by inoculating the urine into the peritoneal cavity of guinea-pigs. In one of these cases where the inoculated animal died, we succeeded in isolating the plague bacillus from its organs after death.

(iv.) Escape of bacteria in urine.
Report of German Plague Commission, p. 273.
Report of Austrian Plague Commission, Part II. B., p. 547.
25,358–62.
17,261–2.
3558.
3647–9.
26,268, 26 336, 26,389.
26,268.

If we omit from consideration, as we are perhaps justified in doing, the observations of Dr. Shrinavasa Rau, we see that the plague bacillus can only very rarely be recovered from the urine. The difficulty arises from several causes. The first of these is that the urine, even when drawn off through a catheter with such aseptic precautions as are practicable, practically always contains contaminating bacteria. Secondly, the bacilli are probably, unless perhaps in the last hours of life, very sparingly present in the urine. Lastly, it would seem that the urine itself may contain substances which are inimical to the growth of the plague bacillus. This is suggested by the fact that in those cases where we succeeded in recovering the plague bacillus by cultivation methods, the colonies were very slow in making their appearance. The virulence of the bacilli which were isolated appears also, so far as we may judge from a single experiment on this subject, to have been considerably reduced.

26,337.

(v.) Escape of bacteria in hæmorrhages.

(V.) The question of the escape of the plague bacteria into the outer world by means of hæmorrhages from the mucous membranes does not require to be specially considered, it being obvious that hæmorrhagic discharges will, where the blood contains plague bacteria, necessarily contain the infective agent.

VI.(3)(*c*).—*Infectivity of Patients convalescing from Bubonic Plague.*

Bacteriological considerations regarding infectivity of bubonic cases during convalescence, on account of persistence of infection in :—

212. We pass now from the consideration of the infectivity of the plague patient during the septicæmic stage to the consideration of the additional possibilities of infection during the period of convalescence of bubonic patients. These possibilities are associated, on the one hand, with the suppuration of buboes and the discharge of pus which supervenes in a large proportion of the cases in which the disease takes a favourable turn, and, on the other hand, with the continued presence of plague bacteria in the sputum and saliva.

(*a*) Suppurating buboes;

The fundamental fact which we have to place before us in connexion with the suppuration of buboes is that the processes of suppuration go hand in hand with processes of destruction of pathogenic micro-organisms. Thus the question which we have here to consider is really the question as to whether a residuum of the plague bacteria which were originally present is still to be found in the bubo when pus comes to be discharged. A considerable number of examinations have been made with a view to determining this question. The German Plague Commission discovered the plague bacillus in two out of 16 samples of pus from suppurating buboes examined by them.

Report of German Plague Commission, p. 265.

Report of Austrian Plague Commission, Part II. B., 509–10.

—The Austrian Plague Commission obtained 7 positive and 3 negative results in 10 examinations of pus obtained from suppurating buboes; these buboes, however, had not opened spontaneously.—Drs. Calmette and Salimbeni state that suppurating and open buboes always contain the plague bacillus, characteristic colonies being invariably obtained from them by cultivation methods.—Dr. Cayley examined a considerable number of samples of pus from suppurating buboes. With these he obtained only negative results. From the parts of the bubo, however, which had not yet broken down, he succeeded in cultivating plague bacteria.

Annales de l'Institut Pasteur, December 1899, 879.

25,370.

The general conclusion to be drawn would appear to be that a residuum of bacteria is discoverable by cultivation methods in the pus from suppurating buboes. It is manifest from what has been said in previous sections, both with regard to the presence of contaminating micro-organisms in suppurating buboes, and also with regard to the difficulty of isolating the plague bacillus in the presence of such contaminating bacteria, that plague bacilli must be present in an even larger proportion of cases than would appear on the consideration of the results of the bacteriological examinations which have been detailed above. No bacteriological observations are available which would allow of our fixing the extreme limit of time within which plague bacteria may be found in the pus of suppurating buboes.

(*b*.) Sputum;

With regard to the length of time during which the sputum may continue infective, we may infer from two cases observed in Alexandria, and placed on record by Dr. Gotschlich, that this may be very considerable. In the first case, plague bacteria were detected in the sputum for a period of 48 days after the temperature had returned to the normal. In the second case, which was one of secondary pneumonia, following upon an inguinal bubo, plague bacilli were detected in the sputum for 20 days after the temperature had returned to the normal. The patients in the two cases in question had been allowed out of bed for 42 and 6 days respectively before the plague bacteria disappeared from their sputum.

Zeitschrift für Hygiene, 1899, p. 406.

(*c*.) Saliva.

Plague bacteria may escape from the body of a bubonic patient, during convalescence also by means of the saliva. This source of danger, which probably comes into consideration only in connexion with cases of buboes in the region of the jaws, is consequently of subordinate importance. To be noted here in connexion with it is merely the fact that Dr. Cayley in one of his experiments found the presence of plague bacteria in the saliva 10 days after the patient had, so far as the constitutional symptoms were concerned, recovered from an attack of plague associated with buboes in the region of the jaw. The glands, at the time when the examination was undertaken, were still in a swollen condition.

25,355

VI.(3)(*d*).—*Review of Bacteriological Evidence bearing on Infectivity of Patients suffering from Bubonic or Septicæmic Plague.*

Summary.

213. We have seen above that the ordinary bubonic case may be considered as non-infective until the time when the septicæmic symptoms supervene; from that time

onwards the plague patient will become infective by the fact that the plague bacilli escape from the body in the discharges, on the one hand, from the nose, lungs, and intestinal tract, and, on the other hand, from the kidneys. This infective period will, in many cases, last only for a few hours; in other cases it may continue for a few days. In fatal cases, it will, perhaps, roughly coincide with the last 24 hours of the patient's life. In cases which turn out favourably a further possibility of infection will, as we have seen, be afforded by the fact that the pus from the suppurating bubo, as well as the sputum and the saliva, may contain the infective agent.

VI.(4). *Infectivity of Pestis Minor.*

214. In sequel to our discussion on the infectivity of ordinary cases of bubonic plague, we may consider the question of the infectivity of those clinically mild cases of bubonic plague which are classed as pestis minor. In connexion with the infectivity of these we have comparatively few actual observations to draw upon. We shall be compelled, therefore, to deduce our conclusions almost exclusively from bacteriological reasoning. Infectivity of Pestis Minor.

Dr. Choksy, speaking from a very extensive experience of plague, told us that he had never in his private practice seen any spread of infection from cases of pestis minor in houses where such cases had occurred. Two cases, however, one in Ahmednagar, and the other in the Punjab, have been brought to our notice in which plague infection is said to have spread from cases of plague so mild as to fall into the clinical category of pestis minor. No great weight attaches to the case reported from Ahmednagar, inasmuch as no details were given, and inasmuch as the witness who reported it was not a medical man. Considerable weight, on the other hand, attaches to the statement of Captain James, I.M.S., that the plague epidemic in Gobindpur, in the Punjab, originated from the case of a boy who had contracted plague when on a visit to a neighbouring village, and who had it in so mild a form that he was never obliged to take to bed. 20,575. 24,117–8. 10,099.

It will have become plain, in the course of our discussion of the question of the infectivity of bubonic plague in its different stages, that when the infective agent has found its way into the system through a cutaneous wound, and has afterwards become localized in a gland, then stopping short, as it does in the case of pestis minor, of a general invasion of the blood, the patient will, in conformity with the fact that the plague bacilli are entirely shut in within his body, be quite non-infective; this, of course, not applying to the case already considered above, where the infective micro-organisms have been absorbed from mucous membranes, in which they may still be cultivating themselves. If, however, the disease should develop into the severer form, or if suppuration and discharge of pus should occur during convalescence, opportunity for the spread of infection may be afforded. Neither of these contingencies is very probable, and when they do occur it is by no means certain that plague bacteria will escape from the body. In this connexion a case that occurred at Alexandria is very instructive, although the history as reported is not altogether free from ambiguities. A porter engaged in carrying luggage, which there was strong ground for regarding as infected, fell ill, and on the 5th or 6th of July went to Alexandria, an hour's journey by rail, to see a doctor. He was sent to the Government Hospital. He did not then present the appearance of a sick man, except for the fact that he had a temperature of 102° F. His tongue was clean, and his general condition was satisfactory. In his left inguinal region there was a small swollen gland which was hard and not painful to touch. The patient said that the gland had made its appearance 8 or 10 days before, and that he had had fever and headache for three days. The gland was punctured; the result of simple microscopic examination of the contents was negative, but cultures established the presence of plague bacilli. From the time of his admission the patient's temperature fell steadily and definitely reached the normal five days afterwards. Suppuration of the bubo took place, the pus being, in this instance, found to be free from plague bacilli. Report of Plague in Egypt, May 1899 to July 1900, p. 64.

VI.(5)(*a*).—*Epidemiological Facts regarding the Direct Infectivity of Rats.*

215. We have next to consider the possibility of human beings contracting plague by direct contact with infected animals. We need here consider the case of rats only. As far as other animals are involved, such facts as have been brought to our notice are dealt in the section of our Report where we consider the means by which plague is disseminated. Infectivity of rats.

The instances which have been brought to our notice where plague is said to have originated from direct contact with rats are both few and, separately considered, inconclusive. These instances are briefly as follows:—(*a*.) Five cases had come to the
19,020-27. knowledge of Dr. Bhalchandra Krishna, where persons in Bombay had developed plague three or four days after handling dead rats. The first four of these cases occurred during the epidemic of 1896, and particulars with regard to them were not to hand. The fifth was the case of a Parsee lady who lived near Girgaum. "She was," said Dr. Bhalchandra Krishna, "taking out rice for cooking, and a dead rat came on to the palm of her hand, and next day she got plague." The patient was under the witness' treatment, and he believed that she had acquired the disease through the uninjured epidermis of the hand by mere contact with the rat. But it must be remembered that, in addition to the disease being prevalent in the City, the house itself had probably been infested by plague-infected rats. It is, therefore, possible that the infection was otherwise acquired than by direct contact with the dead rat in question. (*b*) Mr. Katrak gave us the following history with regard to
17,926. the origin of a plague case which came under his observation. He said:—"A rat was found in one of the rooms. The patient, before she got the infection, took the rat and threw it out, and then with plain water she scrubbed the ground with her hands. In about six or eight hours she felt a sense of chilliness, then fever began, and she went through the whole course and died." He had heard of another case in which a porter was reported to have removed two or three dead rats from a house, and, "within five minutes." had a sense of chilliness, and subsequently developed plague.
16,513. (*c*.) A case occurred at Daman of a young woman placing her foot on a dead rat when she returned to a vacated house. That evening she was attacked by plague.
17,996. (*d*.) Dr. Godinho gave the instance of his own servant boy who developed plague eight days after playing with a dead rat. The witness believed that the boy could
11,418. not have otherwise been exposed to infection. (*e*.) Colonel McConaghy, I.M.S., informed us that a young apprentice fitter in the railway workshops at Karachi, on being ordered to remove a dead rat found in one of the drains, swung the rat about his head and played with it. Three days afterwards he was admitted into the Civil Hospital with plague. There was no plague in the neighbourhood of his house.
20,418. (*f*.) Major Dimmock, I.M.S., recited the case of a Goanese servant who removed a dead rat from a floor and burnt it. He was attacked with plague four days afterwards.
Annales de l'Institut Pasteur, Oct. 1898. (*g*.) Dr. Simond recounts that at the commencement of the plague epidemic in Bombay a considerable number of dead rats were found one morning in the storerooms of a spinning mill. Of the 20 coolies who were ordered to remove these rats, some 10 contracted plague in the course of the next few days, while the persons who entered the storerooms without handling the rats remained free from infection. Mr. Hankin,
Annales de l'Institut Pasteur, Nov. 1898, p. 722. to whom the facts were recounted by Mr. Wadia, the owner of the mill in question, gives the number of the coolies attacked as 12.

In connexion with these cases, where there are colourable grounds for supposing that contact with a plague infected rat may have been instrumental in the communication of the infection to man, there must be taken into account the infinitely more numerous instances in which the handling of dead rats has not resulted in any evil effects. As an instance of what was constantly happening we may cite here the
19,437-41. clearing out of granaries filled with dead rats. We were told by Professor Muller, in connexion with a particular granary which was cleared by him, that none of his gang of 30 coolies was attacked, in spite of the fact that no shoes were worn and
II. 334, para. 118. no special precautions against infection were taken. We hear, also, in Dr. Planck's Report on Mahamari in Kumaun of "certain children, all of whom have remained well, who, as a matter of pastime, cooked and ate one or more of the rats," found dead in the course of an outbreak of plague in the village of Bintola.

We have, in short in reviewing all the facts which have come to our knowledge, little reason to suppose that ordinary casual contact with plague-infected rats, dead or alive, is especially liable to convey plague.

Communication of infection by bites of rats. 1181. "Lancet," 1900. 216. The case is entirely different where we have to deal with a direct inoculation of infective material by the bites of infected animals. We may refer to a case at Bombay observed by Dr. MacCabe Dallas, where plague is said to have been communicated by a rat bite, and to a similar case reported in detail from Hong Kong. Incidentally, in connexion with the inoculation of plague by the bites of animals we may refer here also to a case in Poona, communicated to us unofficially, where infection is said to have originated in the bite of an infected pet squirrel.

VI.(5)(*b*)—*Bacteriological Data with regard to the Infectivity of Plague-infected Rats.*

Bacteriological data with regard to the infectivity of plague rats.

217. Before bringing to a close our survey of the infectivity of rats, it will be appropriate briefly to reconsider the question in its bacteriological aspect. It will be unnecessary to recapitulate the general pathological principles which in the case of animals, as in the case of man, furnish us with indications as to the possible channels by which plague bacilli may escape from the infected organism. We may pass directly to the consideration of the bacteriological data bearing on the question of the presence of the plague bacillus in the excreta of rats.

Dr. Gibson, without giving the details as to the number of samples examined, informed us that he had been able to discover the plague bacilli in the urine of rats, 17,248-9.
and that he had also, by cultivating a series of agar tubes, succeeded in detecting the plague bacillus in the intestinal contents of these animals.—Dr. Mayr told us that he 17,410.
had in one instance found the plague bacillus in the urine of rats, and that he had sought for it several times with negative results.—The German Plague Commission Report of German Plague Commission, p. 284.
put on record the fact that the plague bacillus was found by them in the intestinal contents of rats fed on plague infected material—The Austrian Plague Commission made experiments to determine whether plague bacteria were to be found in the fæces of Report of Austrian Plague Commission, Part II. C, 634.
rats. In the case of four rats inoculated in the peritoneum, and in one rat inoculated subcutaneously, the results were negative; similar negative results were obtained in the case of a rat fed with the organs of a rat which died of acute plague, though the animal which was the subject of experiment died with numerous plague bacilli in its blood. In the case of another rat, fed on the organs of a mouse which had died of plague, bacteria were found in large numbers both in the blood and in the contents of the intestines. In the case of another rat,—the last of the series infected by the intestine,—bacteria presenting the morphological characteristics of plague were found in the intestinal contents, and numerous plague bacteria were recovered from the spleen: a portion of the contents of the intestines injected subcutaneously into a mouse communicated plague to that mouse.—Mr. Hankin told us that he had, on 8602.
several occasions, sought unsuccessfully for the plague bacillus in the urine of rats: on one occasion he found the intestinal canal full of blood containing the plague bacilli.

Plague bacilli do not appear to have been discovered in the bronchial excretion of rats. It would, however, appear to be probable, in view of the fact that pneumonic changes have been found in the lungs of rats, and in view of the fact that the plague bacillus has been found in considerable numbers in the bronchial secretions of guinea-pigs hypodermically inoculated with plague, that the bronchial secretion may, also in the case of rats, be infective.

As in the case of man, so also in the case of rats, plague bacteria have been recovered from the pus from suppurating buboes. In the opinion of the Austrian Plague Commission the disease may not infrequently be disseminated in this way by rats that are recovering from the disease. Report of Austrian Plague Commission, II. C, 686.

VI.(6)(*a*).—*Epidemiological Facts regarding the Infectivity of Houses.*

Infectivity of plague houses and data with regar to persistence of infection.

218. We now proceed to discuss the question of the infectivity of houses and rooms in which plague cases have occurred, or which have been infected by animals. Proof of the infectivity of such houses can be obtained by showing that the entry or re-entry of healthy persons, who have not otherwise come into contact with plague into empty houses, vacated because of the disease, has been followed by the occurrence of cases among them.

Instances of infectivity and persistence of infection in houses where plague cases have occurred.

219. Examples of plague infection due to re-entry into infected houses are available in connexion with village epidemics where the inhabitants were allowed prematurely to return from evacuation camps to their villages. Instances in point are set forth in that section of our Report which deals with evacuation.

Other instances have been placed on record in connexion with the return of people to individual houses which have been inadequately disinfected; some of them are set forth here. Mr. Giles, the Commissioner in Sind, put before us several instances of this kind.—In one of the instances which he cited plague occurred in a house on the 11,266.
27th of March 1897; the room in which the case occurred and the adjoining room were disinfected and left empty till the 15th of April. A man who went to live in the room about this time was attacked by plague on the 18th of April. The whole range of rooms was then again disinfected, and the roof was entirely removed and burnt, permitting the sun to pour in, the burnt walls and the beams alone being

Para. 632 of this Report.

left standing. On the 14th of May, three weeks from the date of the last case, a man who had gone to the house but had not slept in the infected room, and who used the room merely as a cooking place for two or three days, was found there ill with plague.—
11,271. In another instance, a family which had been segregated, was, after the disinfection of their clothes, allowed to return to their house, which had been chemically disinfected and vacated for some time. The number of people who returned to the house was seven, and five or six days after their return one member of the family was
11,276. found dead of plague and three others were found to be ill with the disease.—In another instance, a small house was disinfected and its roof was removed and burnt on the occurrence of a plague case on the 5th April 1897. On the 20th April the residents of the house were allowed to return to it from the segregation camp. On
11,276. the 24th April another case of plague occurred in the house.—Similarly, a woman contracted plague four days after return to a house which, on the occurrence of a plague case, had been disinfected and unroofed and remained empty for between
11,276. three and four weeks.—Another instance given us by Mr. Giles was that of a large and not ill-ventilated house which was, on the occurrence of a case of plague in it, very thoroughly disinfected; about a week after the people of the house returned to it
11,276. from the segregation camp another case of plague took place in the house.—In another instance, Mr. Giles said that a house was disinfected on the occurrence of a case of plague in it, and remained closed for about a month; the man who then reoccupied it was at once attacked by plague. Mr. Giles summed up his experience
11,268. of Karachi by saying: "I was out pretty nearly every day, and one kept on coming across cases of people going back and getting plague."—Similar to the experience
Capt. James' Report, p. 107. of Mr. Giles was that of Captain James, I.M.S., in the Punjab. "It is astonishing," says this observer, "how long infection remains in and about the rooms and buildings where cases of plague have occurred—that is, when nothing has been done to kill the contagion. In Mahlgahla, people returning to the village 40 days after the evacuation were attacked; at Khan Khanan 38 days; at Kariha 30 days; at Mallupota 31 days; and at Bahrwal 24 days after the village had been emptied."—
22,384–5. A remarkable history is that of the house of the jailer at Satara, regarding which
22,535. evidence was given to us by Mr. Dodgson, Lieut.-Colonel Hay, Captain Thompson,
22,137. I.M.S., and Mr. Ghanekar. A case of plague occurred in January 1898 in this house,
22,330. which is a large one of stone, well built and well ventilated. After this the house
22,549. twice underwent disinfection. Another case occurred in the house on the 20th
22,062. December 1898, and the house was then again thoroughly disinfected, but ten days
20,331–8. after the return of the inhabitants two more cases of plague took place.—Remarkable also in connexion with the local persistence of infection are the facts brought to our notice in connexion with the outbreak of plague in the Umarkhadi Jail at Bombay. Four cases occurred in the jail at the commencement of the outbreak, all in prisoners who had been sleeping in one particular spot. The barrack in which they had been sleeping was then evacuated and carefully disinfected. When the barrack was re-occupied, three weeks after, three more cases occurred in other prisoners who slept in the same portion of the building as those first attacked. It is to be noted that the prisoners in the jail are continually moved about, and are never allowed to sleep con-
22,549. tinuously in one particular place or one particular barrack.—Mr. Ghanekar told us that he
24,451. knew of about ten cases in Satara where plague kept recurring in a house.—Mr. Moore
23,196. had noticed one or two cases of the same kind both in Bulsar and Surat.—In Igatpuri, Major Anderson, I.M.S., found that plague cases frequently occurred in infected houses that had been disinfected. He instanced particularly the case of two chawls, each of which, though it had been disinfected four or five times, continued to furnish cases until the inhabitants were removed.—Lieut. Firth noticed, in the case of Bombay
25,790. chawls which had been emptied and disinfected, that when the people returned in a "fortnight or month, or any length of time, as soon as you put them back again they got plague just as badly."

Instances of infectivity and persistence of infection in houses infected by plague rats. Para. 192 of this Report.

220. There can be no reasonable doubt that what holds true with regard to houses which have harboured plague patients holds true also of houses which have been infected by plague rats. In the latter class of cases it will, however, be more difficult to discriminate between infection contracted from the house and infection contracted directly from contact with rats. Such a case, for instance, is that of Chak Kalal, an evacuated village in the Punjab, already referred to as a village where a serious outbreak of plague occurred among the inhabitants on their re-entry into their houses. It is not clear whether the infection was contracted

directly from rats that had died of plague, or from the houses that had been infected by those rats. In the absence of a history of direct contact with rats, it would in such cases seem reasonable to assume that the infection was contracted from the house. Instances of the contraction of infection on a smaller scale from rat-infected houses are the following.—In the case of Mahlgahla a severe epidemic of plague broke out among the inhabitants who returned to disinfect their houses, which were uninfected when vacated some month previously, and in which dead rats were found at the time of disinfection.—Further observations regarding the occurrence of plague among disinfecting staffs employed on infected houses will be found in a subsequent section of this Report.—In the village of Holihosur, dead rats were found in December, and as a precautionary measure the village was then evacuated; no one suffered from plague until a month afterwards, when the people, thinking they were safe, went back to the village. Plague cases then began to occur.—Three people were attacked by plague in a house in Karachi three or four days after mortality among rats had been reported in that house.—Speaking of a particular house in Bombay City, Lieut.-Colonel Weir, I.M.S., informed us that rats were found dead in it. Thereupon, the inmates, with the exception of three persons, vacated the house. One of these three persons was attacked by plague ten days afterwards, no connexion with any other source of infection being traceable.—In another house in Bombay, where the rats were found dying and where the European inhabitants neglected to take the proper precautionary measures, some of the servants were attacked with plague, and a few days later one or two also of the Europeans were attacked.—The appearance of dead rats was, as we were told by Mr. Shroff, in the case of houses on Malabar Hill, followed by cases of plague; among these was notable the case of a man who persisted in sleeping in a room where dead rats had been found.

10,348. Paras. 192 and 273 of this Report. Para. 303 of this Report. 22,943. 11,421. 1,180. 20,416. 18,189. 18,196.

It is, however, important to note that the finding of dead rats has not in all cases been followed by the occurrence of plague among men. Doongri may be mentioned as a place where there was no plague among men, though dead rats were found there and there was plague in the neighbourhood. In the village of Sherli, near Bombay City, an epidemic of plague occurred among rats which was quite unaccompanied by any plague among men. In the Fort of Daman, "in about 40 houses, dead rats were found. Notwithstanding this, no single case of plague occurred in the Fort." No doubt most of the residents of the Fort were inoculated, but this, in view of the general results of inoculation in Daman, can hardly have accounted for the complete absence of plague. Again in Kankhal, though in the month of June, 1897, some 30 dead rats were found chiefly in and about a particular grain store, no plague cases occurred among men in association with this rat epidemic, and no dead rats were found for five or six weeks before the first cases among men occurred.

14,377. 18,966. 15,960. Sect. IV.(3) of Chap. IV. of this Report. 9291–3. 8590. Annales de l'Institut Pasteur, Nov. 1878, p. 749.

Conclusion that infection persists for long periods in houses in which it is introduced by men or rats.

221. To sum up, the universal experience of plague in India proves—and we shall deal with this matter when treating of the question of disinfection—that houses into which the infection of plague has been imported, whether by men or by rats, are infective, this infectivity being so marked that many of the officers who have had most experience of the disease, have come to the conclusion that the principal source of infection is, as would appear to hold true in the case of yellow fever, and possibly also of typhus, to be found in the houses into which the infection of plague has been introduced. The general experience on this question is summed up in the expression current in India that plague is essentially a "disease of locality." 1650, 18,108.

VI.(6)(*b*).—*Bacteriological Observations regarding the Existence and Persistence of Infection in Houses and in particular in Earth and Cowdung Floors.*

Bacteriological observations relative to the presence of the plague bacillus in floors.

222. It would undeniably be of advantage to obtain definite information, first, as to the presence or absence of the plague bacillus in houses suspected of being infective, and, secondly, to obtain light on the question as to whether the infective properties of a house where plague has occurred depend merely upon the continued survival of bacilli which have escaped from the organism, or are due to a multiplication of such bacilli by saprophytic growth. Many series of bacteriological investigations have been undertaken with a view to throwing light upon these questions. The material which has been subjected to examination has been for the most part material derived from the earth and cow-dung floors of native houses, which, since there is very little furniture in native houses, and since the people eat, sit, and sleep, on the floor, would appear to be the most likely carrier of infection. The investigations in question have been already considered by us in discussing the special technical methods employed. The general

Paras. 160-2, and 167 of this Report. 25,374-88.

results of the examinations referred to—and we may note that, in conformity with these, are the results obtained by Dr. Cayley in a series of 30 to 40 examinations of samples of earth collected from plague-infected houses, to which examinations we have not previously referred since details as to the technique employed are wanting—may be summed up in the statement that in the very numerous series of examinations made in India, the plague bacillus has never been detected in the floors by any trustworthy observer. Needless to say these results do not in any way establish the absence of the plague bacillus from the material examined. They prove merely that the plague bacillus cannot, with the present methods, be detected in earth, and that inferences made in particular cases as to the infectivity of houses cannot at present either be confirmed or refuted by bacteriological investigations.

Bacteriological observations regarding the persistence of plague infection in earth.

18,865-80.

223. In default of methods allowing of the detection of the plague bacillus in earth under the conditions which obtain in nature, information of a useful character, bearing on the question of the persistence of infectivity in houses, can be obtained from experiments in which sterilised materials are artificially infected by cultures of plague. Dr. Marsh has conducted a series of such experiments. These showed that when the plague bacillus is introduced into moist sterilised garden earth, it is capable, under the particular circumstances which obtained in Dr. Marsh's experiments, of surviving there for a period of 13 days. The period of survival of the plague bacillus in moist sterilised cow-dung was found to be practically unlimited, the bacillus being still recoverable by cultivation after a period of many months. A further interesting fact elicited by Dr. Marsh, and already incidentally referred to, which has an indirect connexion with the question of the growth of the plague bacillus in earth and cow-dung floors, is the fact that agar cultures when buried under the floors of native houses were found to grow in a particularly luxuriant manner this luxuriant growth being, as further experiments demonstrated, probably due to the presence of carbonic acid in the ground air. These experiments would seem to suggest that the plague bacillus may find certain conditions which are favourable for its development in the floor of native houses.

Para. 162 of this Report.

Bacteriological observations regarding saprophytic growth of plague in earth.

26,132.

224. With a view to testing in a preliminary manner the possibility of a saprophytic growth of plague in earth, we performed the following experiment:—We took 12 small glass tubes, each pierced with a lateral hole, the holes in different tubes being arranged in such manner that earth could be withdrawn from the different tubes at progressively increasing depths from the surface. These tubes were filled with sterilised earth and plugged with cotton wool. The lateral hole of each tube was closed by a tight fitting india-rubber band. The earth in six of the tubes was moistened with sterile water; the six other tubes were kept dry. A drop of fresh plague culture was dropped on the surface of each tube, and the tubes were kept at the ordinary temperature of the room in a diffused light. When they were examined after four days, the plague bacillus was neither to be found on the surface of the tubes, nor in the earth withdrawn at different depths below the surface. Manifestly very little importance attaches to an experiment like this. So far, however, as it goes, it does not lend support to the view that the plague bacillus finds in the earth very favourable conditions for saprophytic growth.

VI.(7)(*a*).—*Epidemiological Observations bearing on the Infectivity of Clothes and Fomites and the Persistence of Infection in them.*

Infectivity of clothes and fomites; epidemiological data. Para. 239 of this Report.

11,961.

225. There can be no doubt, on general considerations, that clothes and fomites must be infective, and may retain their infection for a long time.

Striking proof of such infectivity is furnished, by specific instances of infection from clothes. Most of these are cited in the section of our Report which deals with the importation of infection into uninfected places. One interesting example, however, which was furnished to us by Lieutenant Law, I.M.S., may be set forth here. In July 1897 at Kiamari a case occurred among the Mahrattas, after a considerable interval during which there had been no plague in the neighbourhood. It was ascertained that the patient had three months previously come into possession of some clothing deriving from a person who died of plague in another house. These clothes had apparently been kept in a box, which was opened only shortly before the patient who came under Lieutenant Law's observation fell ill.

In addition to the specific instances to which we have just referred there are also certain general observations pointing to a more general spread of plague in camps by means of clothes. It will be manifest that where in the case of evacuated villages effectual precautions have been taken to prevent the access of people to their houses, and where

the sick among the evicted population have been carefully segregated and the infection in their huts destroyed, the continued occurrence of cases of plague in camp can hardly be imputed to anything else than the persistence of the infection in clothes and fomites. Confirmation of the view that the continued occurrence of plague in camp under these conditions is due to the presence of infection in clothes is afforded by cases when the plague ceased in camp after the clothes and effects had been carefully disinfected.

A notable instance in point is that of Shikohpur, an infected village in the Punjab. With reference to this village, Captain James, I.M.S., says:—"The people were all got out into the camp by February 2nd. . . . Even after the people came out, dropping cases occurred among them, in consequence of which Dr. Walton had all the goods and property in all the camps disinfected. The result was immediate; the disease stopped, and, with the exception of one isolated case of a man on the disinfecting staff, who was attacked on March 3rd, no new cases occurred." Captain James told us that he had, in the case of health camps, seen several instances of a similar effect following the disinfection of personal effects, Mallupota and Bahrwal being specifically mentioned as places in which plague was suppressed in the health camps by the disinfection of clothes and the removal of the camps to fresh sites. In the wide experience acquired in the course of his plague operations in the Punjab villages, Captain James, I.M.S., found that, in order to prevent their reintroducing plague from camp into their disinfected villages, it was, generally speaking, necessary to keep the people out for three weeks after the last case in camp. "In the middle of the cold weather, and in February, when the disease did not die out so quickly after people went into camp, we," says Captain James, "allowed a month to elapse after the last case before permitting the people to return to the village."

9994.

10,145-6.

Captain James' Report, p. 114.

To return to the question of the suppression of plague in camp by the disinfection of clothes, we may refer to the evidence given to us by Lieut.-Colonel Fawcett, R.A.M.C. This witness told us that on the 5th of October 1897 cases of plague began to occur in the Transport lines in the Poona Cantonment. By the 4th of November 24 cases had occurred, so the whole of the followers and their families—altogether about 400 persons—were moved into camp. This measure, however, was not of itself found sufficient to stop the disease. Fourteen cases occurred in the camp, so on the 13th of November it was decided to move the camp a short distance, and to apply a thorough process of disinfection to the persons of the inmates of the camp, to all their clothes and property of whatever kind, and to their tents. This decision was carried out. After the careful disinfection of everything, the camp was moved a few yards across the road. Two children who were found to be ill before the removal were isolated, and after the disinfection and removal had been accomplished not a single instance of plague occurred among those who crossed the road.

21,251-7.

The conclusion that the wholesale disinfection of clothes and personal effects, and not the removal of the camp, was probably the operative agent in stopping plague would appear to be supported by the consideration of the contrasting case of the three Native Infantry Regiments which were about the same time attacked with plague at Poona. The first of these regiments, the 19th Bombay Infantry, was attacked with plague and moved into camp on the 29th of September 1897. Nineteen cases occurred in their camp, and they were then, on the 15th of October, moved to another camp without disinfection of their effects. In the second camp seven cases occurred before the plague stopped. The 2nd Bombay Grenadiers, on being attacked with plague, were moved into a camp on the 29th of September; between 29th of September and the 23rd of October 11 cases occurred among them in camp, and on the 23rd of October their camp was moved, without disinfection of their effects, to another position. After two more attacks had taken place the plague stopped among them. The depôt of the 14th Bombay Infantry was, on the outbreak of plague, moved into camp on the 28th of October, and five cases occurred in camp before the 18th of November. The camp was then, without disinfection, moved further from the Cantonments, and no more cases took place. If we take into consideration here the fact that the removal of the camp, combined with precautions against the re-introduction of infection and with disinfection of clothes was followed by the complete extinction of plague in the case of the Transport followers who had 14 cases of plague in their first camp, while the removal of the camps combined with precautions, as in the case of the Transport followers, but without disinfection, was followed by nine cases in the case of the regiments, which had in their first camps an aggregate of 35 cases, it would appear that a certain amount of infective material must have been harboured in the clothes.

21,521-3

Paras. 304 and 307 of this Report.

The inference that clothes are infective would appear to be confirmed by the frequent occurrence, noticed elsewhere in this Report, of plague among washermen and others who are brought into close contact with clothes which may derive from plague patients.

VI.(7)(*b*).—*Bacteriological Observations regarding the Infectivity of Clothes and Fomites.*

Bacteriological data relating to the infectivity of clothes.

226. In view of the difficulties associated with the isolation of the plague bacillus when mixed with contaminating bacteria—difficulties which have been already sufficiently brought forward in connexion with the bacteriological examination of earth,—the detection of the plague bacillus in clothes and similar effects has only in a few instances been attempted. Mr. Hankin examined, by inoculation into mice, a certain number of specimens of the clothing of people who had plague, or who had died of plague: he failed to find the plague bacillus.—Dr. Gibson examined, by cultivation methods and by inoculation into animals, two specimens of wearing apparel from plague-infected patients. In neither case was the plague bacillus detected.—Dr. Cayley also examined clothing of plague patients, obtaining only negative results. The plague bacillus was, however, detected by Dr. Cayley in a portion of matting which had been used for the bedding of a plague patient, and was soaked with his discharges. This, however, was a case of gross contamination.

8627–8.

619–20.

25,347–51.

25,393–6.

Para. 659 of this Report.

Details regarding the length of time for which the bacillus of plague may survive in clothing into which infection has been artificially introduced will be set forth later in connexion with our discussion of the disinfection of clothes.

VI.(8).—*Infectivity of Merchandise.*

Infectivity of merchandise.

227. In connexion with the question as to whether merchandise is to be regarded as infective, certain *à priori* considerations may, perhaps, appropriately be set forth here. The presence of the infection of plague in articles of merchandise is conceivable only when these have been contaminated either by the excreta of plague patients or of infected rats. The only merchandise which comes into consideration in connexion with contamination by men is rags; the merchandise which comes into consideration in connexion with possible contamination by rats is grain and other food stuffs and merchandise which has been stored in close vicinity to food stuffs. Thus such merchandise as hides, prepared with arsenic, as the Indian hides are, and unused gunny bags, will be little liable to contamination.

In determining whether any particular articles of merchandise are infective or as to how long infection may persist in them we must rely chiefly upon the facts which have come to light with regard to the importation of plague into uninfected places, and with regard to the spread of plague within infected places and the question of the special liability of traders to contract and spread plague. Certain isolated observations, however, which do not come naturally into the discussion of the matters just referred to, but would seem to throw a light on the possibility of infection through merchandise, may be set down here. "In a village which I came to about 10 days ago," said Lieut. Kidd, who was engaged on plague duty in the Belgaum district, "a Banniah had got the plague, and a woman who bought some grain from him, got plague four days afterwards. The Banniah's daughter had died of plague, and I could not trace how this woman had got plague in any other way, as she was living in a hut at least 100 yards away from any other hut, and she had not been in the village for a month, and none of her relations had died of plague." In response to a question as to whether the woman might not have contracted the infection by direct contact with the Banniah's daughter while she was still alive, the witness said, "Yes, that is possible. This Banniah was living in a hut, and he had all his stores outside the hut on a little verandah, and I do not think the women who went to buy grain went into the hut; she would have stayed outside." Lieut. Kidd moreover, told us that in the village of Mohol, where "many Banniahs had the plague, some Mangs stole their grain, and a few days many Mangs got the plague." In reply to a further inquiry, Lieut. Kidd said, "of course they might have caught it through going into an infected house." The following was reported to us by Lieut. Niblock, I.M.S. On the 27th of May two cases occurred among Musalmans in the village of Reri, near Karachi, in which plague had broken out among Banniahs connected with a particular shop. The cases in question were those of a boy and a girl who were not related to each other, who had not been to Karachi for months, who lived in huts far from each other and from the shop of the infected Banniahs, but who bought their eatables and grain from the shop in question.

Sections VII.(4) and IX.(?) and paras. 262 and 301 in this Chapter.

23,005.

23,006.

23,004.

12,446–7.

228. Coming now to bacteriological observations made with regard to the infectivity of articles of merchandise, we may note that Mr. Hankin subjected to bacteriological investigation a formidable number of different articles of merchandise, in particular, seeds and other vegetable products, and that he obtained in every case a negative result. Lieut.-Col. Lawrie, I.M.S., basing himself on cultures and inoculation experiments made by Dr. Mullanah, claimed in the public press that the bacillus of plague had been discovered in raw cotton packed for transport. It appeared, however, from our examination of Dr. Mullanah that this witness considered that there was not sufficient evidence for identifying the bacillus isolated by him from cotton with the plague bacillus. Dr. Shrinavasa Rau, who, as we have seen, claims to have isolated the plague bacillus from so large a proportion of cases of fæces and urine examined by him, claims to have detected the plague bacillus in specimens of coir suspected of having conveyed the infection into Bangalore Jail. It will probably be quite unnecessary at this stage of our report to point out that the negative result of Mr. Hankin's experiments affords us no warranty for inferring that the plague bacillus was in reality absent from all the samples of merchandise examined.

Bacteriological data regarding infectivity of merchandise.

25,903–9
24,820–94.

3482–6.

229. The question of the length of time for which the plague may survive when artificially introduced into articles of merchandise, will be most appropriately dealt with in connexion with the spread of plague over sea, and with the question of the application of measures of disinfection to merchandise.

Persistence of infection in merchandise.

Paras. 262 and 659 of this Report.

VI.(9).—*Concluding Remarks regarding the Infectivity of Plague.*

230. Before bringing our consideration of the infectivity of plague to a conclusion, it will be useful briefly to summarise what has been said above.

Concluding remarks with respect to the infectivity of plague.

Observations on the sick in hospital have shown us that in plague the risk of direct infection from patients, taken in the aggregate, is a small one. We have, however, seen that primary pneumonic plague constitutes an exception to the general rule, and that this type of the disease is highly infective. The same must be assumed to hold true of those exceptional cases of plague in which specific plague rashes, of a bullous or pustular nature, appear on the surface of the skin. Quite of a different order is the infectivity of the ordinary bubonic form of plague. In this, which is by far the most common form of plague, the infective material is not thrown off from the surface of the skin, or to any considerable extent from the lungs, but appears only in the excretions, and this only in the last stages of the disease.

We saw that what holds true with regard to the escape of infective material from the human plague patient probably applies, in a general way, also to the escape of infective material from the organism of plague-infected rats.

With regard to the indirect infectivity of plague, we have seen that the house in which plague patients or plague rats have died, and the clothes and other effects which have been soiled by the excretions of plague infected men and rats, are infective. All these, when they have once been contaminated, may remain infective for very considerable periods. Thus plague, which is in certain of its manifestations formidable from the point of view of its direct infectivity, is formidable also in all its manifestations, except that of pestis minor, from the point of view of its indirect infectivity, especially from the manner in which infection clings to contaminated houses and personal effects. We shall see that the danger which accrues to those who live in infected houses would appear to be much greater than that which accrues to persons who merely come in contact with plague patients, and this is in harmony with the doctrine of chances, seeing that the bacillus escapes from the body, for the most part, only shortly before death, while it is capable of surviving and communicating infection for long periods after it has escaped from the organism.

231. The full measure of the infectivity of plague has not, we think, been at all fully disclosed in India, inasmuch as in the overwhelming majority of cases measures have been taken either to extinguish the infection in infected houses or to remove the people from these the most considerable sources of infection. In exceptional cases, however, where generally owing to the fact that the presence of plague has been concealed from the authorities, or where plague measures were suspended owing to heavy rains, and where, consequently, the people have remained exposed to the unabated force of the infection, plague has had an opportunity of doing its worst. In illustration of what the ravages of plague can be when the people remain in their houses and in contact with the sick under conditions which are also otherwise favourable to the spread of the disease, we may point, on the one hand, to the facts set forth in paragraph 125 of this Report with regard to the terrible mortality recorded in certain places in India, and, on the other hand, to individual instances which were brought to our notice, such as the following:—In a case which came under the personal observation of Mr. Cappel, the Collector of Hubli, nine uninoculated persons died of plague in the

Measures adopted in India have prevented infectivity of plague exerting its full force.

1725.

23,383. family of one of his clerks. Only four members of the family, and these all inoculated, survived.—In a case in Hubli, reported to us by Lieut. Thomas, nine persons out of a family of 13 died in a house before the existence of plague became known to the authorities.—In a case cited by Mr. Cadell 13 out of 18 inhabitants of three adjoining houses in Karachi succumbed to plague.—In a case in Karachi, reported by Lieut. Niblock, I.M.S., in spite of the attempts which were made to isolate the patients in their own room, 24 out of 40 persons in one house succumbed to plague.—In a case cited by Mr. Stevens 9, or 11, persons—the exact number was uncertain—died of plague in one house in a village in the Hyderabad State.—In a case in Calcutta, reported by Major Green, I.M.S., where the patients were not removed to hospital, eight out of a family of 15 succumbed to plague.—In another case in Calcutta under the same conditions 11 persons died in one hut, and of these, four, all members of one family, died in one room.

3698. 12,333. 5498. II., p. 537.

While facts like the above are of the utmost importance as an indication of the full measure of the infectivity of plague, it may reasonably be assumed that there must have been many cases in which the introduction of plague into overcrowded chawls has not been followed by any spread whatever of the disease.

VII.—Conveyance of Plague from Infected to Uninfected Places.

Information regarding agencies by which plague is conveyed into uninfected places not obtainable from history of large towns, but of villages. Paras. 11–13, 41, 57, 61, 74, 92, 95, 102, and 105, of this Report.

232. It has been clearly indicated in the historical section of our Report that the origin of the outbreaks of plague in Bombay and other large towns in India has never been satisfactorily elucidated, and is a problem which it is impossible to resolve. This is not surprising, for in the case of the large cities in India the authorities were either taken unawares by the advent of plague, or were unable to take any effective measures anterior to the advent of plague for the detection of the first case that might be imported, or, in default of such an imported case, the first local case which might originate from infection otherwise introduced. If we look beyond the limits of India, we see that attempts made to elucidate the manner in which plague was imported have been no more successful in Hongkong, Alexandria, Oporto, Sydney, Glasgow, and other places, than similar attempts made in the large towns of India. It will thus be manifest that no light will be thrown upon the question of the mode in which plague spreads from place to place by an examination of the history of plague in large towns. In the case of each such town, we remain in doubt whether the infection was imported by a patient who was already infected elsewhere, or by infected rats, or indirectly by means of infected clothes or merchandise. The important problem as to how far each of the agencies just referred to is concerned in the spread of plague from place to place is, however, one which can be elucidated by studying the mode in which plague was introduced into villages throughout India. In villages the conditions for the discovery of the mode of infection are comparatively favourable, and in many cases the mode of infection was actually ascertained.

VII.(1).—*Introduction of Infection by Human Communication.*

Conveyance of infection from place to place by human communication.

233. The evidence which has been placed before us shows, in the clearest manner, that in many cases plague has been introduced into a place by a person arriving from a plague-infected locality. In many cases this person has himself sickened of plague; in other cases, the person who introduced the infection escaped, while others contracted the disease.

Captain James, I.M.S., who was in charge of the plague operations in the Punjab, told us that in that Province, out of a total of some 90 villages infected, the origin of the disease had in 67 cases (including cases of re-infection) been traced to the importation of the infection by human agency. Under the heading of villages which were infected by human agency, Captain James included not only the villages which were infected by the arrival of a plague patient, but also the villages which were infected by the introduction of the infective material brought by a person who did not himself develop plague. Captain Wilkinson, I.M.S., whose experience covered the same field as that of Captain James, expressed the same opinion, observing that the facts supported the idea of the importation by the human conveyance of plague "strongly. In a large number of villages the first people to be attacked were," he said, "people who were known to have gone to infected villages."

10,078-9. 10,391.

Again, Major Anderson, I.M.S., who had great experience of plague, both in the rural and the urban areas of the Bombay Presidency, told us:—"In most places, as n Kurla, Bassein, Chembur, Trombay, Malad, Chinchni, Thana, Bhiwandi, Bhayndar, &c., we have a history of persons coming sick from an infected locality, or falling sick a day or two after their arrival prior to the occurrence of local cases." Major Anderson placed before us the following specific instances:—"In Agashi, the first local case

23,207. 23,207.

occurred in the person of a Shimpi woman, to whose house some friends from Bombay had come to live. None of these Bombay people were sick or were afterwards attacked In Kelwa, also, the first local cases occurred in the person of two Shimpis to whose house some Shimpis from Bombay had come five days before. These Bombay people were in good health, and after staying three days returned to Bombay . . . . At Versova a striking instance of the disease being carried by an apparently healthy person occurred. The first imported case at Versova occurred on 30th January in the person of a Brahman who came sick from Bombay. The Brahman schoolmaster of Versova visited this man while he was sick, and attended his funeral on 31st January. The schoolmaster lived in the village Talati's house. On the 2nd February the Talati's nephew, who lived in this house, was attacked by plague, while the schoolmaster himself was not attacked till the 5th of February. In Marol also the first case occurred in a house to which a number of people had come from Bombay to attend a wedding. These people were in good health, and after staying some days they returned to Bombay in good health . . . Again, in the village of Madhan, an isolated case occurred prior to the outbreak there, in the person of a man to whose house a man from Bombay had come to live."

Six cases of plague broke out on the 2nd of October in a village called Ugargol, situated in an uninfected part of the Belgaum district. It appeared that the owner of the house in which the outbreak occurred had, in the middle of August, returned to the village from Dharwar, which was then infected; he had not since been to Dharwar, and Mr. Carmichael satisfied himself that he must have introduced the infection from Dharwar in some infected article, six weeks before the disease broke out in his house. 22,878—85. 22,879.

Both Major Anderson, I.M.S., and Mr. Carmichael thought that in the cases cited by them and similar cases the infection must have been imported, not on the person, but in clothes or some other article. In these cases, however, unlike those cited later in connexion with the importation of plague in clothes and merchandise respectively, no particular article was incriminated. 23,206. Sections VII.(3) and (4) of this Chapter.

In the Dharwar district, where, after the evacuation of the chief town of the district the epidemic was very widespread in the villages, Mr. Cappel told us that the plague was conveyed from village to village by human agency. Mr. McNeill, Mr. Cappel's successor in charge of the Dharwar district, who had also been employed in measures against the plague in the Surat district, said :—"As far as my experience goes, I think it was carried by human beings. That seems to explain almost every case that I came across." Captain Colomb, on his experience of plague in the Dharwar district, came to the conclusion that "as between villages, so between huts or groups of huts, the spread of infection can always be traced to human agency." Rao Bahadur Bhardi, Deputy Collector, who also was employed on plague duty in the Dharwar district, said that infection was carried without doubt from village to village by human agency. 1641. 2039. 23,626. 23,502.

In the Sholapur district also, and in the adjoining parts of the Hyderabad State—which became severely infected after the evacuation of Sholapur City—the influence of human agency in spreading plague was shown in the most complete manner. Mr. Stevens, the Deputy Plague Commissioner for the Nizam's territories, gave it as his opinion that plague is conveyed "by people suffering from plague going into villages and giving it to others." He had discovered many refugees suffering from plague in the villages under his charge. Lieut. Bruce, who made observations on the spread of plague in the villages of the Sholapur district, thought that the disease was spread mainly by human agency. 5498. 24,016

Lieut. Steen, employed in the infected villages of the Satara district, thought that infection was conveyed "almost entirely by the visits of people of infected villages to non-infected villages." In seven of the villages under his charge infection was clearly traced to human beings; in no case was it traced to any other source. 22,678. 22,690.

Similar evidence as to conveyance of the infection of plague into uninfected places was placed before us in connexion with Bulsar. "I think," said Major Dyson, I.M.S., "it is spread by human agency from place to place. In the case of Bulsar, plague was undoubtedly introduced from Bombay, and, I think it was introduced in this way into Surat also, though I cannot speak positively about that." Mr. Moore, who had experience of outbreaks both at Surat and at Nasik, was of opinion that Surat became infected by people who went to Bombay, "became infected in Bombay, and then came back to spread the plague in Surat." Mr. Stewart, from his experience of Nasik, said that plague is, as a rule, brought into a place "by people who are infected." He remarked that "people come from infected places and live in clean houses and apparently after some time they infect the houses." Dr. Maynard, employed for some time as Civil Surgeon at Nasik, thought that the spread of plague was caused "by the 19,941. 24,409. 16,966.

14,693. villagers in their ordinary course of moving from one village to another, generally by the class which corresponds to that of grocers, people who deal in small quantities of grain. It was noticed at first, more particularly among the grain-sellers in Nasik."

14,694. He said: "I do not think it has anything to do with what they deal in. I think it is because the people travel from one village to another."

24,089. Mr. Hudson, the Collector of Ahmednagar, thought that the conveyance of plague from village to village was nearly always "by human agency." Similarly,

15,200. Mr. Dhurandhar, the Sanitary Commissioner for plague operations in the Baroda State, gives it as his opinion that the danger of the spread of plague by infected persons from place to place was very great, and added that the infection was carried "by human agency mostly."

VII.(2)(*a*).—*Introduction of Infection by Rats.*

Conveyance of infection into an uninfected place over land by the agency of rats.

234. In dealing next with the question of the conveyance of plague into an uninfected place by rats, we will in the present section confine ourselves to the question as to how far rats play an active part in spreading the plague from one village to another.

In view of the facts already recited above and from the analogy of other diseases, it is manifest that there will, in the case of every infected place, in the absence of evidence pointing to the contrary, be a strong presumption that the infection was introduced by man. When, therefore, it is alleged in any particular case that plague has been imported by rats, it would seem necessary to show that plague cannot have been imported either by direct human communication or by means of infected articles. It is obviously impracticable in any given case absolutely to exclude these possibilities, or, in other words, definitely to prove conveyance by rats. It will, however, in any particular case, be reasonable to assume that plague was introduced by rats, if it can be shown that its conveyance by infected articles or by direct human communication is, in the particular case in question, more improbable than the conveyance of the infection by rats. For instance, in cases where there is a presumption that infection has been brought into a village from a particular plague-infected village, and where human communication between the two villages in question can, with probability, be excluded on account of difference of caste or some other circumstance, there will be sufficient reason for considering the importation of plague by rats probable, especially when the two villages in question are situated at no great distance apart, and there is a history of mortality among rats in both, and the rat infection in the village to which plague is imported has preceded the outbreak of the disease there among men.

VII.(2)(*b*).—*Question whether Rats move out from Infected Villages.*

Observations showing that rats may leave a village and travel some distance afield.

235. Before enumerating cases in which plague has been conveyed from one village to another by rats, we may cite a few instances to show that there is a possibility of plague-infected rats travelling some distance afield. We are told, for instance, that in the Sirohi State, when the village of Kalindri was evacuated, dead rats were found in heaps of recently harvested corn collected round a well which was, we understand, about 1½ miles from the village. As a sequel, six of the people camped round the well and engaged there in the winnowing of the corn were attacked by plague.

9718-24.

9860.

A somewhat similar instance came under the observation of Lieutenant Kidd in the Belgaum district. Here plague is stated to have been brought out by rats from a village evacuated on the appearance of plague to the people encamped in the fields. So impressed was Lieutenant Kidd with this fact that he thought that evacuation camps should always be placed at some distance from the evacuated village.

23,012.

A wandering out of rats, similar to that noticed by Lieutenant Kidd, was referred to by Captain James, I.M.S. He stated that he had trustworthy information that rats had been seen round a well in an evacuation camp close under the walls of a village.

10,120.

Again, Mr. Westropp mentioned a case in which rats appear to have carried out plague to some houses situated about half-a-mile from an infected village.

2116-9.

VII.(2)(*c*).—*Questions as to Occurrence of General Migration of Rats on Outbreak of Plague among them.*

Observations adduced in favour of theory that rats migrated from the Mandvi quarter in Bombay City.

236. The cases which we have cited above furnish evidence, if such were needed, that individual rats may travel some distance afield from plague-infected places. It has, however, been suggested, and the opinion has been widely held, that a general movement of migration may set in among the rats of an infected place as soon as plague breaks out among them. This theory has come under our notice particularly in connexion with Bombay City, where it was believed that shortly after the outbreak

of plague in the Mandvi quarter the rats of that quarter migrated in a body and dispersed all over the City and beyond it into the neighbouring villages. Plague is said to have broken out everywhere in their train. We are informed that it was ascertained by a series of inquiries that rats entirely disappeared from the quarter of Mandvi, and that they appeared everywhere on the outskirts of the town. It is difficult to know precisely how much weight is to be attached to information collected by such inquiries. But even supposing it were quite trustworthy, this information, would not necessarily plead in favour of the theory that rats migrate when plague breaks out among them. The disappearance of the rats from the Mandvi quarter may have been due to the fact that they had all died out there, while the fact that their presence forced itself upon attention in the outlying quarters of the town may have been due, not to a fresh inroad of rats, but to the fact that rats already present emerged from their holes when they became infected with plague. The only fact which is actually established is that plague attacked the rats of the various quarters of Bombay in regular succession, moving outwards from the Mandvi quarter. Now, this outward spread of plague from an infected centre is, in the case of a human community, regularly brought about, apart from any general movement of the population, by the to and fro movements of the individual members of the community. It would, therefore, not be justifiable to assume, as was done in Bombay, that the outward spread of plague from Mandvi, even it was due to rats, was necessarily the result of a definite movement of migration among these animals.

Snow's Report on Plague in Bombay 1896–7, para. 12.
998, 1168, 1260–1.
Snow's Report, para. 12.
19,247.

Evidence to establish the theory that rats do migrate in a body after plague appears among them must, therefore, be sought elsewhere than in Bombay. Such evidence was carefully sought for by Captain Colomb in the Dharwar district, and by Captain James, I.M.S., in the Punjab. Captain James could find no evidence that rats ever migrated in a body, while Captain Colomb said, "As far as I can make out they never move out of the village." On the other hand, Lieut.-Colonel Adams, I.M.S., and Captain Grant, I.M.S., who had experience of plague in the Sirohi State, seem to have been of opinion that plague was carried from village to village by rats, and the latter told us that "after the epidemic was at its height there was not a rat to be seen in any of the villages." The opinions of these last two witnesses appear to be based upon the observation, already noticed above, that the infection of plague was carried out to a well at Kalindri by rats, and upon the disappearance of rats from villages after plague occurred in them. The former observation, however, does not prove more than the fact, which is substantiated by other evidence also, that isolated rats will leave their homes and occasionally wander afield; and the disappearance of rats from the villages may have been due to the practical extermination by plague of the whole rat population.

Observations made elsewhere than in Bombay City bearing on the question of movements of migration of rats.
10,107.
23,596.
9784.
9904.

We think, on a review of the whole of the evidence, that the observations which have been adduced do not in any way support the theory that a general movement of migration sets in among rats when plague breaks out among them. On the contrary, the observations placed on record in a subsequent section of this Report, regarding the enormous numbers of rats found dead day after day in the Mandvi quarter of Bombay, and in the Joria Bazar at Karachi, would seem rather to suggest that the rat population when infected by plague may remain on the spot till exterminated by the disease. Finally, we would note, though it has no direct bearing on the occurrence of migrations among rats when these become infected with plague, that rats swarmed in some of the plague hospitals in Bombay.

Conclusions that theory of general movement of migration among plague infected rats not established.
21,008–10.

VII.(2)(*d*).—*Cases in which the Introduction of Infection was attributed to Rats.*

237. We are satisfied that there may in some cases be sufficient intercommunication between the rats of adjoining places to allow of the occasional conveyance of infection by means of rats between such villages, but we think that this can hardly occur except when they are within the range of a few miles of each other. We base this opinion on a general review of the following facts.

Evidence regarding instances in which plague was introduced into villages by rats.

The village of Hamerowal, in the Punjab, was evacuated in consequence of the fact that mortality was noticed among rats. It would appear certain from the careful inquiries which were made that the infection was imported into Hamerowal from the closely-adjoining village of Punian, about 100 yards distant, where also rats had died. The conveyance of infection by human intercourse could in this case be practically excluded, as the inhabitants of the two villages belonged to different classes who had no intercourse whatever with each other. There are thus grounds for imputing the importation in this case to rats, if indeed we are justified in speaking of an importation where the villages concerned were only such a short distance apart.—

Facts observed in the Punjab.
10,113.
10,400.

10,109. Another village in the Punjab, said to have been infected by rats, is the village of Chak Kalal. In the case of this village, no history of intercommunication with any of the surrounding infected villages could be traced. Epidemics of plague were, however, occurring among the rats of the villages in question. In view of the careful inquiries that were made in this case, it would seem probable that plague was imported into Chak Kalal by rats.

Observations made by Major Anderson, I.M.S., in the Bombay Presidency.

Other instances of the infection of villages by rats came under the observation of Major Anderson, I.M.S., in the course of his plague duty in the Thana district of the Bombay Presidency. In the group of villages near Bandra, mortality occurred among rats before any mortality occurred among men. Although the histories of the cases are not recorded in sufficient detail to allow of an independent judgment being arrived at as to whether rats did or did not import the disease, it may be of interest here to subjoin Major Anderson's statement:—

23,210. "In Bandra, which is practically a suburb of Bombay, the first local case occurred on 3rd December in a house in which dead rats had been found about one week before, and the disease at once spread in this locality. On the same date another local case occurred in another part of the town in a house where no rats had died. The disease did not spread in this locality, and it is possible that the infection had been caught elsewhere. In Pali dead rats were seen on the 6th or 7th of January, and the first case occurred on 13th January. In Chuim rats died in the beginning of February, and the first case occurred on 9th of February, although it was not reported till the 12th February. In Danda in the same way dead rats were found in several houses prior to the outbreak of the disease on 11th February. The above four villages, separated from each other by a distance of about half-a-mile, are included in the Bandra Municipality. In Santa Cruz, two miles distant from Bandra, dead rats were found on 12th February, and on 15th February the two first cases occurred in the house in which the dead rats were found. In Joo, which is separated from Danda by a creek, dead rats were found in a house about the 10th of April. The people thereupon removed to a shed for 10 days, and then returned to the house, which had not been disinfected, and three days after their return the first occurred in this house. It would appear that the Bandra rats spread the disease in the adjacent villages, as they entirely disappeared from the town, and were only, in the beginning of June, re-appearing. Dead rats were in the same way discovered at an early stage of the local outbreak in the villages of Vela Parla, Andheri, Goregaon, and Padali, which form an almost continuous line of houses along the Bandra-Ghodbandar Road, the intervals between the various villages being at that time filled up by the sheds of Bombay refugees. The disease appears to have been carried along this line of road as it was carried in Bombay itself. In Old Kurla village, which is only separated from New Kurla town by less than half-a-mile of open fields, no case had occurred, though the disease was very prevalent in New Kurla during January, February, and March. On the 4th of April rats died in the house nearest New Kurla, and although that and some of the adjoining houses were vacated and disinfected, the first cases occurred in the village on the 8th April. Here the presumption is that rats came from New Kurla and carried the disease with them." Major Anderson's opinion that the villages were probably infected by rats seems to us to be entitled to the greatest respect, and, indeed, on general grounds alone, it is obvious that an observer who has investigated the matter on the spot is in the best position to arrive at a correct judgment as to the likelihood of plague having been imported by one means or another.

Other observations adduced.

14,377. Of interest also is the case cited by Mr. Petigara in the following words:—"I was on plague duty at a village called Utan, and plague appears to have broken out at Bhaynder. On inquiry into the cause of plague at Bhaynder, no importation of plague from outside could be traced, but dead rats were found at Bhaynder and at a village called Doongri at the same time. There were no plague cases at Doongri, even though dead rats were found, but after the appearance of dead rats at Bhaynder plague cases originated there. The inference drawn was that rats appear to have carried it to Bhaynder, *viâ* Doongri, but plague did not originate at Doongri though it did at Bhaynder," the nearest infected place, viz., Utan, being nearly five miles distant.

There are many other cases of villages on record into which the plague is assumed to have been imported by rats on apparently no better grounds than that the disease broke out among rats before it broke out among human beings, and that no other

14,263. importation of infection had come to light. In particular we have in view such cases as that of the Post Office at Karad. This Post Office is situated near the railway, at a distance of two or three miles from Karad, which was infected with plague. The first circumstance noticed was that rats died at the Post Office: a few days afterwards two cases of plague occurred among the inhabitants. On these

grounds the importation of plague was imputed to rats. Similar cases are reported by Lieut.-Colonel Hay from Umbowda Budruk, and Areh. The mere fact of the occurrence of mortality among rats before the occurrence of mortality among human beings in a village does not, however, suffice to establish the fact that rats were instrumental in importing the infection, for as we shall see below, in many cases the infection was imported by human agency in infected articles, and was communicated by these to the rats. 22,223–30. Para. 271 in this Chapter.

238. Even after the exclusion of all those cases where the possibility of the introduction of the infection by infected articles or by direct human communication does not appear to have been duly considered, there appears to us to be a residuum of cases in which there is sufficient evidence to make it probable that rats have carried the infection of plague from one village to another situated at no great distance from it. Conclusions that rats have in some cases carried plague infection between neighbouring villages.

No facts whatever have come to light which would suggest that plague has in any case been conveyed from one place to another by plague-infected rats carried on railways. Since rats seem to us unlikely to travel in trains, we think that the possibility of plague being spread by infected rats along the railways hardly deserves consideration.

VII.(3).—*Introduction of Infection by Means of Clothes.*

239. When considering the question of the importation of plague by human communication, we have already seen instances in which travellers from infected places, who did not themselves contract plague, imported the disease into uninfected places, and we have incidentally referred to the likelihood that the infection in these cases was imported in clothes. These cases might, since there is very little opportunity for the infection to be carried on the person as distinguished from the clothing, have been enumerated here. But we have thought it better to cite here, in our discussion of the importation of infection by means of clothes and personal effects, only those cases in which the importation of infection was specifically imputed to infected clothes. Conveyance of infection by means of clothes. Para. 233 in this Chapter.

The following are instances in point:—A sweeper from Chinkoa, an infected village in the Punjab, worked in Kulewal, in the house of a person who died of plague. The sweeper received as a present some of the patient's clothes. He took these home to his non-infected village and gave some of them to a neighbour. This neighbour was attacked by plague shortly afterwards, and he appears to have been the first person who developed plague in Chinkoa. Inquiry did not reveal any other source of infection, except the clothes which had been given to him.—In Kariam, in the Punjab, the first case occurred in a man who, six weeks before he was attacked, had visited infected villages in which there had been plague three months previously, though not since that time. The man admitted he had brought clothes from these villages, and no other source of infection could be traced. It is stated that the man had not been out of his village for six weeks previous to his attack.—A woman, a beggar by profession, living at Sotran, in the Punjab, begged from the surrounding villages, including Khankhanan, an infected village, from which she received clothes before the village was cordoned. On the 8th or 9th of March she received some clothes from the house of a sister-in-law, who lived in an infected village. On the 13th of March three or four rats were seen in her house in a dying condition, staggering about as if they were drunk. She was attacked with plague on the 18th of March, and died on the 24th of March. Unfortunately there is no evidence to show that the woman was not exposed to infection between the 8th of March and the 13th of March. Though she was an old woman, and not likely to go far, yet she would hardly follow her profession as a beggar without going into many places and houses.—At Dahan, in the Punjab, the first case of plague occurred in a house in which property and clothes from an infected village had been deposited on the 12th of February 1898. The first case did not take place till 23 days later, on the 7th of March. A number of dead rats were afterwards found in this and neighbouring houses. A similar case is said to have occurred at Parowal.—Dr. Godinho told us of an instance where a man developed plague in a fatal form in Bombay from using the blanket of his uncle who had a month before succumbed to plague at Lanauli. There was no local source of infection in the house at Bombay in which the case of plague occurred.—Mr. Dhurandhar, an officer of the Baroda State, cited the following instance. Three persons left Bombay to escape from the plague there, and on their way to their village in the Baroda State stopped for three days at Ahmedabad. They left some clothes in the house in which they stopped, and three days after they left that house plague cases occurred in the room which they had occupied. The refugees themselves did not contract plague.—Mr. Dodgson told

Instances in which introduction of plague infection attributed to clothes. 10,747. 10,394. 10,082. 10,091. 10,093. 17,994. 15,202–4. 22,024.

us that in 1897 a store-keeper in the camp at Satara went, after the plague had ceased, to live in the village of Kamatpura. A month or two afterwards plague broke out at Kamatpura, and the general opinion was that this man had been stealing the clothes from the dead bodies of women in the camp at Satara, and had given them to his own wife. His wife was the first person who got the plague, and clothes that had belonged to the dead, which he could not account for, were found at his house.—Another similar case, also at Satara, came under the notice of Mr. Ghanekar. This
22,527. was the case of a man employed at the burning place at Satara, who was reported to have taken clothes from dead bodies and given them to his relations. The man himself did not get the plague, but the disease is said to have been carried by the clothes from
20,066. Satara to a village in its neighbourhood.—A man took with him to the village of Ladgarh, in the Ratnagiri district, the clothing of his wife who died of plague in Bombay. Shortly after this, three persons in the man's family developed plague. The man himself appears to have remained well.

1,0870—99. A case was reported by Captain Davidson, I.M.S., as having occurred in Multan, a town in the Punjab, remote from any plague-infected place. It appears that a Eurasian female Assistant Surgeon fell ill on the 21st of December 1898. On that date she was suddenly seized with fever and nausea, and within a very short time a painful tumour developed in the left groin. The patient had high fever and headache. The gland began to enlarge, and the pain was so great as to prevent sleep. The patient was seen on the evening of the 24th of December by Captain Davidson, who found that her temperature was 101½° F., and that there was a great deal of effusion, so much so that the gland could be felt only with difficulty. The fever continued till the 27th of December, when the temperature returned to normal, and after rising again in the evening returned to normal again on the 28th. When fluctuation was detected the bubo was opened and about half an ounce of pus was evacuated. The temperature then finally became normal, the patient being discharged on the 9th of January. Captain Davidson was unable to find any cause other than plague to account for the symptoms, and had no hesitation in diagnosing the case as a genuine case of plague. This diagnosis is entitled to considerable respect from the fact that Captain Davidson had had wide experience of plague. On inquiry it was discovered that the patient obtained all her clothing from Bangalore. In October she obtained three blouses from Bangalore, where plague was then epidemic. She had, however, worn one or more of these blouses for a considerable time before she was attacked. It was stated that plague cases had occurred in the family of the tailor who made the blouses, but there is considerable uncertainty on this point.

Conclusion that plague infection may be imported in clothing. Supplement to the 26th Annual Report of the Medical Officer to the Local Government Board, England, 1896-7, pp. 129-38.

240. Such cases as those quoted above are not absolutely conclusive as to the conveyance of plague into uninfected places by clothes. In each and all of these cases a doubt may be entertained whether clothes constituted the only possible means of infection. That plague can be conveyed by clothes to distant places is, however, placed entirely beyond the reach of reasonable doubt by the fact that two Goanese stewards, on their arrival in London in September 1896, contracted plague in a fatal form from wearing clothes which they had brought with them from Bombay, and kept in a box until their arrival in England.

VII.(4).—*Introduction of Plague by Means of Merchandise.*

Circumstances of the up-country trade from Bombay. Paras. 12, 95, 41, 57, 61, and 74 of this Report.

241. We have already seen, in connexion with our discussion in Chapter II. of this Report on the origin of plague epidemics in the larger towns of India, that the suggestion that plague had been imported in articles of merchandise was one of the suggestions brought forward to account for the introduction of the disease into Bombay, Calcutta, Karachi, Hubli, and Bangalore. We saw also that this suggestion was in the case of each of these towns quite unsupported by anything in the nature of positive evidence. At first sight, it might appear astonishing that infection had not been conveyed to many places within India by means of merchandise, inasmuch as Bombay is a distributing centre from which thousands and thousands of tons of
Para. 227 of this Report. merchandise pass inwards to all parts of India. We may, however, in this connexion, again place before ourselves the consideration, already brought forward in the part of our Report which treats of the infectivity of plague, that the goods which are most likely to become infected are food stuffs which have been stored in places to which infected rats may have had access, and rags which may have been infected by plague patients. When we consider the broad features of Indian trade, it will be seen that the merchandise that is distributed from Bombay to other parts of India consists for the most part of imported goods and of textile fabrics manufactured in mills, neither of which are at all likely to be contaminated with plague: and the only item of

merchandise passing up-country from Bombay which might fall under suspicion in connexion with the conveyance of the plague to inland centres are gunny-bags, which are brought into Bombay filled with grain, and returned to the districts empty for refilling.

Gunny-bags suspected of being involved in introduction of infection into Bhujpur.

In addition to the case of Karachi, where, as we have seen, the suggestion that the infection was imported in gunny-bags was quite unsupported by evidence, we find that the suggestion has been made also in the case of the village of Bhujpur in the Cutch State.

The facts concerning the origin of the outbreak in Bhujpur, so far as they were communicated to us, are as follows. The first case of plague in the village was that of a Banniah who traded in gunny-bags, which he imported from Bombay. He fell sick with plague on the 31st of July 1898. On the same day two other Banniahs, who were said to have purchased gunny-bags from him, also fell ill of the disease. There was no plague in the immediate vicinity, the nearest infected place being Cutch Mandvi, some 18 miles distant. It was affirmed that the Banniah who first fell ill had not recently been at Cutch Mandvi. The gunny-bags which he sold must have been imported from Bombay some two months or more previously, as communication with Bombay had been interrupted for that time owing to the prevalence of the monsoon. If the circumstances of this outbreak are correctly reported, we should then have in Bhujpur an instance—and we would note that this is the only reported instance—in which a primâ facie case has been made out tending to incriminate gunny-bags as having been responsible for the importation of plague. But, in view of the fact that the three Banniahs who contracted the disease must have had trade relations with Cutch Mandvi, the possibility of the plague having spread to Bhujpur by means of human communication or of clothes, is not excluded.

13,463—71.

Circumstances of the trade between infected and uninfected districts up-country.

242. Passing now from the consideration of the Bombay trade to the trade which is carried on between infected and uninfected districts up-country, consideration will show that the only trade operations which might fall under suspicion as being possibly involved in the spread of plague are the small operations of trade by which grain is conveyed from an infected to an uninfected village. But such conveyance of grain from one village to an adjacent village is hardly likely to occur apart from direct human communication between the two villages, which, as we have already seen, is chiefly responsible for the spread of plague. For this reason, in most cases, it would be difficult to obtain evidence of the conveyance of infection by grain, even if such conveyance had taken place. As a matter of fact, no case has been brought before us in which the importation of plague into any village has been attributed to grain.

Outbreaks of Mahamari in Kumaun attributed by local opinion to importation of infected grain.

App. XXV., paras. 120, 130, 218.

App. XXV., paras. 159, 165, 233.

Vol. II., p. 342, Appendix XXV., para. 130.

243. We think it, however, interesting to note that in the Kumaun Hills, where, as we gather from Surgeon-General Planck's Report, grain is frequently carried from village to village, the origin of plague epidemics is not infrequently ascribed by local opinion to the importation of grain from infected villages. Surgeon-General Planck inquired into the origin of the outbreaks in 40 villages, and out of the 16 cases of the villages in which any theory could be advanced as to the cause of the outbreak, grain was by the people incriminated in three cases, while in two cases the outbreak was attributed to the importation of grain and ghi, and in the sixth case to the importation of ghi. The only one of these cases which appears to us to be worthy of citation is the following, as narrated by Surgeon-General Planck in his Report :—"Some time in November last, a deaf young man, nephew of Rup Singh, went to Tanda, taking radishes to exchange for rice. The rice he obtained was taken out of the house in Tanda to which a case of gola disease (plague) had been brought from Kausani, as will appear hereafter. The deaf nephew carried the rice to Nainoli, and it was stored in Rup Singh's house. On the 15th January this rice was husked by the women of Rup Singh's house, and it was cooked and eaten by all the members of the family. On the 18th of January, Rup Singh's daughter, aged 12, who had never left the village in her lifetime, was taken ill with symptoms of continued fever, ending in delirium, insensibility, and death on the fourth day. Before she died, a small glandular enlargement was seen in her groin. On the 19th of January, Rup Singh's son, a boy, was taken ill and died on the fourth day also. On the 21st of January, Rup Singh's wife was taken ill and died on the sixth day. And on the 25th of January, Rup Singh's daughter, aged 16, was taken ill and died on the fourth day. The symptoms in all four cases were alike, and bubo was seen in three out of the four cases. After the fourth death occurred, Rup Singh, his third daughter, and the deaf nephew—all the remaining members of the family—hastily vacated the house, and took up their abode in a shed on the northern aspect of the mountain, far from the village site." The other cases referred to in Surgeon-General Planck's Report do not call for special consideration.

VII.(5).—*Concluding Remarks regarding Means by which Infection of Plague is introduced into Uninfected Places.*

Infection conveyed to uninfected places chiefly by travellers.

244. We now proceed to summarise the results of our examination of the evidence bearing on the question as to how the infection of plague is introduced into uninfected places. We see that it has been spread chiefly by the agency of travellers. In many cases these travellers were persons who, having contracted the infection in infected places, sickened of plague when they arrived in uninfected places. In other cases these travellers did not themselves sicken of plague, but carried the infection in their effects and communicated it to persons living in uninfected places who had not themselves been in direct contact with plague cases.

Spread of plague from Bombay has taken place along line of railway and steamship communication, but this spread has been slow.

245. A glance at the maps illustrating the distribution of plague in India (which are printed as an Appendix to this Report) will, in itself, suggest that plague must have been conveyed in this way. That series of maps would seem to indicate that plague spreads along the lines of human communication, and, in particular, along the lines of railways and steamships. Inconsistent with this suggestion would appear to be the circumstance that, though all parts of India are within a very few days' railway or steamship journey of Bombay, plague has, as a matter of fact, spread out only in a very gradual manner in a centrifugal direction from the infected centres, attacking in each successive year a belt of country just beyond the confines of the last infected tract. The combined influences, however, of two circumstances satisfactorily account, in large part, for the fact that the spread of plague outwards from infected centres has been slow. These circumstances are, on the one hand, the fact that owing to the low degree of infectivity of plague in general—a characteristic which has already been demonstrated above—a considerable number of actual importations appear in most cases to occur before plague becomes indigenous in a place, and, on the other hand, the fact that a system of measures was put into force to prevent the travelling of plague-infected persons, and to intercept them *en route* and at their place of arrival, or finally to detect them if they succeeded in effecting an entry.

This slow rate of spread is due, in part, to railway measures. Section IV.(3) of Chapter VI. of this Report.

246. The facts with regard to the number of plague cases stopped while the system of examination on railways was in vogue are dealt with in a subsequent section of this Report, devoted to the consideration of measures adopted to prevent the spread of plague by railway. All that we need say here with regard to the interception of plague patients *en route* will be that one reason why plague has been imported into places near infected centres rather than to places at a distance from such centres is that the incubation period of plague would, in the case of patients proceeding on long journeys, expire before they reached their destination, while this would only rarely occur in the case of persons proceeding on relatively short journeys. Some estimate of the number of incubating patients, who, in the case of towns situated at only a short distance from infected centres, in spite of the precautions taken to intercept them *en route*, had, in the period antecedent to the outbreak of indigenous plague in those towns, succeeded in arriving at their destination, may be formed from the following figures.

12,856. 12,859. 12,862. 21,687–8. 13,906. 15,556. 15,562. 15,750. 18,086. 18,088. 12,023. 12,025. 24,225, 24,227.

At Poona the first imported cases were detected in October 1896. In December 1896, 63 railway passengers from Bombay were discovered at Poona to be suffering from plague; it was only at the end of that month that the first indigenous case occurred. Again, in the period between May 1898 and February 1899, which elapsed between the end of the second and the commencement of the third epidemic, 30 imported cases of plague were discovered at Poona, but no indigenous case occurred.—The case of Ahmedabad is even more striking. In October 1896 there were 10 imported cases and two indigenous cases. In the next three months there were 62 further imported cases of plague detected either at the railway station or in the town, and no further indigenous cases arose. In the succeeding four months, in which there were 27 fresh importations, there were 26 indigenous cases. After this, indigenous plague died out and the importations became fewer. Only 15 such importations were traced in the next 11 months, when only one case of indigenous plague occurred.—The first imported case detected at Surat was found on the 8th of December 1896. No indigenous case of plague was found till the 21st of February 1897. In the interval 32 more imported cases had come to light. Again, between March 1898, the date when the second outbreak at Surat ceased, and February 1899, 16 imported cases were found, and there were no indigenous cases.—About 35 imported cases were discovered in Hyderabad (Sind), the first in January 1897; the first local case was found at the end of February 1897.—At Rohri, in Sind, which is separated from Sukkur only by the Indus, an imported case was first detected on the 7th of March 1897. Twenty-two cases imported from Sukkur were detected before a local case of plague was discovered on the 17th of April.—In 1896, 26 imported cases were found at Ahmednagar. No local case

was reported till the end of October 1897.—At Nasik, according to one witness, 26, and, according to another witness, 35 imported cases of plague came to the notice of the authorities between March 1897, when the first case was detected, and October 1897, when local cases began to occur.—A similar sequence of events was observed at Cutch Mandvi, which communicates by sea with Karachi and Bombay. Between the 3rd of October 1896 and the 28th of February 1897, 47 imported cases were detected in Cutch Mandvi; local cases did not begin to occur till March. 16,966, 14,705. 23,319. 13,360. 13,362. 13,760.

247. These figures do not differentiate between patients intercepted on arrival and persons found sick in the towns, but since they relate for the most part to persons who were intercepted at railway stations and were then isolated, and practically for the whole of the remainder part to persons whose detection in the towns was promptly followed by measures of isolation and disinfection, the figures given above represent importations which are not likely to have been instrumental in actually spreading plague. These figures, therefore, cannot be legitimately cited as conclusively establishing that a large number of importations did, in the towns in question, occur before the plague established itself in an indigenous manner. They are, however, valuable as affording an indication of the number of plague-infected persons who must have been endeavouring to introduce themselves by railway into the towns in question, not to speak of the many who endeavoured to introduce themselves surreptitiously by road. Even under the most favourable circumstance, it is obviously unlikely that anything like the total of the incubating travellers who got into an uninfected town would be detected. The true number, consequently, of plague cases introduced into towns along the railway in the neighbourhood of Bombay must have far exceeded the numbers of plague patients set down in the official returns as found on the railways or in the towns. The deduction thus seems to us plain that, in addition to the railway arrangements, the comparatively slight infectivity of plague is responsible for the slow rate at which plague has, in spite of the railways, spread outwards from infected centres. It would seem to be the rule for each town in turn to become infected when it becomes so near to an infected centre as to receive from it a very considerable number of incubating patients.

This slow spread is due also to low degree of infectivity of plague.

What applies to the spread of plague by means of railways manifestly applies also to towns connected together by means of sea or road communication.

248. Before dismissing the consideration of the causes of the slow rate at which plague has spread from infected centres in India, we may notice that some observers have put forward the suggestion that plague cannot be conveyed a long distance per saltum, either by rail or steamship, because the bacillus, in coming from a distant place, is not acclimatised to the new surroundings into which it is introduced. This theory is briefly considered later in this Chapter in connexion with the epidemiological problems which arise in relation to the spread of plague.

It has been suggested that it is due also to time required for acclimatisation of the bacillus to new conditions. Section XI.(G) of this Chapter.

249. In comparison with travellers, all other agencies which have been engaged in the distribution of plague from place to place are quite unimportant. Rats, as we have seen, may, in a few isolated instances, have carried the infection, but probably only between closely neighbouring villages. Exceptional also are the cases where clothes, apart from travellers, have carried infection. Quite as exceptional is the origination of an epidemic by the importation of the infection in articles of merchandise.

In comparison with travellers, all other agencies by which plague may be introduced into uninfected places are unimportant.

In conclusion, we think that we cannot better summarise the relative importance of the various agencies which have been operative in the spread of plague throughout India than by reviewing in synoptical form the very careful observations collected by Captain James, I.M.S., with reference to the spread of plague in the Punjab. These observations are, in brief summary, as follows :—

Report on the outbreak of Plague in the Jullundur and Hoshiarpur districts of the Punjab, 1897-8.

Total Number of Villages, out of 91 investigated, where the source of Infection was traced.	Number of Villages into which Infection was imported by Persons who themselves developed Plague after Visits to or Residence in infected Villages.		Number of Villages into which Infection was imported by Persons who themselves developed Plague after coming into contact, in an uninfected Place, with People from an infected Place.		Number of Villages into which Infection was imported in infected Articles.		Number of Villages in which all the Sources of Infection could be excluded, except Rats or infected Clothes.	
	No.	Per cent.	No.	Per cent.	No.	Per cent.	No.	Per cent.
65	42	65	5	8	16	24	2	3

VIII.—Spread of Plague over Sea to Countries outside India.

VIII.(1.) *The Ways in which Infection may be conveyed on Board Ship.*

Spread of plague over sea to countries outside India.

250. In passing now, from the consideration of the means by which plague is imported into uninfected towns and villages in India, to discuss the question of the spread of plague from India to countries over sea, we may begin, before discussing individually the various ways by which plague might be carried over sea, by placing before ourselves as complete a catalogue as possible of the various ways in which the infection might be conveyed on board an outward bound ship.

I. Means by which the infection might be conveyed on board.

The infection of plague might be received on board a ship by the embarkation—

(*a*) of a person who was actually suffering from plague;
(*b*) of a person who, though not sick at the time of embarkation, had already contracted the disease;
(*c*) of plague-infected clothes or other personal effects;
(*d*) of plague-infected merchandise and articles of commerce; and
(*e*) of rats which had previously to their arrival on board contracted the infection of plague.

(*a.*) Measures taken to prevent the sailing from Indian ports of persons suffering from plague have been very effective. Section IV.(1) of Chapter VI. of this Report. 25,479.

251. In discussing the measures which have been put in force to prevent the spread of plague from India over sea, we shall show that the system of medical inspection of passengers and crews which is carried out on shore previously to embarkation is so carried out as to make it practically impossible for a person who is actually suffering from plague to be received on board. Not only is the system of medical examination directed to stop persons who show overt signs of plague, but it is directed also to the stoppage of all cases of bubo or of fever which may seem in any way suspicious. So much is this the case that out of 25,011 persons, which was the total number of persons prevented from sailing from Bombay to all parts between the 1st of January 1897 and the 15th of March 1899, only 243 persons, or about 1 per cent., were afterwards known to have developed plague. The examination thus is conducted in such a manner as to obviate all danger of the admission on board ships of persons suffering from overt signs of plague.

(*b.*) Impossibility of taking effectual measures against embarkation of incubating persons.

252. On the other hand, no precautions short of placing everybody in quarantine for the full period of incubation before embarkation could possibly prevent the embarkation of persons who were in the earlier incubation stages of plague. Very few persons, however, appear to have embarked at Indian ports in the incubation stage of plague.

Enumeration of the very few cases where persons have embarked in incubation stage of plague on ships bound from India to Europe.

The following isolated cases, which have occurred on board ships proceeding from Bombay to Europe, Aden, and Red Sea ports, appear from the information at our disposal, to have been the only cases admitted while in the incubation period of plague on board ships sailing for these destinations.

Pekin. 752. *Dilwara.* 755. *Carthage.* 757.

Our list begins with two cases which occurred before the pilgrim traffic from infected ports in India was definitely stopped. These cases occurred on board the *Pekin*, a pilgrim ship, in December 1896, between Bombay and Aden. No further cases developed on board.—Next, we have two cases which occurred on the troopship *Dilwara* in March 1897, before medical inspection was extended to passengers leaving Bombay on troopships. The cases of plague on the *Dilwara* occurred during the voyage between Bombay and Suez. No further cases occurred on board.—Thirdly, we have a case which occurred on board the *Carthage*, a mail steamer. On the 6th of July 1898, four days after starting from Bombay, a native member of the crew was attacked. As we shall see presently, a second case diagnosed as plague afterwards took place on board.—Fourthly, on the 18th of April 1898, a case of plague developed on board the *Clyde* before its arrival at Aden, five days after this ship had sailed from Bombay. No further cases occurred on board.—Fifthly, two cases which were pronounced to be possible cases of plague were taken out from the *Caledonia* at Suez in November 1898.

Clyde. 761. *Caledonia.* 25,489.

We have thus, on vessels proceeding from Bombay to Aden, the Red Sea ports, and Europe, eight cases of illness which, in default of exact information, we have reckoned as cases of plague contracted on shore and embarked while incubating the disease. Even supposing the full number of eight to have been genuine cases of plague—and the diagnosis appears, in the cases on board the *Caledonia*, to have been doubtful—these constitute quite a minimal fraction of the passengers who embarked in Bombay for Aden, the Red Sea ports, and Europe. If we exclude from consideration the cases on board the pilgrim ship *Pekin*, which did not fall within the period covered by the

figures presently to be cited, and on board the troopship *Dilwara*, whose passengers are not included in the statistics furnished to us by Major Crimmin, I.M.S., Port Health Officer of Bombay, we arrive at the result that out of a total of 132,000 persons examined on sailing from Bombay for Europe, Aden, and the Red Sea ports, between the 1st January 1897 and the 15th March 1899, only four cases of plague occurred.

Enumeration of cases of incubating plague embarked on board ships sailing from India to destinations other than the Red Sea ports and Europe.

In addition to the cases mentioned above, a few other plague cases which have to be included in the present category occurred on ships sailing from Bombay and other ports in India for destinations other than Aden, the Red Sea ports, and Europe. Our information regarding these cases is as follows :—

One case of plague occurred on board each of the ships *Ballarat* and *Bengal*, in August and October 1898 respectively, between their departure from Bombay and their arrival at Colombo. No further cases occurred on board these ships.—In January 1899 a case of plague occurred on board the *Buldana* bound from Bombay to Rangoon. There appears to be evidence to show that the patient probably contracted the disease on shore.—In July 1899 two cases of plague were found on board the *Dictator* at Suez, the vessel having sailed on the 14th June from Bombay, *viâ* Karachi. This fact was brought to our knowledge unofficially, and we have no accurate information as to the source of infection; it is possible that the infection may have been contracted on shore.—A number of cases occurred in December 1898 on board the *Bhundara*, a vessel which was conveying coolies from Karachi to East Africa. Three of the coolies who developed plague on board appear to have contracted the disease on shore. One of them fell ill on the day after the ship left Karachi, and two appear to have contracted the disease within five days after leaving port. Subsequently, five further cases occurred on board.—A case of which we have information only through an indirect source is that of the *Annie Maude;* it appears that, about the beginning of March 1897, a fatal case of plague occurred on board shortly after the vessel left Calcutta, and two other cases were said to have occurred subsequently.

Ballarat.
Bengal.
765.
Buldana.
25,491.
Dictator.
Bhundara.
12,250.
25,495-6.
Annie Maude.
26,934.

(c) Little risk of infection being carried on board ships leaving India by conveyance in infected articles.

253. We shall have occasion hereafter to describe the precautions taken at Bombay and other Indian ports to prevent infected clothing being carried on board by native crews, deck passengers, and other persons likely to carry the infection. How little, under the present arrangements, is the risk of plague being carried on board in infected clothes from the infected ports in India, will be obvious from the fact that between September 1896, when plague broke out in Bombay, and July 1899, there appear to be in all only 15 instances on record in which the disease is said to have occurred on ships that have sailed from India. In 11 of these cases—these being the cases to which we have already referred—there is a possibility that the disease had been contracted on shore. In the four remaining instances, to be referred to presently, the infection must have been contracted on shipboard. In two of these instances—we refer to the cases of the *Shannon* and the *Patna*—dead rats were, as we shall see, found on board, and the disease appears to have been derived from these; but it does not appear whether the rats infected themselves on board or on shore. The third instance was that already mentioned in the section of this Report dealing with the infectivity of clothes, in which two Goanese stewards contracted plague in 1896, on their arrival in London. Finally, in December 1898, a case of illness, which was diagnosed as pestis minor, occurred on board the *Golconda*. Here (always assuming that the diagnosis arrived at was correct), we think that the infection must have probably been contracted from infected material conveyed on board.

Para. 693 of this Report.

Para. 258 of this Report.

Case of Goanese stewards.
Para. 240 of this Report.
Golconda.

Incidental mention of a plague case on a ship arriving in Bombay from Hong Kong.

In this connexion, we may refer to an interesting case of the conveyance of plague to India from Hong Kong. On the 20th of March 1899, a Chinaman was landed in Bombay with plague, from the steamship *Bormida*, which had come from Hong Kong, after touching at the non-infected ports of Singapore and Penang. The Chinaman in question, who was shipped in Hong Kong as a cook on the 26th February, two days before the ship sailed, was found on arrival at Bombay to be suffering from plague. Nothing satisfactory could be elicited either as to the source of his infection, or with regard to the date on which he sickened. The diagnosis of plague was, however, placed beyond doubt by the bacteriological examination undertaken by Lieutenant Liston, I.M.S.

Bormida.
25,506.

(d.) Conveyance of infection on board by embarkation of merchandise.

254. The question as to whether the infection of plague has been conveyed on board by means of infected merchandise, is one which we can hardly expect to resolve from considerations relating to the occurrence or non-occurrence of plague on ship-board, inasmuch as it is hardly likely that cargo could, during transit, give origin to plague

cases in man on board the ships in which it was being carried, unless possibly indirectly by communicating the infection first to rats. The question whether infected merchandise has been shipped from India is thus a question which can be determined only in an indirect manner, by seeing whether Indian merchandise has ever been instrumental in spreading infection in countries over sea. This will be considered later.

Para. 262 of this Report

(*e.*) Conveyance of infection on board by embarkation of infected rats.

255. With regard to the suggestion that plague may be conveyed on board by the embarkation of infected rats, we have to note that if this suggestion had any force in it, plague would in 1896, when plague was raging among the rats in the Mandvi quarter, infallibly have been rife on board the ships when lying in or after leaving the Bombay Docks. It, however, appears that no plague cases occurred among men in the ships in the harbour, and that plague mortality among rats was noticed on out-going ships only upon the *Shannon* and the *Patna*, in both which cases, as already noted, the infection may quite well have been conveyed on board in infected clothes or goods. There would thus appear to be no confirmation of the suggestion that plague may be conveyed on board by the embarkation of plague-infected rats.

Para. 252 of this Report.

VIII.(2.) *The Spread of Plague on Board Ships.*

II. The question of the spread of plague on ship-board.

256. Having considered the various ways by which plague may be conveyed on board, the next question which emerges in connexion with the conveyance of plague over sea is the question as to whether the disease shows any tendency to spread on ship-board. The consideration of this question will best be approached by passing in review the course of events in the outbreaks of plague enumerated above as having occurred at sea on ships leaving India.

Spread of plague on board ships when incubating patients have been embarked.

On the *Clyde*, the *Ballarat*, the *Bengal*, the *Buldana*, and the *Golconda*, the outbreak of plague was limited to the occurrence of a single case. On the *Pekin*, the *Dilwara*, the *Carthage*, the *Caledonia*, and the *Dictator*, the outbreak was in each case limited to two cases, and it is to be noted in passing that the diagnosis was doubtful, in the case of the second patient on board the *Carthage*, as well as in the case of both the patients who suffered on board the *Caledonia*. On board the *Annie Maude* three cases of plague occurred, and on board the *Bhundara* eight cases. The above *resumé* clearly shows (especially when it is considered that the *Pekin* was crowded with pilgrims, and the *Bhundara* with coolies) that plague has manifested very little tendency to spread on ship-board, even in cases where it must have been impossible or difficult to apply elaborate measures of isolation and disinfection.

Spread of plague on ships on which rats were infected with plague

257. If we look to what occurred in the case of the two ships on which mortality was noticed among rats, we learn the same lesson.

Shannon. 759.

In the case of the *Shannon* in March 1898, dead rats were found on the ship on a voyage from Bombay to Aden, and also afterwards on the return voyage to Bombay. When the ship was returning from Aden to Bombay, and nine full days after the commencement of the outward voyage from Bombay, a postal official, whose cabin was near to the place where some dead rats were found, fell ill with plague. We must assume this postal official contracted plague at sea from the infected rats. There was no further spread of the disease.

Patna. 763, 12,248.

On the *Patna*, when on a voyage in March 1898 from Bombay to the Persian Gulf *viâ* Karachi, a fireman fell sick of plague and was landed at the latter port. Some days previously the cabin passengers complained of a smell such as would have been produced by dead rats. Later, after the vessel had left Karachi, two dead rats were found by one of the ship's officers. According to one account, three men then sickened, one with typical symptoms of plague and the two others with symptoms which were suggestive of plague; according to another account, there were on board the vessel after it had left Karachi four cases of plague, of which two ended fatally.

VIII.(3.) *The Ways in which Plague Infection might be carried from an infected Ship on Shore at the Port of Arrival.*

III. Plague might be carried on shore from an infected ship by—

(*a.*) Disembarkation of patients suffering from

258. The next question which comes up for consideration is the question as to how plague may be conveyed on shore from a ship in which the infection of plague has been carried.

We need hardly discuss the possibility of a patient found suffering from manifest plague being landed elsewhere than at a Quarantine Station. Only two contingencies

have, therefore, to be considered in connexion with the landing of plague patients. These are, first, the contingency of a patient landing while suffering from pestis ambulans, and, secondly, the contingency of a patient landing while in the incubation stage of plague. clinical and well characterised plague.

259. In connexion with the first, it is to be remembered that pestis ambulans is a very exceptional manifestation of plague. It is thus much more likely that cases which are not due to plague will be diagnosed as pestis ambulans, and be referred to a Quarantine Station, than that real cases of pestis ambulans will be allowed to land as unsuspicious. Again, if a real case of pestis ambulans were allowed to land, the risk of the infection being spread by such a case would, as we have seen in discussing the infectivity of pestis ambulans, be very small, inasmuch as in such cases the bacteria neither effect an entrance into the blood stream, nor do they, as a rule, escape from the body in pus from the bubo. In fact, the danger of spread from the landing of cases of pestis ambulans would hardly require consideration, were it not for the fact that, in exceptional instances, cases of mild plague may, owing to a subsequent weakening of the resisting power of the organism, assume a serious form, accompanied by a generalisation of the plague bacillus throughout the body. (*b.*) Disembarkation of cases of pestis ambulans. Para. 214 of this Report.

260. In connexion with the second of the contingencies just referred to, it is to be noted, firstly, that the incubation period of plague does not, as we have shown, exceed five days, and, secondly, that the nearest port of call on the route to Europe from India is, even in the case of mail steamers, five days' sail from Bombay. It will thus be impossible for any person who has contracted plague in India to land in the incubation period, even at the first port of call, much less at any port in Europe. The danger of the introduction of plague into any European country by the landing of a person in the incubation stage of plague may therefore be excluded, unless in a case where the infection has been contracted on board ship. It is not likely that infection should be so contracted, unless in the case of ships in which human plague cases have occurred during the voyage, or on which mortality has been observed among rats. And even here, as will be clear from the fact that plague, as a rule, shows no tendency to spread on ship-board, it is very unlikely that any person would be incubating plague at the time of landing. Even less to be apprehended than the landing of a person in the incubation stage of plague from a ship on which plague cases have occurred among men, or mortality has occurred among rats, is the likelihood of the landing of a person in the incubation stage of plague contracted from infected clothes or personal effects on board a ship on to which infection may have been carried in clothes or personal effects. Such small danger as might be associated with the disembarkation of passengers in the incubation stage of plague seems to us most appropriately dealt with by keeping all passengers who land from ships on which men or rats have suffered from plague under surveillance for a short period after their disembarkation. (*c.*) Disembarkation of incubating patients. Para. 197 of this Report.

261. The risk we have just considered, in connexion with the possibility that persons who have come into contact with infected clothes might contract the disease on board and be in the incubation stage of plague at the time of landing, is a risk of the same order as the risk of the direct disembarkation of infective material in infected clothing. Experience, however, shows that this risk is so small as to be practically negligeable, no case being on record of plague being contracted on shore from the landing of infected clothing. The nearest approach to such an event occurred in connexion with the case of the Goanese stewards previously referred to, but it is to be noted that that case occurred before plague was discovered in Bombay, and, therefore, anterior to institution of measures for the prevention of the spread of plague from India. In addition to measures of precaution which are now applied in India, in the enforcement of the disinfection of suspected clothing, it is to be noted that the sanitary regulations of many European countries prescribe the disinfection of all personal linen on board all ships before arrival from infected ports. (*d.*) Disembarkation of infected clothes. Para. 240 of this Report.

262. The question of the possibility of plague being carried into countries over sea by infected merchandise is a very wide question, affecting as it does the general operations of trade. It would seem to us that the most important point to consider with regard to this question is the fact that, though plague might conceivably be conveyed in this manner, millions of tons of Indian merchandise have been imported into the great European ports, without giving origin to any outbreak of plague, either among the stevedores who handled the cargoes in bulk, or among the operatives who dealt in detail with the merchandise. The event has in these cases conclusively (*e.*) Disembarkation of infected merchandise.

Para. 659 of this Report.

confirmed the anticipations based on the bacteriological observation that, in the case of merchandise which has been artificially infected by plague cultures, the plague bacillus is rapidly destroyed by desiccation. On the other hand, it would not be legitimate to take our stand on the fact that the introduction of infection into countries over sea has in no case been brought home to merchandise. It would, in other words, not be legitimate to argue, from the non-occurrence of any outbreak directly traceable to merchandise that the introduction of infection in merchandise is impossible. On the contrary, a general survey of the facts relating to the outbreak of plague in Europe and other countries outside India, indicates that the introduction of plague infection during the last two years may, in a number of places, have taken place altogether independently of the landing of any plague-infected patient or his effects—we may instance the cases of Madagascar, Oporto, Sydney, and Glasgow, in 1898 and 1900. In other cases it is plain that the human plague patient who was discovered ill of plague contracted the disease from infective material carried on board—we may instance the cases of plague which were found in South Wales and in Hamburg in 1900. It is difficult to explain these importations in any other way than by supposing that merchandise may, under some unknown, and happily rarely realised, conditions, retain the infection for prolonged periods.

IX.—Agencies by which Plague is disseminated within the Limits of an Infected Place.

IX.(1.) *Spread of Plague in infected Places by Human Agency.*

Importance of human agency in spreading plague within infected places.

Section VI. of this Chapter.

263. We have already, in the portion of our Report which deals with the infectivity of plague, seen that a man can contract the infection of plague—(i) by coming into contact with plague patients (and patients who are suffering from the pneumonic form of plague, or who are in the last stages of bubonic plague, are those whose infectivity comes specially into consideration here), (ii) by coming into contact with infected rats, (iii) by coming into contact with clothes and other infected articles, and (iv), lastly— and this would appear to be the commonest source of infection—by visiting, or residing, in a house which has been infected either by men or rats. Again, in the section of our Report in which we have dealt with the various agencies by which plague may be imported into uninfected places over land, we have seen that man himself must be regarded as an agent of predominant importance in the distribution of the infection of plague, and that infection has been carried about from village to village and from town to town pre-eminently by persons who have themselves contracted the infection, or, who, failing that, have carried back with them infected articles. It will not be necessary, therefore, in this section to labour either the general point that man must be an important agent also in carrying about plague within the limits of an infected place, or to establish by a series of specific instances that within the limits of towns and villages the infection has been carried about in connexion with visits paid to plague patients, in connexion with attendance at plague funerals which may involve, among other things, contact with the corpse, and in connexion with visits to infected houses in which plague has occurred either among men or among rats. We may, however, briefly advert here to a few incidental facts which bear upon the points to which we have just referred.

Section VII. of this Chapter

Spread of plague by attendance on funerals.

264. In illustration of the spread of plague in connexion with attendance at funerals, we may consider the following histories.

9702–9.

The infection was brought to the Sirohi State by a Banniah from Poona. He died the day after his arrival. A largely attended funeral feast was given and the clothes of the deceased man were distributed. As a sequel, twenty-six cases of plague occurred within six weeks of the funeral feast in the village to which the deceased came and in three other villages within two miles of it. All these cases occurred among Banniahs. The somewhat similar cases which occurred in the village of Gungaur, and in connexion with outbreaks of pneumonic plague at Backergunge and elsewhere, have been cited above in our discussion of the infectivity of that form of disease. In the case of Calcutta, Dr. Justice noted that "funerals are powerful factors in spreading the disease among the neighbours." Mr. Moore said that in the evacuation camps at Surat, if any one died, "his caste people came and sat round him and lamented and accompanied the corpse to the burning pyre," with the result that plague was kept alive in the camps. He also stated

9796–802.

9764.

Paras. 202–3 of this Report.

II. 538.

24,441.

that plague was kept alive in the camps because people went out and "used to hire themselves out as plague corpse bearers." 24,422.

265. Closely connected with the dangers of attending funerals, are those which are incident to death ceremonies in India. These are described by Dr. Godinho, one of the Assistant Health Officers of Bombay, as follows:—"So far as the Hindus are concerned, they have got special ceremonies which the female kind undertake. First they make an ablution, and then afterwards put some stuff into their mouths and noses and other orifices. Muhammadans close the different orifices by a plug of cotton, camphor, and spices: a rupee or some coin is placed in the mouth; afterwards this is taken off and replaced; and I have seen the womankind suffering from plague through handling these coins." Spread of plague by death ceremonies in India. 17,993.

266. The practice of certain therapeutic ceremonies would also appear to conduce directly to the spread of plague. We hear of women in the Punjab, in order to take the pain away, making passes over and touching with their clothes the buboes of infected persons. A ceremony such as this might, owing to the possibility of contamination with the excretions of the patients, conduce to the spread of plague. A similar sort of danger would seem to exist in connexion with the circumstance that barbers practice as surgeons, this practice being rendered dangerous not only by the fact that the healthy are thus placed in communication with the sick by the intermediary of the barber himself, but also by the fact that the plague may be spread by means of the razors which, after use for opening plague buboes, are, without previous disinfection, used for shaving uninfected persons. It has been suggested that similar danger may arise in connexion with the carrying about of leeches from plague patients to persons suffering from other diseases. In the case of Bilron in the Punjab, it appears that the original infection and the re-infection of the village was traced to visits made to plague patients with a view to applying leeches. Spread of plague by therapeutic ceremonies and native medical treatment. 10,104. 10,051. 17,962. III. 44. 9253. 22,287. Captain James' Report, pp. 85, 90.

IX.(2.) *Spread of Plague in infected Places by Clothes.*

267. We may pass now to the spread of plague within the limits of an infected place by clothes. Spread of plague by means of infected clothes.

In this connexion we might refer to particular instances, such as the case cited by Captain Leumann, I.M.S., in which plague broke out in a number of successive families who came into possession of a particular set of infected clothes. We might further refer to the case narrated by Captain Chaytor-White, I.M.S., where plague broke out in a camp in the neighbourhood of Kankhal in the person of a man who opened a box of clothing which had been removed from that town. Other similar cases might be referred to. But isolated instances such as these do not throw much light upon the question as to the spread of plague by means of clothing, more especially as the possibility of the infection being otherwise contracted could not in any of these cases be satisfactorily excluded. 3121. 9483. 23,957.

More conclusive with regard to the rôle played by clothing in the distribution of plague in an infected place are the opinions formed by certain experienced plague officers. Of these it will suffice to quote the following:—Mr. Winter, who directed the plague operations in Hardwar, Kankhal, and Jawalapur, had noticed that "the people removed their clothing from the houses in which cases had occurred, or from adjoining houses, to other parts of the town at Kankhal, with the result that fresh cases sprung up in different parts of the town, whereas at Jawalapur, where our measures made it less easy to so remove the clothes, the spread of plague was more easily checked."—Mr. Moore, who directed the plague operations in Surat, said, "people, when they are ordered to bring out their kit and to have it disinfected, or in a case of clothes belonging to the patient to have them burnt, often conceal a part, and it is smuggled off by a friend or neighbour, and the friend or neighbour by so doing takes plague to his own house." 8780. 9256. 24,393.

IX.(3.) *Spread of Plague in infected Places by Merchandise.*

268. We have already seen, in connexion with the infectivity of articles of merchandise and the part played by merchandise in carrying plague to uninfected places, that foodstuffs and other articles of merchandise may be infective, and that in a few cases there is reason to suspect that they have originated attacks of plague. No facts have been adduced to show that within an infected place there has been any spread of Influence of merchandise in the spread of plague within infected places. Paras. 227 and 241–3 of this Report.

plague by means of infected foodstuffs or other infected merchandise. On the other hand, Mr. Giles, the Commissioner in Sind, in reply to a question as to what were, in his opinion, the chief modes of the dissemination of plague, after having referred to
11,278. the predominant influence of rats and clothes, said: "Then there is something in the retail grain shops, the nature of which is uncertain. I do not, for a moment, say that the grain gives the infection, but the Banniahs' shops, where the grain is sold, and the flour mills, are dangerous places. That infection is spread by these grain-sellers, I think there is no doubt about at all. It kept on recurring among them."
11,430. Lieut.-Col. McCloghry, I.M.S., was of the same opinion. He said: "The petty dealer purchases his stores in bulk from infected godowns or shops . . . . and with them infects his own dwelling and the rats therein. The rats in neighbouring houses frequent his shop for food, and also become infected and spread the germs. Thus, there is a spread of the epidemic by a material brought from infected quarters. What the material is I cannot suggest."

IX.(4.) *Spread of Plague in infected Places by Rats.*

Facts illustrating severity of epidemics among rats.

269. We pass now to consider the influence of rats in the distribution of plague within the limits of an infected place. It will be convenient to commence by setting forth certain facts with regard to the severity of plague epidemics which have occurred among rats in certain places.

22,646. Lieut. Anderson, speaking of the second epidemic in the Joria Bazar of Karachi, told us that as many as 105 dead rats were found in one house; in two other houses he spoke of having found as many as 75 and 65 dead rats. With reference to the same epidemic, Mr. Giles spoke of the number of dead rats as an extraordinary sight:
11,198–205. "I used to go down every morning, and morning after morning you came to the same godown, and perhaps one morning there would be 10 dead rats lying on the ground, another morning there would be 11, and so on continuously; although the godown had been swept out and disinfected, yet the very next morning there would be a lot more dead rats on the ground. In the bags full of grain and other material, you would see the rats popping out of the bags and running in every direction and scuttling across the street by you. . . . In one house that Lieut. Anderson visited, from which the people had been cleared out about six weeks, you really could not take a step without treading on a dead rat. . . . On the staircase of that house, on one step there would be two dead rats; on the next, one; and on the next two dead rats; and so on the whole way up. You could not tread on the floor without treading on a dead rat."—The rat mortality in the Mandvi quarter, at the outbreak in 1896 of the first epidemic in Bombay City, appears to have been on a similar scale, it being
8704. possible, as Mr. Hankin told us, to pick up "bucketfuls of dead rats every morning in
14,437. the infected quarter of the town at the commencement of the epidemic." "To give an example," said Professor Muller, "of the abundance of dead rats, when Mr. Hankin arrived he drove down to Mandvi and asked for some dead rats. In five minutes I got him twenty in and around the houses in Dongri Street. He said he wanted some freshly killed rats. I went into a house and turned over some bales. Rats ran out in all directions, and I pursued and killed some with my riding-whip. They were all sick. I noticed rats on the Masjid Bandar Bridge running about as if they were drunk; you could see them lying about the streets in quantities. I have never in any part of the town seen sick, dead, and dying rats in such quantities. Mandvi being the centre of the grain godowns (stores), there are perhaps more rats there than
2269. anywhere else."—The mortality among rats in Sukkur also is said to have been "fearful"; we are told that in a particular temple 35 dead rats were found.—In a
3117. particular period of the epidemic in Hubli the mortality among rats was on a very large scale; hundreds of rats were found dead in a goods shed of which the floor was
22,125. taken up.—"In one of the worst infected quarters of Bhiwandi," said Lieut.-Col. Hay, "I remarked a bad smell emanating from a shop which had been closed for two months. I forced the door open, and on entering discovered several dead rats. One rat was actually dying. It was crawling across the floor, and I killed it with a stick. I ordered the rat holes to be opened up, and I discovered no less than 57 dead rats in various stages of decomposition. Some of them were skeletons. They had burrowed into the walls and into the floors. Plague was raging on all sides of this shop, which apparently was the centre of infection from which the disease radiated. It seemed to be the exact centre, as plague was very bad all round there."

Bacteriological observations made in connexion with above epidemics.

270. A few isolated bacteriological examinations were made in connexion with the rat epidemics referred to above. Dr. Gibson, in the case of the epidemic in the Joria

Bazar at Karachi, examined a few "mummied" rats, but this was months after the epidemic had taken place; as might have been expected, the results of these belated observations were purely negative. In the case of the rats found dead at Hubli, Captain Leumann, I.M.S., found typical plague bacteria. The fact that the rats that died in Bombay City died of plague was frequently verified by bacteriological observation, in the different epidemics, by many observers, including ourselves. The only systematic series of observations, however, on rats found dead in Bombay is a series of 18 observations placed on record by the Austrian Plague Commission. Of the 18 rats examined, 16 came from the Mandvi quarter, and the two others from other parts of the City. Of the 16 Mandvi rats, in eight it was ascertained with certainty that death was due to plague; in six others, owing to their having been received in a putrid condition, definite conclusions could not be arrived at; in one case death was due to tuberculosis; and the last of the 16 had died from violence. The two rats obtained from other parts of the City had both died of tuberculosis.

11,210. 3117. 8705. Report of the Austrian Plague Commission, II. C. 693.

271. The next question, which we have to consider in connexion with the spread of plague by rats, is the question as to how soon after the importation of infection an epidemic among rats breaks out, and, in particular, the question as to whether the rat epidemic precedes or follows upon the spread of the disease to men. In this connexion the following cases may be considered.

Question as to how long after importation of infection rats are affected and whether before or after men.

In the village of Sotran, in the Punjab, referred to above as an example of a village which was infected by the importation of clothes, rats were found in a dying condition, staggering about as if drunk, five days after the incriminated clothes reached the village. The first human cases occurred five days after diseased rats had first come under observation.—In Baroda, as we were told by Mr. K. B. Jadhav, when a locality known as Dave Mola "was evacuated at the end of March, three boxes of clothes and other kit were taken to a house in Anawala Mola. After about a fortnight dead rats were found in that house, and two or three people died of plague."—At Ladghar, in the Ratnagiri district, also previously referred to as a village which was infected by clothes, rats are said to have begun to die two or three days after the infection was brought to the village. Human cases did not occur till later.—In the case of Khandwa, in the Central Provinces, plague was imported by a traveller from Bombay who fell ill of plague seven days after his arrival. Three dead rats were found in his house within about 12 days of his arrival, the first being found two days after his death. Three days after the first dead rat was observed, a boy, who lived in the verandah of the house, fell ill of plague.—At Nasik dead rats were found three days before an indigenous case was reported.—At Ghoti, in the neighbourhood of Nasik, three or four dozen rats were found in a house in which the first imported case occurred. Dead rats were found scattered over the village before the next human case occurred.—At Bulsar dead rats were discovered about 12 days after the importation of the infection. A week subsequent to this a human case occurred.—In the village of Umbowda Budruk, in the Satara district, dead rats were found seven days before plague cases occurred among men.—In Agashi, Kelwa, Marol, Jegapur, Bandra, Pali, Chuim, Danda, Santa Cruz, and Old Kurla—all villages which came under the observation of Major Anderson, I.M.S.—mortality among rats in each case preceded the occurrence of plague cases among men. In many of these cases the dead rats were found within two to five days after the infection had been imported by human agency. The human cases which occurred subsequently appear, so far as the records enable us to judge, to have occurred generally within two to five days afterwards.

Specific instances bearing on the question. Para. 239 of this Report. 10,082. 15,170. 20,066-73. Para. 239 of this Report. 5958-61. 14,705. 14,695. 16,164 22,223. 23,207. 23,210.

In addition to the specific instances quoted above, and those quoted later in this section of our Report in the consideration of the dissemination of plague by rats broadcast through villages, we have the general experience of a number of plague officers, who were engaged in plague duties in villages scattered over many of the infected districts. In the opinion of these officers—and we may particularly mention as holding this opinion Khan Bahadur Bomanji Edalji Modi, Mr. Durandhar, Colonel Henderson, I.M.S., Mr. Shepherd, Mr. Monteith, Lieutenant Steen, Mr. Carmichael, and Lieutenant Bruce—mortality among rats generally precedes the occurrence of indigenous cases of plague. Each of these officers, however, had met with instances in which plague developed first among the human inhabitants, while it occurred in rats later on.

Opinions of plague officers on above question. Paragragh 275 of this Report. 14,072-8, 15,199, 18,107, 18,792, 19,650, 22,694, 22,922, 24,044

It would thus appear to be clear, both from the specific instances cited above and from the experience of plague officers quoted, that plague, in many instances, breaks out among rats before it breaks out among the human community.

Human epidemics not necessarily associated with rat epidemics. Annales de l'Institut Pasteur, October 1898. Para. 220 of the Report.

272. An epidemic of plague among rats does not, however, as certain witnesses who appeared before us, and M. Simond, in a paper already referred to, have assumed, in every case precede and lead up to an epidemic among men. There are, on the one hand, many instances of severe plague epidemics among men occurring independently of the outbreak of any epidemic among rats, and there are, on the other hand, as we have seen in discussing the infectivity of plague-infected rats, isolated instances of epidemics which have not been followed by plague cases among men.

Instances of epidemics unaccompanied by death of rats. 21,723–4; 15,886; 15,964, 16,998; 13,153–5; 24,090, 24,276–7; 13,929; 12,057; 9,675; 13,461.

Instances of human epidemics which have been unaccompanied by noticeable rat epidemics are the first and second epidemics in Poona and the epidemics which occurred in Surat, Nasik, Palanpur City, Ahmednagar, Ahmedabad, Rohri, Khandraoni, Cutch Mandvi, and the Cutch State generally. We cannot do better than quote in this connexion the evidence of Major Hyde-Cates, the Political Agent of the Cutch State, in reply to a question as to whether he had heard much of mortality among rats, he said:—"No. They have had dead rats in Bhadresir on two separate occasions. The houses in which these dead rats were found were treated as infected houses. No epidemic followed this death rate amongst rats, and the place has only had four indigenous cases since plague came into Cutch. Though I have daily visited the town of Mandvi for months during the two epidemics, and visited hundreds of infected houses, both before the disinfecting parties came to them and during the process of disinfection, I have come across very few dead rats, and have been particularly struck by it. I think I could safely say that I have not seen more than a dozen. At Patri, Bhujpur, and one or two other villages, dead rats have been found prior to an epidemic, but I have never received any report of what might be called an epidemic amongst rats." The epidemic in Cutch Mandvi, to which Major Hyde-Cates refers, was one of the most severe which occurred in India, and among the other epidemics which we have cited above there are several of considerable severity,—noticeably those which occurred in Poona. We may, in this connexion, refer to the fact that in only nine out of a total of forty village epidemics, which are detailed by Surgeon-General Planck in his report on plague in Kumaun and Garhwal, was a history of plague among rats obtained.

II., 361, para. 259.

Instances of epidemics unaccompanied by death of rats succeeded by second epidemic in which rats died. 11,202. 23,067–9. 10,127.

Again, in places other than those we have just mentioned, plague has broken out among rats only in connexion with a second human epidemic, while it has not been noticeable in connexion with the first human epidemic. This, for instance, was the case in Karachi, where mortality was not noticed among rats until the commencement of the second epidemic among men. Belgaum is another instance in point; there the first epidemic among men in October 1897 appears to have run its course without spreading to the rats; in the second and severer epidemic in May 1898 there was a concurrent epidemic among rats. Similarly, in the plague-infected villages in the Punjab there was in the autumn of 1897 very little evidence of the occurrence of the disease among rats; in the spring of 1898, on the contrary, there was overwhelming evidence of rats dying in every direction.

We can hardly suppose that the absence of noticeable mortality among rats in a large proportion of urban and village epidemics can be attributable to an absence or paucity of rats in the localities in question, and it is one of the as yet unsolved problems, in connexion with the epidemiology of plague, why the disease has spread in different degrees among rats in different localities and different seasons.

Influence exerted by rats on dissemination of plague within infected places. Observations made in totally evacuated villages.

273. We may now pass on from these preliminary questions to consider the influence which is exerted by the breaking out of the disease among rats upon the local dissemination of plague within an infected place and upon the severity of the human epidemic. For the most valuable information on the first of these questions we must have recourse to observations made in evacuated villages, where, since the factor of human agency falls away, we are in a position to determine with some approximation to accuracy how far rats are responsible for carrying about infection from house to house.

Case of Mahlgahla. 9987; 10,348.

Of particular interest in this connexion are the events which occurred at Mahlgahla, one of the infected villages in the Punjab. The inhabitants of Mahlgahla, some 2,500 in number, were placed in camp by Captain James, I.M.S., on account of an outbreak of plague which was confined to one special quarter of the village. This quarter having been disinfected without incident, the disinfection of the rest of the evacuated village was taken in hand. In the absence of other available labour the houseowners were here employed upon the disinfection of their own houses. As soon as they were set to work they came upon dead rats all over the village; in one case no less than 15 of these were found in a single room. Five days after the commencement of the work of disinfecting the houses which had been free from plague at the time the village was

evacuated numerous cases of plague began to occur among the disinfectors. So severe was the outbreak of plague among these (the resulting epidemic did not subside till 75 persons in all had been attacked) that the disinfection operations had to be suspended. The quarter of the village in which most of the disinfectors were attacked was the quarter most remote from that in which the original group of plague cases had occurred.

Case of Chak Kalal. 10,110.

Equally instructive are the events which occurred in the village of Chak Kalal, a small village which, while it was still free from plague, was described as an island in the middle of a sea of plague. This village was evacuated as a precautionary measure. A few days afterwards, owing to the downpour of rain, a considerable number of the inhabitants returned to their houses. A number of rats (and these were shown by bacteriological examination to have died of plague) were found lying dead all over the village. Within a few days afterwards, quite a considerable number of people contracted plague. It seems clear that the infection was here disseminated over the whole village as a result of the outbreak of plague in an epidemic form among rats.

Cases of other villages in the Punjab and in Bombay Presidency. 10,143-52.

What occurred in these villages on a large scale appears also to have occurred on a smaller scale in several other villages in the Punjab. In Khankhanan, we are told, the disinfectors contracted plague in a portion of the village which was not known to be infected at the date of evacuation. The dissemination of the infection beyond the limits of the originally infected quarter of the village was by Captain James imputed to the agency of rats. Captain James thought he had presumptive evidence of a similar conveyance of plague from place to place in an infected site also in the cases of the villages of Mallupota, Bahrwal, and Kariha. Mr. Dodgson, the Collector of Satara, observed a similar spread of infection in an evacuated village, and he was convinced that it was due to the agency of rats. "You can," he said, "take a village with a few infected houses at one end, or in one quarter, and turn all the people out, and you will constantly find that when the people go back to the other end of the village, or to a hitherto uninfected quarter, and sleep there they got the plague.' Lastly, Major Anderson's experience led him to a very similar conclusion. He cited in particular the case of a village called Hunsikatti, which he evacuated as a precautionary measure, in consequence of a few dead rats being found there. No cases occurred among the people in camp until three weeks afterwards. Two cases of plague then occurred. It was probable that the patients had visited their houses and contracted the infection there, for on inspecting the houses in the village it was found that seals which had been placed upon the houses of these two persons had been tampered with. In view of the fact that these houses were probably not infected at the time when the village was evacuated, it seemed likely that plague had been conveyed to them by the agency of rats during the absence of the people in camp.

22,065.

23,215-24.

Proof of spread by rats derived from consideration of course of plague in partially evacuated places.

274. So far we have dealt only with the case of a completely evacuated village, where, since the spread of plague through human agency may be excluded, the conveyance of the infective material from place to place can be imputed only to rats; or we may practically leave out of consideration such remote contingencies as its conveyance by means of carnivorous animals, or birds of prey, or the agency of the wind. We now pass to follow the course of events in the case of places which were only partially evacuated. In this connexion the history of the spread of plague at Banga in the Punjab is instructive, as showing the influence which rats may exert on the spread of plague under such circumstances.

Case of Banga. 10,235. 10,256.

Banga is a small town of some 5,000 inhabitants, where, with a view to the detection of the first case, corpse inspection was regularly carried out. As soon as the first case was detected, a brick wall, some eight feet high, was built across the street of the "muhalla" or quarter where the case occurred. The inhabitants of the quarter, 7 in number, were, so as to avoid all contact with the other inhabitants, taken out through an opening specially made in the town wall, a guard being placed on that opening, so as to prevent entrance into the evacuated quarter. The evicted people were taken some distance outside the walls, and a cordon was placed round their camp. Seventeen days after this evacuation had taken place (and at this date the evacuated people were quite free from plague) three further cases of plague were discovered in two houses situated just outside the evacuated quarter, one on either side of it. On the day on which these cases were detected a dead rat was found in one of the houses in question, and a day or two before a dead rat had been seen by the disinfectors when at work in the evacuated quarter. On the discovery of these three cases the 203 inhabitants of the second quarter were evicted. They, too, were taken out through an

opening specially made in the town wall. At the same time the four adits by which the newly infected quarter could be approached from the town were sealed up by brick walls, a guard being placed upon them and upon the opening in the town wall by which the people had been removed. Thirteen cases of plague developed in camp among the 203 inhabitants who were removed from the second quarter. Ten days after the date of this second evacuation, another case of plague occurred in the town just beyond the boundary of the second evacuated quarter. Immediately, the inhabitants of this third quarter, numbering in all some 249, were removed and placed in camp. The adits to this newly infected quarter were bricked up in the same way as had been done in the case of the other quarters. Four more cases of plague developed among the people who had been removed from the third quarter. After this third evacuation there was another interval of nine days, during which no plague cases were found in the town. Two further cases were then found, and on the next day three more, two of these being in a house which a sick rat had been seen to enter. After this dead rats and plague cases were found all over Banga, and the town was completely evacuated.

Instances in which rats spread plague broadcast in villages.

22,934.

275. The dissemination of plague by rats, which is so clearly brought out in the cases of Mahlgahla, Chak Kalal, and Banga, cannot be so clearly followed in places which were not evacuated. There are, however, many instances on record of villages where, after an introduction of plague, rats became infected, with the result that the infection was distributed broadcast through the village. In illustration of this we may cite the following cases :—"In Jegapur, a village in the Dharwar district, a man," Major Anderson, I.M.S., told us, "went to visit his brother's wife, who was suffering from

23,207.

plague at Shelawdi, nine miles distant. Two or three days after his return rats died in his house, and three days after his daughter was attacked by plague and died. Rats also died in the adjoining houses shortly after the girl fell sick, and 29 cases

5048–62.

occurred in the village, which was promptly evacuated."—The village of Guntakal was infected by a woman who died of plague there. After her death her house was shut up for three weeks, no one venturing near it. After that time plague cases began to occur among the inhabitants of the village. A large mortality among rats was reported between the first case and the subsequent cases, and dead rats were found in almost

4167, 5097.

every house inspected after the disease had spread among the villagers.—In Kotala, a small village near Guntakal, the three first plague deaths occurred in one house ; the patients belonged to a caste with which the rest of the villagers would hold no communication. Immediately upon the third death the house was burned down. After an interval of 15 days or 3 weeks, a further case of plague occurred, and it was reported that rats were dying in the village. Subsequently to this, an epidemic broke

13,399–401.

out in the village, there being nine attacks in one day.—In the case of Mundra, a town of 10,000 inhabitants in the Cutch State, the importation of 26 cases in nearly four months was accompanied by the occurrence of only 17 cases of indigenous plague. The town was then free from plague for two and a half months. After this the plague reappeared, four indigenous cases occurring on one day, and the reappearance of the disease being preceded by the death of numbers of rats. During the next

22,223.

four months there were close on 500 attacks of plague.—In the village of Areh, after the discovery on the 1st and 2nd of October of dead rats in two houses, nine persons, on the 3rd of October, were attacked by plague in the houses in question and the houses adjoining them.—In Kausani, a Kumaun village, where an outbreak of mahamari in 1876 was investigated by Surgeon-General Planck, rats were found

II. 347.

dead in every hut of a range containing eight huts. Some days after, plague broke out among the inhabitants of these huts, and of these inhabitants, who numbered in all about 13, five sickened all about the same time.

Opinions of plague officers regarding influence of rats in disseminating plague.

276. With regard to the influence exerted by rats on the course of plague in towns and cities, the general tenour of the evidence which we have collected may be judged from what was told us by the following officers who were engaged in plague operations :—

1002, 1168. 18,004. 8288, 10,961. 25,795.

Lieut.-Col. Weir, I.M.S., the Health Officer of Bombay City, and Mr. Godinho one of the Deputy Health Officers, repeatedly observed that the death of rats in district preceded an outbreak of plague among the human inhabitants. Captain Bingley, Major Ross, and Lieut. Firth had observed a similar sequence of events.

11,313.

Mr. Giles, who directed the plague operations in Karachi, said :—"Almost always dead rats appeared before the plague. The Superintendents themselves generally saw rats before they had cases." Mr. Kaka, the Health Officer of Karachi, referring

11,835–7.

to dead rats, said :—"I have seen them generally precede an outbreak in a house.

Lieut. Anderson, also speaking of the Karachi outbreak, said:—"In the market quarter the spread of the disease was extremely rapid, and was, I believe, conveyed entirely by rats, dead ones being found in almost every house in the quarter." The experience of Lieut. Law, who was also employed in Karachi, was similar to that of the officers just quoted. 22,646. 11,974.

Mr. Madhava Rau told us of Bangalore, that when plague got into "the principal grain mart of the City, rats became an important factor in the dissemination of the disease . . . rats being found as many as 10 in a house, immediately followed by plague attacks." 3242.

277. It is unnecessary to make further quotations from the evidence, which seems to us to be conclusive, that rats in some places, but by no means in all, have been active agents in the dissemination of plague. It has been clearly established that when the infection of plague has been imported into a town or village rats have, in many cases, contracted the infection before any indigenous cases occurred among men. Again it has been clearly established that, when rats have become infected, they have been instrumental in spreading plague in evacuated villages, and in carrying the infection outwards from evacuated quarters. Hardly less clear is it that rats have been frequently at work in disseminating the infection of plague in inhabited towns and villages, and that in some cases they have been responsible for the occurrence of an explosive outbreak of plague among men. It is, however, important to note that the outbreak of plague among rats has not in all cases been followed by the occurrence of plague among men, and that explosive outbreaks have sometimes been due to causes other than the spread of the infection by rats. Summary regarding influence exerted by rats in disseminating plague.

IX.(5.) *Spread of Plague in Infected Places by Animals other than Rats.*

278. Among the animals which are eminently susceptible to plague and liable to contract the disease in their wild state must, as already mentioned incidentally, be reckoned not only rats, but mice, squirrels, and monkeys. It is important, therefore, to consider here what influence these animals may have in the dissemination of the infection of plague. Dissemination of plague by means of animals other than rats.

With reference to mice, it is to be noted that laboratory experiments have shown that these animals are exceedingly susceptible to plague, succumbing when even minimal doses are injected under the skin. Mice may be infected by the mouth also, our experience in this matter, though differing from that of the German Plague Commission, being in agreement with that of the Austrian Plague Commission. In spite, however, of their great susceptibility to plague which is testified to by laboratory experiments, the instances in which deaths among mice are recorded in connexion with plague epidemics are rare. Lieut.-Colonel Weir, I.M.S., told us that he knew only of one case in Bombay in which mortality had been observed among mice, viz., in the Pydhownie Police Office in Bombay. Unfortunately the mice which were found dead in this place were not bacteriologically examined, and it was not conclusively proved that they had died of plague. Dr. Shrinivasa Rau informed us that in Bangalore many mice died from plague, and that he had himself dissected 10 mice which had died from the disease. In Porbandar more mice than rats were found dead during the epidemic of plague, and in two localities the death of mice was reported before any case occurred among human beings. In Timmencherla dead mice were found during the plague epidemic. In Karachi, where, as we are told, there was only a small mortality among mice, the conveyance of the infection was in a particular instance traced to mice. The case in question was that of an old woman who was not in the habit of going out, and who developed plague four days after dead mice were found in her house, this last being at some distance from the parts of the town which were at that time infected. The above would seem to be the only case where the conveyance of plague to man was directly traced to mice. Mice. 26,323. 8677. 26,324. Report of German Plague Commission, pp. 285–6. Report of Austrian Plague Commission, II. C. p. 701. 908. 3594–5. 13,699–700. 5047. 11,422–6.

279. With regard to the occurrence of plague among squirrels, we ascertained that mortality among these animals was observed in Hubli, in Bangalore, in Poona, in Baroda, in the Kaira district, in Ankleshwar, in Bhiwandi, and in the town of Banga in the Punjab. In Bangalore there seems to have been considerable mortality among these animals; squirrels were seen to fall down dead when trying to jump from one branch of a tree to another; in particular, we are told of a great number of dead squirrels being found in and around a plague-infected house. In Hubli squirrels appear to have been exterminated. In Poona the mortality among them was so heavy that Colonel Fawcett, R.A.M.C., said: "In December 1897, when Squirrels. 3116; 2601, 2654; 3597; 21,295; 14,641; 14,076; 14,492; 14,493; 10,136.

plague was very bad, I noticed squirrels crawling along the road, and not able to go up the trees. Then I heard some people say they saw three squirrels drop off the trees dead. After that I could at any time find the corpse of a squirrel about here if I looked for one." In view of the above facts, it can hardly be doubted that squirrels, which are, in India, both numerous, and tame enough to go into houses, may possibly constitute one among the less important agencies by which plague may be disseminated.

Monkeys.

93; I. 378; 9119; 8590.

2444, 3113-5. 14,079; 14,243, 14,503; 14,641, 15,381; 19,643; 23,604.

19,643.

8779.

280. Epidemics of plague have been observed among monkeys, both among the brown monkeys (macacus radiatus) and the more susceptible grey monkeys (semnopithecus entellus). In particular, an epidemic of plague occurred among the monkeys at Hardwar and the neighbourhood, the plague bacillus being found by Mr. Hankin in nine out of 26 monkeys examined. Mortality among monkeys was reported also from Hubli and the adjoining town of Gadag, and, further, from Ankleshwar, Baroda, Dharwar, the Thana district, and the village of Dhunadra in the Kaira district. With reference to Dharwar, we were informed that the deaths occurred among "the brown monkeys which live about houses, and will go into the interior and get food." Speaking of a village in the Thana district, Mr. Monteath said: "When the village was evacuated, large numbers of monkeys used to enter the houses by the gaps made in the roof seeking what they could steal. I noticed, on riding into the village on one occasion, a monkey drop dead from a tree above the chawdi. On inquiry I learned that 10 to 12 had died similarly, some in the houses. Coming again to the village a few days later, I was told that five or six more had died." From the above facts, it will be apparent that plague-infected monkeys may come into sufficiently close communication with human beings and their habitations to constitute a possible source of infection. This being so, it is clear that Mr. Winter was well advised to take steps, during the plague epidemic among monkeys at Kankhal, to rid the town of these animals by having them trapped, with a view to their being carried away into the jungle after they had been kept in confinement for a period sufficient to ensure that they were free of plague.

Cats.

26,316.

Report of Austrian Plague Commission, II. C., pp. 707-8.

11,311, 11,839; 18,990, 24,310; 1187-8, 20,423; 14,641; 14,079.

14,484.

281. Among the animals which may play a part in the dissemination of plague must be reckoned also cats. The disease can be communicated to these animals not only by hypodermic inoculation, but also by feeding them on plague-infected food. In connexion with the possibility of the disease being contracted by cats in this manner, reference may be made to the fact that the Austrian Plague Commission succeeded in communicating plague to a series of three cats by feeding them on the bodies of animals dead of the disease. It is to be noted, however, that only one of these cats contracted the disease in an acute and rapidly fatal form; in the case of the two others the disease manifested itself in a very chronic form, characterised by the development of buboes in the neck and great wasting. The epidemiological observations made in India seem to harmonise with these bacteriological observations. Disease among cats in association with plague was noticed particularly in Karachi and Ahmednagar, and also in Bombay City, Baroda, and Umreth; the animals are described as having suffered from buboes in the neck and from wasting. In one case, it was suggested that the infection of plague was directly conveyed to a person by an infected cat; here, however, there were other possible sources of infection.

Dogs, jackals, and birds. Para. 134 of this Report.

7187: I. 474.

II., 338.

Report of the Austrian Plague Commission, Part II., C. pp. 704-5.

282. Since, as we have seen, dogs, jackals, and birds are very insusceptible to plague, the influence of these animals in disseminating the infection can come into consideration only in connexion with the possibility that the bodies of animals dead of plague or portions of these may be carried about from place to place by carnivorous animals, and in connexion with the possibility that plague bacilli ingested in food may pass through the intestinal canal of these animals without loss of virulence. With regard to the first contingency, it will suffice to remark that plague may, in exceptional cases, be spread in this way. To exemplify this we may refer, on the one hand, to a case observed in Calcutta, where a dog carried into a a room a rat dead of plague and laid it on a bed, and, on the other hand, to the case of the infected villages in Kumaun, where, as we learn from Surgeon-General Planck's Report on mahamari, the dead were, owing to the dread of plague, often left unburied, with the result that jackals came and dragged the bodies about through the village site. In connexion with the possibility of plague bacteria passing unchanged through the intestinal canal of carnivorous animals, it will suffice to note that the Austrian Plague Commission, though they failed to infect dogs by feeding them on plague-infected material, were able to detect highly virulent plague bacilli in their excreta.

283. In addition to the animals we have noticed above, insects may, it has been suggested, carry about the infection of plague. We have already considered the case of suctorial insects, and there remains to notice only the possibility that ants and flies may carry about infective material. The possibility of plague being conveyed by ants was pointed out by Mr. Hankin, who in one or two instances discovered the plague bacillus in ants which had been at work on the body of plague-infected rats. The conveyance of plague by infected flies is adverted to by Dr. Nuttall. This observer considers that since flies succumb when fed on plague-infected material, the disease may possibly be spread by infected flies dropping into food. In view, however, of the fact that plague does not, at any rate in the case of man, appear to be contracted by means of the alimentary canal, we do not think that any great importance in connexion with the spread of the disease can be attached to the contamination of food by infected flies.

Insects.
Section IV.(4) of this Chapter.
8530–1.
Johns Hopkins Hospital Reports, Vol. VIII., 1899 p. 16.

IX.(6.) *Question whether any Agencies other than those already discussed Play a Part in the Spread of Plague.*

284. The question as to whether there are any agencies other than those already considered by which plague may be carried about now comes up for consideration.

Some witnesses, and Lieut.-Colonel Waters, I.M.S., in particular, entertained the opinion that the wind was a very important agent in spreading the infection of plague. His views on the subject appear to us to lack all serious basis. They would seem to be based, first, on the fact that the Umarkhadi Jail, which was under Lieut.-Colonel Waters' superintendence, remained for a long time free from plague, though the disease was raging in the surrounding quarter of Bombay; and, secondly, on the fact that some of the first rooms in which plague occurred in Bombay were situated in corner houses or in upper storeys. The interpretation which Lieut.-Colonel Waters placed upon the circumstance that cases did not for a long time break out in the Umarkhadi Jail was that the conveyance of the plague into the jail by the wind was prevented by the high wall which surrounded the jail. Similarly, the interpretation which Lieut.-Colonel Waters placed upon the occurrence of the first cases of plague in certain corner houses and upper storeys was that these places are particularly exposed to "the prevailing breeze" blowing from the Mandvi quarter. We may remark that it was only in connexion with a few of the first cases in Bombay that corner houses and upper storeys were found to be specially attacked. Of the other witnesses who came before us Major Corkery, I.M.S., who also seems to have thought that plague is spread by the wind, had an impression that in Sukkur and in the Dharwar Jail infection was carried by the wind along the ground.

The question as to whether there are any other agencies by which plague may be carried about.
Wind.
17,033-7.
17,015–6; 17,032.
2252.
2275.

285. It is in connexion with the frequent reappearance of plague on the confines of an evacuated area that the suggestion has been most forcibly brought forward that agencies other than those of men and animals may be involved in the dissemination of plague.

This idea has forced itself, in particular, upon Mr. Cappel in the case of Hubli, and upon Mr. Winter in the case of Jawalapur. Mr. Winter was apparently of opinion that plague spread outwards, after the evacuation of an infected area, in a centrifugal manner from the original focus of infection in the evacuated area by means of some agency other than that of men or rats. With a view to placing before us the exact facts on which he based his opinion, Mr. Winter caused a map to be prepared, upon which were indicated the houses in the neighbourhood of the evacuated area where the successive cases occurred subsequent to evacuation. The map which was furnished to us is printed as Appendix No. XXXVI. to our proceedings. A study of it seems to show that plague, though in the case in question did spread outwards from the evacuated area, spread out in a somewhat irregular manner, such as might have been expected if men or rats were the agents in distributing the infection. This conclusion is, we may note, in conformity with the opinion of Captain Elphick, I.M.S., who studied the events on the spot. In connexion with the occurrence of plague cases on the confines of the areas successively evacuated in Hubli, it was suggested that the infection might have spread outwards by growing along the ground. This particular suggestion must, of course, be rejected as inconsistent with bacteriological facts.

Suggestion that spread of plague by agencies already considered does not account for reappearance of plague on confines of evacuated areas.
1645-7; 9276-8.
9580-2.
1707.

To return to the more general issue, it will be manifest that before the dissemination of infection by means of the wind, or by means other than men or rats, can legitimately be brought in to furnish an explanation of the reappearance of plague in the confines of an evacuated area, the agency of men or rats must be excluded. It is conceivable, for instance, that, before evacuation, clothes from the evacuated area may have been deposited in the neighbouring houses, or that

people from the evacuated area may have transferred themselves to those houses, and again, it is possible that the evacuated area may not have been effectively cordoned, and that visits may have been paid by the people on the confines of the evacuated area to the houses within that area after the evacuation had taken place.

Water.

286. Before passing on to summarise what has been said above with regard to the various agencies which are at work in disseminating plague in an infected place, we have still to consider the possibility of the infection being disseminated by means of drinking water. This suggestion was originally put forward in connexion with Hong Kong, where it was claimed that the plague bacillus had been discovered in well water. The only fact which has come to our notice which would harmonise with this suggestion is the fact that Dr. Marsh succeeded in cultivating the plague bacillus from water, which had been previously artificially infected, after 44 days in the case of sterilised water, and after 6 days in the case of ordinary tap water. In this connexion we may observe that in the case of Malpur, an infected village in the Punjab, the water of a particular well in the evacuation camp was regarded with suspicion by the natives because plague-infected rats had been seen to jump in and drown themselves in it. It must, however, be carefully noted that this is the only instance brought to our knowledge where water fell under suspicion, and even here it appears to have been suspected only on *à priori* grounds. It would seem, therefore, to be established that water plays no part in the dissemination of plague.

18,881-2.

10,120.

IX.(7.) *Summary regarding the Agencies by which Plague is spread in an Infected Place.*

Difference of opinion regarding the relative importance of men and rats in disseminating the infection of plague.

287. All that has been said above has gone to show that plague is practically always spread, either by human agency (and for present purposes we include under this term the agency of infected clothes or merchandise carried about by man), or by the agency of rats. It remains, in conclusion, to endeavour to arrive at an estimate of the relative importance of these two agencies in the dissemination of the infection of plague within the limits of an infected town or village. On this question a variety of different opinions have been expressed.

Opinions that, apart from rat epidemics, human plague epidemics cannot occur.

17,837.

Appendix XLVII. II. p. 477.

Annales de l'Institut Pasteur, October 1898.

288. One witness went so far as to say, "I believe if there was a single house which contained infection, as soon as there is a colony of new rats they get themselves infected and spread the plague. After the plague has been over the place, and all the rats are dead, for the time being the plague subsides. Then there comes a fresh colony of rats."—The opinion of Captain Mason, R.A.M.C., is essentially to the same effect. "In no village," says this officer, "in which there has been no epidemic among rats has plague spread among the people. In other words, I firmly believe that imported cases of plague do not cause epidemics, provided the rats be not infected."—Dr. Simond, as we have already seen, takes the same view. He takes it as certain that epidemics of plague do not occur in villages unless preliminary epidemics of plague occur among rats, and, like the first witness quoted above, he attributes the seasonal increase of plague, which is so noticeable a feature in many plague-infected places, to a seasonal immigration of rats, which is followed in its turn by an increase of plague among men.

Captain Mason's experience of plague, and the same holds true to a large extent also of Dr. Simond's experience, was based on observations made in the Cutch State. It is therefore, important here to call attention to the fact that Major Hyde-Cates' evidence already cited above, to the effect that there was very little mortality among rats in this part of India, would appear to throw serious doubt on the correctness of Captain Mason's and Dr. Simond's views. Moreover, we have evidence derived from places outside Cutch which shows that the extreme views of these observers cannot be upheld and we have cited cases of outbreaks, both in towns and villages, which have been either quite, or at any rate in large part, independent of the occurrence of epidemics among rats.

Para. 272 of this Report.

II. 480.

Opinions that influence of rats in spreading plague greater than that of man.

11,278.

11,414.

17,925.

1005, 1168.

22,618.

289. Apart from opinions which entirely subordinate the agency of man in the spreading of plague to the agency of rats, there is a large divergence of opinion as to the relative importance of the roles which are played by rats and men respectively in the distribution of the infection. Mr. Giles considered the influence of rats in spreading plague much more important than any other agency.—Lieut.-Colonel McCloghry, I.M.S., shared this opinion.—Mr. Katrak regarded man as a "much less important factor than rats" in the spread of the disease.—Lieut.-Colonel Weir, I.M.S., the Health Officer of Bombay was, as we gather, also strongly of the same opinion.—Lieut. Anderson, speaking of the spread of plague in the villages in the Satara District

stated that in his opinion rats were the chief agents in carrying plague from house to house within a village. His experience of the second epidemic in Karachi was that the disease was "conveyed entirely by rats."—Lieut. Steen said, "Rats, I think, there can be little doubt, disseminate the plague in a village, once the infection has been imported by human agency. Communication may exist, and often does, amongst the careless villagers, but when cases of plague are found occurring in totally different and well separated parts of the village, and amongst the highest and lowest castes at the same time, there can, I think, be little doubt that rats have spread the plague over the villages, especially as large numbers of these vermin are, as a rule, found dead in the village, either before or at the same time as the occurrence of cases amongst the inhabitants." 22,646. 22,693

290. On the other hand, a certain number of witnesses, although numerically they constitute a comparatively small minority, have either not observed that any great influence on the spread of plague exercised by the existence of plague among rats, or do not believe that its influence is important. Mr. Stewart, who worked in Bombay and Nasik, had never been able to trace the spread of infection by the agency of rats. He said that in Nasik he had found practically no rats in plague-infected houses—in fact, he said, "We have had no signs of conveyance by means of rats in Nasik at all."—Major Reade, R.A.M.C., who was in charge of the plague operations in Poona, said, in his first examination before us, that there had never been any great mortality among rats in Poona, but when he appeared again before us, a month afterwards, he observed: "We have evidence now with regard to rat infection, and it shows that in a small radius there is undoubted infection by rats, though a localised one. Rat infection has not played a large part in the recent outbreak in Poona. As a source of infection to men it is one I have had little knowledge of up to the present date"—(Major Reade was, we understood, speaking also in the light of his experience of plague in Hong Kong)—"but from our experience at Poona I am inclined to believe the influence of this infection has been much exaggerated."—Dr. Clemow states that in the course of a systematic inquiry in Calcutta he could trace a history of dead or sick rats in seven only of 32 houses in which plague had occurred.—Lieut.-Col. Nariman, I.M.S., who was speaking of Surat, had never seen a case of plague which could be traced to a rat.—Mr. Judge, who had experience in Surat and Daman, was convinced that plague spread along the lines of human communication. In the case of Surat, Mr. Judge furnished us with a map based on a systematic investigation into the causes of a whole series of plague cases. In practically every instance these cases were found to have originated from human intercourse.—At Ahmednagar the experience was that plague was communicated almost entirely by human agency.—In the case of plague in Baroda City, Dewan Bahadur V. M. Samarth said that "people at a long distance from one another were found to be infected, and the reason often traced was that they had paid visits to one another, or that an actual plague case was removed clandestinely and put into another house." Opinions that influence of rats in spreading plague is not important. 16,960, 16,962, 16,998. 21,723-4. 25,831. 7188. 15,886. 16,045. App. No. LIV. 24,090; 24,303. 14,608.

291. We have cited above the most important opinions expressed to us with regard to the relative influence of rats and men considered as agents in the dissemination of plague. Some further light may, we think, be thrown on the question by considering certain observations regarding the manner in which plague has spread from house to house in an infected place, and regarding the question whether social intercourse between the residents, or whether contiguity of position is the factor which exerts the greater influence on the spread of plague from house to house. Mr. Lord, in answer to a question as to how plague appeared to be carried from one house to another within a town, replied: "I suppose by human agency, but I must say, that as far as I could see, in big blocks of houses, it did not pass from one house to other houses adjoining it in other streets (*e.g.*, touching back to back, with doors in different streets) but travelled up the street, *i.e.*, it probably was brought through the doors and not the walls."—On the other hand, with regard to this particular point, Captain Wilkinson, I.M.S., gives two specific instances, in the villages of Lidhar Kalan and Bika in the Punjab, where the agency of rats must have been involved, inasmuch as plague spread between houses whose doors were approached from different sides and whose inmates could have had no communication with each other. Maps explaining the special features of these two cases have been furnished to us and are published in our Proceedings. A similar case, also illustrated by a map, was reported from the town of Garshankar in the Punjab.—The evidence, however, relating to Kariha, also a village in the Punjab, points in a contrary direction. We are told that this village is inhabited by five sections of people, three of which were agreed in holding Observations bearing on part actually played by human agency and by rats in spreading plague in particular places. 19,003. 10,454. 10,456. App. XXXVII. App. XXXVIII. 10,845-54 App. XXXIX. 10,123-6.

no communication with the other two. Here, though rats were infected, the outbreak was in large part confined to the two sections who held communication with each other.—Mr. Shepherd, however, the Assistant Collector of the Thana district, observed in his district that plague did not spread among particular classes of the community as it did in Kariha. He drew his experience from a district where plague was epidemic both among rats and among men, and, in stating his opinion that the dissemination of plague by contact with infected persons was extremely rare, said: "If dissemination was chiefly by contact, considering the way the people live in the villages, we should get a certain class of people infected first. If it began among a certain caste, several members of that caste would be infected before others; but, as a matter of fact, we do not find that to be the case."—Finally, we may remark here that human agency, and not the agency of rats, must almost wholly have been responsible for the spread and continuance of plague in evacuation camps, which we have noticed elsewhere in this Report.

18,789.

Sects. III.(2) and (3) of Chapter VI. of this Report.

Conclusions regarding relative importance of men and rats as agents in disseminating plague in an infected place.

292. It will be obvious that, in view of the conflict of opinion disclosed above, it is difficult to obtain a basis for safe generalisation as to the relative importance of rats and men as agents in the dissemination of plague. In some places men, and in other places rats, appear to have been the agents which were most directly involved in the dissemination of the infection. The conclusions which we have derived from a study of the evidence may, perhaps, usefully be presented in the following form:—

(1.) In some places, where severe epidemics have occurred among men, rats have not been infected at all,* and in many places, where there have been plague epidemics among men, the disease has not appeared among rats in epidemic form;

(2.) Occasionally, where there has been considerable mortality among rats, there has been an absence of plague among men;

(3.) Even when there has been the largest amount of plague among rats, the risk to the persons who came in contact with these infected rats appears not to have approached that undergone by persons inhabiting houses in which there have been plague patients, or persons coming into contact with pneumonic plague; there are, however, cases, such, for instance, as the case of Mahlgahla, and the case of Chak Kalal, where a very considerable number of plague cases in a village has been directly attributable to the dissemination of infection consequent on an epidemic of plague among rats;

(4.) Since the number of plague bacilli escaping from the infected human organism must be presumed to be greater than the number which escape from a plague-infected rat, *à priori* it would seem clear that a single infected man must constitute a greater danger than a single infected rat; but, on the other hand, seeing that the excreta of rats may be deposited anywhere throughout a house, and that the number of rats which are affected may many times exceed the number of men who are affected, a severe epidemic among rats must, we think, tend considerably to the spread of infection;

(5.) The chief importance of rats in the epidemiology of plague seems to us to arise in connexion with the first outbreak of the disease in an infected place, for we have seen that in many instances rats become infected before men, and they scatter plague broadcast over an uninfected place;

(6.) When plague is once established in a place, we have no doubt that human agency is a more important factor in spreading the disease than the agency of rats.

Slow rate of spread and persistence of infection in localities once infected marked feature of spread of plague within infected places. 1645.

293. Before leaving this part of our subject, we think it advisable to emphasise the fact that the most striking characteristic in connexion with the spread of plague through a place is its slow and steady advance from one group of houses to another, and its long persistence in a quarter that has once become infected. As Mr. Cappel said, in speaking of the way plague spread in the commencement of the epidemic at Hubli:—"The cases were all grouped together, and for weeks, and I may say for months, we were engaged in combating the plague in the particular area which was contiguous to the original place of infection in the town itself, and that infection moved steadily on from that centre. . . . . It spreads very slowly from locality to locality within the town." Mr. Cappel considered that this slow movement of plague showed that the disease was not spread mainly by human agency, for he said:—"If

* It is noteworthy in this connexion that the minute bibliographical researches of Dr. Rudolf Abel, extending over the whole field of classical, mediæval, and modern, literature, bring out clearly that there are no records of epidemics among rats occurring concurrently with plague among men. The only striking reference to plague mortality among rats which Dr. Abel has succeeded in discovering is the following from Avicen (A.D. 1000), "Et de eis quæ significant illud [the approach of plague] est ut videas mures et animali quæ habitant sub terra fugere ad superficiem terræ et pati *sedar*, *i.e*, commoveri hinc inde sicut animali ebria."

Zeitschrift für Hygiene, 1901.

the infection were mainly spread by human agency within given town limits, we should expect to find the infection appearing in all parts of the town at the same time, but that was not the case either at Hubli or Dharwar." This assumption, it will be noted, is precisely the same assumption as that which came under review as an objection that might be urged against the spread of plague by human communication along the lines of railway; but just as there we saw that the slowness of the spread of plague throughout India, and its gradual progress by short steps, are probably due to the fact that it generally was not till after a considerable stream of infection had flowed through a town that the disease was able to establish itself there, so here, in the case of an infected town itself, we think that, leaving out of consideration the exceptional cases, a large inflow of infection must take place into each quarter or house in order to establish the plague there, and we consider that this fact explains the slow and gradual spread of plague within the limits of infected places. 1645. Section VII.(5.) this Chapter.

X.—Circumstances which influence Individual Liability to Plague.

X.(1.) *Influence of Age on Liability to Plague.*

294. Having discussed the general question of the manner in which plague spreads, we may pass to consider the more important of the circumstances which may influence the liability of the individual to contract plague. Influence of age on liability to plague.

In this connexion we may consider first the question of the influence of age on liability to plague. We have collected below in tabular form all the more important information which is at our disposal with regard to the incidence of plague at different ages of life. Before proceeding to set forth these figures, attention may be drawn to the following points. (*a.*) The figures giving the incidence of plague refer, for the most part, to attacks; when, however, the number of attacks has not been furnished, the figures for deaths have been employed for determining the incidence of plague at different ages. The inclusion of these figures, in a table designed to exhibit the incidence of plague at different ages, would appear to be justified, for, though the prognosis in the cases of children is slightly better than in the cases of adults, the case mortality of the disease is, as will be clear from a reference to Table III. in our Statistical Appendix, not influenced to any important extent by the age of the patient. (*b.*) With a view to furnishing a standard of comparison by which to determine whether there is or is not any increased or diminished susceptibility to plague at different ages, we have, in the case of each age group, given figures representing the number of persons of that age group to be found in the community. With regard to these figures, it is to be observed that, in the case of Bombay and other places where it was practicable, we have given the percentage of persons of each age group as deduced from the census figures of 1891 for the particular places in question. In the case of places in the Punjab, we have taken the figures for the Province as given in the census of 1891. In other places we have taken the figures representing the percentage of persons in the age groups in question, as deduced from the census returns of 1891, for the whole of India. Observations regarding statistical matter bearing on relation of age and plague. Appendix I. to this Report.

The table we have prepared is shown on the next page.

It will become manifest, on comparing the columns denoted by (*a.*) and (*b.*) respectively, in the table under each age group, that the incidence of plague during the first five years of life is much less relatively to the number of persons of this age in the population than it is at any other period of life. There would seem to be—though the figures are less clear on this point—a similar, though less marked diminution in the incidence of plague at the other extreme of life. As a necessary consequence of this diminished rate of incidence at the two extremes of life, there is a correspondingly increased rate of attack in the period of adult life. Conclusions from above table.

A point of minor importance which may be noticed in connexion with the table, is the variation in the ratio in which children were attacked in different places. The ratio which the number of the children who succumbed to plague bears to the number of the children in the population is, in the case of Bombay City, represented by the ratio of 1 to 5·7; the ratio borne by the number of the children attacked to the total number of the children in the population is, in the case of the Jullundur villages of the Punjab, represented by the ratio of 1 to 2·3. The reasons for this variation are not altogether clear. It may, however, be surmised that the difference in the ratios is conditioned, to some extent, by the fact that there was greater opportunity for concealment in the case of Bombay City than in the villages of the Punjab, where the staff employed on plague measures was adequate to secure the discovery of most cases even among children, in whose case concealment is most easily effected. Moreover, the population of children on which the incidences are calculated is that of 1891, and, Remarks regarding variations in ratio of children attacked in different places.

Table setting forth Incidence of Plague at different Ages of Life.

Place of Observation.	Whether the Figures refer to Cases or Deaths.	Total Number of Persons who were attacked by or who died of Plague.	Percentage of (*a*) Plague Cases or Deaths, and of (*b*) the Population under Observation which falls to each Age Group.										Authority for Figures.
	Age Groups		0–5.		5–10.		10–50.		50–60.		Over 60.		
			(*a*)	(*b*)	(*a*)	(*b*)	(*a*)	(*b*)	(*a*)	(*b*)	(*a*)	(*b*)	
Bombay City	Deaths	1,936	1·4	8·0	7·3	8·2	84·3	74·7	4·4	5·6	2·6	3·5	Snow's Report, 153.
„ Arthur Road Hospital	Cases	4,000	1·4	„	4·7	„	87·3	„	4·2	„	2·4	„	Evidence III., 459.
„ Grant Road Hospital	„	1,230	2·7	„	8·1	„	84·6	„	3·2	„	1·4	„	Gatacre's Report, p. 111; Campbell's Report, p. 140.
„ Parel Hospital	„	1,296	2·1	„	7·4	„	84	„	4·5	„	1·5	„	Campbell's Report, p. 115.
„ Mahratta Hospital	„	650	1·7	„	10·3	„	83·6	„	Over 50 = 29 per cent.				Gatacre's Report, p. 48.
„ Jain Hospital	„	510	0·8	„	3·0	„	92·7	„	2·2	5·6	1·3	3·5	Gatacre's Report, p. 151.
Ankleshwar	„	331	2·1	„	9·0	8·6	79·1	74·9	6·6	4·5	3·2	3·3	Evidence, 14,394.
	Age Groups		0–5.		5–60.						Over 60.		
			(*a*)	(*b*)	(*a*)				(*b*)		(*a*)	(*b*)	
Baroda City	Deaths	1,620	1·7	9·9	96·3				83·9		2·0	6·2	Evidence, 14,982; 15,364.
Baroda Villages	„	1,094	4·3	12·0	89·6				85		2·1	3·0	Mr. Mehta's evidence; App. LII.
Calcutta	Cases	183	3·3	7·3	93·6				84·2		7·1	8·5	Evidence, 6518; I. 474.
	Age Groups		0–5.		5–10.		10–50.		50–60.		Over 60.		
			(*a*)	(*b*)	(*a*)	(*b*)	(*a*)	(*b*)	(*a*)	(*b*)	(*a*)	(*b*)	
Jullundur Villages	Cases	2,706	7·3	17*	11·5	14*	71·0	56·8*	7·0	6·5*	3·2	5·7*	Evidence, 10,330.
Banga	„	103	8·7	„	12·6	„	63·2	„	14·6	„	·9	„	„ 10,178.
Daman	Deaths	625	6·2	14·7†	10·6	14·1†	70·0	59·8†	8·3	6·0†	4·9	5·4†	„ 16,511.
Karachi Hospital	Cases	515	4·1	„	11·1	„	79·2	„	3·9	„	1·7	„	„ 12,343.
Kotri	„	100	4	„	14	„	75	„	7·2	„	0	„	„ 12,649.
	Age Groups		0–10.				10–50.		50–60.		Over 60.		
			(*a*)	(*b*)			(*a*)	(*b*)	(*a*)	(*b*)	(*a*)	(*b*)	
Poona, Sassoon Hospital	Cases	192	4·7	28·8†			88·5	59·8†	4·7	6·0†	2·1	5·4†	Evidence, II., 192.
Umreth	„	110	17·3	„			75·5	„	7·2	„	0	„	„ II., 233.
Palanpur	„	815	11·8	„			78	„	7·5	„	2·7	„	„ II., 200.
	Age Groups		0–5.		5–60.						Over 60.		
			(*a*)	(*b*)	(*a*)				(*b*)		(*a*)	(*b*)	
Porbandar	Deaths	357	2·2	14·7†	90·8				79·9†		7·0	5·4†	Evidence, II., 484.
Bangalore, South Camp Hospital	„	256	5·8	„	88·8				„		5·4	„	„ 2923.

* These figures have been calculated from the 1891 Census Returns for the whole of the Punjab.

† These figures have been calculated from the 1891 Census Returns for the whole of India.

as will be seen in connexion with statistics regarding the incidence of plague as influenced by sex, there is reason to believe that in Bombay and other large towns the population of males is increasing in greater proportion than the population of females. Hence it may be assumed that the percentage of children in the population of Bombay is at present less than it was in 1891, and that the number of children assumed to be present, and adopted in our table as the basis for the calculation of the incidence of plague in the first ten years of life, is unduly high. On the other hand, there is nothing to suggest that in rural areas—such as the portion of the Punjab which has been visited by plague—the number of males is increasing relatively to the number of females. For the Punjab, therefore, the census figures of 1891 are likely to represent more correctly than for Bombay City the actual percentage of children in the population.

Para. 295 of this Report.

X.(2.) *Influence of Sex on Liability to Plague.*

295. The next point that comes up for consideration is the question of the relative incidence of plague in the different sexes.

Influence of sex on liability to plague.

Before setting forth the statistical matter which we have collected on this question, it will be useful to call attention to certain facts with regard to the proportion of women in the population of India. The percentage of women, worked out on the census figures of 1891, amounted to 47·9 per cent. of the total population of India. In large cities the percentage of women to the total population was considerably less; in Bombay it amounted to 36·9 per cent., in Calcutta to 36·5 per cent., and generally, in the whole population of all the Indian towns which contain more than 100,000 inhabitants, to 35·3 per cent. Since no later figures than those of the census of 1891 are available, it must be borne in mind that the census referred to bore witness to an increase in the previous decennial period of males in the population of large cities at the rate of 12·89 per cent., while the increase of females in the population of these cities was represented by the figure of 7·6 per cent. only. It is, therefore, probable that the census figures of 1891, which we have used for the purposes of comparison, exaggerate the proportion of females in the population of large towns. The plague incidence in the female population ought, therefore, to be calculated on a basis of a smaller number of women.

Points to be borne in mind in considering statistical matter regarding relation between plague and sex.

296. These points having been premised, we proceed to set forth for Bombay City the figures compiled by Lieut.-Colonel Weir, I.M.S., regarding the incidence of plague among men and women respectively. We may note that the death returns, as being more accurate than the returns relating to the number of attacks, have been made use of for determining the relative incidence of plague in the two sexes. The figures are as follows:—

Statistics for Bombay City bearing on the respective liability to plague of the two sexes.

TABLE showing the NUMBER of DEATHS in BOMBAY from BUBONIC PLAGUE in different CASTES, and the DEATH RATE per Mille of the Population of BOMBAY CITY in the Year 1896.

(Snow's Report, pp. 152–3.)

Classes.	Males.			Females.		
	Population.	Number of Deaths.	Rate per 1,000.	Population.	Number of Deaths.	Rate per 1,000.
Jains	19,317	168	8·69	5,908	40	6·77
Brahmins	26,554	70	2·63	11,418	20	1·75
Lingaèts	462	6	12·98	322	—	—
Bhatias	4,344	19	4·37	3,547	4	1·12
Hindus of other caste	288,569	811	2·79	162,896	379	2·32
Hindus of low caste	25,888	78	3·01	19,281	34	1·76
Mussulmans	94,975	113	1·18	60,126	41	·68
Parsees	24,705	41	1·65	22,753	26	1·14
Jews	2,590	6	2·31	2,431	4	1·64
Native Christians	20,040	55	2·74	9,672	15	1·55
Eurasians	2,294	2	·87	2,036	2	·98
Europeans	8,118	1	·12	3,172	—	—
Negro Africans	90	—	—	56	1	17·85
Chinese	137	—	—	53	—	—
Other and unknown castes	— (*sic*)	—	—	—	—	— (*sic*)
Total	518,093	1,370	2·63	303,671	566	1·88

Lesser incidence of plague among women due to over-estimate of female population and to concealment of cases among women.

These statistics show a much smaller incidence of plague among women than among men, the rates of incidence calculated upon the figures for 1891 standing to each other in the proportion of 71 to 100. The causes of this smaller recorded incidence of plague are to be found not in a lesser case mortality among women, (as will be clear from a reference to Table II. in Appendix I. to this Report), but in the over-estimation of the female population resulting from the relative increase of the male population in large cities referred to above, and also in the fact that cases of plague occurring in women were more often concealed from the authorities than those among men.

Concealment of cases among women established on comparison of admissions to hospital with death returns of City. Gatacre's Report, pp. 48, 111, 124; Campbell's Report, pp. 115, 139, and 150; Evidence III. 459.

The concealment of cases of plague among women is testified to by two facts. The first of these facts is that the admission returns to the Bombay Plague Hospitals show that the number of women admitted stands to the total number of admissions in the proportion of 1 to 4 in a total of 6,970 cases, while the recorded death rate for the total female population stands to the recorded death rate for the total male population in the ratio of 1 to $2\frac{1}{2}$ nearly, thus witnessing the reluctance of the people to disclose plague among women, unless where it may be absolutely necessary for the purposes of carrying out the obsequies. The frequent concealment of plague cases among women is testified to further by the fact that the per mille death rate in the male and female population is entirely different in the various communities and castes which are included in the population of Bombay. The fact that the figures set forth in Lieut.-Colonel Weir's table, given above, testify to the concealment of plague cases among women appears clearly when the figures are exhibited in the following form:—

Testified to also by different rates of attack in different native communities.

Community.	Proportion in which Plague Deaths recorded among Women stand to the Plague Deaths recorded among the Total Number of Persons in the Community.	Proportion in which the Number of Women in the Community stands to the Total Number of Persons in the Community.	Ratio in which Deaths among Women stand to the Number of Deaths which might have been expected on the Assumption that the Proportion of Deaths among Women would correspond to the Proportion of Women in the Community.
Hindus	*477 in 1,629* or 1 in 3·4	*203,372 in 568,506* or 1 in 2·8	82 per cent.
Muhammadans	*41 in 154* or 1 in 3·75	*60,126 in 155,101* or 1 in 2·6	69 per cent.

It will be seen from these figures that the proportion of plague deaths among women to the total plague deaths in the particular communities which we are considering is considerably less in the case of the Musalmans, who practice the rigid seclusion of their women, than in the case of the Hindus, who do not to the same extent insist upon such seclusion. If further proof be required that there was a considerable amount of concealment of plague cases occurring among women in the Musalman community, we may obtain such evidence from the consideration of the statistics of the Bombay Plague Hospitals. To take the figures of the Arthur Road Hospital, which is the largest of the Bombay Hospitals, we find that, whereas the number of Musalman men who were admitted into hospital was 229, the number of women of that community admitted was only 34, while, on the basis of the census figures of 1891 which set forth the relative numbers of men and women in the Musalman community in Bombay, 88 women might, on the assumption that men and women are equally liable to plague, have been expected in the hospital. It would thus appear that the recorded lesser incidence of plague among women in Bombay City must in large part, in particular in the Musalman community, be due to the concealment of cases among women.

App. LXI. in Vol. II. of our Proceedings.

Further consideration of influence of concealment.

On consideration, however, of the table given above, and further, of the hospital statistics, it would appear to be almost certain that concealment does not in every case account for the smaller rate of attack which is set forth in the official statistics. It will be seen that in the case of Native Christians—a community which does not practise seclusion among women, and which is least likely to conceal the occurrence of plague among them—the number of plague deaths among women (which amount to little over 20 per cent. of the total number of plague deaths in the community) is very much smaller than might have been expected from the relative numbers of Native Christian men and women (women constituting about 33 per cent. of the census population of the community in Bombay). As a matter of fact, the deficit of plague deaths among women is a little greater among the Native Christians than among Muhammadans. We cannot suggest any explanation of this circumstance.

297. Passing now from Bombay to the consideration of figures relating to the incidence of plague among women in other places, we may set forth at this point in tabular form the statistics which we have collected. The table is as follows:— Statistics for places other than Bombay City bearing on relation between sex and plague.

TABLE SHOWING INCIDENCE OF PLAGUE IN MEN AND WOMEN RESPECTIVELY.

Place of Observation.	Authority for Figures.	Whether Figures refer to Cases or Deaths.	Total.	Male.	Female.	Per-centage of Females in Total.
Calcutta City	I., 474	Cases	190	152	38	20
Surat	15,645	„	114	78	36	31·6
Karachi City	II., 424	„	3,238	2,174	1,064	32·8
Bangalore Hospitals	2,913–2,923	„	468	280	188	40·2
Baroda City	15,219	„	1,620	910	710	44
Sassoon Hospital, Poona	II., 192	„	3,584	1,860	1,124	48·1
General Hospital, Poona	Report for period from 1/6/97—31/3/98					
Kotri	12,649	„	100	65	35	35
Umreth	14,047	„	110	64	46	42
Satara	22,279	Deaths	644	344	300	46·6
Baroda Villages	15,219	Cases	2,065	1,085	980	47·3
Palanpur	13,159	„	672	290	382	57
Porbandar	II., 484	„	463	200	263	57
Mandvi Hospital	I., 352	Deaths	2,953	1,226	1,727	58·5

The most striking point in this table is the great difference in the figures relating to different places. To begin with the case of Calcutta: we find that here, out of the very small number of cases that came to light, those among women amounted to only 20 per cent. of the whole. This can hardly be due to anything else but concealment, even if we make an allowance for the fact that the percentage of 36·5 of females in the population which was shown in the Census of 1891 must be somewhat in excess of the present percentage of women in the City. In the case of Surat and Karachi, the percentage of cases among women to the total cases is 31 and 32 per cent. respectively. These figures approximate fairly closely to the figure 35·3, which, as we have seen above, represented in 1891 the percentage of women in the populations of Indian towns containing over 100,000 inhabitants. Unless, therefore, it is conceivable that in Surat and Karachi only a very few more cases were concealed among women than among men, the statistics for these towns would seem to indicate that the incidence of plague is at least as high among women as among men. We find confirmation—and more than confirmation—of this indication when we consider the other large towns for which we have figures. In Bangalore, Baroda, and Poona—each of which contains well over 100,000 inhabitants—the percentages of plague cases which occurred among women, computed on the total of plague cases, are 40, 44, and 48 per cent. respectively—percentages which are in excess of the average percentage, 35·3, of women in the population of Indian towns of over 100,000 inhabitants. In view of the fact that we are, in the case of Poona and Bangalore, dealing with hospital figures, which always show an unduly low percentage of female cases, and further seeing that we have had direct evidence of the concealment of cases among women in Bangalore, it will be obvious that the real percentage of women attacked must have been in excess of the already large reported percentages. The cities in question, therefore, supply evidence of a greater incidence of plague among women than among men. Not so decisive, but pointing in the same direction are the rest of the figures in the table given above, which refer to a number of smaller towns and to a group of villages in the Baroda State. It will be seen that—with an exception in the case of the small town of Kotri—the percentages of cases among women to the total plague cases ranged in the places in question from 42 per cent. upwards, averaging probably about 50 per cent., and reaching 58 per cent. in the case of Cutch Mandvi, a town of 38,000 inhabitants. The high percentage among women in the case of Cutch Mandvi is so striking a phenomena, that it has been suggested in explanation that it must have been due to the flight of more men than women from the infected town. With regard to this particular point there is, however, no exact information available. Returning, therefore, to the inferences that may be drawn from the figures relating to the whole series of small towns and villages, it would seem probable—even making allowance for a certain excess in the female population of small places over the percentage 47·9, which was in 1891 the percentage of females in the total population of India—that the percentage of plague cases among women has been quite as great as the percentage of plague cases among men.

6576.

25,022.

I. 352

298. We may now sum up our conclusions regarding the statistics which we have considered in the last two paragraphs. We may reject the figures for Bombay and Calcutta, as so vitiated by concealment of cases among women as to be useless as Conclusions regarding the relation between sex and liability to plague.

indications of the respective liability to plague of men and women. The figures which are available for other places, as they stand, would seem to indicate that the percentage of cases among women is at least as great as the percentage of cases among men, and when we take into consideration the circumstance that the factor of concealment must in all cases to some extent have tended to decrease the number of cases recorded among women as compared with the number of cases recorded among men, we may conclude with probability that the percentage of cases among women has been greater than the percentage among men. Even if this conclusion were established beyond doubt, it would, however, be far from affording proof of any special susceptibility to plague attaching to women; for consideration will show that by reason of their more confined life women would more than men be exposed to the risk of contracting the disease from the infection lurking in infected houses, from which as we have seen the greatest number of plague cases originate.

X. (3). *Influence of Race on Liability to Plague.*

Influence of race on liability to plague.

299. We now pass on from the consideration of the influence which is exerted on liability to plague by sex to the influence which is exerted on liability to plague by race. In dealing with the influence of race, we may limit ourselves to discussing the question whether there is any difference of susceptibility between Europeans, Eurasians, and the natives of India collectively considered. The only statistical data which we were able to collect on this question are data relating to the City of Bombay and to the Civil and Military Station of Bangalore, which are the only two plague-infected places with a large population of Europeans for which figures are available. The facts with regard to the incidence of plague in Europeans, Eurasians, and Natives, respectively, are set forth in the following table :—

TABLE SHOWING INCIDENCE OF PLAGUE IN DIFFERENT RACES.

Place of Observation.	Period of Observation.	Authority for Figures.	Whether Figures refer to Cases of Deaths.	Rate per Mille among		
				Europeans.	Eurasians.	Natives of India.
Bombay - -	From the outbreak of plague to the end of 1896.	Lt.-Col. Weir, I.M.S., (Mr. Snow's Report, p. 152).	Deaths.	·08	·92	2·4
Bombay - -	From the outbreak of plague to Feb. 1899.	Dr. Blaney, Evidence, 17,681–5.	„	1·06	7·30	Ranging from 15 p. c. (Native Christians) to 38·05 (Jains).
Bangalore Civil and Military Station.	From 15/9/98 to 28/2/99.	Mr. Cadell, Evidence, 25,015.	Cases.	3·6	18·4	42·5

It will be manifest, from a consideration of this table, that the incidence of plague has been very much less among Europeans than among Eurasians, and that it has been very much less among the Eurasians than it has among the Native Community. An impression has accordingly grown up in India that Europeans must possess something in the nature of a racial immunity against plague. But the extremely severe epidemics which have occurred in Europe in former times, and also the fact that one-half of the European population fell a victim to plague in the course of the epidemic which devastated Bombay in the 17th century, make it incredible that anything in the nature of a racial immunity comes into play. This inference is confirmed by the long record of plague cases which have occurred during the present epidemic among Europeans who were specially exposed to infection, and by the circumstance that the number of cases of plague occurring among Europeans in attendance in plague hospitals has not been less in proportion to their number than the plague cases that have occurred among native attendants. Again, it must be remembered that if a racial immunity is imputed to the Europeans, on the ground that they have suffered comparatively little from plague, it might, on this ground, be imputed also to the upper classes of the Parsees, for whom no such immunity has been claimed. We are of opinion that the relatively small incidence of plague among the European community (and the same would apply in a lesser degree to the Eurasian community and to some extent also to certain of the higher classes of natives) must be accounted for by the fact that the European is protected against wounds by his clothes, and particularly by his boots. Further, a great deal must, in the case of the educated classes, be claimed as the result of education, which secures that they shall not unnecessarily remain in contact with infection.

The President does not think that much weight can be given to these explanations. The former has, no doubt, been advanced by several of the witnesses; but, as it has just been stated, the case incidence in many European epidemics has often been extremely large, and the Europeans who suffered were well shod and even more heavily clothed than are the Europeans in India. The latter explanation appears to him inadequate to cover the many instances in which Europeans have in India been day after day in close relationship with infected persons and infected houses with almost complete immunity. The explanation which appears to him more probable has been stated in a separate Memorandum on the influence of insanitary conditions on plague. App. III. to this Report.

X.(4.) *Influence of Religion, regarded as an Index of varying conditions of life, on Liability to Plague.*

300. We may pass from the discussion of the question of the existence of a racial immunity to consider whether religion, prescribing as it may a particular code of habits, has had anything to do with liability to contract plague. Influence of religion, regarded as index of habits, on liability to plague.

We have already, in the course of our discussion on the incidence of plague according to sex, printed a table which exhibits the rates of attack among different religious communities in Bombay. It will be seen, on referring to this table, that if we exclude the Lingaits (whose numbers are quite inconsiderable), the Jains show a rate of attack from plague which is double that of all other classes of the community. This high incidence of plague among the Jains is testified to also by Dr. Blaney. According to this witness, in three years the plague mortality among the Jains was 38 per mille, as contrasted with a mortality of 22·5 per mille among the other classes of Hindus, and with a much smaller mortality among the other classes of the community. Dr. Blaney spoke as follows with regard to this matter:—"The rate of mortality among the Jains, 17,681. surprisingly large as it is, is real, and it is due to the fact that animal life is sacred among the Jains. They will not sweep their staircases, or sweep their sleeping rooms out, or their cooking rooms very often, lest they should destroy some animal life, so scrupulous are they. Only one half of the vegetables which are used by the natives of Bombay are eaten by these people, because animal life has been found in them. There are sundry other social peculiarities about them which render their sleeping-places and cooking places particularly susceptible to the attraction and preservation of the infection. Not that these places are very dirty. They are a wealthy people, and 17,682. their places look comparatively clean." We are not precisely informed as to what is the nature of the social peculiarities spoken of by Dr. Blaney; possibly, however, the reference may be to the fact that the Jains, owing to their aversion to taking life, are said to be infested with parasites. They are also said to foster animals in their neighbourhood by providing them with food, and it is conceivable, therefore, that their houses are more rat-infested than those of the rest of the population.

When we pass from the case of the Jains, to consider whether there is any difference in the incidence of plague among the Hindus taken collectively, the Muhammadans, the Parsees, and Native Christians, we find that the more important data which are available for the resolution of this question can be presented in the form of a table. The table is as follows:—

Table showing Incidence of Plague in Natives of various Religious Communities.

Place of Observation.	Period of Observation.	Authority for Figures.	Whether Figures refer to Cases or Deaths.	Rate per Mille among			
				Hindus.	Muhammadans.	Parsees.	Native Christians.
Bombay - -	From outbreak of plague to end of 1896.	Lt.-Col. Weir, I.M.S. (Snow's Report, p. 152).	Deaths	2·86	·98	1·41	2·36
Bombay - -	From outbreak of plague to end of February 1899.	Dr. Blaney, Evidence, 17,681–5.	„	From 20 per cent. (Brahmans) to 38·05 (Jains).	9·44	9·42	15
Baroda City -	From 1st October 1896 to 30th June 1898.	Evidence, 14,989; 15,219.	Cases	16	7·9	0	—
Surat - -	From 8th December 1896 to 11th March 1897.	Evidence, 15,633; 15,642.	Deaths	1·31	·39	1·87	—
Bangalore Civil and Military Station.	From 15th September 1898 to 28th February 1899.	Evidence, 25,015 -	„	45·5	36·5	—	40·5
Ankleshwar -	From April to November 1898.	Evidence, 14,448 -	Cases	33	24	38·3	0
Karachi City -	The period of the first Karachi epidemic.	Evidence, 11,827 -	„	45·4	40	9	?
Mysore City	From 1st October 1898 to 28th February 1899.	Evidence, 25,015 -	„	22·9	38·2	—	—

The figures given in this table for the different communities differ considerably in the different places. In the first six of the seven places for which statistics are set forth in the table, the incidence of plague among Hindus is recorded to have been greater than the incidence among the Muhammadans. The only town where the contrary has been recorded is Mysore City. An explanation of the smaller rates of attack recorded among Muhammadans suggests itself in connexion with the fact, already adverted to above, that plague cases among women are to a larger extent concealed in the case of the Muhammadan community. The contrary instance in Mysore is probably explained by the fact that, as there were no stringent plague measures applied in Mysore, there was nothing in this city to be achieved by concealment. Except for the circumstance that religion influences the recorded rate of attack among Muhammadans by engendering among them a desire to keep cases which occur among their women concealed, there would appear to be nothing in the habits and customs enjoined by the different religions which affects the plague incidence as exhibited in the statistics. It would, however, appear from the testimony of observers in many different parts of India that the fatality of plague is greater in the case of Hindu patients than in the case of Muhammadan patients. The difference may stand in relation to the robuster physique and better circumstances of the Muhammadan populations in question.

I. 352.
I. 349.
III. 459.
11,898, 27,041.
13,478–9,
15,632–4.

X.(5.) *Influence of Occupation on Liability to Plague.*

Liability to plague of traders who deal in grain.

301. We have already, in connexion with the question of the importation of infection in merchandise, incidentally noticed the point that plague has in certain places broken out first among traders, and we have also, in connexion with the question of the spread of plague in an infected place, cited the opinion of Mr. Giles and Lieut.-Colonel McCloghry, I.M.S., with regard to the dissemination of the infection from Banniahs' shops. In proceeding now to inquire more in detail whether traders are specially liable to contract plague, or whether they exercise any special influence in disseminating the disease, we find that the only further facts and considerations which have a bearing on the question are the following. In Molagavelli, Kajurgi, Sirohi, Reri, Asu, in the case of the first and the second epidemics at Palanpur, further, in Ilkal and Jalgaon, and, lastly, in Poona in the third epidemic, the disease was in each case introduced by a trader who came into the place and there sickened of plague. Again, the first case that came under the notice of the authorities in Calcutta in 1898 was that of a small shopkeeper or hawker, and in Kotri also the first case was that of a Banniah. Moreover, in certain places plague appears to have broken out first among, or to have been most severe among, the merchants. This was the case in the Mandvi quarter of Bombay City, where the severe epidemic among rats was a noticeable feature of the outbreak. Here not only the Jains, many of whom are grain traders, but also the Banniahs, Bhatias, and Brahmans, of whom also many are engaged in the grain trade, were among the first to be attacked and suffered very severely from plague. The same was noticed also in the case of Karachi, where the first and second epidemics broke out among traders. In the second epidemic at Karachi, the Joria Bazar, which was occupied mainly by retail sellers of grain and other materials, was the first focus of infection and was the scene of one of the most severe rat epidemics on record. In Bulsar the first epidemic commenced in the street inhabited by grain sellers. In Nasik the disease occurred first among the grain sellers. In Baroda City plague first appeared in the grain market. In Ankleshwar and Kotri plague appears to have carried off most victims among the Banniahs. In Gadag, as we are told by Captain Colomb, seven out of a total of 12 cases which occurred in December 1898, occurred in Banniahs.

Section VII.(4) of this Chapter. Para. 268 of this Report.

4980; 5501; 6414, 9702; 12,446–7, 12,486; 12,449, 12,451; 13,054, 13,059; 19,512; 24,529; 25,860.

6462, 12,596.

911.

11,197, 11,359.

16,316.

13,578, 14,693.

15,459.

14,448, 12,578, 12,597, 12,640.

23,608.

When we come to consider what may be the cause of the occurrence of the first cases among traders—here we have in view chiefly traders who have dealings in grain and and foodstuffs—and the greater prevalence of plague among them in certain places, several different explanations suggest themselves. In the first place, it is manifest that the superior liability of traders to contract plague may be attributed to the circumstance that the traders, since they travel more than any other class of the community, must more frequently visit infected places. Again, the fact that the traders necessarily come into contact with a large number of people immediately suggests that the greater prevalence of plague among traders may be due to their coming into contact in their places of business with infected persons. Lastly, the fact that the grain in which the traders deal is liable to have been contaminated by infection and is likely to attract rats to the places in which it is stored, may be invoked as an explanation of the prevalence of plague among traders and of the spread of plague in

10,244, 13,422, 23,607.

3785-90, 3831–6, 11,430, 15,459, 15,471, 17,837 22,645–6.

the neighbourhood of their shops. It is possible that each and all of these factors may come into consideration in connexion with the occurrence of plague in the trading community.

Classes other than traders likely to come specially into contact with infection.

302. In addition to the Banniahs and other traders, certain other classes of persons might be supposed to be particularly liable to come in contact with the infection of plague. The classes we have in view are, firstly, persons employed in the disinfection of plague-infected places; secondly, Dhobis, or washermen; thirdly, the Acharaj and the Domes and corpse bearers who assist in the laying out of dead bodies and in the carrying out of funeral ceremonies; and, lastly, scavengers and sweepers.

Liability to plague of persons employed in the disinfection of plague-infected houses.

303. Persons employed in the disinfection of plague-infected houses are, by the very nature of their employment, exposed to a greater risk of contracting the disease than persons who are not brought into direct contact with places which contain infective material. The frequent occurrence of plague cases among disinfectors is, therefore, a circumstance that we might naturally expect to come under observation. Indeed, the instances brought to our notice in which disinfectors were attacked are not few. The instances to which attention must here be more particularly directed, as affording the best indications of the amount of risk incurred by disinfectors in general, are those in which a number of plague cases has been observed to occur among the members of regular disinfecting gangs, and persons in close connexion with them. Such instances are the following. In the North Western Provinces, out of 188 plague cases which occurred in Jawalapur, Kankhal, and Jamalpur, 36 were cases of disinfectors, or of persons connected with disinfection: such cases were very numerous in Jawalapur, where they amounted to 29 out of the total of 116 plague cases that occurred in the course of the epidemic. In the Punjab, 50 cases out of a total of 3,407, were, it is estimated, cases of disinfectors, presumably including men employed regularly as disinfectors, and also those engaged on special occasions in the disinfection of their own houses. In some of the Punjab villages, noticeably Mahlgahla, Khankhanan, Mallupota, and Bahrwal, cases among disinfectors were particularly numerous; but throughout the plague-infected villages of the Punjab, attacks among disinfectors were common, and from a statement relating to 561 cases, which occurred in villages other than the four we have named above, it would appear that 21 of these cases were those of disinfectors. The conclusion arrived at by Captain James, I.M.S., from his experience of plague work in the Punjab, was that "disinfection is the most dangerous work." Of the 100 cases that occurred in the second outbreak at Kotri, five cases were those of disinfectors. In Cutch Mandvi, eight out of the 300 disinfecting coolies employed in the first epidemic were attacked: in the second epidemic there appear to have been cases among disinfectors, but particulars of their number were not supplied to us. In Palanpur, Captain Rainier, I.M.S., said, after the return of the people to the City in the end of December 1897, seven or eight imported cases of plague occurred "chiefly among members of the disinfecting parties sent to the villages." In the Hyderabad State it was noticed that many coolies engaged in disinfecting work contracted plague. Other instances were brought to our notice by Surgeon-General Bainbridge and Mr. Edalji Bomanji Modi. Further, we were told that five out of 20 coolies engaged in the disinfection of one particular temple at Sukkur were attacked.

8929, 9178, 9217; 9221; 9158. 10,001, 9981. 9987, 10,139, 10,349–51. 10,143, 10,145–6. 10,625. 10,000. 12,647. 13,480. 13,100. 1. 442. 220, 14,067. 18,403.

The above facts are sufficient to prove that the risk of infection which is run by disinfectors is considerable. So serious is it that the question of adopting special precautions to lessen the risks which are incurred by disinfectors becomes one of the greatest practical importance. In connexion with this question the following cases are of interest. Lieutenant Anderson made a statement before us, which was quoted and confirmed by Mr. Kaka, that in Karachi, in a gang of from 50 to 55 disinfecting coolies, before inoculation was resorted to as a protective measure, 20 cases of plague occurred, while no cases occurred after the inoculation of the gangs. A similar fact was observed by Captain Clarke, I.M.S., in the Hoshiarpur district of the Punjab; before the inoculation of disinfectors "cases had been occurring in the disinfecting gangs in every village that was being disinfected"; after inoculation was adopted, three cases only occurred, all among uninoculated persons in gangs that contained about 880 inoculated and 170 uninoculated persons. In Satara, we are told by Lieutenant Steen, the coolies, who during four months had been employed in disinfecting houses that had been left for three months vacant and partially unroofed, were free from plague; but when 24 coolies were employed in the disinfection of a small village only three weeks after its infection, seven of the coolies were attacked, and the coolies employed in other places also were attacked when they were careless, as in three small

22,668, 11,769. 10,681–6. 22,720–7.

10,907.

wadis of Tasgaon, where four coolies died of plague. Similarly, in Surat, Mr. Judge found that his disinfecting coolies did not contract plague if they were careful, though they did so if they were careless, and he instanced the case of a carpenter who contracted the infection because he worked on the disinfection of houses with a cut upon his foot. These cases, while they too, like the cases first quoted, show that the risk which disinfectors run is great, show also that that risk can be practically removed by the taking of proper precautions.

Liability to plague of washermen 11,314.

304. With regard to the liability of washermen to contract plague, the following facts have been placed before us: An epidemic of plague occurred among a group of 70 washermen in Karachi, and after the removal of five persons who were sick at the time when plague was discovered among them, no other cases took place in this community. In Bombay, up to December 1896, out of a total of 1,838 deaths, which had been classified according to the occupations of the persons who had died, there were 19 deaths from plague (1 per cent. of the total) recorded among washermen. Between the 24th of September 1896 and the 10th February 1899, out of 4,000 patients treated in the Arthur Road Hospital in Bombay, 31 (0·75 per cent. of the total) were washermen. In Satara, out of 749 plague cases classified according to the occupations of the patients, four cases (0·5 per cent. of the total) are noted as having occurred among washermen. There was then apparently a considerable amount of plague among washermen.

I. 392-3.

III. 466.

21,269.

Liability to plague of persons whose employment brings them into contact with persons sick or dead of plague, and their effects. Sections VI.(1) to VI.(4) of this Report. Para. 191 of this Report.

305. Next, as to persons whose employment brings them into contact with persons sick and dead of plague, and their effects.

As to the liability of doctors and nurses and sick attendants generally to contract plague in the course of their attendance on the sick, a sufficiency of facts has already, in the section of this Report dealing with the infectivity of plague patients, been set down to allow of a judgment to be arrived at as to the amount of risk which is involved. Similarly, in connexion with the discussion of the incubation period, we have set down facts which show clearly that there is a considerable risk involved in making post-mortem examinations on persons who have died of plague. Further cases might also have been cited in this connexion, if details of these cases had been available. The risk of infection at post mortems, however, as will be seen on a study of the cases we have collected, is specially great in the case of unskilled attendants who fail to take the necessary precautions.

9459.

Much less serious are the risks incurred by persons who are concerned in the laying out or disposal of the dead. We were, however, informed that, at Hardwar, a certain number of cases occurred among the Acharaj, who get the clothes of the dead. It is not certain that in these cases infection was derived from clothes, or from contact with the dead when assisting at funeral ceremonies; but we may note that Mr. Winter informed us that plague among the Acharaj ceased when precautions were taken to prevent the distribution of the clothes of the dead.

9271, 9460.

Liability to plague of sweepers and scavengers.

306. Passing to the question whether special risks attach to the occupation of sweepers and scavengers, we have not sufficient data to permit us to arrive at a definite decision. Isolated cases among sweepers are reported from two or three places in India; these may manifestly have been contracted in the ordinary course independently of the special occupation of the patients. On the other hand, the immunity of sweepers during the first epidemic at Bombay was so marked as especially to attract attention, and the suggestion was put forward that a certain amount of immunity was acquired by them in the course of their work, or that the plague bacillus was killed when it was introduced into dirt.

18,642.

1004.

X.(6.) *Influence of particular Habits of Natives of India on Liability to Plague.*

Influence on liability to plague of natives' habits.

307. Closely connected with the question of the special liability to contract plague attaching to particular occupations, is the question of the influence exerted on the spread of plague by certain native habits and practices. We may, before leaving this part of our subject, briefly refer to a few of these here.

(i.) Of sleeping on the floors;

First of the native practices which may conduce to the spread of the plague, and which may in part account for the much greater severity of plague among the natives as compared with the European community, is the fact that in many parts of the country it is the common practice for the natives sleep on the floor. It will be obvious that the absence of beds must not only conduce to the contamination of the floor by sick persons, but it must also bring the native more directly in contact with the floor, which may have been soiled by man or rats. The only specific observation which was

brought under our notice in this connexion is one adduced by Lieut. Anderson. This witness had had under observation in Karachi a particular house where every inmate was attacked by plague, "except one who slept on the only bed." 22,655.

Another habit that probably conduces to the spread of plague is the custom which prevails everywhere in India among the natives of going barefoot, at any rate in the interior of their houses. The habit of going barefoot in houses may, we may assume, tend to increase the opportunity of contracting plague from contaminated floors, especially as cuts or abrasions are commonly to be found on the feet of natives. (ii.) Of going barefoot;

Further possibilities of contracting the disease from the floor are provided by the native habits of squatting on the floor, and by the habit, which would appear to be prevalent in certain places, of eating food directly from the floor. Mr. Dodgson saw food being eaten off a floor within a very few feet of a place where a plague patient was lying. (iii.) Of squatting on, and eating food off, the floor;

The practice of cleaning out brass vessels with mud from the floors has also been noticed as a practice calculated to lead to accidental infection. (iv.) Of cleaning brass vessels with earth.

Lastly, the suggestion of possible infection by earth has also been made by Lieut.-Colonel Weir in Bombay, in connexion with the fact that a certain section of the community are in the habit of using earth balls in lieu of the ablutions which are customary elsewhere in India. (v.) Of using earth-balls for certain cleansing purposes. 25,778.

XI.—Epidemiological Questions which suggest themselves in connexion with the Spread of Plague.

XI.(A.) *Question as to whether a Prolonged Interval intervenes between the Importation of the Infection and the Outbreak of Plague in a Place.*

308. Having in the foregoing sections of this Chapter set forth the main body of facts which bear on the spread and communication of plague, we have still to consider a series of epidemiological problems which arise in connexion with the spread of the disease. The first of these is the question as to whether a long interval occurs between the importation of the infection of plague and the outbreak of plague in an indigenous form. The opinion that such an interval usually does occur is held by a number of witnesses who came before us. Among these may be mentioned Surgeon-General Bainbridge, Surgeon-General with the Government of Bombay. "It seems," said this witness, "that between the date of the importation of plague into a town or village an interval of several weeks elapses before the disease becomes localised." In the opinion of the witness, "the germs lay dormant for a time, or took some considerable period to acquire potency." Captain James, I.M.S., gave it as his experience that it was extremely common for a long period to elapse between the arrival of imported cases and the occurrence of the first indigenous cases in the infected villages of the Punjab. Mr. Kabraji, the Acting Collector of Kaira, estimated that nearly three or four weeks elapse before an imported case gives rise to indigenous plague in a previously uninfected locality. Dr. Simond, in a paper already referred to, and Mr. Hankin, in a paper on the spread of plague referred to in his evidence, have also discussed the question of the occurrence of an interval between the importation of the infection and the occurrence of local cases. Dr. Simond, in particular, asserts that it is a rule for an interval of 20 to 50 days to occur. Mr. Hankin expresses himself more guardedly; he considers that the question deserves careful attention.

Opinion held by many observers that long interval usually intervenes between importation of infection and outbreak of epidemic.

214.

10,223.

19,514.

Annales de l'Institut Pasteur, October, 1898. Annales de l'Institut Pasteur, November, 1898. 8564–5; 8632.

309. Before dealing with the evidence which has been adduced by Dr. Simond and others in support of the theory we are now considering, it is important to realise that the question, as to whether an interval elapses between the importation and the spread of the infection, opens up only a particular case of the more general issue as to whether a prolonged interval may occur between a human case of plague and the human case next in series. This issue has already been incidentally considered in other forms.

The question under discussion is a particular issue of a wider question. In view of fact that long interval may occur between a human case and case next in series, interval may be expected also between importation of infection and outbreak of epidemic. Paras. 212, 219–20, 225–6, 184–6, 220, and 659 of this Report.

We have seen, when discussing the mode in which the plague bacillus may leave the infected organism, that, in the case of plague patients who convalesce with suppurating buboes or after developing pneumonic complications, the discharges are in certain cases capable of conveying the infection for weeks after all the patients have recovered their usual health.

Again, we have seen, in discussing the infectivity of houses and clothes (and we shall deal more fully with this question in connexion with disinfection), that the infective material may lie dormant in houses and clothes for prolonged periods.

Lastly, we have seen, in the course of our discussion of the mode in which the plague bacillus effects an entrance into the organism, and also of the infectivity of houses

infected by rats, that the plague bacillus may be transmitted from animal to animal through a series of generations without the intervention of any human case.

The circumstances to which we have just referred show that it is quite possible for a prolonged interval to occur between a human case of plague and the human case next in series.

We may go further. There are other circumstances which suggest that the intervention of a prolonged period between two human cases may not be very infrequent. We have here in view the circumstance that the bacteria in cases of bubonic plague escape, from the infected organism only in very small numbers, remaining alive outside the body for a considerable period. It may thus, on the doctrine of chances, be expected that a considerable interval will, in many cases, elapse between the time when the bacillus leaves the body and the time when it comes in contact with another human organism in such a way as to give origin to an attack of plague.

In view of the above considerations, which apply generally, it is *à priori* to be expected that cases will be met with in which an interval will occur between the importation and the spread of the infection.

Instances of intervals observed by Captain James, I.M.S., in the Punjab.

9919-30.

310. We are now in a position to consider the instances in which a prolonged period is said to have intervened between the importation and the spread of infection. Captain James, I.M.S., furnished us with the following instances. In the case of Khatkar Kalan, the very first village which was infected in the Punjab, the infection appears to have been brought by one Ram Saran, who died on the day of his arrival in the village on the 29th of April, 1897. During the next two months no case of suspicious illness was observed in the village; but from July onwards isolated suspicious deaths began to occur. In October the deaths became so frequent as to cause alarm; the matter came to the knowledge of the authorities, and the disease was recognised as plague. All inquiries failed to trace any possible channel of infection other than that mentioned above. It would thus seem that the introduction of plague must have been due to Ram Saran, and that an interval of at least two months must have occurred between his death and the infection of the next patient in series.—The

10,224

village of Aur furnishes another instance in point. On the 5th of April 1898, a case of plague occurred at Aur, which was some miles distant from any infected village. No other cases occurred, and the village was officially declared free from

10,549-51.

plague on the 19th of April. On the 23rd or 24th of July, four mild cases of plague were discovered, no external source of infection being traceable.—In the case of

10,099-01, 10,224.

Gobindpur the disease was imported by a resident of the village, a boy who went to see a friend at an infected village. On his return in the first week of March, 1898, this boy fell ill of plague. The attack was not a serious one. The next patient was a man who lived next door. He died on the 14th of April. In this case the interval between the attack of the person who imported the infection and the first

9987, 10,196, 10,348.

local case was 37 days.—In Mahlgahla the history of the outbreak was as follows. A girl arrived from the infected village of Mallupota on the 12th of February, and she remained well till the 22nd of February. She then fell ill, and was on examination by an English doctor certified to be suffering from plague. She made a rapid and uneventful recovery. There was no other case in the village till the 15th of March,

10,091. 10,224.

21 days after the original attack.—In the case of Dahan, clothes were brought into the village from Mallupota on the 12th of February, and were deposited in the house

10,224 and Capt. James' Report.

of a woman. This woman was attacked by plague 23 days afterwards.—In Laroya the first plague case occurred in the person of a man who had been in the infected village of Piragpur. We are not told whether the patient recovered or died. The next case in

Do.

series was that of his wife, who was attacked 26 days after his attack.—In Jhingran the first case was that of a woman who had been on a visit to an infected village.

Do.

The second case was attacked 25 days after the first.—In the case of Birampur, in the Hoshiarpur district, plague was imported from Khatkar Kalan by a woman who had been on a visit to the latter place, and who, returning to her own village on the 20th of October, 1897, died there a few days afterwards. The next person attacked was her mother, who fell ill on the 5th of November; the third case did not occur till

Do.

December.—At Hansaron the first case is stated to have occurred on the 14th of March,

Do

and the second case on the 13th of April.—At Masani the first patient died of plague on the 27th of February, 1898, and the second on the 3rd of March. There was then no further case till the 27th of March, when the wife of a man who was employed in disinfecting the house of the second victim was attacked.—In the case of Bhaura

lso there was, as we were told by Captain James, a long interval, but the exact dates ere not available.—In the village of Aujla, a case, which was suspected of being a ase of plague, was followed, after an interval of a month during which no cases ccurred, by a typical case of plague. 10,224 and Captain James Report.

When we consider these instances which have been adduced by Captain James, we ee that they include the cases of twelve out of a total of 63 villages where the history f the importation was traced. In other words, a prolonged interval intervened between he importation of infection and the outbreak of plague in the case of the villages in he Punjab in less than one out of five cases. It is possible that the proportion of ases in which a true interval intervened between the importation of infection and the utbreak of plague was somewhat less than this, for although the history of infection as in each case very carefully investigated, and although the infected villages were ordoned, we must not overlook the possibility that, in some of the villages in which an nterval apparently occurred, there may have been some undetected re-importation of ne infection. If this were the case, the fact that a considerable period elapsed etween the first and the subsequent cases of plague could obviously not be adduced s evidence to establish the existence of an interval between the importation and the pread of infection. Again, in certain villages, such, for instance, as Aur and obindpur, where the first patients recovered, it seems to us possible that the plague acillus may have persisted in the organism of the first patient for a long period after onvalescence, and that the first patient directly infected the next. If this was so, here would in these villages be no question of a real interval between the first and econd cases of plague. Even, however, making allowance for possibilities such as hose just mentioned, it would seem justifiable to conclude that, in at least a certain umber of the cases cited by Captain James, a long period actually did intervene etween the importation and the spread of the infection. In particular, the occurrence f such an interval appears to be established in the case of Khatkar Kalan, in the ase of Dahan, and in the case of Masani.

We have no means of ascertaining how the interval in question was bridged over the case of Khatkar Kalan; in the case of Dahan we may presume that the infection y dormant in clothes; and in the case of Masani that it persisted in the house. In one of the twelve cases cited is there reason to believe that the interval was bridged y an epidemic of plague among rats.

311. Mr. Kabraji, the Acting Collector at Kaira, who seems to have paid considerable attention to the question here under consideration, gives us the following examples of the occurrence of a prolonged interval between the importation of infection and the occurrence of indigenous plague. The first case cited by him is that of Igatpuri. Some Muhamadans came from Bombay to Igatpuri, and a death from plague in the latter place occurred among them on the 21st of July 1898. After an interval of nearly twelve days certain further deaths from plague took place in the locality. In this instance, the actual interval was not more precisely stated in Mr. Kabraji's evidence.—This witness further cited the case of Ilkal. "On the 31st of July 1898, a Marwari anniah came from Hubli and died at Ilkal. That death passed unnoticed. Then e man he had been living with died on the 21st August, and his wife on the next ay. That started the plague at Ilkal."—Mr. Kabraji further cited the case of obleshwar, a large village in the Bijapur Taluka. In the course of an inspection this village, which was recognised as being in imminent danger of infection, asmuch as it was situated between two infected villages, "I came" said Mr. Kabraji, upon a house where that very morning (17th December) a man had died, and I as informed that his wife had died in the same house on the 23rd November. fter a great deal of cross-examination it was brought out that the woman had en to a funeral at Kakhindki, one of the infected villages in question (with which e people had declared that they had cut off all intercourse). Two days after her turn she had died. . . . . I came to the conclusion that the husband had died of ague." The inference that this death was due to plague seems to win support om the fact that "in another house at the back of the first one, the wife of the cupant was absent, but was traced to her father's house and found to be feverish, d on the third day following she died, apparently of pneumonic plague. Two of the ns of the first case, as well as his brother, also developed plague (as pronounced a Hospital Assistant) in quick succession."

The facts detailed in the instances of Ilkal and Bobleshwar seem to point to a certain erval having occurred between the first indigenous case and the imported case from ich it was derived. There is no record of rats having died in the interval. Instances of intervals observed by Mr. Kabraji in Bombay Presidency. 19,509. 19,512. 19,513.

Instance of interval observed in the Hyderabad State. 5501.

312. A further interesting case was cited to us by Colonel Lawrie, I.M.S., and Mr. Stevens from the village of Kajurgi in the Hyderabad State. "A Banniah arrived in the house 45 days ago from Umarghi. He died of plague three days after his arrival. The people state that there was no other case of sickness in the house afterwards for 20 days. The whole of the Banniah's family there got plague one after the other, and the Banniah himself and eleven males, making with the first case from Umarghi thirteen in all, died. . . . . That was the clearest case of a man coming from an infected district and dying, and there being a total absence of plague in the village for three weeks—twenty days clear. In that time it seems to have got exceedingly virulent, because the whole family were wiped out, including servants. All that remained were the old grandmother, the wife of the first man who died, and two small children." Mr. Stevens was positive that no dead rats had come under observation in that village.

Historical evidence adduced by Mr. Hankin in support of theory of interval. Annales de l'Institut Pasteur, November, 1898, pp. 753–4.

313. Mr. Hankin, in his paper already referred to, first proceeds, with regard to the outbreak of plague in London in 1665, and with regard to the outbreak of plague in Marseilles in 1720, to adduce historical evidence tending to prove that in the case of both these two epidemics nine and six weeks respectively intervened between the importation of infection and the outbreak of the disease. Of recent instances in India, Mr. Hankin discusses the history of the introduction of plague into Kankhal, Satara, and Hubli, and he concludes that in each of these cases a considerable interval elapsed between the importation of infection and the beginning of the consequent epidemic.

Consideration of the Indian cases cited by Mr. Hankin in support of theory of interval. Kankhal. 8914, 9065. 8916, 9077.

The history of the outbreak in Kankhal, which was given to us in detail by Mr. Winter, is a curious one. The last case of plague in the neighbouring city of Hardwar occurred in June, 1897. The only two cases of plague which had occurred in Kankhal up to that time were, first, the case of a beggar woman who had died in a garden, some little way out of the town, and, secondly, the case of Ram Kishan, a Hindu priest, who had contracted the infection at Hardwar and had died in Kankhal on the 16th of May. No other case of plague was discovered until the 16th of September, and it appears certain that no case of plague arrived in the town during that interval. In the middle of June it was reported that rats were dying at Kankhal. 8774, 9074. Bacteriological investigation showed that these were dying of plague. Careful inquiry was then made into all the deaths that had occurred in Kankhal since June. With 9253. reference to this inquiry, Mr. Winter, the Collector, remarked: "The number of deaths was very small; it was so small that one could inquire into the circumstances of each case. I traced suspicious cases back to the 3rd August, but before that I have no reason to suspect that there were any. None of the deaths before that date were connected with any of the deaths which occurred later on; they were not related to the people who died later on. They were mainly old people, or young people who would die in the ordinary course, whereas these special cases were very closely connected, and they died after very short illness. One of the Hospital Assistants or the Assistant Surgeon reported that a child had died of mumps. A person does not often die of mumps, and the probability was it was a case of plague. In 9080. another case a man had a bubo and had had leeches applied to him." Every one of these cases which were noted as suspicious occurred in the immediate neighbourhood of the house in which Kishan Ram had died. It is impossible, of course, to say for certain that there were no undetected cases of plague, but the inquiry made by Mr. Winter was evidently made very carefully and under very favourable circumstances and it seems to us probable that no case of plague did occur during the two and a half months that elapsed between the importation of plague by Ram Kishan and the first indigenous case in Kankhal. It will be noticed that this interval, though it may have been bridged over in part by the transmission of the plague bacillus from rat to rat, was not entirely bridged over in this manner. No mortality from plague 9290–3. came under observation among rats subsequent to the month of June. There was thus an interval of about six weeks between the last date upon which dead rats were seen and the date of the first human case of plague.

Hubli.

Para. 61 of this Report.

The case of Hubli, which Mr. Hankin relied upon as another instance of a place where a long interval occurred between the importation of the infection and the outbreak of the disease, does not seem to us at all conclusive. We have already seen how uncertain is the history of the course of events at the beginning of the outbreak in Hubli. On the one hand, it is possible that there was a re-importation of the infection immediately before the severe epidemic in May 1898 occurred; on the other hand, it is possible that this epidemic was connected with the original importation of infection in October 1897 by a continuous series of sporadic plague cases.

The third case cited by Mr. Hankin, that of the town of Satara, seems to us also very inconclusive; for, even taking the history exactly as it is set forth by Mr. Hankin, it would appear that during the interval of five weeks which elapsed between the first discovered indigenous cases of plague (which were found on the 3rd of October 1897) and the subsequent epidemic outbreak of plague in the neighbourhood of the first infected houses, a few cases of plague were occurring in the camps. There was, then, not a clear interval between the first cases of plague and the subsequent epidemic outbreak.

Satara. Annales de 'Institut Pasteur, Nov. 1898.

314. We next pass to consider the instances adduced by Dr. Simond. Dr. Simond in his paper seeks to establish that it is the rule for a considerable interval to occur between the importation of the infection and the outbreak of the disease; he seeks, further, to establish that "the period of latency which separates the imported case of plague from the beginning of the human epidemic is the period which is necessary for the development of the disease among rats."

Evidence adduced by Dr. Simond in support of theory that interval occurs and is bridged by epidemic of plague among rats. *Loc. cit.* p. 648.

The evidence adduced by Dr. Simond on this question is of a two-fold character. First, with a view to establishing the existence of a prodromal or "incubation" period during which the disease spreads among the rats, Dr. Simond cites three specific instances.

He first cites the history of the outbreak of plague in the village of Muska in the Cutch State. The history of this outbreak was, according to Dr. Simond, as follows. A woman was infected in Muska in February 1897; another woman who assisted in nursing her caught the infection and conveyed it to her own family, ten members of which succumbed to plague. After these deaths (the dates of which are not given) had occurred, there were no further cases in the village till the 13th of May. We find certain further details with regard to Muska in a report by Lieut.-Colonel Wilkins, I.M.S., which is printed in General Gatacre's Report on the Plague in Bombay in 1896-7. The date of the original case does not seem to have been ascertained with accuracy. Lieut.-Colonel Wilkins writes: "About March last a few cases occurred in Muska." Referring to the cessation of the outbreak after the woman who had nursed the original case and ten other members of her family had died, he said: "Segregation and isolation stopped the plague there; no further cases occurred for a time." When the next case occurred, plague was raging at Cutch Mandvi. Muska is quite close to Cutch Mandvi, being less than 3 miles distant. Since thus there is a possibility of Muska having been reinfected in May from Cutch Mandvi, we think that the history does not warrant us here in concluding that there was a prolonged interval in Muska between the importation of the infection and the subsequent epidemic. But even assuming that we have a prolonged interval, it would appear that there was no epidemic among the rats. Major Hyde Cates' statement is quite definite on the subject; "the cases at Muska," he says "were certainly not due to rats."

Muska. *Loc. cit.* p. 647. General Gatacre's Report, p. 102. II. 480.

The only two other specific cases adduced by Dr. Simond are the cases of the villages of Cherra and Woter, which are both in the Karad Taluka of the Satara District. In the case of the first, Dr. Simond relates, citing the authority of Dr. Robertson, that there was an interval of "some weeks" between the importation of the infection and the subsequent cases. During this interval rats were found dead in the neighbourhood of the house where the first patient died. In the case of Woter, an interval of about a month is said to have occurred between the first group of cases and the subsequent case. No further details are given. In view of the fact that in the earlier stages of the outbreak in this neighbourhood no European officers were present, and that the means did not exist for collecting facts with accuracy, we do not think it is possible to be certain that there was, in these cases, a true interval between the importation and spread of the infection.

Cherra and Woter. *Loc. cit.* p. 647.

Waiving, however, this question, we think it is deserving of attention—seeing that the three cases given above are the only cases cited to establish the theory that the interval between importation and spread is bridged over by an epidemic of plague among rats—that only one of the three histories placed on record by Dr. Simond makes mention of a mortality of rats occurring in the interval between importation and the occurrence of indigenous plague. In short, Dr. Simond's theory, of the bridging over of the interval by an epidemic among rats, wins but little support from the particular cases cited by him.

315. With a view to establishing the general proposition that a prolonged interval regularly occurs between the first importation of the infection and the subsequent epidemic, Dr. Simond publishes in his paper two maps of the Bombay Presidency, on which there are inscribed, for each of the towns which had, at the date of the preparation of the maps, become infected, two dates, the first of these being the

Maps of the Bombay Presidency showing dates of importation and outbreak of plague. *Loc. cit.* pp. 634–5.

date of the first imported case which was detected, and the second being the date on which the first indigenous case was detected. Dr. Simond appears to regard these dates, which are given on the authority of Major Grayfoot, I.M.S., as constituting conclusive evidence of the theory contended for by him that the infection of plague, when imported into a place, does not spread to man immediately, but spreads only after an "incubation period," during which the disease is propagated among rats. No evidence whatever is adduced by Dr. Simond in support of the tacit assumption that in the towns inscribed in his maps an epidemic among rats intervened between the first imported and the first indigenous case. Further, there is not in any of these cases any evidence to show that the first imported case recorded in the map was connected by any fact of causation with the first indigenous case. Now this cannot be assumed without proof,—first, because in the large majority, if not in all the towns inscribed on Dr. Simond's map, a considerable number of imported cases, any one of which might have given origin to the outbreak, came to light before a single indigenous case was detected; secondly, because the particular cases recorded as the first imported cases may, as has been explained elsewhere in the Report, have been cases which were detained at railway stations and which never entered the towns; and thirdly, because the plague outbreaks in many cases almost certainly originated from imported cases which did not come to the notice of the local authorities. The fallacies of the argument which is based by Dr. Simond upon the information supplied by Major Grayfoot are, therefore, much under-rated by this last witness when he told us: "To say the period between the first imported case and the first local case is the time it takes to make a place infected is not strictly accurate." Rather we may conclude that the dates set down in Dr. Simond's maps to establish that a prolonged period intervenes between the importation of the infection which originates an epidemic and the resulting outbreak of plague are, considered as evidence of the truth of that theory, entirely fallacious.

Para. 246 of this Report.

17,135

Conclusions regarding occurrence of interval between importation of infection and consequent outbreak of plague.

316. To sum up, the facts adduced above suffice to establish that a prolonged period does in some cases intervene between the importation of infection and the outbreak of plague. We have, however, no reason for supposing that the occurrence of such an interval is anything but exceptional. When a true interval does occur between the importation of infection and a spread of plague consequent on this importation, we think that this interval is much more often bridged over by the persistence of the plague bacillus in the soil or in clothes, than by the propagation of the bacillus through a series of rats. While thus there is no element whatever of mystery attaching to the occurrence of this interval, we think that a knowledge of the possible occurrence of such an interval is of the utmost importance as indicating that the danger of the spread of plague cannot be regarded as over until a prolonged interval has elapsed after the last human case has come under observation.

XI.(B.) *Question as to whether Plague manifests itself at the Outset of Epidemics in Atypical Clinical Forms.*

Question whether plague manifests itself at the outset of an epidemic in atypical clinical forms.

317. The question which we now proceed to consider is whether plague manifests itself at the outset of epidemics in atypical clinical forms. This question is of great theoretical interest as bearing on the mode of origin of plague epidemics, and it has, at the same time, a practical importance in connexion with the difficulty which is not infrequently associated with the recognition of plague when it breaks out in a new place.

Prodromal epidemics of "mumps." 21,653–56.

Noteworthy in connexion with this question are the following observations. Assistant Surgeon Barucha remarked that in a village which had been under his observation an epidemic of "mumps," occurring in men, women, and children, preceded the outbreak of plague. This observer had a similar experience in Poona. Here, at the outbreak of the second epidemic, he had seen 20 to 25 children brought in from one house with symptoms of "mumps." In the case of one of these children symptoms of plague, among which was specified the occurrence of a bubo in the groin, afterwards presented themselves. We ourselves in Poona saw a case closely resembling "mumps" occurring in association with cases of plague. Again, at Kankhal, where, as already shown, a very strict watch was kept upon the mortality with a view to obtaining the earliest possible information of the appearance of plague, a case of death reported as due to the "mumps" occurred in close association with a series of suspicious deaths which appear to have been the first cases of indigenous plague in the town. In another place, to wit, in the small town of Banga, in the Punjab, where Captain James, I.M.S., was on the watch with a view to detecting the first cases of plague that might

9077, 9253.

10,235

occur, an epidemic of "mumps" occurring before the outbreak of plague "gave rise to anxiety." In Garshankar also an epidemic of buboes behind the ear is said to have occurred before the outbreak of plague. While bacteriological data are in all these cases absent, it would seem possible that these prodromal epidemics may have been associated with an invasion of the glands in the neighbourhood of the mouth, nose, and throat, by an attenuated plague bacillus, which had previously been cultivating itself on the mucous membrane of those regions. We may bring into association with this the interesting observations made by Major Dimmock, I.M.S., in Bombay. "A group of Parsees," he said, "came in from Thana, and the children were suffering from nasal catarrh of a most severe type. The symptoms were, apparently, very acute—coryza, with a high temperature. There was no glandular enlargement in the early stages, but, subsequently, the mucous membrane of the nose and of the pharynx swelled up to a tremendous degree. Eventually, the lymphoid tissue became infected in the same way and the patients apparently died rather suddenly, of suffocation, within three or four days—in the severe cases. I took three of the children to Professor Haffkine's laboratory, and he investigated the cases most carefully, and finally decided that he could find no evidence of their being plague. At first I thought they were some peculiar cases of diphtheria, but diphtheria is not very common. It does occur during the rainy season, but even then it is not very common. The cases then passed out of my sight—they came to the out-patient department of my hospital and passed out of my sight and went to reside in Mazagaon. From the Health Officer, Dr. Weir, I understand that they set up a very severe epidemic in that region—that they were a focus of a severe epidemic of plague in Mazagaon."

10,832. 20,394. 20,532.

In connexion with the difficulty of differential diagnosis between mumps and certain clinical forms of plague, we may refer here also to the fact that in the case of one inoculated man in the Byculla Jail at Bombay, and in the case of two inoculated men in the Umarkhadi Jail in the same City, the symptoms were such as to render it doubtful whether the men in question suffered from plague or mumps. The same difficulty appears to have arisen in some other instances. Captain Walton, I.M.S., in particular, drew attention to the difficulty of diagnosing cases at the beginning and end of an epidemic, instancing a case which could be diagnosed as mumps only by observation of subsequent cases arising in connexion with it.

III. 659 (*f*) I. 9; 7236–8. Captain James' Report, p. 166.

318. With regard to the wider issue, as to whether it was the rule for a larger percentage of cases of pestis minor to come under observation at the outset as compared with the other phases of plague epidemics, very little exact information is at our disposal. In addition to the cases quoted above, in which epidemics appear to have begun with the milder forms of plague, only one isolated instance has been brought before us in which an outbreak appears to have begun with cases of pestis minor. This is the instance of the outbreak in Hyderabad, Sind. Here, as we are told by Lieut.-Col. Henderson, I.M.S., mild cases were more frequently observed at the beginning of the epidemic. In adverting to this as the only definite case brought directly to our notice in India, we are not unmindful of the fact that Professor Simpson, who was at the time we have in view Medical Officer of Health for Calcutta, is of opinion that a succession of mild cases of bubonic disease in 1896 preceded the occurrence of genuine cases of plague in that City. On careful consideration, however, of all the evidence that was laid before us in this connexion, we think that there is no reason to suppose that any of the cases which were regarded by Dr. Simpson as cases of pestis minor were genuine cases of plague. In particular, the cases of indolent climatic buboes occurring among the British troops, which were specially noted by Professor Simpson, appear to us to have stood in no connexion at all either with the fact that the Shropshire Regiment had previously been engaged on plague duties in Hong Kong, or with the fact that plague afterwards broke out in Calcutta. The bacteriological evidence which was regarded as establishing that these cases of indolent bubo were cases of pestis minor seems to us to have been fallacious.

General question of the occurrence of cases of pestis minor at the outset of epidemics. 18,102. Paras. 95 and 156 of this Report.

319. In certain cases the advent of a plague epidemic has been heralded by increased mortality from diseases of the respiratory system. Such an increased mortality was a noticeable feature in the months which preceded the first plague epidemic in Bombay. So noticeable was this increased mortality, that Captain Childe, I.M.S., made a special investigation of its causes, in the course of which he established by bacteriological methods of proof that plague may manifest itself in the form of plague pneumonia. In Poona also, antecedent to the outbreak of the third epidemic, an increased mortality from pulmonary diseases came under observation. The data with regard to Poona are in so far more definite as corpse inspection was being

Plague epidemics heralded by increased mortality from pulmonary diseases. I. 375. 21,790; 21,918–9.

regularly carried out in the town, with a view to determining the cause of each death. We learn from the statistical records, which embody the results of these inquiries, that the deaths from diseases of the respiratory system, which had in the months of October, November, and December, 1898, amounted to an average of 99 cases per month, increased to 172 in January, 1899, this being the month which immediately preceded the outbreak of the third epidemic of plague.

21,416.

21,788.

The occurrence of an increased mortality from pulmonary diseases at the commencement of a plague epidemic is not a new observation. We find attention already directed to the matter in a paper by Thomas Whyte, Esquire, published in the "Transactions of the Bombay Medical and Physical Society," for 1838. "On its first visitation," says the observer in question, speaking of the outbreak of plague in Kathiawar in 1820, "that form (of plague) noticed above, as distinguished by fever, slight cough, pain in the chest, and hæmorrhage, has been the most common, but it has disappeared for some time. This variety seems to have appeared first at every place which has yet been visited." Mr. Whyte notes in this connexion the fact, already referred to, that plague at Athens appears to have been ushered in by an epidemic of a fatal form of pulmonary disease. In de Chauliac's *Chirurgia Magna*, already quoted by several authors in connexion with the general history of Plague, there occurs the following passage: "But the mortality"—the author is speaking of the epidemic of plague in Avignon in A.D. 1348—"began among us in the month of January, and lasted for seven months, and presented two types. The first lasted for two months with continued fever and spitting of blood, and these died within three days. The second lasted for the rest of the time, also with continued fever, and apostems and carbuncles on the external parts, especially in the axillæ, armpits, and groins; and they died within five days. And it was so contagious, especially when it was attended by spitting of blood, that not merely by living with the patient, but even by looking at him, one person caught it from another." To return to the case of Bombay and Poona, it would appear to be probable that the increased mortality from pulmonary disease, which was here observed anterior to the outbreak of definite plague, was referable, not so much to typical pneumonic plague, as to the occurrence of cases of mixed infection in which the plague bacillus played only a secondary rôle, the primary rôle being played either by the tubercle or the influenza bacillus, or by the pneumo-coccus.

Occurrence of the septicæmic form of plague at the outset of an epidemic. Para. 368 of this Report. Appendix XXV. in Vol. II. of these Proceedings.

320. In certain places plague has manifested itself at the outset only in a septicæmic form, buboes appearing, as is the rule in septicæmic cases, only where the disease ran a favourable course, and then in the later stages of the disease. This mode of origin of a plague epidemic has come under observation particularly in connexion with the plague outbreaks in the regions of Kumaun and Garhwal, where the disease is, as we shall see, endemic. It was, as we learn from a study of Surgeon-General Planck's record of a large series of village epidemics in these regions, the exception rather than the rule for plague to appear at the outset in the typical bubonic form; in most cases, the symptoms of the disease as first observed were those merely of high fever, followed shortly afterwards by the death of the patient. The typical bubonic form generally made its appearance only after the occurrence of a succession of such septicæmic cases. It is hardly necessary to point out that, when an epidemic of plague begins with a succession of septicæmic cases, the character of the disease may readily be mistaken, unless bacteriological examination is resorted to.

Causes which may determine the clinical type with which plague epidemics commence.

321. It may be profitable, in conclusion, briefly to consider the possible causes which may determine the particular clinical type in which plague appears at the outset of an epidemic. When, as ordinarily has been the case in India, the disease has been introduced by the importation of a human plague case from outside, and when the first indigenous case or cases have followed without any very considerable interval upon the escape of the plague bacilli from the body of the patient who gives origin to the subsequent outbreak, the disease may reasonably be expected to manifest itself in exactly the same guise as in the case from which the infection has been derived. In other words, the disease may be expected to manifest itself either in the ordinary bubonic or in the pneumonic or septicæmic type, according as the original patient may have suffered from the one or the other of these forms of plague. Where, on the contrary, there has been a break in continuity,—where, in other words, the virulence of the infective agent has either been diminished or exalted after its escape from the human organism,—it is comprehensible that the disease might at the outset manifest itself in an altered clinical form. It would, for instance, be intelligible that where the action of prolonged desiccation had come into play, the plague bacillus should be attenuated, and that it should manifest its effects at the outset in a less virulent

type of attack. Possibly the comparative mildness of many of the cases observed in Oporto and Glasgow may admit of such an explanation. Again, where the plague bacillus finds conditions for growth which are favourable to the exaltation of its virulence for man, it is intelligible that plague should manifest itself at the outset in an unusually virulent form. It is interesting to note in this connexion that, so far as can be gathered from the indications furnished in Surgeon-General Planck's Report, already referred to, the outbreaks of plague, which arose apart from an introduction of the infection *ab extra*, were almost uniformly associated with the occurrence of a preliminary series of septicæmic cases. The large majority of the particular village outbreaks in question, were, as we shall see, proceeded by mortality among rats.

Para. 375 of this Report.

XI.(c.) *Question as to whether there is any Variation in the Fatality of Plague in different Phases of an Epidemic.*

322. We may now, in following out the scheme set forth at the commencement of this Chapter, pass on to consider the question as to whether there is any variation in the case-mortality in the various phases of an epidemic – a question which is manifestly of considerable importance in connexion with the rise and decline of plague epidemics. Among the statistical data relating to this question which were furnished to us, by far the most important are contained in the detailed records which set forth the case-mortality from plague in the Arthur Road Hospital at Bombay for each monthly period from October 1896 to January 1899. We have in the chart printed on the following page plotted out for the period in question the curve of the variations in the hospital case-mortality, corrected by the omission of cases of convalescent patients transferred from other hospitals. With January 1899 our detailed record of case-mortality closes. We have, however, with the help of indications contained in a report by Dr. Choksi dealing with the subject of serum therapeutics, carried on the curve to April 1899. In the latter period, however, the case-mortality is to some extent influenced by the results of the treatment. For the purposes of comparison we have also set forth, for the period in question, the curve of the rise and fall of the total mortality in Bombay City, that mortality affording the most trustworthy indications of the course of the successive epidemics.

Question as to whether there is any variation in the fatality of plague in different phases of an epidemic.

Records of case-mortality of the Arthur Road Hospital at Bombay.

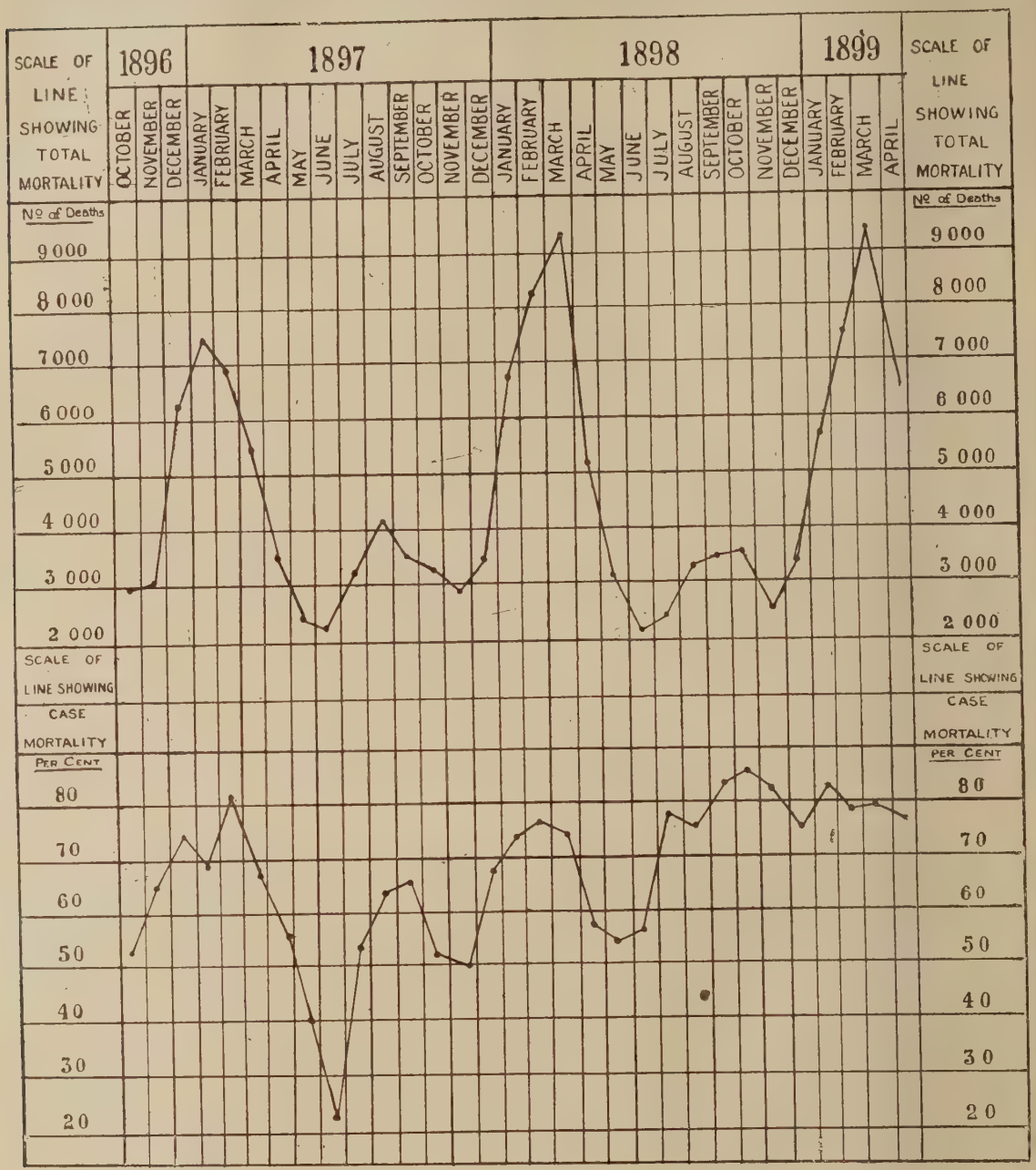
457.

III. 461.

On consideration of the chart, it will be seen that in each successive epidemic the curve of the case-mortality shows a very close agreement with the curve which represents the rise and fall of the epidemic. Thus, in October 1896, at the outset of the first epidemic of 1896–7, the case-mortality stood at a little over 50 per cent.; it rose to over 80 per cent. at the climax of the epidemic, and fell off to a little over 20 per cent. at the end of the epidemic in June 1897. With the recrudescence of plague in July and August 1897, the case-mortality again rose and then sank again until it reached 50 per cent. in November, when plague once more was at a minimum. In the case of the epidemic of 1897–8, the coincidence between the two curves is equally close. The same thing holds true of the recrudescence which occurred towards the end of the rains in 1898 and ended in November, except that the curve of the case-mortality here started from a higher base line, and did not at any time reach a very low level. From November onwards to the month of January 1899 another, but a small, rise in case-mortality marks the beginning of the epidemic of 1898–9. The case-mortality in January reached the high figure of 80 per cent., and it showed a tendency to fall when the epidemic began to subside.

The striking concurrence of the rise and fall of the case-mortality with the rise and fall of each of the successive plague epidemics which have occurred in Bombay City would appear to point clearly to the conclusion that the rise and fall of the case-mortality is connected by some fact of causation with the increase and decline in the spread of the plague. Beyond this we cannot, however, go, as we have no means of arriving at a decision as to which of the phenomena in question is cause, and which is consequence. It is, on the one hand, quite conceivable that the rise and decline of a plague epidemic may depend upon a successive exaltation and attenuation of the plague bacillus in its passage through a series of men and animals. Just as easily conceivable is it, on the other hand, that the successive acceleration and retardation, which must, concurrently with the rise and fall of the epidemic, take place in the transfer of the plague bacillus from the organism of an infected patient to the patient next in series, may, by affording in the one case a shorter, and, in the other case, a longer, interval for the action of desiccation to be brought into play on the bacillus outside the organism, condition a variation in the case-mortality in the different phases of an epidemic.

DIAGRAM showing TOTAL MONTHLY MORTALITY in BOMBAY CITY and the AVERAGE MONTHLY CASE-MORTALITY among PLAGUE PATIENTS in the ARTHUR ROAD HOSPITAL, BOMBAY.



Records of case-mortality from other large centres.

323. When we pass from the records of case-mortality relating to the Arthur Road Hospital, Bombay, to consider the records of case-mortality which have been furnished to us from other large centres of population, we realise that these are practically of no value, inasmuch as they are based, not upon observations in hospital, but upon reports of sickness and death collected from an unwilling population which endeavoured so far as possible to conceal the fact of the existence of plague among them. For this, among other reasons, we have here excluded from detailed consideration the records of case-mortality furnished to us in connexion with the epidemics of plague in Hubli, Bangalore, Calcutta, Surat, and other places. The general tenor of the information collected at one of these places—Belgaum—may, however, briefly be placed on record here, as exemplifying some of the sources of fallacy which complicate such statistics. In the first epidemic at Belgaum in December 1897 and January 1898, the case-mortality of plague, as deduced from the records for the town, works out at over 90 per cent. We are told that during this epidemic there was much panic and concealment, such sick persons as were able to move leaving the town in order to escape the plague measures; we have here a factor which may account for the high case-mortality deduced from the consideration of the records of attacks and deaths. In the second and more considerable epidemic of September, October, and

23,127–8.

23,135–40.

November 1898, when we are assured there was very little concealment, the case-mortality, which according to the reports received at the outset of the epidemic stood at 70 per cent., varied during the height of the epidemic between 70 and 80 per cent. With regard to the case-mortality at the outset and decline of this epidemic, it may be noted that, again according to the reports received, there was a slight rise in case-mortality at the outset, and that the decline of the epidemic was marked by a rise of the case-mortality to 90 per cent. This last feature of the curve of case-mortality is, however, probably due to the carrying over of deaths from the month which witnessed the great decline of the epidemic to the next monthly period, when there were very few new cases.

Records from certain of the smaller towns.

Porbandar. II. 484.

Ankleshwar. 14,404.

Ilkal. 19,493.

Igatpuri. 19,493.

324. The records of case-mortality furnished from the smaller towns, where the plague attacks and deaths could be kept under better observation, are, it may be presumed, more trustworthy. Four of such records are available to us. In the case of Porbandar in 1898, where a total of 465 attacks were recorded in a period of seven months, the case-mortality rose from 66 per cent. at the outset of the epidemic in May, to 84 per cent. in August; it then fell to 73 per cent. in October, after which there were only four cases with two deaths. In the case of Ankleshwar we may omit from consideration the figures which apply to months when cases were numbered only by units, and consider only the records for July, August, and September 1898. In July, when the cases numbered 21, the case-mortality was equivalent to 52 per cent; in August, when the cases numbered 191, the case-mortality works out at 73 per cent; in September, when the cases numbered 103, the case-mortality works out at 75 per cent. When, however, we consult the detailed daily abstract of plague cases and deaths which is available in this case, and note that in many cases which ended fatally the attacks were recorded in one month and the deaths in the next, we arrive by a process of inference at a probable case-mortality of 52 per cent. for July, of 69 per cent. for August, and of 69 per cent. for September. In the epidemic at Ilkal in 1898, 284 plague cases were recorded. Of these, 31 cases occurred in September, 25 in October, and 28 in November. The case-mortality for September works out from the records as 63 per cent., that for October as 87 per cent., and that for November as 78 per cent. If, however, we make the corrections for the carrying over of deaths which seem indicated by the abstract of daily attacks and deaths, we arrive at case-mortalities of 74 per cent., 87 per cent., and 68 per cent. respectively for the successive months. At Igatpuri, in 1897, the plague epidemic covered a period of almost exactly three months. There were 126 cases in the month of August, 298 cases in September, and 86 cases in October. The case-mortality for the successive months works out at 63 per cent., 87 per cent., and 78 per cent. respectively. There are, however, as is manifest from consideration of the daily abstract of cases and deaths, considerable corrections to be applied for the carrying over of deaths. It is difficult to say precisely what the numerical result of these corrections would be; there is, however, no doubt that the corrected curve of case-mortality would rise from a comparatively low figure at the outset of the epidemic, would reach a maximum at the climax of the epidemic, and would towards the termination of the epidemic fall off much more markedly than would appear from the record as it stands. Thus, in all these small towns, the case-mortality rises and declines, as it does in Bombay, with the rise and fall of the plague epidemic.

Paras. 527 and 542 of this Report.

Lastly, we may note the fact that evidence of a great falling off in the case-mortality at the end of a local outbreak is incidentally supplied in the case of statistics cited in Chapter V. of this Report in connexion with the case-mortality of cases not treated with serum in Karachi and Bangalore.

Opinions of plague officers regarding severity of plague in different phases of an epidemic. 14,634, 14,667. 15,113. 15,907.

12,512.

325. If we pass now from statistical results and consider the experience of individual plague officers, we find that five out of the six witnesses who expressed an opinion in this matter stated that they had noticed a diminution in the severity of cases occurring towards the end of an epidemic. The witnesses in question are Lieut.-Colonel Nariman, I.M.S., who was in medical charge of the plague operations in Surat; Diwan Bahadur Samarth, whose observations were made in the City and villages of Baroda; Mr. Jadhav, the magistrate of the Naosari District of Baroda; Mr. Deva and Mr. Cooper, who acquired their experience in the course of their medical duties in Porbandar and Ankleshwar respectively, two of the towns whose statistics have been discussed above. Lieut. Niblock, I.M.S., however, told us that Karachi plague cases were just as virulent during the subsidence, as in the beginning, of the epidemic.

Bacteriological observations pointing to possible diminution of virulence of plague bacillus during last phase of epidemic. 8519. 8669.

326. In conclusion, we may note here that Mr. Hankin observed, in the case of the cultures obtained by him at the end of the plague epidemic at Jawalapur, a distinct diminution in their virulence for animals, as compared with the virulence of the plague cultures obtained by him in Bombay. In association with this diminution of virulence, Mr. Hankin observed that the cultures obtained at the end of the epidemic at Jawalapur were less sticky than the Bombay cultures; the Jawalapur cultures, however, regained the characteristic of stickiness after having been passed through a series of four or five mice. Interesting as Mr. Hankin's observations are, we would note that we have no exact information as to the cases in Jawalapur from which the attenuated cultures were obtained. Apart from this, Mr. Hankin's observations lose a great deal of their value in the present connexion; for, on a study of the list of Jawalapur plague cases which was furnished to us by Mr. Winter, we find that the epidemic at Jawalapur was not characterised by any regular decline of the case-mortality towards its close, unless we can see indications of the diminution of the virulence of the disease in the circumstance that the three last cases, which occurred sporadically within the month subsequent to the close of the epidemic at the end of April, all ended in recovery.

II. 45-8.

XI.(D.) *Question as to whether there is any Inter-relation between Meteorological Variations and the Increase and Diminution of Plague Mortality.*

Total plague mortality in India exhibits marked seasonal variations.

327. A notable feature in the mortality from plague in India has been the fact that, in every place where the disease has established itself in a more or less continuous manner, plague epidemics have alternated with periods of freedom, or comparative freedom from plague. The periods of high and low plague mortality in the case of the places which have contributed most largely to the plague mortality in India have, in the main, synchronised, and synchronised so closely that the plague mortality for India, as a whole, shows a very remarkable and regular seasonal rise and fall. This will be seen by reference to the Chart on the opposite page, on which we have plotted out the curve of plague mortality for the four successive years which have elapsed since the introduction of plague into India. In the month of June, as will be seen, plague has in each year sunk away to a minimum, the plague mortality for the whole of India declining to considerably less than 500 per week, and in three of the years to which our chart relates to very considerably less than this. From June onwards, plague has in each year increased up to a maximum which has fallen on some date between the first week in October and the second week in November. This maximum, except in the case of the year 1900, has represented the absolute maximum of weekly plague mortality for the year. From this autumn maximum the plague mortality has then in each year declined to another minimum which has fallen between the last week of December and the last week of January. The minimum mortality at this period has not fallen below 1,100 per week. After sinking away to this winter minimum, the plague mortality has in each year increased to another maximum in March. We have thus two periods of maximum plague mortality which fall on or about the midsummer and midwinter solstices, and two periods of minimum plague mortality following upon the spring and winter equinoxes.

No salient features which distinguish the weather at periods of high plague mortality from the weather at periods of low plague mortality.

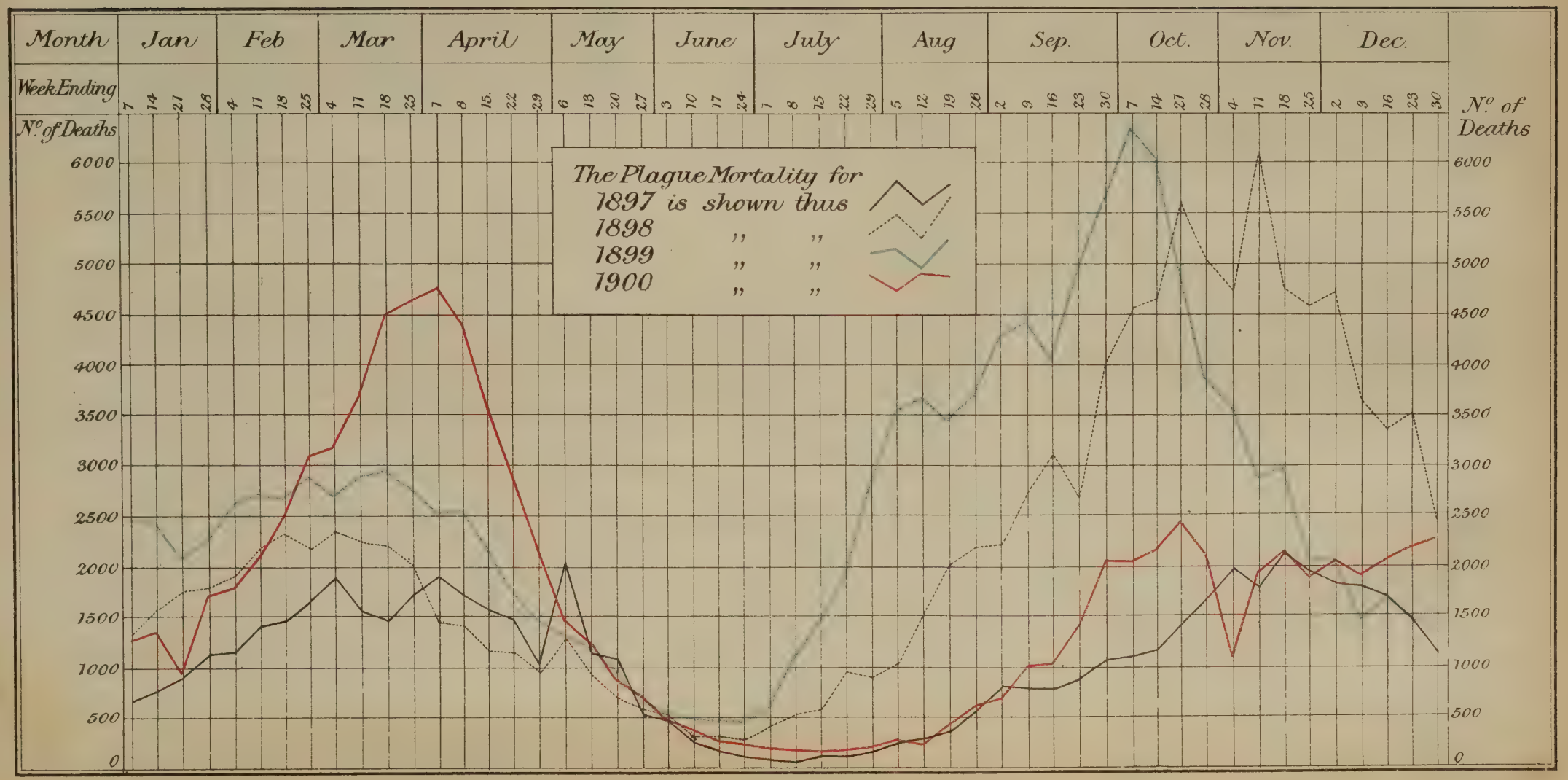
328. When we inquire as to what the meteorological variations are which correspond with the seasonal variations in plague mortality, we come face to face with the following facts. The atmospheric conditions at the period when plague reaches its absolute minimum are those associated with the commencement of the rains; in other words, the temperature is high and the air is saturated with moisture. Essentially similar are the atmospheric conditions when plague reaches it autumn maximum; the rains are not yet over and the air is still hot and moist. When plague has declined to its second minimum, the meteorological conditions are entirely different; the temperature has fallen and the air is relatively dry. Lastly, in March, when the second period of maximum mortality is reached, the weather is hot and the atmosphere is dry. We have thus, in India, two periods of greatest plague mortality, corresponding, the one with hot and moist, and the other with hot and dry, atmospheric conditions; and two periods of least plague mortality corresponding, the one with hot and moist, and the other with cold and dry, atmospheric conditions. It will be manifest, in view of these facts, that there is no direct inter-relation between any particular type of weather and the period of highest and lowest plague mortality.

History of epidemics in particular places confirms conclusion

329. Confirmation of this conclusion is afforded by the study of the history of plague in many places, particularly in Poona and Karachi. For the two cities ju

CHART

Showing Rise and Fall of Mortality from Plague throughout India in each Week during the Years 1897, 1898, 1899 and 1900.

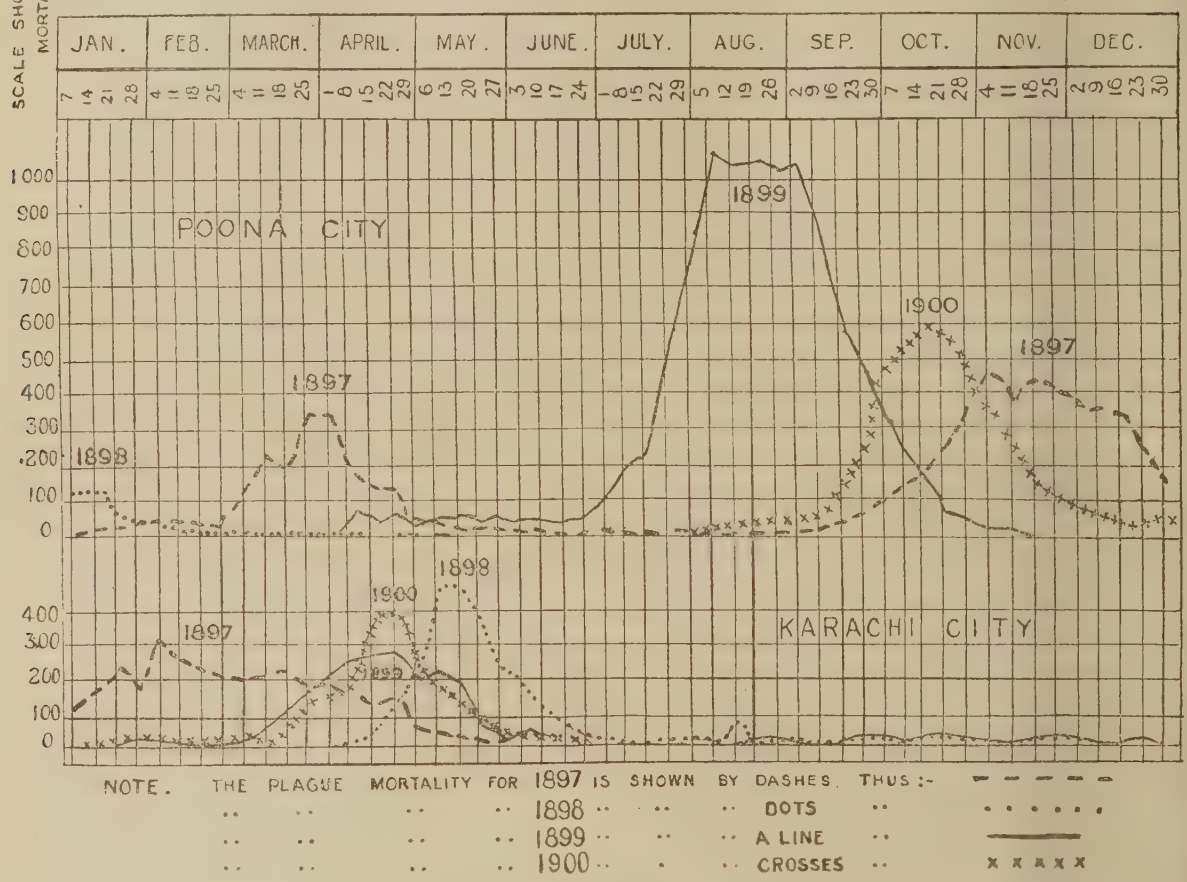


that plague may be epidemic in all kinds of weather.

mentioned, we have plotted out the course of plague for the years 1897 to 1900 inclusive, in the following chart :—

CHART

SHOWING RISE AND FALL OF PLAGUE MORTALITY IN 1897, 1898, 1899 AND 1900 IN POONA CITY AND KARACHI CITY



It will be seen on reference to the chart that plague has spread in an epidemic manner in Poona and Karachi under the most diverse meteorological conditions, the period of maximum plague mortality having fallen indifferently in periods of cold dry, hot dry, and hot moist, weather.

Influence of variations of temperature and humidity on prevalence of plague.

330. A few further points may be brought out in connexion with the general question of the influence of hot and dry, and moist and cold, atmospheric conditions upon the prevalence of plague in India.

In connexion with the influence of the first set of metereological conditions, we may draw attention to the following facts. Plague was epidemic in Sind in the very hot dry weather, the climax of the epidemic falling in March and April respectively in two successive years, 1897 and 1898. Again, plague, though it was introduced into the Punjab in the autumn of 1897, did not become epidemic till the spring of 1898, the climax falling in the very dry and hot month of May. Lastly, in the years 1897, 1899, and 1900, the plague was epidemic in the State of Cutch only in the hot dry weather.

In connexion with the question as to whether the general decline of plague in India in December stands in association with the colder, and in some places damper, atmospheric conditions prevalent at midwinter, we think it interesting to note that plague outbreaks not infrequently occur in Kumaun in the midst of ice and snow.

Prof. Hunter Stewart's opinion on the question.

Further, we would call attention to the fact that Professor Hunter Stewart, of the University of Edinburgh, who kindly undertook the critical examination of the meteorological data bearing on this question, arrived at the conclusions that: "In the areas affected by plague (1) the temperature conditions are suitable for the epidemic appearance of plague at all seasons, and (2) the amount of moisture in the atmosphere has little, if any, influence."

331. Before pursuing any further the consideration of the general question of the influence of the meteorological conditions upon the spread of plague, and in particular upon the length of time for which the bacillus of plague may survive outside the organism, we may, perhaps with advantage, pass in review such detailed comparative records of meteorological variations and plague mortality as are available from plague-infected places in India. We may deal first with the smaller places, and then proceed to consider the case of Bombay City.

Detailed comparative records of meteorological variations and plague mortality available from certain places in India.

In the case of the epidemic in the Sirohi State, we were furnished by Captain Grant, I.M.S., with a chart on which are set down the mean daily temperatures for the period covered by the epidemic and the number of plague cases which occurred daily. On a study of this chart, which is printed as Appendix XXXII. in Vol. II. of our Proceedings, it will appear that, with hardly an exception, a fall in the mean temperature was followed, in the course of a period varying from one to four days (*i.e.*, after an interval which may possibly represent the period of incubation), by a rise in the number of plague attacks. Similarly, each rise in the temperature appears to have been followed by a fall in the number of plague cases, while the final rise of temperature at the end of the chart was associated with the extinction of the epidemic. The interest of these particular observations is unfortunately diminished by the fact that they apply to an epidemic where the maximum number of cases did not on any day exceed seven. The meteorological observations made in Bangalore are, for an entirely different reason, almost devoid of interest. Here the variations in the temperature and humidity of the atmosphere were, in the period covered by the epidemic, quite insignificant. The figures relating to Baroda cannot be utilised here inasmuch as grave doubt as to the correctness of the plague mortality recorded is aroused by the fact that, after subtracting the plague mortality from the total mortality, the figures given indicate a mortality from causes other than plague which varies in the seven months to which the statistics relate from about 24 per mille to 120 per mille.

Sirohi. 9902; App. XXXII. in Vol. II. of our Proceedings.

Bangalore. App. LXXI. (iii.) in Vol. II. of our Proceedings.

Baroda. 15,238–46.

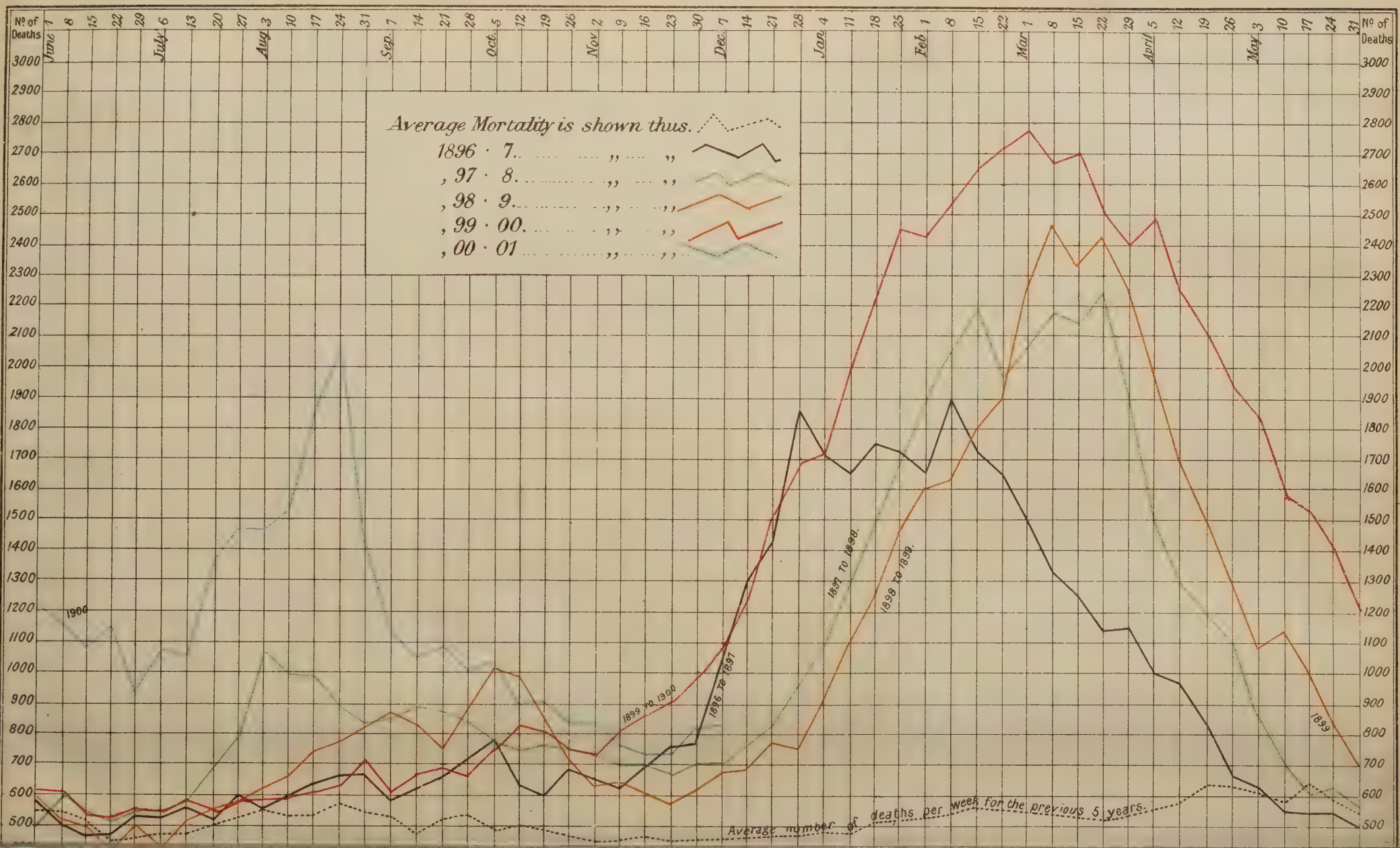
332. We may pass now to consider the detailed records of plague mortality and meteorological variations which are available in the case of Bombay City. Here, as in the case of India generally, a very remarkable seasonal periodicity of plague has come under observation, as will be seen on reference to the charts of total mortality published in Volume III. of our Proceedings as Appendices Nos. LXXVII., LXXVIII., and LXXXIV. This last chart we have brought up to date and reproduced on the following page. It will be seen from this chart that, in each successive year since plague was introduced into Bombay, the weekly mortality of the City has risen in the early months of the cold weather, and has increased up to a maximum, which has fallen in 1897 in January, in 1898 in February and in 1899 and 1900 early in March. After reaching its maximum, the weekly mortality has in each year fallen away rapidly, reaching a minimum in the month of June. At this period, except in the year 1900, the total weekly mortality has been only very slightly higher than the average weekly mortality for the City in the years anterior to the advent of plague. In the end of July or in the month of August the curve of mortality has in each year risen—most markedly in 1900, and in the other years to a less extent. The curve then sinks away towards a second minimum in the beginning of November.

Seasonal variations observed in the different epidemics of plague in Bombay City.

333. The most important data bearing on the question of the influence of meteorological variations on the plague mortality in Bombay will be found embodied in the chart printed as Appendix LXXVII., which was put in by Mr. James, the Drainage Engineer of Bombay. Upon this chart have been entered for each day, since the advent of plague to the end of May 1899, on the one hand, the total mortality for Bombay City, and on the other hand:—(*a*) the maximum and minimum temperature; (*b*) the degree of humidity of the atmosphere; (*c*) the amount of rainfall; (*d*) the velocity of the wind; and (*e*) certain data with regard to the presence and amount of cloud. On consideration of this chart, it will be seen that, although the annual curve of plague mortality coincides in a general way with the annual curve of the temperature, yet the correspondence is by no means a very close one. Even if we quite leave out of sight the relatively inconsiderable rise and fall of the curve of mortality between July and November in the years with which the chart deals, our attention cannot fail to be arrested by the higher range of temperatures which were associated with the climax of the second and third, as compared with the first annual epidemic. Whereas the climax of plague coincided in 1897 with minimum daily temperatures which ranged between 60° and 70° F., the corresponding temperatures during the

Consideration of Mr. James' comparative chart of meteorological variations and plague mortality for Bombay City.

DIAGRAM, Shewing weekly Mortality in Bombay June, 1896 to Dec. 1900.



climax of plague in 1898 ranged from 65° to 70°, and in 1899 almost always between 70° and 80° F. The absence of correspondence which here emerges as between the annual curves of temperature and of plague mortality comes out in a much more distinct manner on a consideration of the daily and weekly variations. A study of the daily and weekly rise and fall of the temperature and mortality curves will show that it would be as easy to interpret the curves in the sense of a rise of temperature conditioning a rise in plague mortality as in the opposite sense. What holds true of the daily and weekly variations of temperature holds true, as the chart shows, also of the variations in humidity. If, therefore, the rise and fall of plague mortality is in reality dependent upon the meteorological variations, it seems quite clear that the inter-dependence must be a very indirect one.

334. A variety of theories has been put forward as to the cause of the seasonal variation of plague in Bombay. Among these the following are perhaps deserving of mention :—

Theories which have been put forward to account for the seasonal variation of plague in Bombay :—

(*a*) It has been suggested that the periodical increase in the incidence of plague is due to the fact that, at the period of the rising epidemic, new pabulum is provided for the plague bacillus by the arrival in Bombay of immigrants from uninfected centres. This theory, if it rests upon any foundation of fact at all, would appear to have a very limited sphere of application, for, as will be seen, on a study of Mr. James' Chart, printed as Appendix LXXVII., an exodus took place in each successive year when plague began to spread in an epidemic manner in the City. The diminution of the population was particularly well marked in the case of the first epidemic. Moreover, this theory would not account for the rise of plague mortality so noticeable in July and August in 1899 and 1900.

(*a*) Suggestion that it may be due to seasonal variation in population.

(*b*.) It has been suggested that the rise and fall of the plague mortality may depend upon a greater or less prevalence of plague among rats and the consequent greater or less dissemination of the infection through the agency of rats. In connexion with this theory it will suffice to note that it rests on casual impressions only, and not on any basis of systematic observations. Even if the seasonal periodicity of plague in Bombay were brought into association with a seasonal periodicity of plague among rats, the underlying causes of the observed seasonal variation would be still to seek.

(*b*) Suggestion that it may be due to seasonal increase of plague among rats.

(*c*.) It has been suggested that the seasonal variation in plague in Bombay may be dependent upon the variation of the ground evaporation, which is conditioned by variation in the atmospheric condition.

(*c*) Suggestion that it may be due to variation in ground evaporation.

Mr. Moos, the Director of the Government Observatory in Bombay, has endeavoured to establish the existence of such an indirect association. He has ingeniously shown that when, in addition to the temperature and humidity of the atmosphere, certain data relating to the temperature of the soil are taken into consideration, and when upon the basis of these data the amount of ground evaporation is computed for a series of successive weekly periods, a curve is arrived at which shows an extremely striking correspondence with the curve of the total weekly mortality. As may be seen, on consideration of Mr. Moos' Chart B., printed in Appendix LVIII. in Volume III. of our Proceedings, the coincidence extends even to the smaller undulations of the curve, so much so that accidental association would appear to be excluded. When, however, we go into the methods which were employed by Mr. Moos in calculating out his curve of evaporation, we find the correspondence between the curve of evaporation and the curve of mortality somewhat less convincing, as to the existence of a close connexion between the amount of ground evaporation and the course of plague mortality. We learned in the course of our examination of Mr. Moos that the minor coincidences of the two curves, which appeal in such a forcible manner to the eye, represent the result arrived at by calculations from data selected upon a principle which seems to us to be open to criticism. Mr. Moos told us that he had selected as the basis of his calculations the particular series of ground temperatures taken at a depth of 5 feet, because the curve of evaporation which was deducible from these particular readings coincided most closely with the curve of mortality. In brief, we learned from Mr. Moos that if he had selected any other series of thermometric readings than those of this particular depth of 5 feet, the smaller correspondences between the curve of ground evaporation and the curve of plague mortality would have disappeared, though the general agreement between the two curves would still have been preserved. Another point in Mr. Moos' calculations which seems to us deserving of note relates to his assumption that there is a free interchange of vapour between the ground at considerable depths, such as that of

20,276–20,309. App. LVIII. in Vol. III. of these Proceedings.

20,296.

20,298–30.

5 feet, and the air. We think that this assumption, though it might hold good in a calculation affecting only the broad features of the annual variation, could hardly hold good in calculations affecting the question of the vapour which would be given off from the ground in a daily or weekly period. We pass to a more general criticism. It appears to us that if Mr. Moos' theory of the causal association between the rise and fall of the curve of ground evaporation and the rise and fall of plague mortality furnishes a true explanation of the seasonal periodicity of plague in Bombay, the theory ought to be applicable also, in a general manner at least, to other places, but the fact that in many places severe plague epidemics have occurred in the rains, when ground evaporation is largely suspended, would seem to indicate that this factor, if it plays a part in determining the spread of plague, cannot in all places be a factor of prime importance. That the problem is far from clear, and that there are many difficulties in the matter will be apparent from the fact that Dr. Buchan, the Secretary of the Meteorological Society of Scotland, to whom we referred Mr. Moos' meteorological work for technical criticism, sums up his study of the evidence in the following terms: "This connexion between the curve of mortality and these temperatures' curves opens up a field of research to the physician and the bacteriologist, and the research is all the more likely to lead to good results when it is kept in mind that the principal maximum mortality of the year occurs when the weather is coldest and driest and the secondary maximum when the weather is moistest and wettest, and the temperature very high."

III., 437.

(*d*) Suggestion that it may be due to influence exerted by weather on habits of the people. 26,104.

(*d.*) Further, it has been suggested that the seasonal variation of plague in Bombay may be dependent upon the influence which cold weather exercises in driving the people into their houses at night. The possible importance of this factor as a means of spreading plague will be immediately appreciated when it is realised that an estimated number of 350,000 people (*i.e.*, a number equivalent to nearly a half of the normal population of Bombay) sleep, in the warm weather, in the open, and in the cold weather, in their houses. It may reasonably be assumed with regard to the inmates of plague-infected houses that these, when sleeping indoors, would be more exposed to the risk of infection. We have unfortunately no means of testing the actual effect of this factor by reference to exact observations. There is not in the case of Bombay anything in the nature of a trustworthy record of the daily plague attacks available, such as would enable us to judge of the influence exerted by a cold night. And though Mr. James' Chart shows in practically every case a rise in mortality as occurring within a few days after a cold night, the general character of the curves is so irregular, and there is so much uncertainty as to what interval would elapse between plague infection and death, that it is impossible to correlate any particular rise of mortality with an antecedent fall in the temperature. Thus, no definite conclusion can be arrived at as to the effect produced by the driving in of the population into their houses at night by the cold. It is, however, remarkable that the advent of the rains, though it must obviously drive the people into their houses at night, does not, in Bombay City, go hand in hand with any increase of plague.

(*e*) Suggestion that it may be due to longer or shorter survival of plague bacillus outside the organism.

(*e.*) Lastly, it has been suggested that the seasonal rise and fall of the plague mortality in Bombay may be directly or indirectly dependent on an influence exerted by seasonal meteorological variations on the survival, growth, or virulence, of the bacillus outside the animal organism.

This last theory we propose to consider in its relation to the wider question of the seasonal variation of plague in India generally.

Weather may influence plague indirectly by its effects on the human community; in particular, by confining people to their houses and interfering with plague measures. 13,485. 16,646.

335. Before dealing with the special question of the influence of meteorological conditions on the survival in a virulent form and eventual multiplication of the plague bacillus outside the body, we may emphasize the point, which has already emerged in connexion with Bombay City, that metereological conditions may, in some cases, determine the greater or less prevalence of plague by influencing the habits of life and activities of the human community. The fact that in the Sirohi State plague cases were more numerous after a cold night, and that the same was observed at Daman and Cutch Mandvi after the occurrence of a Scotch mist or a heavy dew at night, may possibly be explicable in this way. Again, we must bear in view that certain meteorological conditions—and we have in view here in particular the occurrence of heavy rains—may render all measures for the suppression of plague difficult or impossible to carry out. The heavy mortality which, as will be seen from the chart printed as Appendix LXVI. in Volume III. of our Proceedings, occurred in the Karad villages after the downpour of heavy rains, may very probably have been

due to the necessary suspension of the evacuation operations. In the same way the phenomenally high plague mortality which occurred in the Dharwar villages in the heavy rains, is probably accounted for by the suspension of evacuation proceedings. In harmony with this is the opinion expressed by Professor Hunter Stewart that "the rainfall seems to exert its effects chiefly or entirely by interfering with remedial measures." 1601, 2192, I. 92-3.

336. A few words will dispose of what it is necessary to say here concerning the question of the possible influence of meteorological conditions on the vitality of the plague bacillus outside the organism. Influence exerted by meteorological conditions on the survival of plague bacillus.

We have already seen, in a previous section, that the plague bacillus is capable of surviving exposure, on the one hand, to the winter frosts of Russia, and, on the other hand, to the tropical heats of a summer on the plains of India. It will thus appear that variations of temperature which occur in India generally, not to speak of those which occur in the equable climate of Bombay, could not of themselves directly affect the vitality of the plague bacillus. Paras. 141 and 146 of this Report.

Much more important, as we have seen, than the direct influence of temperature is the influence of temperature as determining a variation in the humidity of the atmosphere. In conformity with this it might have been supposed that plague would under damp atmospheric conditions everywhere spread in an epidemic manner, and that it would die out when the air became dry. This, however, as we have already seen, does not hold true. Not only has plague spread in an epidemic manner in the hot dry weather in some of the hottest and driest regions of India, but it has, taken generally, died out or decreased about the time of the advent of the rains. We can only explain this by supposing that the influence which atmospheric humidity exerts in the direction of favouring the survival of the plague bacillus in external nature must in very many cases be compensated for, and more than compensated for, by the increased multiplication of saprophytic bacteria which takes place in damp surroundings. It will be remembered that many of the common saprophytic bacteria exert an influence which is unfavourable to the survival and multiplication of the plague bacillus.

In connexion with the theory of the influence exerted by the ground evaporation, and in particular by the ground evaporation from a sewage impregnated soil, on the rise and decline of the plague mortality, we may immediately reject the idea that the ground evaporation could be directly responsible for the conveyance of the disease to man. Bacteria would under no circumstances be carried up by evaporating moisture. On the other hand, it is obvious that the evaporation of moisture from the soil might keep the earth floors of the lower storeys, and through them the atmosphere of the houses, continuously moist. It might thus, given that the other conditions were also favourable, operate in the direction of favouring the survival, or, as the case might be, the saprophytic growth, of the plague bacillus. Further it is just conceivable, in the light of Dr. Marsh's experiments, already referred to in this Chapter, that the ground air of a sewage-polluted soil, containing as it does appreciable quantities of carbonic acid gas, might, when it was carried up by the current of evaporating moisture, favour the saprophytic growth of the plague bacillus in the upper layers of the soil. Para. 162 of this Report.

Certain other considerations relative to the influence of meteorological variations on the survival of the plague bacillus in a virulent condition outside the organism will more appropriately come up for discussion in connexion with the theory of acclimatisation. Section XI.(a.) of this Chapter.

XI.(E.) *Question of the Influence of Insanitary Surroundings on the Spread and Persistence of Plague.*

337. Outbreaks of plague have in India, as elsewhere, often been brought into relation with the existence of insanitary conditions in and about the dwelling-houses. Among the conditions which have been brought into relation with plague in India are: overcrowding, absence of ventilation, damp, defective lighting of the dwelling-rooms, the character of the floor (this consisting in India for the most part of a mixture of earth and cow-dung), and, lastly, defective cleanliness, and, in particular, defective arrangements for the removal of the night soil. Among the conditions external to the houses which have been brought into relation with plague are: sewage pollution of the soil, excess of moisture—gauged by the high level of the subsoil water,—narrow streets, and a deficiency in open spaces. In short, every circumstance, excepting only the character of the food supply and of the drinking water, has been brought into relation with the prevalence of plague. Insanitary conditions which have been brought into association with plague.

We purpose here to consider how far a causal association can be established between the various insanitary conditions which have been particularised above and the prevalence of plague.

XI.(E.)(1.) *Existing Conditions in India in connexion with Sanitation inside Dwelling-houses.*

I. Internal conditions.
I. (*a*) Existing conditions with regard to overcrowding. 25,667.

338. We may first consider the existing conditions with regard to overcrowding. All over India, and particularly in the towns, we have to deal with conditions of overcrowding. In the case of the City of Bombay, the degree of overcrowding may be judged from the fact there is in some of the wards one inhabitant to every 7·2 square yards of superficial ground space. At night the floor space in the large chawls in which the working population of Bombay is housed is, in the cold weather, when the people sleep indoors, covered with sleeping forms to such an extent that the floor is almost hidden from view.

I. (*b*) Existing conditions as regards ventilation.

339. Provision for ventilation can hardly be said to exist in ordinary native houses in India. In the ordinary sleeping rooms in the village houses there are no windows, and such sleeping rooms communicate with the exterior only through the intermediary of other rooms. In many cases cattle stalled within the house contribute to the vitiation of the atmosphere.

In the case of many of the overcrowded houses in towns (we have in view here particularly the chawls of Bombay), the conditions in the matter of the ventilation of the inhabited rooms are worse even than in the village houses. Not only is there less cubic space available proportionately to the number of persons accommodated within the limits of a single room, but there is, in the case of many of the rooms, a system of subdivision, by means of which each family is partitioned off from the others; in addition, purdahs, in the form of curtains or mats, are placed over all the doors to provide for the privacy of the women. And though windows are to be found in the large majority of the rooms, such windows look out in many cases upon dark and narrow gullies, and they are rarely opened. Again, the dwelling-rooms communicate with the exterior only by a system of low, narrow, passages. From this combination of circumstances, the condition of the atmosphere after the evening meal has been cooked in the rooms and the people have retired to sleep, with all the windows fastened, is more easily imagined than described.

If we seek for the reasons for this absence of ventilation from the native houses, we must bear in mind that the absence of windows and other openings in the walls is in conformity with the habits and attitude of mind of the native. Not only are draughts and ventilation of all kinds distasteful for their own sake, but—and this applies in particular to the village houses—the very presence of windows appears to be associated in the native mind with a sense of insecurity and with a sense of want of privacy. In the case of many of the large chawls in Bombay and other towns, an additional factor comes into consideration, this factor being the entire absence of sanitary supervision in the building of the houses. Even in the particular case of Bombay we ascertained that the Medical Officer of Health for the City can exercise no effective restraint on the erection of houses which have no means of ventilation.

I. (*c*) Existing conditions with regard to damp in the houses.

340. There is nothing special to be noticed about the condition of village houses in India with respect to damp. Speaking generally, inasmuch as the floor is raised above the surrounding ground, the houses are dry, except in the rainy season. An exception to this rule is, perhaps, to be found in the case of the houses, numerous in some parts of the country, where the cattle are stalled in the dwelling-house itself. Here we must assume that the atmosphere of the house may be frequently kept damp by moisture derived from the cattle. Especially must this be the case in the Kumaun and Garhwal districts (presently to be referred to as districts where plague is endemic). Here not only are the sleeping rooms purposely placed directly over the cattle stalls, so that in winter the warm exhalations of the cattle may come up through the chinks of the floor, but, in addition, the sleeping rooms are used also as store-rooms for the grain, which is, we are told, here brought in and stored in a moist condition in baskets.

II. 354 (209).
II. 360 (252).

In large towns we have to deal everywhere with the damp which is incidental to crowded houses; that is, with the damp associated with the exhalations from the cooking, and from the bodies of the numerous persons who inhabit the houses. In the particular case of Bombay we have, in addition, to deal with houses which are supplied with an unlimited water supply, which is both lavishly used and wasted by the natives.

341. In a large part of India, the majority of native houses in villages are built without windows, and daylight obtains access to the interiors only through the doors. The sleeping rooms, opening as they almost always do off other rooms, are entirely dark. The reason of this is probably to be found not only in the desire for privacy and security, which is, as we have seen, constantly present to the native mind, but also to the fact that a well lighted house appears to the native to be uncongenial and uncomfortable. I. (*d*) Existing conditions as regards lighting.

In the case of the chawls in Bombay, although the large majority of the rooms are provided with windows, many of these, as before said, open upon dark gullies. Again, there are many rooms which are windowless. These, since the passages leading to them are often low and tortuous, are altogether dark. So much is this the case that it is often necessary, even in broad daylight, to have recourse to artificial light to find one's way about in certain portions of the houses.

342. The floors of Indian houses, even those of upper storeys in many town houses, commonly consist of beaten earth covered over with cow-dung. The floors are renewed every week, as a semi-religious ceremony among the Hindus, by the application of a fresh layer of cow-dung, plastered on wet. When this has been recently done, the floor has a pleasant, smooth, and clean, appearance; it is cool and comfortable to the touch in summer and warm in cold weather, these being considerations of importance to the mass of the people, who go about their houses barefoot. I. (*e*) Existing conditions as regards character of the floor.

343. Taken as a whole, the dwelling-rooms in India are surprisingly clean. This is due to the fact that the floors are periodically cleaned and renovated with cow-dung, that there is hardly any furniture in the rooms, and that the walls are generally not recessed in such a manner as to allow of the collection of dirt. Even where cattle are kept in the houses, the houses are not necessarily dirty, as the animals are not stalled in the dwelling rooms. What applies to the general cleanliness of the houses all over India, applies also to some extent to the chawls in Bombay. In these the majority of the dwelling-rooms—such at least as are in the occupation of single families—are kept neat and clean. On the other hand, the windowless rooms, where the very poor are crowded together, are, naturally, in a very filthy condition. Moreover, the passages and other portions which are used in common by the tenants, and for the cleanliness of which the landlord is responsible, are very much neglected, the walls and the woodwork being begrimed with smoke and grease, and the earth floors being in the wet weather trodden into mud. I. (*f*) Existing conditions as regards cleanliness.

344. Closely connected with the idea that plague is a filth disease is the idea that it may be associated with faulty drains and generally with night soil. I. (*g*) Existing conditions as regards removal of night soil.

In Indian villages there are no special conservancy arrangements. The universal practice is for the villagers to resort to the fields in the outskirts of the inhabited site. Thus, there is no question of the accumulation of fæcal matter in village houses.

In the towns and cities of India the conservancy arrangements naturally vary in different places. It will suffice here to consider the conservancy arrangements in Bombay. The City is divided into drained and undrained districts. In the case of the drained districts the sullage and fluid excreta only are carried off by means of drains. The night soil proper is collected into baskets, which are carried on the heads of sweepers to certain so-called depôts, where the material is shot into the main sewers. In the districts of the City into which the drainage system has not yet been introduced, the sullage and fluid excreta are carried off into cesspools, the night soil proper being, as in other parts of Bombay, carried to the depôts. 25,741-79.

XI.(E.)(2.) *Existing Conditions in India in connexion with Sanitation outside Dwelling-houses.*

345. We may pass on now from the consideration of the insanitary conditions in the interior of dwelling-places to enquire what insanitary conditions external to dwelling-houses have been alleged to have an influence on the spread of plague. In doing this we find that the sewage pollution of the soil in Bombay—due to the presence of cesspits and open drains in the undrained districts, and to faulty connexions in the drained districts, of the town—has constantly been asserted to be the cause of the presence and persistence of plague in the City. Similarly the high level of the subsoil water in Bombay—due to the fact that the City is, in large part, built on land reclaimed from the sea—has been asserted to stand in relation to the continued presence of plague in the City. Lastly, an influence has, in the case of Bombay and other towns, been attributed also to the narrowness of the streets and the deficiency of open spaces in the native quarters. II. External conditions. Sewage pollution of the soil, high level of subsoil water, and narrow streets.

XI.(E.)(3.) *Measures intended to combat Plague by effecting Sanitary Improvements.*

Measures taken to improve conditions—App. XXVI. in Vol. II. of these Proceedings.

346. Attempts to combat plague by measures for the introduction of sanitary improvements in the houses have been made ever since the middle of the century in Kumaun, where under the Mahamari rules the housing of the cattle in separate sheds and the removal of the manure heaps to a distance from the houses have been prescribed. In the same way, the idea that the disease can be combatted by measures of sanitary improvement has to a great extent directed the actions of the sanitary authorities in dealing with the plague in the City of Bombay and many other places in India affected and threatened by plague.

of overcrowding;

An endeavour has been made to relieve to some extent the overcrowding in the most infected quarters of Bombay by instituting health camps, and by inducing the people to establish themselves in these. It has, however, in view of the enormous population which has to be dealt with, been impossible sensibly to relieve the general overcrowding by this means.

of bad ventilation;

Para. 666 of this Report.

Similarly, in connexion with plague operations, efforts on a large scale have been made throughout India to remedy the defects in ventilation which have just been described. We shall presently see that in connexion with plague measures the drastic method was adopted of unroofing whole villages. In hundreds and hundreds of villages where this extreme measure was not carried out, holes were knocked through the roofs and through the walls of the houses for the purpose of improving the ventilation. In the case of Bombay and other cities, every "insanitary looking" house was partially untiled. In the case of crowded chawls in Bombay, the partitions referred to above as having been put up in the rooms were in many cases removed so as to allow of a certain circulation of air. Holes were cut in the walls above the doors. In some cases holes were made also in the ceilings so as to allow of the passage of air from floor to floor throughout the building. No permanent improvement, however, has been effected either in towns or in villages, for the general experience has been that the people soon close up the openings again, and everything reverts to the *status quo ante*.

of damp;

With a view to remedying the excessive damp in the dwellings in Bombay City, the water has, in many instances, been cut off from the houses, and the only supply procurable has been from stand pipes in the streets.

of dirt and uncleanliness;

Under the belief that plague is pre-eminently a filth disease and more likely to occur in dirty than in clean houses, great efforts were in many cases directed to cleaning up the houses, and especially to whitewashing them, with a view to warding off the advent of plague. The whitewash, however, was in certain cases applied only to the outside of the houses. In Bombay, where the authorities thought they were able to foretell the direction which plague would take, particular endeavours were made to clean up the houses along the line which it was assumed that plague would follow.

of defective conservancy arrangements;

Lastly, in accordance with the prevailing idea of plague as a filth disease, and a disease which is connected in some way with fæcal matter, the efforts of the sanitary authorities were, in the case of large cities, directed to the improvement of conservancy arrangements. Latrines and sewers were flushed down, and a spirit of greater activity was infused into the ordinary operations of conservancy. In Bombay thousands and thousands of gallons of carbolic acid were thrown down the drains, and the drain pipes were, in some cases, taken down from the houses and put through a process of steeping in hot water, which was in intention a process of disinfection.

of sewage pollution, high-water level, and narrow streets.

With regard to measures for improving the insanitary conditions associated with the sewage pollution of the soil, the high level of the subsoil water, and the existence of narrow streets in the native quarter, it may be noted that, in the case of Bombay, large schemes for subsoil draining, and for the broadening of the streets, and the rebuilding of certain of the native quarters, have for some time past engaged the attention of the Government in Bombay. Activity in this direction has not been confined to Bombay City.

XI.(E.)(4.) *Discussion of the probable Influence of the various Insanitary Conditions on Plague.*

Effect of overcrowding discussed.

347. Approaching the question of the effect of overcrowding in houses from the *à priori* standpoint, it would seem certain that in a crowded house there must not only be greater opportunities for the direct transference of an infectious disease

om the sick to the healthy, but there must also be greater opportunities for the direct transference of such a disease by means of infected articles or the infected ›or. When, however, we leave these *à priori* reasonings, and come face to face with .e question as to whether as a matter of experience an association has actually been ›served to exist between the crowding of houses and the prevalence of plague, e can, indeed, point to instances such as those already cited in a previous section, here a number of plague deaths have occurred in a single house. We may, if we ease, adduce these as illustrations of the influence of overcrowding on the spread of e disease. But individual instances are, in such a case, inconclusive, inasmuch as any other contrary instances might be adduced in which plague did not spread in ercrowded chawls. The statistical method is obviously the only method by which general proposition, such as that now in question, can be established. Such a ıtistical method has been employed in the case of Bombay.

348. An endeavour was there made to compare the number of plague deaths in e more densely populated districts of the City with the number of plague deaths ıich occurred in the more sparsely populated districts. Figures subserving the mparison in question have been collected by Lieut.-Colonel Weir, I.M.S., Medical ficer of Health for Bombay. These figures, which are published in Lieut.-Colonel eir's Annual Reports, and which were furnished to us by him, set forth, on the one nd, the number of plague deaths which occurred in each ward, and, on the other nd, the average number of persons per house, and per unit of superficial area, in ;h ward. Lieut.-Colonel Weir's figures, so far as they were at the time available, re considered by Mr. Hankin in a paper already referred to. Mr. Hankin has esented the results of Lieut.-Colonel Weir's figures in the form of a series of graphic rves. These curves show clearly that there is no inter-relation between the density the population in the different wards of Bombay, as estimated on the basis of ıut.-Colonel Weir's figures, and the severity of plague in those wards. More than ıt, Mr. Hankin's curves show that plague has, generally speaking, in the case of mbay, been most severe in the wards where the density of the population—as lged on the one hand by the number of people dwelling in each house, and on the ıer hand by the number of inhabitants per unit of superficial area—has been least.

Statistical investigation of effect of overcrowding in Bombay. Deductions drawn by Mr. Hankin from statistical data. 25,665–7.

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Neither of the criteria employed can, as a matter of fact, be considered as an ;urate criterion of the density of population. On the one hand, we cannot take the nber of persons per house as an index of the overcrowding of the houses in the ferent wards, inasmuch as the average size of houses varies very considerably in different wards, those in the suburban districts being only one or two-storeyed, l as a rule comparatively small, while those in the centre of the town are in many es large and many-storeyed. Thus the number of people per house in the case of mbay gives an inaccurate measure of the relative overcrowding of the different ts of the City. Objections of not less weight can be brought against taking as a asure of the overcrowding in the separate wards the figures which are arrived at dividing the population of each ward into the superficial area contained within the d boundaries. This last criterion is, in the case of many of the wards, an olutely fallacious one, firstly, owing to the fact that the average height of houses ies a great deal in the different wards, and, secondly, owing to the fact that there included in certain of the wards quite extensive areas of land not built over. nifestly, in the wards where the taller houses are to be found, the overcrowding ıld *pro tanto* be less than in the wards where the houses are for the most part -storeyed. On the other hand, in the wards which contain much ground not built r, the density of the population would really be greater than would appear in the ıres.

Criticism of criteria of density of population employed by Mr. Hankin.

349. In spite of these considerations, which show that the figures available for ulating the density of the population do not accurately measure the density of the ulation, we have no doubt that they do, as a matter of fact, provide a rough cation of the density per superficial unit of floor space of the population in the rent quarters. For we were assured by Lieut.-Colonel Weir—and indeed it is a question on which there can be any doubt—that the conditions as regards crowding in the suburbs are not in any way comparable to those in the heart he town. It would thus appear that the absence of direct inter-relation between conditions of overcrowding and the prevalence of plague which is exhibited so rly in Mr. Hankin's curves must, paradoxical as that absence of inter-relation may ear, be accepted as a fact of actual observation. We have ourselves calculated out later figures given to us by Lieut.-Colonel Weir, and we have arrived at the

Conclusion that statistics do not show existence of any relation between overcrowding and plague. 25,676–9

same result as Mr. Hankin. The fact of this complete absence of inter-relation betwee density of population and the prevalence of plague is interesting, as showing how litt *à priori* judgments are to be trusted in matters relating to the influence of sanita surroundings upon the prevalence of plague.

Effect of absence of ventilation discussed.

350. *A priori* it would appear possible that an absence of ventilation may exe an effect upon the spread of plague by reducing the vitality of the inhabitants wh live in a vitiated atmosphere. It is, in other words, conceivable (though we have n data for arriving at a decision on this question) that persons living in a vitiate atmosphere may be rendered more susceptible to contract the infection. If they a not directly rendered susceptible to the infection of plague, they may possibly b rendered susceptible to pulmonary diseases, which would increase their liability t contract certain forms of plague. Further, absence of ventilation and a vitiate atmosphere may possibly exert an influence upon the plague bacillus when it h escaped from the infected organism. Inasmuch as the absence of ventilation in a overcrowded house implies the existence of damp, that absence of ventilation ma on the one hand, postpone the destruction by desiccation of the plague bacillus, an on the other hand, may interfere with the carrying away of the infective materia which would, in the case of a thoroughly ventilated room, be effected by currents Para. 162 of this Report. air as soon as the infective material had dried up and become pulverised. Furthe in view of the facts elicited by Dr. Marsh, with regard to the effect of carbonic ac upon the growth of the plague bacillus, it is conceivable that an excess of carbon acid in the atmosphere, such as would be met with in an ill-ventilated or ove crowded house, might be a factor favourable to the saprophytic growth of the plag bacillus. In view of these considerations, it would seem possible that the absen of ventilation may, to some extent, be favourable to the spread of plague.

Even if definite data were available to establish the greater spread of plague in i ventilated than in well-ventilated houses, it would have to be borne in mind that vitiated atmosphere is, as already indicated above, practically always associated wi conditions of overcrowding, which, even in spite of the fact that the statistical eviden in the case of Bombay points in the contrary direction, may nevertheless exert influence on the spread of plague. It would, therefore, be futile to ask from e perience for actual proof or refutation of the conclusion that the absence of ventilatio is favourable to the spread of plague. Proof, however, is adducible to show th freedom from plague is not always associated with the existence of good ventilatio To show that plague may occur in houses whose atmosphere is quite untainted, will suffice to refer to the fact that plague cases broke out in well-ventilated a 11,095. airy houses in Hardwar, Karachi, Ahmednagar, and elsewhere, and that an epidem 24,228. broke out in the lofty and, from every sanitary point of view, unimpeachab buildings of the Byculla Jail in Bombay. With regard to the last, it is to be remark that while the epidemic was not a severe one, the incidence of plague was, relative 25,682. speaking, greater than in many of the most overcrowded chawls in the City. Lieu Colonel Weir, I.M.S., was so struck with this fact that in his Report he calls attenti to it in the following terms :—"Here we have a population, all living under the sa excellent sanitary conditions and under the same regulations, and in a space mo beautifully clean than can be found in city life anywhere, suffering as the people many of the most overcrowded parts of the City."

Again, even though we may take it as established that plague does not show t same tendency to spread in evacuation camps as it does in unevacuated villages, is manifest that we cannot determine how much of the advantage which accrues fr moving out into camp is due to the breathing of an unvitiated atmosphere; h much is due to the spacing out of the people in camp, so that they are brou into less intimate contact than in their homes with infected persons; how much ag to the more rapid desiccation of infective material in camp; and, lastly, how mu to the removal of people from the sources of infection in their houses.

Whatever may be the importance which attaches in the case of camps to factor of ventilation, we have little doubt that the importance of this factor greatly exaggerated by those who urge that plague cannot occur in well-ventilat airy, surroundings, and endeavour to trace back every outbreak of plague t defect of ventilation.

Effect of damp discussed.

351. With regard to the closely related question of the influence of damp on spread of plague, very little in the way of definite information is available. It conceivable that damp may, either by conducing to pulmonary diseases, or in so

ıer way, reduce the resisting power of the human organism, and thus favour the ead of plague. While, however, this point is doubtful, there can be no doubt that amp atmosphere in houses would, as we have already seen in discussing the effects meteorological conditions on the spread of plague, supply one of the conditions ich is favourable to a prolonged survival of the plague bacillus outside the animal ganism. But, as we have indicated in the section just referred to, in damp roundings influences might make themselves felt which, by allowing of the freer wth of saprophytic micro-organisms, would be hostile to the survival of the plague illus.

Para. 336 of this Report.

352. With regard to the effect of dark or ill-lit dwelling-rooms on man, there is no son to suppose that such an absence of light as comes into consideration in the e of Indian houses is by itself injurious to health. On the contrary, it is quite ural that, as a relief from the glare of the bright sunshine, dark rooms should in lia be congenial. Again, there is no evidence to show that the saprophytic growth the plague bacillus (supposing always such saprophytic growth actually to take ce in infected surroundings) would in any way be furthered by the absence of ıt. As will be seen, in discussing the effect of light in connexion with disinfection rations, we did not in our experiments obtain any evidence that diffused daylight retard the growth of the plague bacillus. It is, indeed, conceivable that the iod of survival of the bacillus after its escape from the body may possibly be longed when the bacillus is screened from the light, for the longest periods of vival which have been placed on record are periods observed in the case of plague illi which have been kept in the dark. There are, however, no comparative periments which tell us what is the maximum period of survival of the plague illus when exposed to the influence of diffused light.

Effect of ill-lit or dark rooms discussed

Para. 658 of this Report.

353. From the point of view of the sanitarian, the cow-dung and earth floors, ich are practically universal in India, are obviously very undesirable. Like a nge, they soak up all discharges, and, quite apart from the fact that they are iodically renewed with moist cow-dung, they are naturally very retentive of isture. Obviously also such floors, when they lie directly upon the soil, do not offer same obstacle to the burrowing of rats as floors laid with boards or tiles or ıent. The floors of native houses thus may quite well play an important part h in facilitating the introduction of infection by rats and in keeping the infection plague alive in a house. Again it is quite certain, as we shall see in the section of Report dealing with disinfection, that these floors render the destruction of the gue bacillus by chemical agencies much more difficult and uncertain than it would erwise be. We have, however, practically no data which would enable us to arrive a definite conclusion as to whether plague has actually been more rife in houses h cow-dung floors than in similar houses provided with cement or stone floors. e witness, Dr. Gibson, had, as he informed us, entered upon an investigation with iew to collecting definite information upon this question, but the information which was able to bring forward at the time of his examination was incomplete and onclusive.

Effect of cow-dung and earth floors discussed.

Section III. (4) (*b*) of Chapter VI. of this Report.

17,289–290.

354. When we look around for a justification of the opinion that plague is a filth ase, we are confronted with the fact that the dirt which accumulates in houses is ll probability not a good culture medium for the plague bacillus. Taking the fact t the plague bacillus grows with extreme difficulty in the presence of most sapro- tic micro-organisms, in conjunction with the fact that Dr. Marsh's observations e shown that there is an average of over 3,000,000 saprophytic bacteria in every m weight of dirt scraped from the floor of native houses, we can hardly find justifi- on for the belief that plague is a filth disease in the sense that the growth of its illus is favoured by the presence of dirt. If, therefore, dirt is contributory to the ad of plague, it can only be when, owing to its association with moisture, it plays art in retarding the destruction of the plague bacillus by desiccation. Except in far as dirt in a house may exert an effect in this direction, its presence would ear, from the point of view of the spread of plague, to be a matter of indifference, ss possibly as an indication of the absence of any efforts on the part of the abitants to purge their houses of infective material, supposing such to have been oduced. On the other hand, from the point of view of the sanitarian, a clean room nifestly may constitute as dangerous a nidus of infection as a dirty room, inasmuch he specific micro-organisms of the disease may effect a lodgment or remain in the n in spite of any amount of sweeping and cleaning. These *à priori* conclusions ear to be fully borne out by the teaching of experience. The "cleaning-up

Effect of dirt discussed.

18,862.

operations" which were undertaken not only in Bombay, but also in nearly every other town which has been attacked by plague, have not, so far as we are aware, done anything to divert plague from the cleaned and whitewashed houses.

Effect of fæcal contamination of houses and soil discussed. Para. 344 of this Report.

355. We pass now to the discussion of the question of the influence exerted on the spread of plague by the fæcal contamination of the houses and soil. This question which has assumed so large a measure of importance in the eyes of lay sanitarians may, it seems to us, be disposed of in a few words. Our sketch of the methods by which the night soil is removed in the rural districts generally and in Bombay City will have made it plain that, if the spread of plague were influenced to any important degree by fæcal contamination of the houses or the soil, it would have been much more severe in Bombay City than in the villages where the arrangements practically exclude the fæcal contamination of houses. The percentage, however, of the population attacked has been far greater in infected villages than in Bombay. Again, if contact with night soil was a frequent cause of plague, we should have expected to find an extremely high plague mortality among the sweepers in Bombay, who carry the night soil in open baskets on their heads; on the contrary, that mortality has, as we have already seen, been comparatively light. It has compared very favourably with the mortality from plague in many other classes of the community. It is very noteworthy that the conservancy arrangements, which exert so important an influence in other epidemic diseases, do not appear to exercise any very important influence upon the spread of plague.

Para. 306 of this Report.

Effect of sewage pollution and high level of subsoil water discussed.

356. No exact information has been furnished to us which either confirms or refutes the suggestion that the sewage pollution of the soil and the high level of the subsoil water are responsible for the persistence and severity of the plague in Bombay City but it is clear that plague may occur as severely and persist as long as in Bombay in places such, for instance, as the towns in the Thana district, where the conditions in respect of the inhabited sites, of sewage pollution, and of the height of the subsoil water, are much more favourable than those of Bombay, while the climatic conditions are generally similar.

Effect of wide streets and open spaces discussed.

357. In view of the fact that schemes involving the widening and re-construction of the streets have been set on foot directly with a view to checking the spread of plague we may briefly glance at the question as to whether wide streets and open spaces as such, and apart from the character of the houses, exercise an influence upon the spread of plague in their neighbourhood. *A priori*, this could hardly be expected. None the less, with a view to ascertaining whether any lessons had been learnt in Bombay with regard to this question, we made inquiries from Lieut.-Colonel Weir, I.M.S., the Health Officer for Bombay. Lieut.-Colonel Weir told us that plague had been quite as severe in the newly-opened wide thoroughfares of the Byculla district, and in particular in Morland Road, and in the houses which adjoined large blocks of unoccupied land, as in the houses situated in narrow streets.

25,700–15.

Question as to whether a combination of sanitary defects influences plague.

358. It will, on a review of everything that has been said above, be manifest that we have not been able to discover any clear evidence to connect plague with any one of the various conditions grouped together under the head of sanitary defects. Failing such evidence, it behoves us to see whether the spread of plague can be brought into relation with any combination of the above-mentioned sanitary defects. Opportunity for bringing this question to a definite test would appear to be afforded by the circumstance that in Bombay City a census was taken of the most insanitary houses with a view to the compulsory vacation of those which appeared unfit for human habitation. Mr. Hankin, in the paper already referred to, considered such of the figures of this census as were available at the time of writing. He has plotted out the statistical data thus placed at his disposal in a graphic form, figures representing the number of plague cases in different quarters of the town being placed alongside of figures representing the number of houses condemned as unfit for human habitation in each of these quarters. It is clearly seen, on consideration of Mr. Hankin's diagrams that the severity of plague in the separate districts has not stood in any relation with the number of houses which were condemned as unfit for human habitation in those districts. It appears to us that these comparisons of Mr. Hankin's are, for several reasons, fallacious. In the first place, the number of houses given by him as having been condemned as unfit for human habitation amounts in all to the very small number of 1,903. Further, these 1,903 houses being for the most part quite small houses,—

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in many cases partaking of the nature of cabins or huts or annexes to larger houses,—the number of their inhabitants must have been quite insignificant, and the rate of mortality among these cannot seriously have influenced the mortality of the quarters where they lived. Moreover, there appears to be a serious doubt whether the number of houses condemned was actually as high as that given by Mr. Hankin, for in figures furnished to us which purport to represent the number of houses condemned in a period longer than that specified by Mr. Hankin, we find that the total number of houses condemned is set down as 665. Many other objections suggest themselves to using the number of houses condemned in a district as a criterion of the sanitary conditions of that district as a whole. We may give a few examples. First, the conclusion that the average house in a district is insanitary does not follow from the fact that some houses in that district have been condemned as insanitary; as a matter of fact, as already stated, many of the buildings condemned in Bombay were mere annexes of large and more sanitary houses. Again we understand,—though Mr. Hankin appears to be under a different impression,—that the particular census of insanitary houses, on the results of which he based his arguments, was in different districts committed to different officers, and that the number of houses condemned as insanitary in a district varied to a large extent with the zeal and the standard of sanitary requirements which was set before himself by the individual census officer entrusted with the duty. Lastly—and this is the most serious objection, inasmuch as the figures for plague deaths relate in part to a period subsequent to the condemnation and vacation of houses unfit for human habitation—these figures for districts in which a large number of houses were condemned may be treated as figures applying to districts where sanitary conditions may, by the elimination of the worst houses, have been raised to a higher general average. For all the reasons stated above we must conclude that the figures adduced by Mr. Hankin do not supply either proof or disproof of an absence of inter-relation between defective sanitary conditions and the severity of plague. 25,666.

359. On consideration of everything that has been said in connexion with the influence of sanitary surroundings, both within and without the house, on the spread of plague, it will be clear that we have been unable to find anything in the nature of statistical evidence or in the nature of inference from scientific observations to establish the proposition that any of the sanitary defects referred to, or any combination of them, exercises any marked favouring influence on the spread of plague.

Conclusion that no statistical or bacteriological evidence available showing that sanitary defects influence spread of plague.

360. We may judge of the question also by inquiring into the general history of recent plague epidemics. We may note, in the first place, that Calcutta and Mysore, cities whose sanitary conditions are as bad as, if not worse than, those of Bombay, have suffered much less severely from plague, and this in spite of the fact that the measures taken in these cities to check the spread of the disease have been much less strenuous. Oporto again furnishes another example of a town in which, in spite of the existence of all kinds of insanitary conditions, plague showed little tendency to spread. The sanitary condition of the infected quarter near the river wharfs may be judged of from the following passage taken from the report of Drs. Calmette and Salimbeni, who were sent by the Pasteur Institute in Paris as a Commission to investigate the plague in Oporto. "The Fonte Taurina and Customs quarters, where the epidemic began, are made up of blocks or islands of tumble down houses, divided by narrow lanes, in the form of tortuous staircases, unprovided with either sewers or gutters. Here and there, at irregular intervals, one meets with receptacles containing dirty rubbish of all sorts, which has accumulated for centuries. The sun does not penetrate into these lanes, and the air has a nauseating odour, and is mixed with acrid smoke. Nearly all these miserable dwellings consist of three or four rooms placed one on the top of the other, each occupied by entire families. Sometimes the ground floor provides asylum for animals—pigs, goats, and rabbits,—these being mixed up indiscriminately with the human inhabitants in an area of a few square metres. It is impossible to picture the squalor of these people. One thing only," say these observers, voicing the feeling of almost every one who for the first time observes the course of plague in the midst of insanitary surroundings, "can excite wonder—that plague did not make greater ravages here." Not only did plague not spread in any alarming manner in Oporto, but the disease appears to have quite died out in the surroundings which have been described, and this in spite of the fact that the sick were not compulsorily removed to hospital. Again, the same lesson seems to be taught by the history of plague in the City of Canton and in Hong Kong; the epidemic in Canton declining

History of recent epidemics affords no proof that sanitary defects aggravate plague.

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at the same time as the epidemic in Hong Kong, although in the one case no measures, and in the other case very strenuous measures, were taken for the improvement of general sanitation. We may note further, in the case of Poona and Bombay, that in spite of all the cleaning and opening up of houses and other sanitary improvements which accompanied the first and second epidemics, the third epidemic was much severer than either of the previous ones. The history of particular epidemics in recent times, therefore, does not appear to afford proof that insanitary conditions exercise any marked favouring influence on the spread of plague.

Opinions of plague officers regarding influence of sanitary conditions on spread of plague

361. The only kind of evidence which still remains for us to examine is found in the opinions of plague officers who have had actual practical experience of plague as seen in the houses of the native population. We have, therefore, carefully gone through the evidence, and after so far as possible excluding such opinions as seem to us to have been founded upon theoretical considerations rather than on practical experience, we subjoin the opinions which seem to us to be most deserving of consideration in connexion with this question.

7836. Surgeon-General Harvey, Director-General of the Indian Medical Department, who had opportunities of seeing plague in many infected centres, told us that "as seed cannot grow without proper soil, so this plague seed cannot flourish unless it finds an environment suited to it, and that environment is made by all these insanitary conditions. It is a disease of filth, a disease of dirt, and a disease of poverty. The whole history of plague, so far as we know it, shows that to a great extent."

21,323. Lieut.-Colonel Fawcett, R.A.M.C., who had great practical experience of plague in Poona, said, "It is unfortunately an axiom that plague in its epidemic form is only to be found where dirt, darkness, and overcrowding prevail; in a word, in those places where sanitation is unknown, impossible, or neglected . . . . the bacillus which causes it, though capable of multiplication with extraordinary rapidity in a suitable nidus, has neither the power of resistance, locomotion, or spore formation, and dies at once in the light of day."

23,180–6. Major Anderson, I.M.S., apparently attributed the severity of plague in the Dharwar district to the filthy condition of the houses and their defective lighting and ventilation, and the custom of keeping cattle in them.

10,377. Captain James, I.M.S., who was in medical charge of the plague operations in the Punjab, expressed the opinion that "light and air seem to be inimical to the spread of plague, while dark and badly ventilated places are conducive to it."

11,228. Mr. Giles, the Commissioner in Sind, who had great experience in Karachi, though he had noticed plague occurring in houses of a superior character, said that "there was not the faintest doubt that the ill-ventilated houses where the people were crowded got the plague far the worst. It was quite apparent." He told us also that want of ventilation, dirt, and overcrowding, were in the order given responsible for the extension and virulence of the disease.

11,281.

11,851. Mr. Kaka, Health Officer of Karachi, thought that "where sanitary conditions were good, plague does not assume a very virulent form."

Dr. Clemow, who based his opinion on his experience of one district in Calcutta, and on his observation of the dirty condition of the patients whom he had seen in the Parel Hospital at Bombay, thought that there was "evidence to show that insanitary conditions favour the spread of plague."

7179–80.

16,971–86. Mr. Stewart, I.C.S., who had experience both in Bombay and in Nasik, while unable to say that plague, generally speaking, occurred in the worst houses in a district, remarked that the houses in which cases occurred were defective in some way, either in respect of light, or of ventilation, or of overcrowding.

II. 361. Surgeon-General Planck thought that the prevalence of plague in Kumaun was to a large extent due to the insanitary conditions of the houses and in particular to the custom of keeping cattle in the lower storeys, and of using the sleeping rooms as granaries, and he remarked that the two periods in which energetic steps were taken, under the Mahamari rules, to remedy these defects, were periods which were unaccompanied by any violent outbreaks.

Major Reade, R.A.M.C., who was in charge of the plague operations in Poona, and who had observed plague, not only in India, but also in Hong Kong, when asked whether better hygienic conditions were operative in diminishing the amount of plague when people were removed into camp, expressed himself in the following guarded

21,718–9. way :—"It is a presumption that, if you take people from ill-ventilated houses and

place them in good airy houses, the hygienic conditions are better, and one can say that, under better hygienic conditions, the general health will improve." But this witness was more definite as to the influence of the cow-dung and earth floors in aiding the propagation of plague. He thought they certainly contributed to the spread of the disease. 21,725.

The Bombay Scientific Committee, consisting of Major Lyons, I.M.S., Major Manser, I.M.S., Captain Childe, I.M.S., Dr. Surveyor, Mr. Haffkine, and Mr. Hankin, appointed to make investigation into the nature of plague and the means of its spread, in its Report, states: "All observers up to the present time have connected the occurrence and diffusion of the disease with want and filth, and there seems to be no doubt that these factors do favour it, in so far as they lower the general health and diminish natural immunity. But it will be seen that filth *per se* has but little influence, from the fact that there occurred in the House of Correction, Byculla, where cleanliness is brought as nearly to perfection as is attainable, an outbreak which exceeded in severity that in any of the filthy chawls and tenements around." I. 378.

Lieut.-Colonel Weir, I.M.S., Health Officer of Bombay City, said, "I think the view of associating this disease with dirt, in the sense of refuse, is utterly wrong. As far as I understand, when once the microbe gets into dirt, it becomes harmless. My reason for thinking that is that the classes here who handle dirt and walk in dirt have suffered less than classes who avoid dirt most. I do not see that there can be any other explanation—that microbes when they get into dirt are destroyed by others." 1003-4.

Dr. Nield Cook, who, as Health Officer of Calcutta, had exceptional opportunities for observing the distribution of plague in that City, said, "Some of the cases were in very good houses, in decent sanitary streets; certainly a number of cases were in a poorer class of houses . . . . I think it is very much a matter of where the infection gets carried, and I have no doubt it has a greater chance of being carried in poorer localities, where people mix about in closer contact and are generally crowded together; but I do not think there is sufficient evidence that the contagium morbi prefers foul places to healthy places." 6659-60.

XI.(E.)(5.) *Conclusions regarding the Influence of Insanitary Conditions on Plague.*

Conclusion regarding influence of insanitary conditions on plague.

362. The opinions of those witnesses who hold that plague occurs most frequently in dark, ill-ventilated, and overcrowded, houses, and of those who hold with Dr. Nield Cook, that the disease breaks out wherever the infection may be carried, whether the sanitary conditions be good or bad, are not, it seems to us, in necessary contradiction. They can be harmonized if we assume as we must, even in face of the fact that the plague was severest in the less crowded districts of Bombay, that the factor which more than any other is likely to determine the introduction and spread of plague in a house is the crowding together of a number of inhabitants in that house. While the more frequent occurrence of plague in the crowded, dark, ill-ventilated, houses which serve as dwelling-places for the very poor would be easily explained in accordance with this view, we are very far from suggesting that in certain of these houses there may not be some special insanitary condition which may be contributory to the spread or persistence of plague.

Possibility of effecting improvements in insanitary conditions in India.

363. In the absence of anything in the form of definite knowledge on the question as to what may be the special nature of the existing defect which comes into consideration here, it would seem to us that the great practical question which presents itself for solution is the question of the prevention of overcrowding. In approaching this subject, we must recognise that much of the overcrowding which prevails in India generally is directly attributable to poverty. The overcrowding which is directly attributable to this cause would appear to be irremediable, except in so far as the economist may be able to devise means for increasing the general wealth. But with regard to the overcrowding which actually exists in the chawls and lodging-houses of Bombay, it appears to us that much of this is due, not so much to poverty, as to the fact that the influx of persons seeking work in the mills has, in the absence of any legal restraint on the number of persons who may be accommodated in lodging-houses, afforded unscrupulous landlords an opportunity of overcrowding their houses to the utmost. It would seem possible by a properly devised Lodging House Act to relieve some of this overcrowding and such insanitary conditions as are associated with it. It might be possible, as has been found possible for instance in the case of Hardwar and Simla, to regulate the number of people who are admitted into each room on the principle of allotting to each a certain minimum of cubic space. It would also seem

possible to enact that there should be a certain provision for ventilation, and with it for the admission of light into the houses. The enforcement, however, of general structural improvements in town or country in the case of existing private houses would appear to be out of the question. It would be impraticable to enter on a scheme for rebuilding all India and for bringing it into conformity with modern sanitary ideas, by enacting that every room should be provided with windows, and that cemented or paved floors should be substituted for earth and cow-dung floors. Even if it were possible to carry out such a scheme of reform as this—and it has already been seen that reforms involving large rebuilding schemes have in particular places been carried through in connexion with measures taken against plague—it would not be possible without a legion of inspectors to ensure that the people did carry out the improvements required and that there was no subsequent backsliding. How necessary it would be to attend to this last point will appear from the fact that, where measures for improving the ventilation of the houses were carried through in connexion with plague measures, the people for the most part closed up the ventilation openings at the earliest opportunity.

Directions in which improvement in insanitary conditions might be effected.

Para. 750 of this Report.

364. While thus we are of opinion that measures of sanitary reform in the existing houses cannot be introduced, or cannot, at any rate, be introduced on a large scale, we think that certain important steps in the direction of sanitary reform could be taken in the case of the large towns. Not only could, as we have above suggested, a system of licensing and regulating lodging-houses be introduced, but, as we have elsewhere suggested, it would be possible to give the Medical Officer of Health for each large town a right of vetoing the erection of grossly insanitary buildings within the precincts of his Municipality. Along with this power of vetoing the erection of new insanitary houses ought, we think, to go the power of vetoing the rebuilding in an insanitary manner of the houses which now exist. By taking these steps we think that a gradual revolution in the sanitary conditions in Indian towns might be brought about without undue interference with existing rights or customs.

Chapter VII. of this Report.

It would be travelling beyond our province to discuss the question of possible improvements of sanitary conditions by the institution of subsoil drainage or by the adoption of better conservancy arrangements. Recommendations in connexion with these questions would, it seems to us, fall more suitably within the province of the Sanitary Department, which, as will be seen elsewhere, we think ought to be immediately established in India.

The President disagrees with many of above statements and conclusions regarding influence of insanitary conditions on plague.

365. As the President is unable to agree with many of the statements and opinions contained in this section, (XI. (E.)), he has separately expressed his personal views in a Memorandum printed as Appendix No. III. to this Report.

XI.(F.) *Question of the Existence of an Endemic Focus of Plague in the Foot-hills of the Himalayas.*

History of the earlier investigations into the occurrence and nature of Mahamari in the Hills. Appendix XXV.

366. An interesting side light on the problems connected with plague may be obtained by a consideration of the question of the existence within the confines of British India of an endemic plague focus in the Kumaun and Garhwal districts, which are situated on the lower spurs of the Himalayas. The occurrence in this region of a fatal form of disease of a type unfamiliar elsewhere in India was, as we learn from Surgeon-General Planck's valuable Report on the question (printed in Volume II. of our Proceedings) brought to the knowledge of the Government as long ago as 1836. According to the reports which reached the Commissioner of Kumaun of that day, the disease in question originated in 1823 at Kidarnath—an important place of pilgrimage—in the person of a priest of the shrine. In 1834 to 1835, when the disease was first made the subject of a report, it was (we quote from Dr. Planck) noted upon by the Revenue Officers as follows:—"The appearance of the disease in a village had been observed to be preceded by mortality amongst the rats of the village. The onset of the disease in man is described as sudden, attended with fever, great thirst, and an eruption of buboes or swellings under the arms and behind the knees, with a desire to eat bitter things. After the appearance of the swellings choleraic symptoms appeared, and the disease generally terminated fatally in the space of two, three, or four, days, those who recovered being very much reduced for a long period." 633 deaths occurred in Garhwal from this disease in the epidemic of 1834 to 1835.

Appendix XXV., para. 95

Appendix XXV., paras. 96, 97.

In 1849 the statement that the disease was probably identical with Asiatic plague appears in a Report by Mr. Strachey, the Assistant Commissioner, in which attention is called to the ravages of the disease and to the deplorable condition of the people who had fled out of their houses to escape contagion. In consequence of these

presentations, Dr. Renny was deputed to make inquiry into the disease. Dr. Renny, ho did not succeed in seeing more than a few cases of the malady, formed the pinion that Mahamari, *i.e.*, "the great disease"—the disease had by this time become ficially denoted by this name, although it is locally known only as Gola Rog or hutkia Rog, both meaning "the bubo disease"—was not to be identified with Asiatic ague. He regarded it as "a malignant form of typhus, accompanied by external andular tumours, a very fatal disease, generally terminating in death within three four days after attack." The recorded mortality from the outbreak of 1849 to 350 was 103 deaths in nine villages. The measures recommended by Dr. Renny were ie disinfection of the houses by fire, the lime-washing of all houses (inside and itside), the immediate removal of the bodies of those who had died of the disease, id the removal of the manure heaps from the neighbourhood of the dwellings. urther inquiries into the nature of the disease were made by Dr. Pearson and r. Francis conjointly, in the year 1852–3. These gentlemen, on the basis of "chemical udy" and a certain number of post mortem examinations, arrived at the conclusion at the disease was plague. They recommended as an urgent measure the removal of e cattle from the lower storeys of the houses into separate sheds. Again, in 1877, e epidemiology of the disease and its symptoms were very fully and ably studied by irgeon-General Planck, from whose Report most of the above statements have been dled. Colonel Richardson, I.M.S., Colonel Hutcheson, I.M.S., and Lieut.-Colonel iompson, I.M.S., who, each in his turn, succeeded to the Sanitary Commissionership the North-West Provinces, previously held by Surgeon-General Planck, made further vestigations into the outbreaks of plague which have occurred periodically in Kumaun d Garhwal. As the result of these investigations, a sufficient body of clinical and idemiological facts is at our disposal to justify the conclusion that the disease, iich has long been known to occur in the Hill country, is, in reality, plague.

Appendix XXV., para. 98.

App. XXV. para. 103.

App. XXV. para. 106.

App. XXV.(1), Section V.

367. The more important circumstances which place the identity of the Hill form the disease with genuine plague beyond doubt, are, first, those relating to the period incubation. As the result of observations made in the case of persons who ematurely returned to revisit or reinhabit their evacuated houses, and in the case persons who contracted the disease in their villages after exposure to infection wly imported from outside, it was determined that the incubation period of ahamari may be as short as 24 hours, and that it may, in some cases, extend to five ys. The following cases are of interest in this connexion. A woman who lived in e healthy village of Kheti slept one night in the infected village of Kirsal and was acked by plague next morning while walking home. She managed to reach her home, d next day a child living there was attacked. In Balt "all remained well" among e population who were camping outside their village on account of an outbreak of ahamari "until, a great fall of snow occurring on the 14th of January, the people urned to their homes, and during the succeeding five days four more deaths curred in the houses of the terrace previously infected." On the 15th January, e which had been imported from an infected village was husked in the village of inola: on the 18th, 19th, and 21st January cases of plague occurred in the house which the rice was husked. These facts are, as will be seen on reference to the tion of our Report where we deal with the incubation period of plague, in close reement with those that have come to light in connexion with the disease as now evalent in India.

Facts elicited by investigations referred to above place it beyond doubt that mahamari is identical with plague. (1) Incubation period. II. 362 (3).

II. 338 (111).

II. 342(130).

Section V. of this Chapter.

The sudden onset of the symptoms; the development of buboes either early or, in presumably septicæmic cases, in the latter stages of the disease; the occasional currence of carbuncles; the extreme fatality of the disease; the frequent cerebral nplications, evincing themselves either in the form of passive hallucinations or in an controllable tendency to wander about, which often caused the patients to leave ir houses and to throw themselves down steep places: these all point to the ntity of mahamari, as seen in the Hills, with true plague.

(2.) Clinical symptoms. II. 359 (242) II. 369(6c).

Similarly, the epidemiological data point in the same direction. The fact that a rtality among rats* and mice in some cases precedes the outbreak of the disease ong men; the fact that the disease is very rarely contracted by direct contact with sick, while it spreads rapidly among the people who remain in or revisit their ected houses; the fact that the houses and goods retain the infection for long iods; and, in association with this, the fact that a long interval may occur between importation of the infection and the subsequent spread of plague; and, perhaps,

(3.) Epidemiological data. II. 361 (254 and 259). II. 362 (4). 8832, 26,576, II. 360 (247–9), II. 362 (261), II. 369 (6c). II. 361 (254)

* It is noteworthy that the rat which occurs in the Himalayas, and which is spoken of as being infected by ue, is described by Surgeon-General Planck as being, not the strong "black (*sic*) Norway rat which lives ie sewers of Europe, but a more delicate grey species."

the fact that the disease shows a tendency to recur, as plague in Bombay and other
8827, 8995, II. 363 (6). places has done, in the colder months of the year, and to disappear in the summer
all these again point to the identity of mahamari with plague. In connexion with the
occasional occurrence of an interval between the importation of infection and the
occurrence of the disease, we may note that in the case of Nainoli, just referred to
II. 342 (130). above, an interval of over six weeks elapsed between the date of the importation o
the rice from the infected village and the outbreak of the disease. The following
II. 340 (120). cases also bear on the question. "In the month of November a boy of 15 went into
Balt village to fetch wheat for seed; he remained one night in Balt, sleeping in a
house in which a woman was ill of Gola disease. . . . . Fifteen days after h
came home again, his little sister was taken ill with continued fever and died afte
II. 352-3 (199). three days' illness." In the village of Baret, two suspicious deaths occurred in th
month of November; on the 22nd April, a definite case of bubonic plague occurred i
a second house in the village; in the first half of May two further cases occurred i
the house adjoining that in which the deaths had occurred in November; and, lastly
at the end of May, a second case occurred in the house which had been infected i
April. In none of the intervals which we have cited was any mortality among rat
noticed. In connexion with the influence of the seasons on the occurrence of mahamar
we have ascertained from Surgeon-General Planck's Report, that of the 40 villag
outbreaks which he investigated, 4 commenced in October; 7 in November; 7 i
December; 5 in January; 4 in February; 4 in March; 6 in April; 2 in May; non
in June and July; 1 in August; and none in September. We may, however, not
that the rule which seems to be deducible from these cases does not appear to b
absolute, for in the case of six village outbreaks, for which details were collected b
II. 366-8. Lieutenant Walton, I.M.S., and Lieutenant Douglas, I.M.S., four appeared in th
months of July, August, and September, and two only in the winter months.

Points in which plague, as seen in the Hills, appears to differ from plague as seen elsewhere in India.

368. A few minor points of difference between plague as seen in the Hills, an
plague as it has appeared in the plains, would appear to be deserving of notice. Th
first of these is the fact that in plague, as seen in the Hills, the non-bubonic for
of the disease appears to be much more frequent than the bubonic form. Out of 7
cases chosen from Dr. Planck's Report, from among those in connexion with whic
the necessary indications are supplied, we find that 56 were non-bubonic an
21 bubonic. The frequent mention of diarrhœa also deserves attention, as anoth
circumstance indicating that plague assumes a more virulent form in the Hil
Indeed, the extreme fatality of the disease in the Hills is almost unparalleled in th
plains. Out of 291 cases reported by Surgeon-General Planck, 277 ended fatal
and only 14 recovered, giving a case mortality of over 95 per cent. Lastly, it
remarkable that no symptoms are reported from the Hills from which the occurren
of the pneumonic form of plague could be inferred. Lieut.-Colonel Thomson, I.M.S
8833, 8955. speaks of pneumonia as being unknown in connexion with plague in the Hills.

Points unresolved by the inquiries of earlier investigators.

369. Several very important questions are unresolved by the information collect
by Surgeon-General Planck and his fellow observers with regard to plague as it occu
in the Hills. Firstly, inasmuch as the information in question was collected at
date anterior to the discovery of the plague bacillus, it necessarily leaves unresolv
the question of the bacteriological identity of mahamari and plague. Secondly,
leaves unresolved the question as to whether plague is really endemic in Garhw
and Kumaun, or whether it is periodically imported into these districts from beyo
the borders of the district. Thirdly, it leaves unresolved the question as to how t
infection of plague, supposing the disease to be endemic in the country, is kept ali
in the intervals between successive outbreaks. Fourthly, it leaves unresolved t
problem of the nature of "sanjar," an epidemic disease unassociated with bubo
which, when it appears in these districts, is not always easily discriminated from "g
rog"—true mahamari.

Preliminary steps taken with a view to the elucidation of the problems in question.

370. With a view to filling in, so far as possible, these lacunæ in our knowledge
plague as it occurs in the Kumaun Hills, we, shortly after our arrival in India, approach
the Government with a request that the services of Captain Leonard Rogers, I.M
(who was then in charge of the Bacteriological Laboratory at Muktesar, on t
outskirts of this region), might be made available for the bacteriological study of a
cases of epidemic disease which might be reported in the district.

Captain Rogers' investigation of an epidemic designated as an epidemic of "sanjar."

An opportunity for investigating the nature of an epidemic notified as san
soon presented itself. It may be remarked that the malady locally designated
this name is regarded by Lieut.-Colonel Thomson, I.M.S., as possibly standing
some connexion with plague, this opinion being supported by the observation that,
an instance which came under his notice, outbreaks respectively designated as outbre

gola rog and of sanjar were occurring at one and the same time in adjoining villages. 8825–6.
ptain Rogers, however, availing himself of the opportunity which offered itself for a 8894–5.
ticular study of the disease, had no difficulty in determining, from a consideration the clinical symptoms, that the particular epidemic of sanjar in question was an demic of typical recurrent fever. In the case of one patient, the correctness of diagnosis was confirmed by the fact that the spirillum of Obermeyer was detected the blood.

371. With a view to elucidating the bacteriology of mahamari, and with a view also obtaining more light on the general epidemiological problems connected with the ease than could be obtained by isolated inquiries into such cases of epidemic disease might, from time to time, be officially reported, we, before our departure from lia, suggested to the Government that important light might be thrown on the blem of the endemicity of plague in Kumaun and Garhwal by a more detailed uiry into the nature and distribution of the epidemic diseases prevailing in these tricts, and by special inquiry into the question as to whether the appearance of gue in these regions was or was not associated with a periodical importation of the ease from Tibet. We represented that Captain Walton and Lieutenant Douglas, h of the Indian Medical Service, who had been specially attached to us for the pose of assisting in our work, would be specially qualified to undertake the estigation.

Further steps taken towards the elucidation of the problems associated with mahamari.

372. Captain Walton and Lieutenant Douglas performed the duties which were igned to them in a very zealous manner. In view, however, of the fact that no es of mahamari, or of any other infective disease, except measles, were to be nd in the tract of country through which they passed, they were not in a position make any bacteriological observations upon mahamari. On the other hand, like all ent observers, they were able, by personal inquiries, to convince themselves that disease which breaks out periodically in Kumaun and Garhwal must be recognised genuine plague. With a view to determining whether there was any possibility he periodical outbreaks of plague in Kumaun being due to a periodical importation the infection from the trans-Himalayan country, Captain Walton and Lieutenant iglas proceeded to the foot of the passes which lead over into Tibet. They had re an opportunity of meeting certain Tibetan envoys who, according to custom, II. 369(*d*).
ve at this period of the year to satisfy themselves, before the opening of the le routes, of the absence of epidemic disease in British territory. These envoys, o appeared to be well informed with respect to the occurrence of plague in Kumaun Garhwal, asserted that no disease of this kind was known in Tibet. Further lence pointing clearly to the non-importation of the disease into British territory n Tibet was afforded by a study of the distribution of plague in Kumaun and hwal, which revealed that plague has in no case occurred in the villages which are e up to the Tibetan border. There can thus be no doubt as to the existence of enuine endemic focus of plague in the foot-hills of the Himalayas within British itory.

Results of Captain Walton and Lieutenant Douglas' inquiry. Plague truly endemic in the Hill; not periodically imported from Tibet. App. XXV.(I) in Vol. II. of these Proceedings.

373. The question immediately presents itself for solution as to how the infection plague is kept alive in the country in the intervals which occur between one break and another.

Possible explanations of the endemicity.

t seemed possible that the outbreaks might be accounted for by the continual irrence of sporadic cases, and by the distribution from village to village of the ction derived from these. With a view to obtaining light upon this question, tain Walton and Lieutenant Douglas, on their failure to find any indication of the tence of sporadic cases of plague in the tract of country traversed by them, ected data with regard to the length of the periods of freedom from plague II. 369–70(*e*).
ch intervene in the regions in question between successive outbreaks. A scrutiny he plague records in the office of the Commissioner of Kumaun revealed that, ough serious epidemics have occurred only at very prolonged intervals, it is eptional for a year to pass in which mahamari is not reported from some one other of the widely scattered villages in the districts of Kumaun and Garhwal. s fact, taken by itself, would manifestly be favourable to the view that the emicity of the disease in the regions in question may be imputable to the ribution of the infection from a succession of sporadic cases. But this view, even t is not rendered untenable by the absence of sporadic cases from the region ersed by Captain Walton and Lieutenant Douglas, must be definitely abandoned n the geographical distribution of the outbreaks is considered in the light of the culties of travel, the great distances between the villages which have been

Theory that it may be due to distribution of the infection from sporadic cases considered.

successively affected, and the absence of intercourse between these. Some factor, othe than the distribution of the infection from sporadic cases, must, therefore, be invoked to account for the endemicity of the disease.

Theory that intervals between epidemics may be bridged over by cases of pestis minor.

374. Another suggestion, which has been mooted in connexion with the explanation of the endemicity of plague in other places, is that the intervals between successiv outbreaks may possibly be bridged over by a succession of cases of pestis minor Although it is difficult, in view of the almost certainly non-infective character o ordinary cases of pestis minor, to suppose that the infection is kept alive by thi agency, Captain Walton and Lieutenant Douglas made a careful search for cases o mild bubonic disease. The results of their search were entirely negative.

II. 369 (6c).

Theory that endemicity may be due to the infective agent surviving for prolonged periods in a dormant condition.

II. 354 (209), II. 360 (252). 8827, II. 364 (18), II. 366 (III.). 26,578, II. 361 (259), II. 363 (9).

375. Lastly—and this theory would appear best to explain the facts—it is conceivabl that the endemicity of the disease may be due to the infective material lying dorman for prolonged periods in the soil or in the houses. Survival of the infective agen under such conditions would probably be favoured by the climatic conditions o Kumaun and Garhwal, and possibly also by the presence of damp in the dwelling arising from the stalling of cattle in the lower storeys and the storage of dam grain in the sleeping rooms. Again, the fact that certain villages have repeatedl been attacked by plague while many other villages have escaped; the fact that i the cases of villages where the disease has reappeared after a term of years, mortality among rats has often preceded mortality among men; the fact that ol and tumble-down houses (these being sometimes houses where there was a histor of previous infection) have been attacked in such villages more frequently than ne houses; and, lastly, the fact that the outbreaks have often commenced with wome or children who are unlikely to have come into contact with outside sources infection: all these appear to suggest that we may here be dealing with tr recrudescences, *i.e.*, with outbreaks which originate in infective material which h long lain dormant. An indication, which perhaps points in the same direction as t facts just adverted to, is to be found in the statement of Surgeon-General Planck, th the periods when the Mahamari rules were strictly enforced (these last being measur directed to the improvement of the sanitary conditions of the houses by the remov of the cattle into outhouses and sheds) were periods of comparative immunity fro plague; whereas the periods when these regulations were (owing to pressure circumstances) allowed to fall into desuetude were periods of considerable mortalit While sanitary improvements of the kind in question could hardly render the peop less susceptible to imported infection, they might manifestly exert an influence the length of time to which the infective germ might survive in the houses.

26,575, II. 360 (250).

II. 338 (104–5). II. 361 (256).

II. 377(2).

The President considers that it should not be overlooked that the Mahamari ru above referred to, also require that every room in each house shall "have a windo two feet by two feet opening, on the outer wall of the house, in the wall opposite the doorway for purposes of ventilation."

Facts relating to Kumaun appear to suggest possibility of true recrudescences in Plains.

376. Before bringing to a conclusion our remarks on this question, we may n that although, in India generally, because of the constant inter-communication betwe plague-infected and uninfected places, it has been impossible to establish that outbreak of plague has ever occurred apart from an importation of the infection, th are many facts in the distribution of plague in the last four years for which assumption that plague may recrudesce after lying dormant for prolonged perio would supply the simplest explanation. Such a recrudescence would fully explain history of particular infected places in which periods of freedom from plague h alternated with periods when the disease is epidemic, even when the infected area—for instance, the area infected in the Punjab—is isolated and is confined within lim which do not to any great extent vary from year to year; and such a recrudesce would furnish us, in the case of India as a whole, with the cause of the seaso periodicity of plague, which is marked by the sharp transitions in which periods wh there is apparently a general dying out of infection are succeeded by a quick general reappearance of the disease all over India.

XI.(G.) *Question as to whether the Infective Material of Plague must undergo a Prelimin Process of Adaptation or "Acclimatisation" before it can spread in any Locality.*

Theory that plague, before it can spread, has to be acclimatised to particular

377. We have already, in connexion with our discussion of the means by which pla has been carried from place to place in India, called attention to, and endeavoure account for the slow rate at which the disease has (in spite of the railway commu cation which exists) spread outwards from infected centres. We saw that this slow

of spread was in large part the result of the comparatively slight infectivity of ordinary cases of plague, and that it was no doubt also the result of the steps which were taken to prevent the importation of plague cases into uninfected places and to discover and deal with them when introduced. The possibility that an acclimatisation of the plague bacillus to the particular environment into which it was in each case introduced might possibly be an essential preliminary to the spread of plague was incidentally adverted to as another factor which might conceivably have been contributory, along with the other factors before mentioned, to the slow rate of progress which has characterised the spread of plague through India. This question was, as will be remembered, reserved for subsequent discussion. We take it up at this point, with the remark that the problem could not profitably be adjudicated upon before the facts and considerations which have been set forth in the sections immediately preceding this had been duly weighed.

environment into which it is introduced. Section VII. (5) of this Chapter.

378. Passing now to the observations which the theory of acclimatisation has been invoked to explain, these are succinctly stated by Sir Andrew Wingate. This witness observed, "The point is that the spread of infection appears to me to be the same in the country as you find it to be in a town, that is, very localised at first, and spreading from a given centre. It is precisely the same if you extend your glance over the whole of India. The Bombay Presidency is the infected area, while the rest of India remains practically free. It is very striking. In every map you will see huge areas —highly populated areas—without a single case, or perhaps one or two only over a whole district. That seems to me to indicate that the outflow from an infected area is probably more dangerous close to the centre of infection than at any distance from it—that as the outflow gets further away from the centre, so the danger seems to lessen. Take the case of a man, starting from an infected house: if he goes 10 miles in two hours, he appears to be more dangerous than if he has gone 20 miles in four hours. Or it may be that the local spread of infection is due to entry into an infected area. It may be a market town which is infected, and where you have a market town the people from all the villages round go into the infected area: and, therefore, you get this localised infection. Into Ahmedabad or Broach the exodus from Bombay, I presume, has been as constant and as great in the past three years, but it is only this year that North Gujarat has been attacked. Take the town of Broach. You will find that it was not until Ankleshwar village, within 10 or 12 miles of Broach, was attacked that Broach was infected. That seems to me to point to the fact that the danger to Broach was not the incoming of people from infected areas at a distance, but either the outgoing of the people from Broach to an infected place within their easy reach, or the entry of people from a neighbouring infected village into Broach. You may say that the people of Broach went and came also to and from Bombay. That is so, but there you introduce two fresh elements—the one of distance and exposure to sun and air when going back to Broach, and the other of the medical inspection and the disinfecting arrangements, whether they involve detention or merely inspection. The people who go from Broach to Bombay by rail, and go back, come under those restrictions; and, therefore, are to a certain extent not so liable to introduce infection as when the infected area approaches close to a town. And there is the further danger that it is by the people of the town visiting that infected area that plague is also carried. All I wish to point out is, that as the Commission will examine our maps they will almost invariably find that the infected places are grouped; that when they hear of a district with 30,000 deaths in it, and expect to find the whole district a mass of plague, it is nothing of the sort. The plague is absolutely localised in certain parts of the district. It is just the same when going into a plague-infected town; you will be taken to one or two infected streets, and it is there where you have the chief mortality."

Sir Andrew Wingate's summary of facts which are capable of explanation by theory of acclimatisation 26,537.

379. The view that this slow rate of progression is attributable to the time required for a preliminary process of acclimatisation to the local conditions was advanced by Major Anderson I.M.S., who had exceptional opportunities for observing the course of plague in the Bombay Presidency. He expressed the opinion that in the first instance, on its arrival in India, the disease was a coast disease which required some time to assimilate itself in such a manner as to secure a hold upon an inland place, whether town or village. "We learn," he said, "that many cases escaped from Bombay at the beginning of the epidemic, and even after railway inspection was put on, still a large number got through. For example, in Nasik, before indigenous plague occurred, we had 35 imported cases. Those 35 imported cases did, I think, no harm." He also observed, "One other point which struck me concerning the outbreak in the

Acclimatisation theory put forward by Major Anderson, I.M.S. 23,318–29.

23,319.

23,323.

Thana district was that no indigenous case occurred in any town or village more than five miles from tidal water. The disease, when it first appeared in Bombay, was undoubtedly a coast disease, and required a high subsoil water for its development; a town with a good water supply and no drainage suffering more than an equally dirty town with a deficient water supply." It was his opinion that since the disease had spread about in the interior, it more easily secures a hold upon new places, provided those places be situated within a certain tract of country. He added, "Places in the Deccan are much more easily infected now. Nasik, I understand, this year has been infected from one single imported case, whereas the previous year it took at least 40 imported cases to infect the town." We may remark that the particular case of Nasik does not seem to lend any real support to Major Anderson's case, inasmuch as it appears that 26 of the 36 imported cases which were detected in Nasik before plague became indigenous in October 1897 were cases which had been imported from the neighbouring town of Igatpuri. Quite apart from any question of a superior infectivity of cases of plague imported from neighbouring localities, the probabilities were manifestly greatly in favour of Nasik becoming infected from Igatpuri. Major Anderson, however, came to the conclusion that the risk of infection spreading from an imported case was associated, not so much with the fact of the patient suffering from an eminently infectious form of plague or not, as with the fact of his coming from a locality where the conditions were similar to those of the place into which he introduced the infection. Thus, he considered that an imported case from Bombay would not be so likely to affect Nasik town as a case imported from the Nasik district itself.

23,328.

Resolution of the Government of Bombay, No. 3941 P., 17th June 1899.

Observations by Mr. Lord and Mr. Hudson regarding acclimatisation of plague bacillus in Ahmednagar district. 18,998–19,005.

380. Mr. Lord, who was employed in the Ahmednagar district, had formed an opinion quite similar to that of Major Anderson. He had, on mentally reviewing the origin of plague outbreaks in the district in question, noticed that cases imported from Poona appeared to have taken root much more easily in Ahmednagar than imported cases from Bombay. He expressed his belief that every case imported from Poona or Sirur into the Ahmednagar district led to an outbreak. With reference to this circumstance he observed: "My private opinion is that the bacillus from below the Ghats had got to be acclimatised in a damp place, because Poona is a damp place and so is Igatpuri, more or less, and then Sirur was a little drier, and so the infection was brought gradually to a place in a condition in which it could live. I believe the same thing has been noticed in the Broach district, because cases taken straight from Bombay to Broach did not seem to spread. But when cases were brought into Broach from Ankleshwar, which is just south of Broach, the plague spread at once. I believe there had been several imported cases from Bombay direct into Broach which did not spread."

24,110.

It is to be noted, in connexion with the opinion formed by Mr. Lord, that Mr. Hudson who had also been employed in plague operations in the Ahmednagar district, had not noticed any difference in the infectivity of plague cases imported from neighbouring places, as compared with the infectivity of plague cases imported from a greater distance.

Facts relating to epidemics in Rajputana and Hyderabad State that support the acclimatisation theory.

381. In addition to the observations which have been cited above, there are also other observations which would seem to lend support to the theory of acclimatisation. Among these is the fact that, though numbers of residents of Rajputana fled to their homes on each occasion upon which plague became severe in Bombay, only one small outbreak of plague has occurred in that extensive tract of country. Again, in spite of the fact that many thousands of people dispersed over the country when Sholapur was evacuated, as many as 5,000 of these making their way into the town of Gulberga in the Hyderabad State, the furthest point to which the infection is known to have been carried was not more than 60 or 70 miles from Sholapur.

Instances of the occurrence of plague outbreaks apart from any preliminary acclimatisation of plague bacillus.

382. Observations such as these would appear to conform with the view that the plague bacillus, when carried into parts of the country remote from the original source of infection, may find in the change of environment an influence unfavourable to its survival. The facts, however, do not point in a uniform manner in this direction. Isolated instances pointing in a contrary direction are on record. We may instance the cases of Palanpur, of the village of Khandraoni in the Gwalior State, of Teu in the Sirohi State, and, further, the case of the village of Sangwi and two adjoining villages in the Satara district, where plague epidemics broke out immediately after an imported case had arrived from a distant part of the country. Here, though we may explain the fact to ourselves by assuming that the local conditions in the place in which the infection was imported may, at the date of importation, have been essentially

22,093.

similar to those of the infected centre from which the infection was imported, it would seem certain that no preliminary adaptation of the bacillus can have been required.

Bacteriological observations bearing on acclimatisation theory

383. Before dealing further with the epidemiological observations which may come into consideration in connexion with this question, we shall perhaps do well to turn and inquire whether any bacteriological facts have come to light which would lend any countenance to the theory of acclimatisation espoused by Major Anderson and Mr. Lord. A body of bacteriological facts, which may be brought into relation with the problem which presented itself to these observers, is set forth in a recent paper by Danyz, which deals with the application of bacteriological methods to the destruction of rats. With a view to the practical exploitation of such methods, Danyz addressed himself to the task of modifying a particular species of coli bacillus which was pathogenic for rats, in such a manner as to obtain a culture which should produce a fatal septicæmic infection in these animals when administered to them in food. After having determined, in a series of preliminary experiments, that this object could not be achieved by making a series of passages in which the bacillus was inoculated directly into the rat organism, and having further determined that the cause of failure was, here, the fact that the increased capacity for growing in the organism which was in the course of these hypodermic passages acquired by the bacillus did not go hand in hand with a capacity for maintaining its vitality in the alimentary canal, Danyz preceeded to apply a twofold process of selection. He selected, in the first place, from among the bacteria which produced fatal results when hypodermically inoculated, those varieties which succeeded in adapting themselves to conditions which resemble, to some extent, those found in the alimentary canal. From among these were again selected those varieties which manifested the greatest power of growing in the interior of the organism proper. By a judicious alternation of these two main principles of selection, Danyz obtained a micro-organism which possessed at one and the same time an increased capacity for growing in the organism proper and an increased capacity for surviving in the alimentary canal. By means of food infected with this bacillus, Danyz succeeded—it is beside our present purpose that many others who have been provided with the Danyz bacillus have since failed—in producing a fatal septicæmic disease among the rats in a section of the Paris sewers, which was specially shut off for the purposes of this experiment.

Annales de l'Institut Pasteur, April 1899.

The general principle underlying the procedure adopted by Danyz would appear to have a very great epidemiological importance. It would seem, in particular, to have a great importance in connexion with the epidemiology of plague, since we may assume that the spread of this disease necessarily involves, on the one hand, a capacity on the part of the specific bacteria for cultivating themselves in the organism, and, on the other hand, a capacity on the part of those bacteria for surviving in a fully virulent condition in the particular environment in which they find themselves after they have escaped from the infected organism. The possession of such entirely different sets of attributes is obviously intelligible only on the basis of a dual process of adaptation on the part of the plague bacillus, an adaptation, on the one hand, of the plague bacillus to growth within the human organism, and an adaptation, on the other hand, of the bacillus to survival in a fully virulent condition in the particular external environment of the infected locality. Now, since this external environment varies from place to place, and from one season of the year to another, it is obviously conceivable that one of the essential preliminaries of an epidemic outbreak of plague in any place may be the adaptation, or "acclimatisation" of the bacillus to the external conditions obtaining for the moment in that particular place.

Epidemiological observations which are in conformity with the acclimatisation theory.

384. Having gathered, from a general biological consideration of the question, the general bearings of the "acclimatisation theory," we may come back once more to the question of the conformity of the theory with the epidemiological observations which have been made in India in connexion with plague. In this connexion we may advert to the following points:—

(1.) The slow rate at which plague has spread through India, so far as it has not been conditioned by the factors considered in a previous section of this Chapter, would be satisfactorily accounted for by the "acclimatisation theory."

(2.) The general circumstance that in infected places, almost without exception, epidemics of plague have alternated with periods of freedom, or relative freedom, from plague, would conform with the theory, inasmuch as it must be assumed that the external conditions may at one time be favourable, and, at another time, unfavourable, to the survival of the bacillus in a virulent form outside the human organism.

The seasonal variation of plague would find its explanation in this circumstance. In particular the fact that the plague epidemic which occurs in the latter part of the cold

weather dies out in the altered meteorological conditions which are associated with the advent of the very hot weather (in Northern India generally), or with the advent of the rains (in Bombay and Southern India generally), suggests that a plague bacillus which is adapted for surviving in a particular external environment under the conditions which obtain in the cold weather, is not adapted for surviving in that external environment in the altered conditions of the extreme hot weather, or the rains. Similarly, the fact that the curve of plague mortality rises to a second maximum in the latter part of the rains and goes down again when the cold dry weather supervenes, seems to suggest that a plague bacillus can indeed adapt itself to the external conditions which obtain in the rains, but cannot adapt itself to this set of conditions without forfeiting the power of flourishing outside the organism under the external conditions which are associated with the cold weather.

(3.) Again, the circumstance that plague has in some places not spread to any serious extent, while in others it has been severely epidemic, though at the same time it has manifested itself in both in an equally virulent and fatal form,—Calcutta and Bombay may be usefully contrasted from this point of view,—seems to suggest that in the external conditions there may be something which, in the one case is favourable, and in the other unfavourable, to the survival of the plague bacillus outside the organism. We may remark, in passing, that while in India we made some preliminary investigations, which did not lead to any positive results, to determine whether there was any marked difference in the bacterial flora of the soil of the two cities we have just referred to.

(4.) The circumstance that in Bombay, Poona, and certain other places, the later epidemics have exceeded the earlier ones in severity, seems to suggest that a process of gradual acclimatisation of the bacillus to the external conditions of these places may have been in progress. In suggesting that this interpretation may be placed upon the facts, we have not lost sight of the circumstance that the increasing severity of the successive epidemics may possibly, to some extent, find its explanation in the gradual wider diffusion of the infection in the localities in question.

(5.) Some of the seemingly paradoxical facts, considered above in connexion with the influence of insanitary conditions on plague, may possibly be explained in the light of the theory we are considering. Among these may be enumerated (*a*) the fact that plague has not become severely epidemic in the towns of Calcutta, Mysore, or Oporto, where the evidences of every kind of sanitary neglect are to hand on every side; (*b*) the fact that plague was more rife in the less crowded suburban districts of Bombay than in the overcrowded districts in the centre of the City; and (*c*) the fact that plague claimed considerably more victims in the well-ordered and sanitary surroundings of the Byculla Gaol, Bombay, than in some of the most insanitary and overcrowded chawls in the City. These facts, which at first sight appear so paradoxical, are brought nearer to the understanding, if we reflect that the survival of the plague bacillus in a virulent condition in the external surroundings of insanitary chawls is very far from being, as has been often tacitly assumed, a mere matter of course. On the contrary, there are greater difficulties in imagining that the plague bacillus could adapt itself to the protean surroundings of overcrowded insanitary chawls (differing as these do widely among themselves) than in conceiving of the bacillus adapting itself to comparatively simple external conditions which are common to clean houses as a class.

(6.) Lastly—but here we are treading, if possible, on more uncertain ground—it would seem as if we were entitled to draw certain inferences which are appropriate to the question now under review, from the consideration of the past history of plague, both in India and in other countries. Plague, after being prevalent for a period of years, has, in the past, always gradually diminished, and (except in certain endemic centres) finally become extinct. It would seem from this that in certain cycles the external conditions (we have of course no knowledge as to what particular external conditions are involved) must be relatively favourable to the survival of the plague bacillus outside the body, and that then in certain other cycles (and it is legitimate to hope for the advent of these in India) the external conditions become again relatively unfavourable to the survival of the plague bacillus outside the body and, therefore, to the spread of plague.

The President being unable to agree with many of the suppositions and statements in this paragraph has expressed his views separately.

The President is unable to agree with many of the suppositions and statements contained in this paragraph (284). His views have been expressed in a separate Memorandum. (Appendix III., *see* paragraphs 47–52, 41–43, 84, 13, 11, 59–64.)

CHAPTER IV.

HAFFKINE'S ANTI-PLAGUE INOCULATION.

I.—Review of History of Preventive Inoculation.

Preliminary review of preventive inoculation.

385. It will be of advantage, before we deal specifically with the particular question of Ir. Haffkine's Anti-Plague Inoculations, very briefly to review the history of preventive ioculation. We shall, in the course of our survey, see that general principles have radually established themselves, first in connexion with the question of the application f preventive inoculation in different kinds of diseases, and secondly in connexion with ie question of the procedure which must be adopted to secure the maximum of dvantage from such preventive inoculation.

Inoculation against small-pox.

386. The first step in the application of the method of preventive inoculations was iken when the process of inoculation against small-pox was introduced into Western urope from Turkey in the early part of the last century. In the case of this ioculation, the virus of small-pox, was directly transferred from the patient who as suffering from a clinically mild attack of small-pox into the person who as to be protected. This method of inoculation was resorted to in the expectation iat the patient who was inoculated in this manner would, like the patient who ad suffered from an actual attack of small-pox, be thereafter immune against irther attacks of the disease. This particular expectation was fulfilled. On ie other hand, experience showed that, as a set-off against the advantages which cèrued to the patient, a considerable risk had to be run. Severe, and in some cases tal, attacks of small-pox not infrequently supervened in the persons who submitted iemselves to inoculation.

Principles established.

The principles which can be deduced from the experience of this, the first, system preventive inoculation are the following :—

I. Given the case that we are dealing with an infective disease of which one attack protects against a subsequent infection, it is possible to protect against that disease by inoculating some of the virus derived from a sick patient into a previously unprotected person.

II. Given that the virus which is inoculated is not sufficiently attenuated, the evils which result from such preventive inoculation may, in some cases, exceed the benefits which will be derived.

Inoculation with cow-pox lymph.

387. The first system of preventive inoculation, in which a duly attenuated virus was ied, was the system of preventive inoculation with cow-pox lymph, which was troduced by Jenner. In this case, the virus which is inoculated is, as subsequent searches have shown, the *materies morbi* of small-pox, which has undergone a constant id adequate degree of attenuation in the course of its passage through a series of lves. From the experience of the favourable results which have been obtained by is system of preventive inoculation, the following principle can be deduced :—

III. Given that the virus of a particular disease can be obtained in nature in a constant and sufficient degree of attenuation, that virus may be employed as a vaccine.

Pasteur's researches on anthrax.

388. The next great practical advance, in the matter of preventive inoculation, was ken when Pasteur demonstrated that, in default of a naturally attenuated virus it is possible by artificial means to obtain the attenuated viruses which are required. asteur showed in the case of anthrax that it was possible to attenuate the virus to y desired degree. Further, he showed how the degree of attenuation could be curately measured on animals. Lastly, Pasteur showed that it was possible, by ploying measured quantities of such duly attenuated cultures, successfully to ccinate animals against anthrax.

The principle which was thus established may be formulated as follows :—

IV. Given that we have at disposal in nature only a virus whose virulence is such that its inoculation would entail risks too formidable to be faced, still, if it is practicable artificially to attenuate that virus, the attenuated virus may then be used without risks as a vaccine.

With the establishment of this principle, finality would appear to have been reached in the matter of the *essential* requisites of those living vaccines which Pasteur had in view.

Purity of vaccine an essential condition.

389. In order that we may have an ideal vaccine material, we need only stipulate in addition, first, that the menstruum in which the actual vaccine material is contained shall be quite free from bacterial contamination, and secondly, that such menstruum shall, as far as possible, be free from any toxic substance which is not operative in the direction of producing immunity.

Ferran's inoculation against cholera with living cultures.

390. Having placed these theoretical requisites clearly before us, we may consider the next extension which was made in the matter of the application of preventive inoculation to man. This step was taken in Spain in the presence of the cholera epidemic which devastated certain regions of that country in 1885. This step consisted in the application of the principle of preventive inoculation to the protection of the population against this disease. The anti-cholera inoculations which are in question were introduced by Dr. Ferran. The method of operation consisted in the inoculation of living cholera cultures. These inoculations were very widely applied. An inquiry into them was instituted by a Commission appointed by the French Academy of Medicine. The scope of this inquiry embraced: (*a*) the methods which were employed in the manufacture of the vaccine, and (*b*) the results which were obtained by the inoculation.

Bulletin de l'Académie de Medicine, 1885, pp. 902–33.

It appears from the Report of the French Commission, that Dr. Ferran refused to reveal the details of the methods which he employed in the preparation of his vaccine. The vaccine appears to have been manufactured by growing the cholera bacillus in nutrient broth. The methods which were employed in making these cultures are described as having been crude in the extreme. It would appear that no method of standardizing the strength of the culture and no process of controlling the purity of his culture was adopted by Dr. Ferran.

The members of the French Commission saw a considerable number of persons inoculated with Dr. Ferran's vaccine. They examined these patients after inoculation and they reported that they did not observe, and that they had not learned of, the occurrence of any evil symptoms beyond the fever and the malaise and local pain, which are the necessary accompaniment of such inoculations. Dr. Ferran's statistics were not controlled by the French Commission. The Commissioners, however, state that they learned incidentally from a fellow passenger in the train that these statistics were probably untrustworthy, inasmuch as there was a considerable concealment of deaths in the cholera districts, and inasmuch as the population of Spanish towns was habitually understated by the Municipalities, with a view to evading the poll tax.

General deductions from Ferran's inoculations.

391. These inoculations of Dr. Ferran have an interest, inasmuch as they represent the first extension of the principle of preventive inoculation to the protection of man from diseases other than small-pox. The principles which underly these inoculations may be formulated in the following way:—

V. Given that we are dealing with a disease, such as cholera, which does not itself confer any long-lasting immunity against a subsequent attack, it is still an object to be aimed at to obtain, by a vaccination process, the short-lived immunity which accrues to a patient who has survived an actual attack of the disease.

VI. In a disease, such as cholera, which is caused by micro-organisms, which have no power of proliferating themselves in the blood or tissues of man, the vaccine material which is hypodermically inoculated may legitimately consist of unattenuated living cultures of such micro-organisms.

From the results of Dr. Ferran's inoculations, so far as they are available to us, we may glean also the following facts for our guidance:—

(*a*.) Even in the absence of all scientific precautions for controlling the purity and the virulence of the cultures which are employed, the evil effects which might have been anticipated do not necessarily supervene.

(*b*). No certain judgment can be arrived at with regard to the efficacy of the inoculation process when the statistics are liable to be extensively vitiated on the one hand, by insufficient reporting and the concealment of cases and, on the other hand, by false estimates of the numbers of the non-inoculated population. Both these factors are said to have vitiated the

statistics of the anti-cholera inoculation in Spain: we shall find them detracting from the value of many of the statistics of anti-plague inoculation which were placed before us in India.

Pasteur's work on rabies.

392. Almost contemporaneous with Dr. Ferran's work in connexion with the cholera noculation, is the brilliant original work which Pasteur did in connexion with he elaboration of a method of protective inoculation against hydrophobia. The etails of Pasteur's process, and the new principle which can be deduced from it ith regard to the possibility of inoculating successfully during the incubation period, o not immediately here concern us. The question of successful inoculation during ne incubation period, so far as it has an application to the case of plague, will be ealt with later. Suffice it for our purpose that Pasteur's work in connexion with abies provides us with a further instance of a possibility of conferring protection gainst an invariably fatal disease by the employment of a duly-standardized living accine.

Haffkine's inoculation against cholera with measured quantities of living bacteria of known virulence.

393. The next advance in the history of preventive inoculation is marked by the fact hat Mr. Haffkine, who was inspired by the principles of Pasteur, took up the anti-holera work which had been inaugurated by Dr. Ferran. Mr. Haffkine laid down for imself as a principle that in the case of the anti-cholera inoculations the Pasteurian rescription of operating with measured quantities of bacteria of accurately known irulence was to be strictly observed. The vaccination-dose of cholera bacilli nployed by Mr. Haffkine was the quantity of culture which grew in 24 hours on ne-sixth part of the surface of an ordinary agar tube. The virulence of the cholera ultures which were employed for these inoculations was such that the total peritoneal fusion of a guinea-pig, which had been killed by intra-peritoneal injection of cholera, as lethal to another guinea-pig in eight hours.

Duration of immunity against cholera doubtful. "Indian Medical Gazette," No. 1, January 1895. Report to Government of India, dated Aug. 25th, 1895. "Indian Medical Gazette," Vol. XXX., No. 12, December, 1895.

394. Reports showing favourable results which were obtained from these inoculations ave been published, on the one hand by Mr. Haffkine, and on the other hand by aptain Christian Hare, I.M.S. Inasmuch as these favourable results were obtained in ersons who were exposed to cholera infection within a very short period after the oculation, they tell us nothing with regard to the duration of immunity. A ainstaking endeavour was made by Mr. Haffkine to collect data upon this subject y investigating the incidence of cholera among inoculated persons who were residing the bustees of Calcutta, where cholera is endemic. In accordance with the results these inquiries, the resulting immunity was adjudged to last for over 12 months. the case of the epidemic of cholera which occurred in the East Lancashire egiment in Lucknow, 14 months after inoculations had been done in this regiment by r. Haffkine, the incidence of the disease showed that such immunity as was conferred y the small quantities of cholera cultures inoculated was hardly noticeable.

Inoculation against typhoid, with dead cultures. "Lancet" Sept. 19th, 1896, "British Medical Journal" Jan. 30th, 1897. "Deutsche medicinische Wochenschrift" Nov. 12th, 1896.

395. The next practical extension of the principle of preventive inoculation was made nen the anti-typhoid inoculations were introduced. The first of these inoculations ere undertaken by one of us in July and August 1896. A little later similar oculations were undertaken by Pfeiffer and Kolle in Germany. In both cases the mmunizing material which was recommended consisted of dead cultures of typhoid cilli. In view of this fact, it was possible directly to standardize the vaccine determining the toxicity of a measured volume of the dead culture. It has been termined by comparative experiments on men and animals that the quantity of dead lture which may with safety be inoculated into a man is the quantity of broth lture which is lethal for 100 grammes of guinea-pig.

Haffkine's inoculation against plague. Annales de l'Institut Pasteur, 1895.

396. Next in chronological order comes preventive inoculation against plague. The st experiments made on this subject were made by Yersin, Calmette, and Borrel ijointly in 1895. These observers found that it was possible to confer a certain mount of immunity against plague by the hypodermic injection of dead cultures plague bacilli. Their experiments were carried out on rabbits and guinea-pigs. Mr. Haffkine's anti-plague inoculations represent an extension of this system of eventive inoculation to men.

II.—Preparation and Character of Haffkine's Vaccine.

Questions to be now considered in connexion with Haffkine's anti-plague vaccine.

397. We may consider in connexion with Mr. Haffkine's method of inoculation—

I. The method of preparing mass cultures of the plague bacillus.

II. The method of controlling the purity of these cultures.

III. The method of killing the plague bacilli with the view of converting the culture into an immunizing material.

IV. The method of decanting and bottling the prophylactic fluid.

V. The method of ascertaining that the vaccine is free from contaminating micro-organisms, and the precautions which are adopted for preventing the growth of such organisms.
VI. The method of standardizing the vaccine.
VII. The composition of the vaccine.
VIII. The volume of the prescribed dose.

II.(1.) *Method in use for the Preparation and Standardization of the Vaccine.*

I.—Method of preparing mass cultures of the plague bacillus.

398. In preparing cultures of the plague bacillus on a large scale, Mr. Haffkine had to deal with two difficulties. In the first place he had to exclude from his culture medium all animal products, such as commercial peptone, of which it could not be asserted that they had been prepared in such a manner as to be inoffensive to the feelings of the natives of India. In the second place, Mr. Haffkine had, while cultivating his plague bacillus in mass culture on a liquid substratum, to provide for the efficient aeration of his cultures. Both these difficulties have been overcome.

Culture medium adopted. 26–7.

A suitable culture medium has been obtained by digesting goat's meat in the autoclave at 140° C. with hydrochloric acid, and then neutralizing the acid medium with caustic soda.

Method of securing adequate aeration. 27.

The difficulty which is occasioned in the case of any fluid culture medium by the want of proper aeration in the deeper layers is overcome in the case of Mr. Haffkine's mass cultures by the introduction of a small quantity of oil into the medium. The plague bacteria suspend themselves from the oil drops on the surface of the medium.

Method of cultivation. 27.

The culture medium is inoculated with plague bacilli. This done, the inoculated flasks are cultivated at the temperature of the air. When this temperature sinks below 25° C., the air of the room is warmed up by lighting a certain number of gas jets. The flasks are cultivated in this manner for a period which varies from two to six weeks. During the course of the cultivation, the culture is shaken up every few days.

II.—Method of controlling the purity of the plague culture. 17,276.

399. At the termination of the cultivation, the purity of the plague culture is controlled by transferring a portion of the contents of the flask with a sterile pipette on to the surface of a dry agar tube. The character of the growth which develops on the dry agar tube is then controlled by naked eye observation. The character by which the purity of the culture is judged is the specular appearance of the colonies, when they are seen from the back by reflected light through the thickness of the agar.

III.—Method of killing the plague culture and precautions taken to prevent the growth of contaminating micro-organisms in the vaccinating material. 102·4.

400. After drawing off a sample of the contents for the purpose of controlling the purity of the culture, the cultivation flasks are placed in a water bath along with a control flask. This last is filled with water, and is fitted with a thermometer. The temperature of the water bath is then raised until the thermometer in the control flask indicates a temperature of 65° C. This temperature is maintained for one hour. An addition is then made to each flask of a quantity of carbolic acid amounting to one two-hundredth part of the bulk of the vaccine.

IV.—Method of decanting and bottling. 17,461–6.

401. The decanting and bottling is effected in the following manner. The decanter takes a flask of the dead plague culture and shakes up the sediment which has lodged at the bottom of the flask. He then raises the cotton-wool plug and introduces into the neck of the flask a long glass syphon. There appears then to be some difficulty in starting this syphon. The method which is enjoined involves the fitting on of a mouthpiece to the end of the syphon, and the application of suction. The syphon having been started, the contents are run off into ordinary 4 to 6 oz. narrow-necked and dark-coloured medicine bottles. These are then corked up with corks that have undergone a sterilization process. This done, the necks of the bottles are immersed into a vessel full of melted sealing wax. During the process of decantation, the decanters are instructed to shake up the vaccine material from time to time.

V.—Method of controlling the sterility of vaccine. 17,470–2. 104.

402. It was assumed that the sterility of the vaccine was sufficiently insured by the procedure described above until attention had been drawn to this matter shortly after we commenced our inquiry in India. Since December 12th, 1898, the sterility of each culture has been directly controlled by transferring to a tube of agar the first sample of the carbolized vaccine syphoned off from the cultivation flask.

VI.—Method of standardizing the vaccine.

403. We had some difficulty in ascertaining what was the method of standardization which was adopted by Mr. Haffkine.

72, 77, 78, 182.

We gathered, from Mr. Haffkine's statement when he was first examined before us, that each brew of vaccine was standardized either by Mr. Haffkine himself or by the inoculator by determining what volume of the vaccine would produce an average temperature of 102° F. in a series of ten to twenty patients. Further inquiry, however,

vealed that this method of standardization was not systematically employed. On the
ie hand it was ascertained that Mr. Haffkine was not himself able to carry out this 3023–8.
stem of standardization in the case of the large bulk of the vaccine which was sent
it, and on the other hand we were able to see clearly that the conditions under
hich the inoculations were done at outlying stations were such that it was not possible
r the inoculator to undertake this preliminary task of standardizing his vaccine on
series of patients. Finally, we elicited that the routine practice which was adopted 17,527–46.
Mr. Haffkine's laboratory was to standardize the vaccine by holding up to the 26,467.
ght one or two sample bottles of each brew, with a view to appreciating the opacity
the vaccinating fluid. It was in conformity with the results of this appreciation
at the dose was inscribed on the label of each bottle of the vaccine. The dose was
ere stated on the labels in multiples of a "standard dose." This "standard dose" 4,577.
as an arbitrary quantity: it was assumed to be the equivalent of $2\frac{1}{2}$ c.c. of the original 10,705–6.
ccine with which Mr. Haffkine inoculated himself and his first batch of patients. 33, 4002.
here, for instance, two or three standard doses were prescribed, it was assumed that
e vaccine of which this quantity was prescribed was twice or thrice weaker than the
iginal vaccine.

404. While it appears from the evidence which has been placed before us that the Factors which in practice acted as checks on the method of standardization adopted.
ove is the routine practice which was adopted in the standardization of the vaccine,
is to be observed that the results which were obtained by this method were, in some
stances, controlled by direct observations of toxicity made on persons who presented
emselves for anti-plague inoculation at the inoculation station which was attached to
r. Haffkine's laboratory. We may also note that Mr. Haffkine throws the ultimate 78.
sponsibility for the standardization of the vaccine upon the inoculator, inasmuch
he has given it to be understood that each inoculator, before he proceeds to
oculations *en masse*, should make, in the case of a sample of vaccine, a preliminary
ries of test experiments with the prescribed dose on a batch of some twenty
tients.

405. The samples of the vaccine which have come under our direct observation have VII.—Volume of the prescribed dose.
en estimated by Mr. Haffkine to be respectively 2, $2\frac{1}{2}$, 3, 4, 5, 6, 7, and 8 times
aker than the standard vaccine. The doses of these vaccines were thus respectively
$6\frac{1}{4}$, $7\frac{1}{2}$, 10, $12\frac{1}{2}$, 15, $17\frac{1}{2}$, and 20 c.c. On an average the prescribed dose has
rhaps amounted to something like $7\frac{1}{2}$ c.c.

406. The vaccinating material consists of two constituents. By far the larger bulk VIII.—Composition of vaccine.
nsists of the nutrient fluid in which the plague bacilli have been grown. Relatively 27.
aking, a very small bulk consists of the dead bodies of the plague bacilli which have
en grown in that fluid.

II.(2.) *Criticism of Method employed for Preparing the Vaccine and sending it out for use.*

407. Before passing on to make any criticisms of the methods which are adopted Criticisms of method adopted for preparation of vaccine.
the preparation of the vaccine, we desire to recognise in the fullest manner the
ormous difficulties with which Mr. Haffkine had to contend in working under the
nditions of an Indian climate, with an incompletely trained staff, in an incompletely
ipped laboratory, and under the pressure of great urgency.

408. It seems to us that Mr. Haffkine has solved in a skilful manner the problems (I.) Method of preparing mass cultures.
finding a suitable culture medium and a suitable method of aerating his mass
tures of the plague bacillus. A somewhat similar method to that which he has
ployed for suspending the plague bacilli on the surface of the nutrient fluid has
quently been employed in the cultivation of the diphtheria bacillus.

409. Mr. Haffkine's method of controlling the purity of his mass cultures is the routine (II.) Method for controlling purity of cultures.
thod which is adopted by all bacteriologists, and calls for no remark. The
thod of controlling the purity of the cultures of the resulting growths merely by
ked eye inspection is, on the other hand, a departure from the usual method which
adopted in such cases. The specular appearance of the colonies of plague bacilli,
n from behind by the reflected light through the depth of the agar, is a characteristic
on which we should scruple to place much reliance. Our experience in this matter 8667.
the same as that of Mr. Hankin. Hence we do not think that this method of
trolling the purity of a plague culture is one that ought to be generally adopted.
are, however, very far from disputing that the character referred to may, in the
nds of a bacteriologist who has the special experience of Mr. Haffkine, constitute
rustworthy index of the purity of the plague culture.

(III.) Method of killing the plague culture.

410. The method for killing the cultures of plague bacilli, which has been adopted by Mr. Haffkine, is the routine method which is adopted by every bacteriologist. In view of the fact that no surviving plague bacilli have been detected either by ourselves or by others in very numerous samples of vaccine which have been examined bacteriologically, the method of killing the culture which is adopted by Mr. Haffkine appears to us to be, in the case of plague, as in the case of other non-spore bearing organisms, absolutely trustworthy.

Influence of heating on the bacterial sediment and the supernatant fluid respectively.

The only question which has to be considered in connexion with the application of heat for the purpose of killing the plague bacilli is the question as to the influence which the temperature employed may exercise upon the various constituents of the prophylactic fluid. Experiments upon this question have been made by the German Commission and by ourselves.

Report of German Plague Commission, pp. 312-4.

The German Commission found that the efficacy of the immunizing substances which are contained in the bacterial sediment was perfectly well preserved when the bacteria were killed by an exposure to a temperature of 65° C. The protective power of the bacteria which were killed by the application of that temperature was not less than the protective power of an equal bulk of bacteria which had been killed by chloroform. The immunizing powers of the bacterial sediment were, however, practically destroyed when this sediment was subjected to a temperature of 100° C.

26,432

We, for our part, have tested the effect which is exerted by heat upon the filtrate from a plague culture. These results have been incorporated in Table IV. of Dr. Liston's evidence. It results from these experiments that the soluble toxins, which are under certain conditions developed in the fluid nutrient medium in which the plague bacilli are grown, are apparently not affected by exposure to a temperature of even 100° C.

109.

Mr. Haffkine has not made any experiments on this subject.

Effect of addition of ½ per cent. carbolic to the vaccinating material.

Further, in this connexion we may consider the effect which is produced by the addition of carbolic acid to the vaccinating material.

Report of German Plague Commission. pp. 310–2.

So far as we know, the only experiments which have been made with a view to ascertaining the effect of such an addition, are the experiments on the effect of an addition of ½ per cent. carbolic acid to plague cultures, before and after heating, which were instituted by the German Plague Commission. Two facts were brought out by these experiments. The first of these is that the protective properties of a plague culture which has been killed off by the application of heat are not in any way impaired by the subsequent addition of ½ per cent. carbolic acid. The second of these is the very interesting fact that when ½ per cent. carbolic acid is added to the unheated plague culture, its chemical constituents are so altered that it becomes perfectly useless from the point of view of giving protection.

120.

Mr. Haffkine has not made any accurate experiments on the effect of the addition of the carbolic acid which he makes to his vaccinating material.

(IV.) Method of decanting and bottling.

411. It is manifest that the procedure of decanting and bottling which has been outlined above, and which is prescribed in detail in the instructions which are incorporated in Appendix No. V. in Vol. I. of our Proceedings, is a procedure which even a skilful operator could not easily carry out in an absolutely aseptic manner. The chances of contamination must naturally be materially increased when it becomes necessary to commit the task of carrying out these instructions to a subordinate native agency. The particular difficulties which we have noticed are:—

1st. The difficulty of ensuring the adequate sterilization of the glass syphon.

2nd. The difficulty of ensuring that the syphon shall be started by the prescribed procedure without contaminating the end of the delivery tube.

3rd. The difficulty of corking a bottle in an aseptic manner when the cork has to be held and inserted into the neck of the bottle with no other implement than a pair of ordinary dissecting forceps.

Lastly. The difficulty of sealing a bottle in an airtight manner with a material such as sealing wax, which refuses to adhere to a wet or steamy glass surface.

II.(3) *Question of the Sterility of the Vaccine.*

(V.) Method of securing sterility of vaccine.

412. The above criticisms of the methods of the preparation, decanting, and bottling of the vaccine, acquire importance in view of the fact, which we will now proceed to consider, that the precautions taken did not, in point of fact, secure the freedom of the vaccine from contaminating micro-organisms.

Question of the presence of contaminations in the

Evidence bearing upon this question was brought before us first at Dharwar. It was there brought to our notice that, in view of the fact that certain bottles

vaccine had been found to be putrid, it was the practice of the inoculators to smell each bottle of the prophylactic, with a view effectually to exclude such bottles. vaccinating material. 1754–7, 1781, 1807–9.

The question of the presence of contaminating micro-organisms in the vaccine was gain brought before us at Hyderabad, by Col. Lawrie, I.M.S., and Mr. Mullanah. We ound some difficulty in determining who was really responsible for the experiments vhich were adduced in the support of this assertion, that the vaccine was contaminated. Ve have, however, in view of the fact that Dr. Lawrie controlled the processes nd deductions of Mr. Mullanah, finally arrived at the conclusion that the responsibility or these must rest with Dr. Lawrie. This being so, we are compelled, in view of he want of familiarity with bacteriological methods which was displayed by)r. Lawrie, to receive the experimental data, which were brought before us, with a ertain amount of reserve. 5161, 5379–95 5674, 5762–77, 5914–25.

In addition to the evidence just referred to, evidence on the subject of the ontamination of the vaccine was brought forward both in Hyderabad and afterwards n Bombay by Captain C. A. Johnston, I.M.S. This evidence related to a series of even bottles of Haffkine's prophylactic fluid which were bacteriologically tested. The amples which were employed for the purpose of testing these bottles were drawn off n the bacteriological laboratory at Hyderabad in the presence of one of us. While re are not able in this particular case to guarantee the absolute asepticity of the rocedure which was adopted in transferring the vaccine to the agar tubes, there can e no doubt that the luxuriant growth which was to be seen when these agar tubes ere afterwards produced for our inspection could not possibly be accounted for by ne minor defects which were noted in the procedure adopted. The bacteria which ere obtained by Captain Johnston from one of these samples (Brew No. 5,338) were fterwards inoculated into a rabbit, with fatal results. 5798–821. 24,895–916. Appendix LXX. in Vol. III. of these Proceedings.

Our attention was further directed to the possibly contaminated condition of the accine by the evidence which was laid before us at Wardha. The evidence in question nowed that constitutional and local symptoms of excessive severity had supervened pon the inoculation of Mr. Hogan and his Assistant Surgeon with Mr. Haffkine's accine. 6300–92.

413. In view of the fact that doubt had thus been cast on the efficiency of the procedure lopted by Mr. Haffkine to secure the sterility of his vaccine, a series of samples of nti-plague vaccine were requisitioned from Dharwar, Bangalore, and Hyderabad. ample bottles were also taken from the stocks which were kept at Nagpur and at alcutta. Among the bottles which were taken from Nagpur were some samples of ne brew of vaccine which, according to the information we have received, was the rew (Brew No. 4,746) employed in the inoculation of Mr. Hogan and his Assistant urgeon. Bacteriological examination of samples of Mr. Haffkine's prophylactic at Calcutta and Bombay. 8389.

These various samples of vaccine were submitted to bacteriological examination by r. Nield Cook at the Health Office, Calcutta, under the immediate supervision one of ourselves. The methods which were employed in these examinations, nd the details of the results which were obtained, will be found in Dr. Nield ook's evidence. It will suffice here to note that out of a total of 22 bottles examined, even were found to contain bacterial impurities. This total of 22 included six bottles hich were sent in for examination from Hyderabad. One of these contained npurities. This bottle was derived from the same brew (Brew No. 5,338) which was und by Captain Johnston to contain a bacillus which was fatal to rabbits. In two the bottles which were derived from the brew (Brew No. 4,746), which had been nployed for the inoculation of Mr. Hogan and his Assistant Surgeon, a number of ntaminating bacteria were found. We may note in passing that the number of the ew, as it appears in Dr. Nield Cook's evidence, differs from the number assigned this brew in the note in which Lieutenant Hammond, I.M.S., who also examined rtain samples of vaccine, details the result of his bacteriological observations. In iew, however, of the fact that contaminating bacteria were found both by Dr. Nield ook and by Lieutenant Hammond, and that they appeared on comparison to be orphologically identical, the confusion as to the number of the brew is not of any nportance. 8380–9. III. 567. I. 460.

A further series of 12 bottles, derived from the stock of vaccine which had accumulated Mr. Haffkine's laboratory, was tested under our direct supervision at Bombay. ree of these bottles were found to contain contaminating micro-organisms. 26,375.

17,481. Lastly, a methodical examination of all the brews of vaccine which were available in the Plague Research Laboratory, Bombay, was made by Captain C. J. Milne, I.M.S., acting under the instructions of Mr. Haffkine. This systematic examination revealed the fact that out of 125 bottles examined 26 contained contaminating micro-organisms.

Thus, if we altogether exclude from consideration the evidence which emanated from Dr. Lawrie's laboratory, it is to be noted that the other series of examinations yielded the result that contaminations were present respectively in seven out of a series of 22, three out of a series of 12, and 26 out of a series of 125 bottles examined; in other words, contaminations were found in 36 out of 159, *i.e.*, in 22·5 per cent. of bottles examined.

Source of the contaminations discovered on bacteriological examination. 17,481. III. 645.

414. In view of the fact that in Dr. Milne's series of examinations contaminations were found only in six out of 104 well-sealed bottles, whereas they were found in 20 out of 21 badly-sealed bottles, Mr. Haffkine has urged that the bacterial contaminations which we have spoken of above are almost all imputable to a defect of sealing in the bottles. We cannot, however, accept Mr. Haffkine's view in its entirety, for even when great precautions were taken to exclude all but well-sealed bottles—and such precautions were taken in the case of the bottles which were examined under our supervision at Calcutta and Bombay—the result showed that contaminations were present in 10 out of 34 of these well-sealed bottles. It seems to us, therefore, that many of the contaminations must have been introduced during the process of decantation.

Precautions for the avoidance of contaminations.

415. The questions which have been considered above, suggest two further questions:—

1. They suggest the question as to whether it would be possible to institute a final bacteriological examination with a view to the detection and exclusion of contaminated bottles.
2. They suggest the question whether, failing the adoption of such a process of bacteriological control, it would be possible to re-sterilize the vaccine after it is finally bottled.

Bacteriological control of sterility before vaccine is sent out not applied at period when most required, and does not detect bacteria not checked in development by presence of carbolic acid.

In connexion with these questions, we have to consider the procedure for controlling the sterility of the vaccine which has been adopted by Mr. Haffkine since Decembe 1898. This procedure consists, as we have seen, in receiving directly on the surfac of an agar tube the first portion of the carbolized vaccine which is syphoned o from each cultivation flask. It seems to us that this procedure hardly meets th necessities of the case. The following points suggest themselves to us in thi connexion.

First, the proceeding is one which ensures the control of the vaccine only at tha period when it least needs control, *i.e.*, at the period which immediately supervene upon the process of heating and carbolization. On the other hand, it does not i any way guarantee the sterility of the vaccine at the stage when it most require control, *i.e.*, at the period when it has been exposed to the risks of contaminatio which are involved in decanting, bottling, and corking.

17,302–6

A further criticism relates to the method which is adopted of testing the presenc of contaminating micro-organisms in the vaccine material. It will have been notice that the carbolized vaccine is received for testing purposes directly from the syph upon the surface of the agar, and that it is not subsequently diluted. It seem to us that this method is a method which is calculated to reveal the presence of suc bacteria only as are capable of growing in the presence of $\frac{1}{2}$ per cent. of carbolic aci It may quite well be that these are the contaminations which are most to be dreade None the less, it seems to us that it would be better to adopt as a method of testi a method which would favour the development of each and every species contaminating bacteria. This end might be readily achieved by taking the precauti of diluting the sample of carbolized vaccine which is to be tested, either before after it is transferred to the surface of the agar tube.

Question of final re-sterilization of the vaccine in bottles. 24,917–21, 24,926–31.

In response to a suggestion which was made to Dr. Lawrie during his cro examination before us, experiments on the re-sterilization of the vaccine in bottl after they had been sealed up, were instituted by Captain C. A. Johnston. So far these experiments go, we may note that their results are favourable to the conclusi that it would be possible by re-sterilization at a temperature of 60° C. to secure th the vaccine should be sent out in a perfectly aseptic condition, and this apparen without forfeiting anything of its efficacy.

II.(4.) *Question of the Standardization of the Vaccine.*

VI.—Method of standardization.

416. We have seen above that it was laid down as a principle by Pasteur that no accination should be undertaken unless with a measured quantity of a culture of efinitely ascertained virulence. The above, however, applies only in cases where living culture is employed as a vaccine. In cases in which dead bacterial cultures re used as vaccine, the standardization of the vaccine resolves itself into a question f determining the toxicity of a measured volume of the culture. This determination ay be made by directly testing the toxic effect of the dead culture on men or nimals. Failing that, it may be made by determining in two successive operations, rst, the virulence of the bacteria which are employed in the manufacture of the vaccine, nd secondly, the quantity of effective element which is contained in a given volume f the dead culture.

Two methods of standardization applicable.

Criticism of standardization by temperature reaction obtained in an inoculated patient. Paras. 403–4 of this Report.

417. We have already stated that in some cases Mr. Haffkine's vaccine was standardized y the direct method of determining the toxic effect that was produced on man by the jection of a measured volume of the vaccine. Further, we have stated that it was ntemplated that all of it should be standardized by this method. We may, therefore, nsider the question of the standard of toxicity which was adopted. This standard as, as we have already seen, the production of an average temperature of 102° F. on series of 10 to 20 patients.

With regard to this we have to state that we have no data at our disposal for arriving a judgment as to whether the attainment of that temperature is, in reality, an dex of the acquirement of immunity. It seems to us, however, on *à priori* grounds, at the temperature reaction is probably an untrustworthy index of the protection hich is afforded, inasmuch as the vaccinating material contains, in addition to the oisons which are specific to the plague bacillus, other fever-producing poisons in e form of considerable quantities of peptone.

Practical difficulties in adoption of temperature reaction as standard.

Quite apart from the possible fallacies which attach to the particular criterion of ficacy which has been adopted by Mr. Haffkine, there seem to us to be also certain ractical difficulties which are incident to this particular method of standardization. n the first place, the temperature reactions which supervene upon the inoculation of ecisely similar quantities of one and the same vaccine are notoriously subject to very nsiderable individual variations. We may refer here to the evidence of Captain C. J. ilne, I.M.S., and the observations of Lieutenant Hammond, I.M.S. Secondly, the me of the temperature apparently occurs at very irregular intervals after inoculation. hirdly, it is very difficult to keep the inoculated under close medical observation for e time which is required for determining the ultimate height of the temperature.

17,490–5.

App. XIX. in Vol. I. of these Proceedings.

3023–8, 6321–3, 10,803–7, 11,702–3, 14,808–10.

Confirmation of the truth of these conclusions was obtained by us as soon as we gan to inquire into the question as to how far it had been possible in practice to ploy the method of standardization which was prescribed by Mr. Haffkine. We und that in practically every case it had been found impossible to undertake the escribed series of preliminary test temperatures. In connexion with this, we may mark that, with hardly an exception, the inoculator injected without question the oses of vaccine which were prescribed upon the bottles.

As soon as these facts came to our knowledge, during our tour in the Provinces, we commended to the Government of India that the inoculator should be relieved from e task of standardization, and that all the operations which were required for the ndardization should be carried out in the laboratory before the vaccine was sent t.

Criticism of standardization by estimation of opacity.

418. On our return to Bombay we made further inquiries as to how the dosage which as inscribed on the labels of the bottles was arrived at. Our inquiries revealed, as s already been stated above, that the doses were inscribed on the labels in accordance th the estimates which were formed of the opacity of the particular vaccines when rtain sample bottles of these were held up to the light. In view of the fact that is method is not a procedure for directly testing the toxicity of a measured volume vaccine, but a procedure which is designed to determine the quantity of bacterial diment which is contained in a particular volume of fluid, it became necessary rther to inquire, first, whether a preliminary method of standardizing the virulence the culture was employed, and, secondly, whether the quantity of bacterial sediment ntained in a measured volume of fluid was, as a matter of fact, accurately ascertained the process which has been described above.

Question of antecedent estimation of virulence of cultures employed. 17,118–17,125.

419. The evidence which was laid before us by Dr. Mayr revealed the fact at no systematic method was adopted for ascertaining the virulence of the ltures which were employed for the manufacture of the vaccine. Mr. Haffkine had

previously stated that in lieu of adopting a systematic method of testing the virulence of his cultures, an endeavour was made to employ for the manufacture of the vaccine, so far as it was practicable, only microbes which were directly derived from the bodies of patients who had died of plague. No definite data were communicated to us by
27. Mr. Haffkine, but it appears, however, that it was not always possible to employ cultures which were directly obtained from plague patients. In many cases, as we gathered from Dr. Mayr's evidence, the microbes which were employed for the manufacture of the vaccine were microbes which had become attenuated in the course of prolonged cultivation on artificial media.

Doubt whether inoculation with attenuated plague cultures confers protection.

420. We need not here, in the absence of sufficient experimental data, discuss the question as to whether protective effects can be obtained by the inoculation of non-virulent cultures. We may merely note that the German Plague Commission endeavoured to put this question to the test by experiments on two monkeys. Contrary to what had been found to be the case when sterilised cultures of virulent plague were employed, it was found that the sterilised cultures of non-virulent plague in similar quantities conferred no protection. The German Plague Commission based upon these experiments the conclusion: "That it is absolutely essential that the plague cultures which are employed in protective inoculation should be obtained directly from fatal cases of plague, and, failing that, they should be cultures which have been kept up to the necessary degree of virulence by a continuous series of passages through animals." The evidence for this conclusion does not seem to us to be absolutely cogent, inasmuch as evidence has not been brought forward by the German Commission to show that the quantity of bacteria on an agar culture of non-virulent plague is as great as the quantity which is contained on an agar tube of virulent plague. None the less until it shall have been established that protection can be afforded by vaccines made from non-virulent cultures, it seems to us that it would be desirable to adopt some method of keeping up the virulence of the plague cultures which are employed for the manufacture of the vaccine. The
26,434 C. results of the experiments which are recorded in the evidence of Lieutenant Liston, I.M.S., seem to us to indicate that it would be a comparatively easy matter to keep up the virulence of a plague culture by a series of passages through guinea-pigs.

Report of German Plague Commission. p. 314.

Criticism of method adopted by Mr. Haffkine for determining quantity of bacterial sediment in given volume of vaccine.

421. From this we may pass on to the consideration of the method which is employed by Mr. Haffkine for determining the quantity of bacterial sediment which is contained in a given volume of the vaccinating fluid. It was obviously important to determine whether the method which was adopted was a method which gave approximately accurate results. This question is one which it seems to us must be kept rigorously apart from the question of the virulence of the particular cultures which are employed. For although it is possible that an error committed in the determination of the quantity of bacteria in a given bottle of vaccine might, in some case, be counterbalanced by the fact that the virulence of the culture had, in a particular case, been over or under-estimated, yet it is obviously possible that errors of this kind might be cumulative, and that, on the one hand, an unduly small quantity of an attenuated culture, or, on the other hand, an unduly large quantity of a culture of
26,472-99. excessive virulence, might be prescribed. In view of these considerations, which do not appear to have been sufficiently appreciated by Mr. Haffkine, and, failing the production of experiments to show that no serious results need be anticipated from the inoculation of large doses, and that adequate protection is conferred by the inoculation even of small doses, we cannot but take a serious view of the fact that, in a case like the present where the estimation of the virulence was neglected, the weight of the bacterial sediment in a prescribed dose was in some samples of the vaccine seven times
26,272-6. as large as in other samples. The details of the experiments upon which this statement concerning the weights of sediment is based are to be found in Table III. in the evidence of Lieutenant Stewart Douglas, I.M.S. In view of the results of the weighings, and further, in view of the fact that no mere estimation of opacity would enable a judgment to be arrived at as to the quantity of dissolved toxins contained in a fluid, should any such be present, it seems to us that the routine method of standardizing the vaccine which was employed by Mr. Haffkine, must be pronounced to be imperfect. So imperfect is the method of standardization that we have no data for judging what the relation of strength is between doses of the samples we examined and the original dose which Mr. Haffkine has taken as his standard, nor are we able to form any estimate of the quantity of bacterial sediment which was contained in the original "standard dose." Assuming that the results obtained in our weighings represent the extreme limits of variation, the quantity of bacterial sediment employed may have consisted of any amount from 8 to 56 milligrammes.

II.(5.) *Character and Effects of the Liquid and Solid Elements of the Vaccine and Question of the Dose employed.*

422. It has been seen above that the volume of the vaccinating injection which is prescribed by Mr. Haffkine has varied between $2\frac{1}{2}$ and 20 c.c. Further, we have seen that the volume of the injection may have amounted, on the average, to something like $7\frac{1}{2}$ c.c. In view of the fact that considerable difficulty is experienced in the injection of such large quantities of vaccinating material, and, further, in view of the fact that the menstruum (which, since it contains peptone, is a toxic menstruum) bears here so large a proportion to the vaccinating substance proper, it becomes a matter of importance to consider in detail the composition of the vaccine.

(VII. and VIII.) Criticisms of composition of vaccine and of volume injected. Para. 405 of this Report.

Mr. Haffkine, in accepting these large quantities as necessary in cases where a scanty growth of plague has occurred in his nutrient medium, bases himself on three confessedly *à priori* and provisional assumptions. The first of these assumptions is, that the bacterial sediment protects the patient against being attacked by plague, while it does not in any way improve his chances of survival when he has been attacked. The second of these assumptions is, that the supernatant fluid of his anti-plague vaccine secures to the patient a greater chance of recovery when attacked. The third of these assumptions is, that the best composition of the vaccine is secured when the bacterial sediment and supernatant fluid are present in the vaccine, "in the proportion in which these elements are obtained in the process" of manufacture adopted by Mr. Haffkine. Mr. Haffkine adduces as a basis for these *à priori* assumptions the fact that the case mortality, *i.e.*, the percentage of deaths to attacks, has been reduced in the case of anti-plague inoculation, where the bacteria are injected in conjunction with the liquid menstruum in which they are grown, whereas the case mortality was not reduced in the case of the anti-cholera inoculations, in which the vaccine material consisted of cholera bacteria which were suspended in a merely watery menstruum. But, even if we assume the correctness of this particular statement, Mr. Haffkine's conclusion does not necessarily follow. For so numerous are the points of difference between cholera and plague that there can be no logical justification for asserting that a particular effect, which manifests itself in the case of one of these diseases, and not in the other, is an effect which is imputable to a particular modification of the vaccine which was employed.

Mr. Haffkine's theoretical assumptions regarding vaccine.

29. App. LXXXII. in Vol. III. of these Proceedings. 26,504-8. 26,512-26. 31.

The utility or otherwise of each separate element in the plague vaccine is manifestly a matter which can only be determined by experiment. We may, therefore, set ourselves to inquire in the case of each element, first, whether any specific toxic effect, and, secondly, whether any protective effect, is produced by the introduction of that element into the system.

423. We begin with the consideration of the deposit of plague bacteria which collects in the culture flasks.

Toxic effect of dead plague bacteria forming sediment of anti-plague vaccine.

It has been firmly established by numerous experiments in which dead bacterial cultures have been introduced into the organisms of animals—and in this connexion we may specially note the experiments which were done by the German Commission on monkeys—that the dead bodies of plague bacilli contain appreciable quantities of poisonous substances. Alterations in the blood, hæmorrhages in the gastric mucous membrane, and necrotic changes in the sub-cutaneous tissues at the site of the injection and in the liver, are the pathological effects which most often manifest themselves after the injection of plague bacilli.

Report of the German Plague Commission, p. 302.

424. It is not open to doubt that the introduction of the toxic elements, which are contained in the bodies of the plague bacilli, ultimately confers upon animals a certain power of resisting plague.

Protective effect of the sediment.

Yersin, Roux, and Borrel, in 1895, first brought forward evidence to show that a certain amount of protection could be conferred by inoculations of dead plague bacilli.

Annales de l'Institut Pasteur, 1895.

Further, we may note that this fact was confirmed by the experiments of Dr. Klein. We desire particularly to direct attention to the fact that a certain measure of protection was obtained in these experiments of Dr. Klein, for Mr. Haffkine has drawn from these experiments the conclusion that a protective measure may turn out to be effective in man, even when experiments on lower animals do not appear to contain "any elements justifying its application to men."

Local Government Board Report, 1896-7. 26,515.

The protective efficacy of injections of dead plague bacteria is further established by the many very careful experiments of the German Plague Commission. In particular, it was shown by the German Plague Commission that in the case of the

Report of the German Plague Commission, p. 302-20.

brown monkey (*macacus radiatus*), and in the case of the rat, a considerable measure of protection can be conferred by these inoculations. But the experiments of the German Commission also show that very slight, if any, protection is conferred by these inoculations on the grey sacred monkey (*semnopithecus entellus*).

The protective effect of inoculations of dead bacterial cultures was again confirmed by our own experiments. These were done on guinea-pigs, brown monkeys, and rats.

It is to be noted that in the many series of experiments which have been referred to above, there is hardly to be found an instance (unless possibly we have such an instance in the case of certain experiments on rats which were performed by Lieut. 26,311. Walton, I.M.S.), where a very high degree of immunity has been obtained. In almost every instance, severe symptoms have supervened upon the subsequent test inoculation of living plague bacteria to which the vaccinated animal was subjected. What was achieved in the above experiments was, therefore, merely a modification of the plague attack, and a delayed or diminished mortality. These results (which, be it noted, were obtained by the injection of plague bacilli suspended in a merely watery medium) are in all respects identical with the results which have been obtained on man in cases where Mr. Haffkine's inoculation has not succeeded in averting an attack of plague. Thus the diminished case mortality which Mr. Haffkine imputes, in the case of man, to the effect of the supernatant fluid which forms a constituent of his anti-plague vaccine, is, in the case of animals, obtained in the absence of that supernatant fluid.

Toxic effect of filtrate from plague cultures (supernatant fluid of anti-plague vaccine).

425. The question as to whether the supernatant fluid of Mr. Haffkine's prophylactic causes, when injected into the organism, the elaboration of substances which modify the severity of a plague attack, is a question which cannot be answered until we have determined whether a specific toxin is produced when the plague bacillus is cultivated in a fluid nutrient medium. It behoves us, therefore, first to consider this question.

Loc. cit. Yersin, Roux, and Borrel came to the conclusion that there was no specific toxin produced by the plague bacillus when it was cultivated in a liquid nutrient medium.

The German Plague Commission also investigated the question as to whether specific soluble plague toxins were developed when plague bacilli were cultivated in a liquid nutrient medium. In the first series of experimental injections which were made upon animals by the German Commission with a view of resolving this question, filtrates from 10-day old cultures of plague were employed. The experiments were performed upon rabbits and brown monkeys. In each case, 5 c.c. of the filtrate was injected. No toxic effect was observed to follow from these injections. No appreciable amount of soluble toxins was therefore produced in this instance. In a second series of experiments, the German Commission employed carbolized filtrates from sterilized six-week old cultures. The experiments were in this case done upon two rats. No control experiments were done with carbolized bouillon. The German Commission inferred, from the fact that both rats died, that a small quantity of toxins may possibly after the expiration of six weeks, escape from the macerated bacterial bodies, and pass into solution in the nutrient fluid.

26,378, 26,384–5. Several additional series of experiments on this question were instituted by ourselves. The results of these experiments are incorporated in Tables I., II., and IV. of Lieut. Liston's evidence. In the first of these series of experiments the material which was employed was the (carbolized) filtrate from Mr. Haffkine's prophylactic. The animals which received respectively 2 per cent. and 4 per cent. of their body weight of the supernatant fluid, succumbed to its effects. It is manifest that the carbolic acid in the supernatant fluid may be answerable for this result. In the case of the second series 26,384. of experiments, (*i.e.*, of the experiments whose results are incorporated in Table II.) the material which was employed consisted of the non-carbolized filtrate from Mr. Haffkine's prophylactic. In this case, with the exception of an animal which died from intercurrent disease, all the animals survived. A slight amount of necrotic change was observed to occur at the site of injection in the case of the animals which had been injected with doses of supernatant fluid amounting to 4 to 6 per cent. of the body weight. In view of the fact that these necrotic changes did not occur in the control animals injected with similar quantities of Mr. Haffkine's nutrient medium, it is manifest that a small amount of some specific toxin must here have been present in the filtrate from Mr. Haffkine's prophylactic. We are disposed to believe that the small amount of toxin may have been derived from the macerated bodies of the plague bacilli. The material which was employed in the third series of experiments consisted of a filtrate from a culture of virulent plague, which had been grown in ordinary peptone broth. The results of this series of experiments are incorporated 26,432.

Table IV. of Lieutenant Liston's evidence. It was here established that when the ague bacillus is grown in an ordinary peptone broth toxins which are lethal to uinea pigs may, under certain conditions, be produced. In the experiments in uestion, the animals succumbed when the amount of the filtrate amounted to or ceeded 5 per cent. of their body weight.

426. The first experiments which were made to determine whether any protection was nferred by injections of filtrates from plague cultures were made in 1895 by Yersin, almette, and Borrel. These observers arrived at the conclusion that such injections nferred no protection. No protective effect obtained from filtrates of plague cultures. Loc. cit.

The German Plague Commission re-investigated this question. They experimented on two monkeys. Each of these monkeys received a subcutaneous injection of 5 c.c. the filtrate of a 10-day old bouillon culture. Both monkeys succumbed to a bsequent test inoculation of plague. In the case of one of the monkeys no protection, d, in the case of the other, only a very slight amount of protection would appear to ve been conferred. This latter effect is explained by the German Commission by the sumption that the filtrate which was used in this case must have contained a certain antity of substances which were derived from the macerated bodies of the plague cilli. Report of German Plague Commission, p. 356.

In addition to the experiments which have just been recorded, the German Commission made experiments on monkeys with the supernatant fluid, which was filtered from a sample of Mr. Haffkine's prophylactic, and they compared the effects of e supernatant fluid with the effects of the bacterial sediment. The results of these periments led the Commission to the conclusion that the whole protective power of r. Haffkine's vaccine was resident in the bacterial sediment, as distinguished from the pernatant fluid. Report of German Plague Commission, p. 320.

The above results are in absolute conformity with the results which were obtained ourselves in two series of experiments.

In the series of experiments recorded in Table III. of Lieutenant Liston's evidence, 26,385. will be seen that the only guinea-pig which had survived the inoculation of the rbolized filtrate from Mr. Haffkine's prophylactic fluid succumbed with extreme pidity when tested with a living culture of plague. This guinea-pig had obviously t acquired any power of resisting plague.

The results of the second series of experiments are recorded in Table VI. of 26,434. eutenant Liston's evidence. In this series of experiments the prophylactic power the supernatant fluid of Mr. Haffkine's vaccine was compared with the prophylactic ect of a freshly-prepared sample of his nutrient medium. It was found that the erage period of survival of (*a*) the three guinea-pigs who received only injections the nutrient medium, and (*b*) of the three guinea-pigs who received comparable antities of the filtrate from Mr. Haffkine's prophylactic fluid, was in each case days. It is manifest, therefore, that if there is any specific soluble plague toxin in e filtrate from Mr. Haffkine's prophylactic fluid—and we have seen that this is ubtful—this specific plague toxin must be a toxin which confers absolutely no otection against plague.

427. The evidence which has been reviewed above appears to place beyond doubt e fact that the injection of the bacterial sediment does in the case of animals ercise an effect in the direction of the production of immunity. This immunity ay be absolute where the test-quantum of plague culture which is introduced into e organism does not exceed a very small minimum. In such cases as this, the ague bacilli are destroyed before they can produce any noticeable symptoms. In ier cases, what is achieved is merely a modification of the plague attack. This ect comes under observation wherever the test-quantum of plague culture which is roduced into the system is somewhat larger than the maximum quantity which the stem is capable of effectually dealing with. Summary of criticisms on composition and volume of Mr. Haffkine's anti-plague vaccine.

Further, the evidence which has been considered above indicates that the rate which is derived from the plague culture (the supernatant fluid of Mr. Haffkine's ccine) does not lead to the elaboration of any substances which are capable of vourably modifying a plague attack. It neither confers upon animals any immunity ainst attack, nor does it, so far as can be seen, operate in the direction of reducing rtality. It would appear, therefore, that the supernatant fluid might with advantage be pensed with, in the case of man, and it might readily be eliminated from the vaccine a mere process of decantation. If it was so eliminated, several advantages would be ned. The task of inoculating would be rendered very much less laborious. The luction of the volume of the vaccinating material would tend to diminish the dangers ich might result from the possible contamination of the vaccine. By the elimination the supernatant fluid there would be eliminated from the vaccinating material

a considerable quantity of a poisonous substance (peptone)* which stands in no known relation to the production of immunity.

II.(6.) *Summary of Criticisms on mode of Preparation of Haffkine's Vaccine.*

General summary of criticisms on mode of preparation of Mr. Haffkine's vaccine.

428. While making the criticisms which we have been compelled to make above, on the methods which are employed by Mr. Haffkine, we have not lost sight of the fact that the only essential points in an anti-plague vaccine are, first, that it shall be capable of preventing and of favourably modifying attacks of plague; secondly, that it shall be capable of conferring an effect that will last some time; and thirdly, that it shall be capable of producing these good results without entailing any serious evil consequences.

Of subordinate importance to these are the questions, first, of the complete freedom from contaminations, and secondly, of the correct standardization of the vaccine. These have a practical importance only in so far as such defects may be the cause, on the one hand, of unnecessary suffering to the patient, and, on the other hand, of inadequate protection.

In other words, we recognise that even though a vaccine which is insufficiently standardized, and which is occasionally contaminated, is from the scientific point of view a very imperfect vaccine, yet judged from the standpoint of practical life such defects may very well be overlooked if the insufficient standardization and the occasional contamination of the vaccine have not interfered in a sensible manner with its utility. This standpoint, which is indeed the only reasonable standpoint, is the standpoint which has been taken up by Mr. Haffkine in the statement that was handed in by him, and which is published, at his request, in our Proceedings.

App. LXXXIII. in Vol. III. of these Proceedings.

Innocuousness of contaminating micro-organisms present in anti-plague vaccine cannot be deduced from results of inoculation of small-pox vaccine.

Several of the statements which he makes in that memorandum do not, however, altogether commend themselves to us. We are of opinion, in the first place, that the importance of the presence of the contaminating micro-organisms in the anti-plague vaccine is not diminished by pointing out that such contaminations are present in the vaccine lymph which is employed in anti-small-pox inoculations. On the contrary, we think that the disadvantages which are associated with the presence of contaminating micro-organisms in small-pox lymph cannot be overlooked. Again we are of opinion that while the bacterial contaminations of small-pox lymph cannot at present be entirely avoided, it would be possible in the case of a bacterial vaccine such as the anti-plague vaccine, to take effective precautions against the presence of these contaminating micro-organisms. We consider this the more necessary inasmuch as in the case of anti-plague vaccination considerable quantities of fluid are introduced sub-cutaneously, whereas in the case of small-pox contaminating micro-organisms are only placed on the surface of the skin. In view of this fact, we do not consider that Mr. Haffkine is justified in asserting that the contaminating micro-organisms which find their way into his vaccine are harmless, "as are harmless also those innumerable microbes that we eat and drink and breathe in our daily life, or as is harmless an apple, or an onion, or a mango, or any other living vegetable fruit that we eat uncooked." Manifestly we have here a comparison between things that are not in any respect comparable. The contaminating micro-organisms are, in one case, introduced into the alimentary canal, where they are innocuous, and, in the other case, into the sub-cutaneous tissues, where they may quite well be harmful.

App LXXXIII. in Vol. III. of these Proceedings.

Such statements, however, apart, we find ourselves in agreement with Mr. Haffkine that the good and the evil results of inoculation must be judged, not from experimental data obtained in the laboratory, but rather from the results which have been actually obtained in man.

III. Serious Ill-effects not produced in Man by Inoculation of Haffkine's Vaccine.

No serious ill effects have been produced by inoculation of Mr. Haffkine's prophylactic fluid.

429. We may dismiss in a few words the question of the evil effects which have resulted from Mr. Haffkine's inoculation. We have not had any evidence laid before us which goes to show that any serious ill consequences have resulted from the defect in sterility which was spoken of above. We made careful inquiries into this question in the various centres where anti-plague vaccinations have been performed. In the overwhelming majority of cases no evidence was adduced to show that any injurious effects had supervened upon inoculation. In a few isolated cases, however, there was evidence that abscesses had occurred in the vaccinated at the seat of injection. It is of course, not certain that these abscesses were imputable to the condition of

1753, 4743, 4781, 10,714, 11,705, 16,568.

* We have not made any direct experiments on the toxic effect of peptone on man, nor do we know of experiments made by Mr. Haffkine to determine whether any toxic effects result from the inoculation of culture medium in man. But we conceive that it is hardly possible that a dose of 20 c.c. of a pept solution, which may possibly contain from 3 to 6 per cent. of peptone, can be quite indifferent to the hum organism.

ccinating material. On the other hand, it must be noted that in many cases the ng-continued reaction at the seat of inoculation and the continuance of the fever emed to point to the presence of a certain amount of septic contamination. The erman Commission also had this question under consideration. They state that ey were not able to determine "whether the observed symptoms were in these ses purely toxic effects." Report of the German Plague Commission, p. 320.

We have to consider, in connexion with this question of the possibility of evil sults imputable to the anti-plague inoculation, a few cases in which death occurred ddenly in a few hours after inoculation in the case of persons who before inoculation peared to be quite healthy. We have come to the conclusion that these cases are not be imputed to any defect in the anti-plague vaccine. In addition to the fact that ey have been quite isolated occurrences, there is the fact that no such cases of death ve been reported to us as having occurred in two or more persons who were oculated with the same vaccine. These cases of sudden death have always occurred a time when plague cases were actually occurring, while they have not occurred in absence of a plague epidemic. We are, therefore, of opinion that such sudden aths are probably imputable to the fact of the inoculation having been made into a rson who had already contracted the infection of plague. 2388, 4605. 4973.

Having said this, we may now pass from the consideration of these preliminary estions which relate to the composition and preparation of the vaccine to the nsideration of the results which have been obtained in actual practice by its plication.

History of First Series of Inoculations with Haffkine's Vaccine, with Criticism of Statistics relating to the Results, and of Conclusions drawn by Mr. Haffkine from these Statistics.

IV.(1.) *First Inoculations in Bombay.*

30. Mr. Haffkine first tried the physiological effect of the prophylactic fluid on nself. His description of the effect produced may here be introduced as representing ordinary symptoms which follow inoculation. He says:—"The amount with which as injected was 10 cubic centimetres, of which 6 c.c. were injected into my left nk, and four into my right. The symptoms, as far as they were perceptible, were follows. Three or four hours after the injection my temperature began to rise, reached the maximum 102½° F. in 8½ hours. From that time my temperature an to fall. At the seat of injection, almost parallel with the development of the er, a tenderness began to develop, and reached its maximum next morning, which about 15 hours after the time of injection. The region under my left arm, newhat below the axilla, became tender on pressure, though, in my case, no ndular swelling formed. The seat of the inoculation reddened, and swelled and ame so painful that I had great difficulty in getting up next morning. Still I able to do so, and at no moment was I obliged to interrupt my work. I was culated on the 10th January (1897), and on the 11th an important meeting to be held, presided over by the Director-General of the Indian Medical Service, geon-General Cleghorn. I was able to take part in the meeting and until my ptoms had entirely disappeared, scarcely anyone knew that I had been inoculated, ept the operator (Dr. Surveyor) and the Principal of the Grant Medical College, had witnessed the operation. The fever disappeared between 20 and 24 hours r injection, when my temperature reached normal. The pain at the seat of ulation, however, lasted for several—four or five—days and after the pain ppeared the nodule remained for a couple of weeks longer." Effect of inoculation of prophylactic fluid on Mr. Haffkine. 32.

31. After Mr. Haffkine had thus tested his fluid on himself he proceeded to inoculate nteers from the general public of Bombay, with doses that he estimated to correspond n one quarter of that which had been used in his own case. He says:—"In the se of the first three weeks, from the 10th until the end of January (1897), ral hundred persons of all ages and sexes offered themselves to be inoculated the effects were carefully observed, on them. In no instance did we see any- g which would point to a marked difference from the effect of the cholera ulation, or which would indicate the possibility of evil consequences to the ral health. This result impressed itself so much on the minds of those who erwent the inoculation or studied its effect on others that in the course of the few months 8,142 people in Bombay came to avail themselves of the new ment." Of these at least "356 belonged to the higher classes; 2,119 to the st; and the rest were between the two. The vast majority of these cases persons who read newspapers, and who had physicians to consult. Under these itions the fact of the 8,142 people escaping infection would probably have only ry relative bearing on the question as to the efficacy or otherwise of the First inoculations in Bombay 32. 48-49.

inoculation if it had not happened that out of the 8,142 people 18 did get infected, all natives of India, chiefly of the class of servants, and went through the full course of the disease. Out of the 18 persons 16 recovered, and the two who succumbed had developed unmistakeable symptoms of plague within 24 hours of inoculation. From the history of these two cases it appeared probable, and in one case I know now for certain, that they had symptoms of plague on them already at the time of inoculation."

52. Mr. Haffkine says that it is almost certain that no fatal case, except the two mentioned, occurred among the inoculated, for "a large part of the persons inoculated in Bombay during the first part of 1897 were literate people who read books and newspapers, and were perfectly aware of the novelty of the treatment, whilst the vast majority of the rest were their servants." As, moreover, "the physicians in Bombay . . . kept a perfectly open mind with regard to the efficacy of the inoculation," and "exceedingly few expected any actual good from it . . . cases of plague, especially fatal cases, would not have escaped our attention or the attention of those who were observing the progress of this work." On the contrary, says Mr. Haffkine, "whatever seemed to be abnormal in the immediate effect produced by inoculation, even the most insignificant symptoms, were brought to our knowledge with detail and alarm. . . . However, it is obvious that in a large city like Bombay there was no possibility of our instigating a direct inquiry and following up every one of the 8,142 cases inoculated.

Inoculations at Mora.

52 Mr. Haffkine, however, states that "somewhat more complete data were obtained with regard to a small group of people included in the above number of 8,142, but who lived half an hour from Bombay, across the harbour, namely, in Mora village. The population of Mora when the plague broke out was under 1,000, and out of this number 429 underwent inoculation; the rest remained uninoculated," the number of the uninoculated people being only approximate. Of the inoculated, seven were attacked with plague with no deaths, while among the uninoculated 26 cases with 24 deaths were reported. "I quote this instance," says Mr. Haffkine, "because it produced a great impression upon the inhabitants of Mora, and because its influence was reflected upon Bombay. But owing to the great pressure of work at the time I could not follow up the cases in Mora to such an extent as to be able to guarantee that the figures are absolutely correct."

27,310–8. The attacks which took place appear to have all occurred before the beginning of February 1897. If, for the reasons given by Mr. Haffkine, we eliminate from consideration the two cases that proved fatal, the results with reference to the total number of persons inoculated, for a period which has not been specified, may from Mr. Haffkine's statement, be summarised as follows:—

Number of Persons Inoculated.	Attacks.		Deaths.	
	Number.	Per cent.	Number.	Per cent.
8,142	16	·2	0	0

The results for the group of inoculated persons, included among the above, who were resident in Mora, are, according to the evidence laid before us, as follows:—

——	Number.	Attacks.		Deaths.	
		Number.	Per cent.	Number.	Per cent.
Uninoculated - -	571 (?)	26	4·5	24	4·2
Inoculated - -	429	7	1·6	0	0

There are, however, no accurate data enabling us to form an opinion as to how many of the total of 8,142 inoculated actually escaped. Many of them belonged to the lower classes, and it is by no means certain that deaths from plague among them would have been reported, especially as many may have left Bombay after the inoculation. Moreover, no death was reported among the 16 persons who were attacked, and such reduction of case mortality has never been noticed elsewhere.

IV.(2.) *Inoculations in the Byculla House of Correction, Bombay.*

Her Majesty's House of Correction, Byculla. 34–49, 20,351–2. 432. An opportunity soon offered itself for testing the prophylactic fluid under conditions that permitted of strict observation of the results of inoculation. Plague broke out on the 23rd January 1897 in Her Majesty's House of Correction at Bomb

nd between the 23rd and the afternoon of the 30th of the month, when inoculations ere resorted to, 14 cases of plague had been recognised, of which seven proved tal. The official figures represent that of 338 prisoners present in the jail, 153 were noculated and 185 were not inoculated. But from the detailed list of prisoners III. 445-56.
pplied to us, on which these figures are obviously based, it would appear that from ne number of 185 not inoculated, there ought for the purposes of our comparison be subtracted the 14 persons who had been recognised as plague patients before noculations commenced. Moreover, correspondence which we have had regarding the ficial figures, and which is published as App. No. LXXXVIII. in our Proceedings, has III. 658-61.
nown that one prisoner enumerated in the Jail list as among the inoculated, was reality not inoculated. The numbers of the non-inoculated and inoculated, after ne necessary corrections, would stand respectively as 172 and 152. With these we ay compare Mr. Haffkine's figures, first as given in evidence before us, and secondly given in a communication addressed by him to the Royal Society, and published the "British Medical Journal" of the 1st July 1899. Before us Mr. Haffkine said 34.
ne number of prisoners in the jail was 327, of whom 154 were inoculated. In his mmunication to the Royal Society he gave the total number of prisoners as 337, nd the numbers of the inoculated and uninoculated respectively as 183 and 154. hese discrepancies are of little importance. We have for convenience adopted the ller statement that Mr. Haffkine made before the Royal Society to represent his gures.

The course of the plague in the jail up to the date when it ceased, and the currences of attacks and deaths among the inoculated and the uninoculated spectively, according to the corrected official figures, and also according to the gures laid by Mr. Haffkine before the Royal Society, are exhibited in the following ble in parallel columns :—

—	Non-inoculated.				Inoculated.				Names and Official Numbers of Inoculated Prisoners who contracted Plague.
	M. Haffkine's figures.		Official figures.		M. Haffkine's figures.		Official figures.		
	Cases.	Fatal Cases.	Cases.	Fatal Cases.	Cases.	Fatal Cases.	Cases.	Fatal Cases.	
January 30th, 1897, nclusive.	15	8	14	7	3	3	0	0	
culations performed on anuary 30th, 1897.	M. Haffkine's figures show 183 prisoners not inoculated and 154 inoculated. The Official figures show 172 prisoners not inoculated and 152 inoculated.								*a.* 672.—Keshav Umishankar. *b.* 832.—Rama Lakshman. *c.* 1,222.—Govind Pandu. *d.* 1,356.—Abdul Karim Golam Hussain. *e.* 1,336.—Ganput Babajee. *f.* 1,366.—Shamrao Sittaram.
uary 31st, 1897 - -	2	1	2	1	1	0	0	0	
ruary 1st „ - -	1	1	1	1	0	0	4 *a. b. c. d.*	3 *a. c. d.*	
„ 2nd „ - -	1	1	1	1	0	0	1 *e.*	0	
„ 3rd „ - -	0	0	0	0	0	0	0	0	
„ 4th „ - -	1	1	1	1	0	0	0	0	
„ 5th „ - -	2	1	2	1	0	0	0	0	
„ 6th „ - -	5	1	4	1	1	0	0	0	
„ 7th „ - -	0	0	1	0	0	0	1 *f.*	0	
Total January 31st to February 7th -	12	6	12	6	2	0	6	3	

On looking at this table, two discrepancies will be noticed which have a bearing on nclusions drawn regarding the relative immunity of the inoculated and the non-oculated. One relates to the case of an inoculated prisoner, Ganput Babajee, who pears in the official statement as having been attacked with plague on the 2nd bruary. This man is omitted from Mr. Haffkine's statement, and the omission is III. 659 (*f*).
plained on the ground that the diagnosis of plague was erroneous. But we have not cluded this man from our figures, as he was definitely held by the medical officers the jail to have suffered from plague, and may, like two inoculated prisoners whose es were brought to our notice in connexion with inoculations in the Umarkhadi 69, 7236 (I.) and
l at Bombay, have been suffering from a modified attack of the disease. The (II.).
ond discrepancy relates to three patients who are excluded from consideration by 34, III. 659 (*b*).
. Haffkine in calculating his results, on the ground that they had already contracted gue at the time when the inoculations were performed. If, however, cases are to be

excluded for the reason that the full incubation period had not elapsed between the date of inoculation and the date of attack, we should, assuming that the incubation period may last as long as five days, omit not only the three prisoners omitted by Mr. Haffkine, but also every one who was attacked up to the 4th February. If this was done, and we think that it ought to be done, there would remain seven cases among the uninoculated, with two deaths, contrasted with one case, which recovered, among the inoculated. The comparison would thus be greatly in favour of the inoculated. The results, however, do not seem to justify the conclusion drawn from them by Mr. Haffkine that protection is acquired within 24 hours of inoculation, for during the first five days after the inoculations, there were five cases among the inoculated as compared with four among the prisoners who remained uninoculated.

47.

IV.(3.) *Inoculations in Daman.*

Lower Daman. 52-60.

433. After the experiment in the Byculla House of Correction, inoculation was next tried at Daman, where plague had broken out in March 1897, as already stated in para. 45 of this Report. The circumstances connected with the inoculation operations at Daman were made the subject of a Special Joint Report by Mr. Haffkine and Major Lyons, I.M.S. The Report was based on inquiries made at Daman on the 20th and 21st of May, at which Drs. Koch and Gaffky, the heads of the German Plague Commission, assisted, and upon a house to house visitation carried out at the end of the month by Major Lyons. The results of the different series of inoculations that were done are separately grouped in the Joint Report, where the results that were obtained in the first three series are considered.

App. II. in Vol. I. of these Proceedings.

Normal population of Lower Daman.

Before entering into the results of the inoculations, we must consider what was actually the population of Lower Daman at the commencement of the period of observation. Mr. Haffkine and Major Lyons definitely state that the normal population of Daman Proper, Katheria, and Kariwari, which constitute the town of Lower Daman where the inoculations were performed, is 5,000, 4,700, and 1,200 respectively, amounting in all to 10,900. This statement was not based on a special census, but on information received from His Excellency the Governor that the population amounted to 11,000, and from a Parsee gentleman, who introduced inoculation into Daman, that the population amounted to 10,000. Major Lyons, under examination, admitted that the figures were arrived at "by a guess."

I. 305.

27,214-29.

27,229.

Population at time when inoculations instituted.

From the normal population thus arrived at, the population actually present at the commencement of the inoculations was computed by making certain deductions. A deduction was made for the diminution of the population due to exodus. In the Joint Report it is estimated that 2,000 people had left the place before the 23rd March, 1897, but this figure, like that of the normal population, was a pure guess, apparently made by Mr. Judge, Assistant Collector of Salt Revenue. Another deduction was made for a decrease in the population on account of deaths from plague since the outbreak commenced. This again was made in a somewhat loose manner. It was based upon an estimate formed by His Excellency the Governor that the deaths in February might be neglected, and that the deaths in March might be assumed to amount on an average to 30 a day; but it is clear that there was no system of death registration which was at all trustworthy or capable of affording assistance in arriving at a correct estimate.

I. 305.

27,239-41.

I. 305-6.

Results claimed from inoculation.

The inoculations which were performed in each of the three series, and the results which are claimed for them, may be tabulated as follows from particulars given in the Joint Report, viz.:—

I. 308-9.

FIRST SERIES.

Inoculations performed from the 23rd to the 26th March 1897.

Results claimed to have been obtained for the period from the 26th March to the 23rd April:—

Number.		Cases.	Deaths.	Total Plague Mortality.	Case Mortality.
				p. c.	p. c.
Inoculated - { Dr. Kalapesi - 846		17	3	·4	17·6
Inoculated - { Dr. Poiares - 171		6	3	1·8	50
Total - - -	1,017	23	6	·5	26
Uninoculated - - -	7,213	—	716	9·9	—

Second Series.

Further inoculations performed from the 17th April to the 2nd May 1897.

Results claimed for the period from the 2nd May to the 19th May:—

—	Surviving from First Series.	Inoculated in Second Series.	Total.	Cases.	Deaths.	Total Plague Mortality.	Case Mortality.
Inoculated — Dr. Kalapesi	843	529	1,372	52	18	p.c. 1·3	p.c. 34·6
Inoculated — Dr. Poiares	168	99	267	12	9	3·3	75·0
Total			1,639	64	27	1·6	42·0
Uninoculated			5,869	—	674	11·5	—

Third Series.

Further inoculations performed from the 21st to the 23rd May 1897.

Results claimed for the period from the 23rd May to the end of May:—

—	Surviving from First and Second Series.	Inoculated in Third Series.	Total.	Cases.	Deaths.	Total Plague Mortality.	Case Mortality.
Inoculated, Dr. Kalapesi and Dr. Da Cunha.	1,354	552	1,906	1	1	p.c. ·05	p.c. 100
Inoculated, Dr. Poiares	258	0	258		2	·8	66
Total			2,164	4	3	·1	75
Uninoculated			4,643	—	93	2	—

Mr. Haffkine and Major Lyons sum up the results as follows:—"During the three periods of observations put together, between the 26th March and the end of May, 4,033 uninoculated had 1,482 deaths (24·6 per cent.), while 2,197 inoculated had 36 deaths (1·6 per cent.). . . . This represents a difference in mortality of 89·2 per cent." I. 308.

Criticism of figures tabulated above.

It is to be noted that between the dates when the first period of observation closed and the second period opened there is a gap of eight days, while there is a similar gap of three days between the second and third periods of observation. But a scrutiny of the statements shows that the figures of the population—and this is particularly important in connexion with the uninoculated part of the population—at the opening of the second and third periods of observation respectively, can be arrived at from the figures of the preceding period by a simple subtraction of the deaths set down for that period, and by a transference of the number of newly inoculated persons from the category of the uninoculated to that of the inoculated. It is obvious that Mr. Haffkine and Major Lyons have altogether omitted to take into account the deaths which must have taken place in the gaps which we have noticed. We have no means of telling exactly how many deaths have thus been left out of account, but we may remark that there is a table in the Joint Report itself from which we have ascertained that 327 deaths are recorded as having occurred between the 25th April and the 2nd May. Further, we have not been informed how the inoculated who were operated upon between the 17th April and the 23rd April inclusive were brought to account in case any plague occurred among them. Moreover the number of inoculated persons with which the third period of observation begins includes 54 persons who had been attacked by plague during the two preceding periods, and were, for the most part, probably convalescent. These persons ought after their attack to have been excluded from further consideration. A similar deduction ought to have been made for the number of the uninoculated persons who had been attacked. As the number of attacks among the uninoculated is not given, we have, I. 306.

taking as our guide the case mortality given for the uninoculated in the Daman investigation sheets, presently to be referred to, estimated that the number of uninoculated persons who had been attacked in the first two periods of observation must have been about 417. Lastly it is important to notice that the method of computation adopted in the Joint Report admits of another serious cause of error, inasmuch as it makes no allowance for diminution of population by exodus.

Method of obtaining cognisance of deaths at Daman.

I. 305.

The next point to consider is whether the data concerning attacks and deaths incorporated in the Joint Report are themselves accurate. Up to the 25th April, which covers the first period of observation, the daily mortality "was recorded officially by information from the inhabitants coming to the registration office to declare cases of deaths in their families." To show how inaccurate the official registration of deaths was at that time, it may be stated that "on the 25th April the Governor placed a guard of his own on the burial and burning grounds, and on that day, though the number of deaths reported by the inhabitants was 11, the number of bodies disposed of in the presence of the guard was 34." It is evident that during the first period of observation the number of deaths as registered by the authorities is quite inaccurate. From the 25th April an official guard was kept permanently on the burial and burning grounds by His Excellency the Governor. From this date onwards the official information available regarding the number of bodies disposed of is, we may presume, more accurate. But the official measures provided no means for ascertaining the occurrence of plague till after death, and in view of the fact that inoculation was not introduced on the initiative of the Portuguese Government, no official steps appear to have been taken to ascertain whether any of the dead had been inoculated.

I. 306.

Mr. Damaun Valla's statistics.

The official information regarding the mortality and attacks in Daman is, however, supplemented by the inquiries instituted and carried on by a Parsee gentleman, Mr. Seth Sorabji Damaun Valla, who was particularly interested in the inoculations. We must pay particular attention to the investigations of Mr. Seth Sorabji Damaun Valla, for the Daman figures appear to be almost entirely dependent on his statistics, or rather on the figures gathered by his peons and servants. He states

25,427.

that he kept a watch over the people inoculated in Daman by employing about half-a-dozen agents who knew all the people. Whenever people were inoculated, his servants inquired every day to ascertain how they were getting on. Those who were inoculated also had instructions to let him know if there was any sickness among them. Then he had his sepoys and servants at the burial and burning grounds at the

25,430.

beginning of plague, and they brought information if his other people failed to give it. They inquired whether bodies brought for disposal were inoculated, and ascertained the name, and reference was afterwards made to the inoculation registers. Asked how he was satisfied that the answers they gave him were true answers, he stated that:

25,432–3.

"Because whenever they were ill I used to know it, and gave them medicine as I gave to others also. Generally people came to me running whenever they got ill and my men always used to go and inquire who was ill amongst the inoculated people." We can hardly believe that the information thus obtained can be accurate, and, in support of this opinion it will only be necessary to quote an answer given by Mr. Seth Sorabji Damaun Valla to a question, as follows:—

25,446.

Q. "Amongst the 2,200 people inoculated in the first season have there been, up to date, any deaths from causes other than plague"?

A. "Amongst the inoculated I have not found a death from any cause other than plague."

We are not disposed to admit that a population of 2,200 people had no death among them for two years. But it should be noted here that the authors of the Joint Report in referring to the statistics collected by Mr. Seth Sorabji Damaun Valla state that

I. 306.

"these details were found afterwards, in the course of a close investigation by us, to be scrupulously correct." Mr. Haffkine states that "considering the small extent of the

I. 306.

town, the limited number of inhabitants, and the number of medical officers who were looking after the place, it appeared probable that not many attacks among the inoculated escaped their notice, as such attacks usually excite much comment among these inhabitants, and attract general attention. Indeed, the gentlemen present at the meeting (on May 20th) seemed to know between them every place in the town where inoculated people lived, and every house which had suffered from plague or happened to escape. As has already been mentioned, they stated at the time that there was a small number of inoculated attacked whom they did not see, but of whom, they informed us a record existed in the office of Seth Sorabji Damaun Valla. . . . He had a personal knowledge of the people who were inoculated, and kept a

watch over the effect of the operation, collecting with the help of his office establishment a large number of details in this connection." Mr. John Moniz, a medical officer, who was in the Portuguese Government service during the year 1897, and is one of those referred to in the Joint Report, in answer to the question, whether it was possible to find out regarding each dead body whether the person had in life been inoculated or not, stated that: "In the year 1897, there was so much confusion and panic that it was impossible to take down these particulars." We can hardly doubt that this statement of Mr. Moniz correctly represents the state of affairs, if we note the extreme virulence of the plague in Lower Daman. In a population estimated to number ordinarily about 11,000 or 12,000, the official figures, defective as they may be, record a total of 2,189 plague deaths from the time of the outbreak in February to the close of the period of observation in May, and a house to house inquiry made by Mr. Seth Sorabji Damaun Valla resulted in the confirmation of 2,093 deaths up to the 19th May 1897.

16,667.

I. 306.

Conclusions of the German Plague Commission regarding Daman.

Report of the German Plague Commission, p. 333.

We may note that Professors Koch and Gaffky have expressed their opinion that "the material which was collected left much to be desired from the point of view of completeness and trustworthiness, as indeed could only have been expected." "Still," they say, "an impression which may, probably, be taken to be approximately correct, was obtained" by them regarding the results of inoculation at Daman. This impression appears to have been strengthened by the results of Major Lyons' enquiries which are incorporated in the Joint Report. The German Plague Commission have generally adopted the statistics set forth in the Joint Report.

Conclusions regarding statistics for Daman as a whole.

Our own conclusions as regards the general effects of inoculation in Daman may be stated as follows. Considering (1) that the estimate of the population of Daman, on which statistics regarding plague incidence in the town are based, is only approximate; (2) that the total number of inoculated and non-inoculated people who left Daman is unknown; (3) that the total mortality in Daman for the period of observation, and particularly up to April 25th, that is during the height of the epidemic, is unknown; (4) that the proportion of deaths reported to have occurred among inoculated and non-inoculated respectively, at Daman, is based almost, if not entirely, on the reports of the peons, sepoys, and servants of Mr. Seth Sorabji Damaun Valla; and (5) that there are serious errors in the methods by which results have been calculated in the Joint Report; we are of opinion that no exact conclusions can be drawn from the data at our command which relate to the town as a whole. Nevertheless, while unable to accept the statistics as accurate, we attach some importance to the fact that Mr. Damaun Valla and the medical men who observed the effects of inoculation during the outbreak were satisfied that it exerted a great protective influence.

Inoculation among Parsees at Daman.

I. 306–7.

Having now discussed the figures for the town as a whole, we may consider what conclusions can be drawn from particular series of figures obtained from a narrower field of observation. The first series of data which we may consider in this connexion relates to the Parsee community of Daman. The results as collected by Mr. Seth Sorabji Damaun Valla with reference to 306 members of the community are as follows:—

—	Number under observation.	Cases.	Deaths.
Non-inoculated - -	29	—	4
Inoculated - -	277	8	1 (probably infected before inoculation).

These results are very favourable to inoculation, but in the absence of other details are not sufficient to form a basis for a conclusion.

Daman investigation sheets.

I. 307.

16,862–77.

27,210–3, 27,284–6, 27,297, 27,301.

Another series of data is that contained in the record of Major Lyons' house to house investigations. This officer went to Lower Daman on the 26th May 1897, the object of his visit being to find out accurately by minute inquiry in each house the results of the anti-plague inoculations which had been done. For this purpose he divided the town into four sections, and during four days, between the 26th and 30th May 1897, visited all the houses section by section, and he made inquiries at every house inhabited by inoculated persons which was pointed out to him as one in which a plague case had occurred. In each case he obtained information at the house visited from some one inmate who was "fairly intelligent." As a number of the

dwellings had been evacuated and the inhabitants were encamped in the fields outside the town, Dr. Lyons visited them in their encampments and collected information from them, in the same way as he collected information from the houses.

I. 310-30. The total results of the inoculations which we have summarised from Major Lyons' investigation sheets regarding 89 houses are as follows:—

—	Number.	Attacks.		Deaths.		Case mortality Per cent.
		Number.	Per cent.	Number.	Per cent.	
Uninoculated - -	123	55	44·7	38	30·9	69
Inoculated - -	382	91	23·8	36	9·4	39·6
Diminution of mortality in favour of the inoculated - - -					21·5	

Out of the 89 houses to which these figures refer, in 62 houses both inoculated and non-inoculated were present, and in 20 of these the inoculated alone were attacked, while in 22 the non-inoculated alone suffered. In 20 houses both inoculated and non-inoculated were attacked.

In these 62 houses, in which uninoculated controls were present, the results obtained are as follows:—

—	Number.	Attacks.		Deaths.		Case Mortality Per cent.
		Number.	Per cent.	Number.	Per cent.	
Uninoculated -	123	55	44·7	38	30·9	69
Inoculated - -	255	50	19·6	20	7·8	40
Diminution of mortality in favour of the inoculated - - -					23·1	

The results thus are very favourable to inoculation. It is, however, to be remarked that of the people whose names appear in these investigation sheets none were inoculated till the last week of March, and more than half were not inoculated till the climax of the epidemic had passed. The effects of the inoculations are thus exaggerated, inasmuch as the period of exposure to infection was in the case of the uninoculated greater than in the case of the inoculated.

Comparison between results in investigation sheets and in Daman.
I. 308.

The total mortality from plague among the uninoculated in the houses where both inoculated and non-inoculated persons lived was only $1\frac{1}{4}$ times larger than the mortality, 24·6, which, according to the Joint Report, is the mortality among the uninoculated in the rest of the town, whereas the mortality among the inoculated people in these houses was about 5 times as great as the mortality, 1·6, which is the figure given in the Joint Report as that of the mortality among the inoculated in the whole of the town. This result may in part be imputable to the under-estimation of the mortality among the inoculated in the town. With reference to houses in which more than one case occurred, we think that the comparatively large proportion of the inoculated residents who were attacked is capable of explanation on the assumption that inoculation is not nearly so protective to persons who are brought into close and continuous contact with infection in their own houses as it is to those who run merely the risk of infection that is incident to living in a plague infected town.

Further results at Daman.
25,441.

To complete the history of inoculation in Daman, it may be added that, according to Mr. Seth Sorabji Damaun Valla, of the 2,200 people inoculated at Daman during the first season, 46 died of plague in the first season (1897), 19 in the second season (1898), and two in the third season (1899). Of the 1,115 people inoculated in 1898 at Daman, and of the 200 people inoculated during the same season at Katheria, 12 died of plague in 1898 and four in 1899. Of the 397 people inoculated in 1899 no one had died of plague up to March 15th, 1899.

IV.(4.) *Inoculations in Lanauli.*

434. The next series of inoculations were performed at Lanauli, a hill station within four hours of Bombay on the way to Poona. Mr. Haffkine and his assistants proceeded to Lanauli in July, 1897, about a month after plague had broken out again after a temporary subsidence. The total population was then below 2,000, and consisted "largely of railway officers who lived in separate quarters away from the rest of the population. Their quarters remained almost entirely free from plague, and there were only two wards in the town which got the plague badly. . . . . The whole town was divided into four wards, and . . . . the officers of my laboratory," says Mr. Haffkine, "took over the two severely affected wards. We began first of all by taking a detailed census of the population, and I have the investigation sheets referring to each family separately, and giving the ages and sexes and names and other details of the members of every household."

Inoculations in Lanauli. 60-1, 26,535. 60.

The following table has been put before us by Mr. Haffkine to show the results obtained at Lanauli:—

Occurrences of Plague in C. and D. Wards of Lanauli (put together), between the 24th July, 1897, and the end of September, 1897. III. 352.

Date.	Number of uninoculated present.	Attacks.	Deaths.	Number of inoculated present.	Attacks.	Deaths.	Date.	Number of uninoculated present.	Attacks.	Deaths.	Number of inoculated present.	Attacks.	Deaths.
24.7.97 -	711	4	4	45	—	—	13.8.97 -	316	1	1	370	—	—
25.7.97 -	636	5	5	116	—	—	14.8.97 -	315	1	—	370	—	—
26.7.97 -	621	4	3	126	—	—	17.8.97 -	314	1	—	370	—	—
27.7.97 -	568	2	2	175	—	—	19.8.97 -	313	1	1	370	1	1
28.7.97 -	544	3	3	197	—	—	20.8.97 -	312	1	—	369	—	—
29.7.97 -	472	2	1	266	—	—	22.8.97 -	311	6	5	369	—	—
30.7.97 -	460	6	4	276	—	—	23.8.97 -	305	—	—	369	1	1
31.7.97 -	430	3	2	300	1	1	26.8.97 -	305	1	1	368	—	—
1.8.97 -	398	8	6	328	3	2	3.9.97 -	304	1	1	368	—	—
2.8.97 -	373	8	6	342	3	1	4.9.97 -	303	1	—	368	—	—
3.8.97 -	341	1	1	363	1	—	6.9.97 -	302	1	1	368	—	—
4.8.97 -	336	3	2	336	1	—	7.9.97 -	301	3	3	368	—	—
5.8.97 -	331	1	—	368	1	—	13.9.97 -	298	1	1	368	—	—
6.8.97 -	329	3	1	367	—	—	23.9.97 -	297	1	1	368	—	—
8.8.97 -	323	1	—	370	—	—							
9.8.97 -	322	1	1	370	—	—	Total -	—	78	57	—	14	7
10.8.97 -	320	1	—	371	1	—							
11.8.97 -	319	1	1	370	—	—	Average daily strength -	377	—	—	323	—	—
12.8.97 -	318	1	—	370	1	1							

From these figures the results deduced by Mr. Haffkine are as follows: — 61.

—	Average Daily Strength.	Cases.		Deaths.		Case Mortality Per cent.
		Number.	Per cent.	Number.	Per cent.	
Uninoculated -	377	78	20	57	15	73
Inoculated - -	323	14	4	7	2	50

Conditions under which average strength may be used as a basis for calculating effects of inoculation.

The method of calculating results which is here adopted by Mr. Haffkine differs from the method which was applied in the case of the inoculations done in the Byculla Jail and at Daman. There the inoculations were all completed before the period of observation began, and the incidence of plague could be calculated on the absolute strengths of the inoculated and uninoculated groups. Here, on the contrary, the inoculations were continued into the period of observation, and there was consequently a transference during that period of uninoculated persons to the inoculated group; therefore, the incidence of plague must, at least for the period during which the inoculations were continued, be calculated, not on the absolute strengths, but on the average numbers of inoculated and uninoculated persons present. Inasmuch as very considerable complication is introduced when the incidence of plague has to be calculated on the average instead of upon the absolute strengths, it will be convenient here, before considering Mr. Haffkine's calculations, to discuss the conditions under which calculations of this kind will afford accurate indications of the amount of protection conferred

by an inoculation. The first point to note in this connexion is that it is essential for the criticism of any calculation which is based on average strengths to have before us in the form of a statement, such as that given above, the numbers of inoculated and uninoculated persons actually ascertained to be present by a series of periodical enumerations, and the numbers of the plague occurrences among them in the intervals between the successive enumerations. In the event of the inoculations having been commenced after the advent of plague, it is desirable to have the figures from the commencement of inoculation down to the end of the outbreak. Having from such a statement deduced the average number of inoculated persons present during the period of observation, the date should be ascertained at which the number of inoculated persons found present approximates most closely to their average strength. This date may be taken as dividing the epidemic into two phases. Throughout the phase preceding this date the inoculated are less in number than their average strength, while throughout the phase which supervenes upon this date their number would be more than the average strength. The direct contrary of this would hold, of course, of the uninoculated. This point having been clearly appreciated, it will appear, on consideration, that the incidence of plague upon the average strengths of the two groups will accurately represent the relative susceptibility of the two groups only when, on the one hand, the excess of the uninoculated in the first phase is accurately balanced by the excess of the inoculated in the second phase of the period of observation, and when, on the other hand, the severity of the plague in the earlier phase is neither greater nor less than the severity of the plague in the later phase. We may note in passing, first, that we may gauge the relative severity of the epidemic by calculating the percentage incidence of plague upon the average strength of the uninoculated for each separate phase, and secondly, that a longer period of less severe plague may be taken as counterbalancing a shorter period of severer plague. Only when the conditions referred to above are complied with will there be a guarantee that the excess of the uninoculated during the earlier phase of the epidemic were exposed to a risk of infection which was neither greater nor less than that which was incurred by the excess of the inoculated in the second phase of the period of observation. On the contrary, when the two phases of the epidemic do not counterbalance each other in the matter of severity, the incidence of plague on the two groups will fail to represent correctly their relative susceptibility to plague infection. Where, on the one hand, the plague was severer in the first phase, the comparison would, owing to the greater exposure to infection of the uninoculated, be unduly to the advantage of inoculation. Where, on the other hand, the plague was severer in the second phase, the comparison would, owing to the greater exposure to infection of the inoculated, be unduly to the disadvantage of inoculation. It will therefore be necessary to apply a correction in cases where, owing to the unequal severity of plague in the two phases of the epidemic, a consideration of the incidence of plague on the two groups does not give accurate results. This correction can be applied by striking off one or more rows of figures from the tabular statement which supplies the figures for the calculation. By striking off figures from the top or the bottom of the list of figures we plainly, in the one case, increase and, in the other case, reduce, the average strength of the inoculated, so that the opening day of the second phase of the period of observation will fall upon a later or, as the case may be, an earlier date than that upon which it falls when the average strength has been worked out upon the entire series of figures. By an adjustment of this kind it will be possible to divide up the period of the epidemic into two phases of more equal severity. It is obvious that this process of correction is anything but a simple one. For, in striking off one or more rows from the table, there will be struck off along with the figures which represent the number of inoculated and uninoculated persons, also the corresponding figures for plague attacks and deaths. Inasmuch as the estimate of the severity of plague, in the particular phase to which the eliminated figures refer, will have been based in part upon those figures, it may be necessary to compensate for this by a secondary correction. It will, in short, be necessary to resort to a system of "trial and error" to determine what period of observation will yield an average strength, such that the two phases of the epidemic which are, as we have seen, disposed on either side of the date on which the inoculated attained the average strength upon which the incidence is calculated, may be as long as possible, and may, as nearly as possible, represent periods of equal exposure to infection.

Consideration of results at Lanauli.

In accordance with the principles above laid down, we may, in connexion with Mr. Haffkine's tabular statement regarding Lanauli, first consider the question of the necessity of resorting to a calculation on average strengths. A consideration of the table will show that, owing to the practical completion of inoculations on that date, a calculation on absolute strengths would be applicable from the 5th of August onwards

to the end of the epidemic. If this had been applied the incidence of plague in the inoculated and the uninoculated groups would have worked out as follows:—

—	Number.	Attacks.		Deaths.		Case Mortality. Per cent.
		Number.	Per cent.	Number.	Per cent.	
Uninoculated - -	330	29	8·8	18	5·4	62
Inoculated - -	370	5	1·3	3	·8	60
Diminution of mortality in favour of the inoculated - - - -					4·6	—

Mr. Haffkine has instead resorted to a calculation on average strengths, and we may now examine his calculations with a view to see how far they conform with the principles enunciated above. Inspection of the tabular statement will show that the number of the inoculated reached the average strength, 323, on which Mr. Haffkine calculates the incidence of plague, on the 1st of August. Consequently this date marks the point at which the period of observation is divided into its two phases. In the first of these phases—the phase throughout which the inoculated were below their average strength—the severity of the epidemic is measured by the fact that of the average strength of the uninoculated during that phase 5·2 per cent. were attacked. In the second phase—in which the inoculated were throughout in excess of their average strength—the severity of the epidemic is measured by the fact that 15·8 per cent. of the average number of the uninoculated present during that phase were attacked. In view of this, it will be seen that the comparison which Mr. Haffkine has made is unduly to the disadvantage of inoculation. We may, however, note that the comparison is not as inequitable as might have been expected from the inequality in the severity of the plague in the two phases of the period of observation. This is probably due to the fact that, very early in the second phase, the number of the inoculated reached its maximum, and that during the long period which intervened between the attainment of this maximum and the end of the epidemic, the numbers of the inoculated and of the uninoculated did not differ very materially from the average numbers taken over the whole period of observation. That this is the real explanation of the fact that Mr. Haffkine's calculation is not more inequitable to inoculation will be obvious if we consider what his results would have been if he had taken only the period before the 5th of August. A calculation on average strengths from the figures up to this date would give the following results:—

—	Number.	Attacks.		Deaths.	
		Number.	Per cent.	Number.	Per cent.
Uninoculated - -	491	49	10	39	7·9
Inoculated - -	239	9	3·7	4	1·7

Results more unfavourable to inoculation would thus be obtained, and this is in accordance with the fact that the excess of the inoculated over their average strength in the severer phase of the period of observation which ends on the 4th of August, is relatively greater than their excess over their average strength in the severer phase of the longer period of observation which was taken into consideration in Mr. Haffkine's calculation.

We may now pass on to consider the more important question of the trustworthiness of the data on which the above calculations are based, and the method which was employed for determining the numbers of inoculated and uninoculated present in Lanauli on each day during the period of observation. A scrutiny of the figures representing the numbers of the inoculated and uninoculated present on the successive days in Mr. Haffkine's table given above shows that they cannot have been based upon a daily census. The figures given for each day can be computed, in every case but two, from those of the preceding day by the method of computation which we have already noticed as that by which the numbers of the inoculated and uninoculated in each of the three series of inoculations in Daman were computed from the preceding series. In one of the cases where this is not so—on the

Para. 433 of this Report.

4th August—the discrepancy is probably due to a clerical error; in the other case—on the 5th August—there is a difference of one, and it is to be noticed that this same difference of one re-appears in a comparison of the opening and final figures of the statement. The number of the inoculated and uninoculated present on the last day of observation, added to the figures showing the total number of persons attacked during the outbreak, is one more than the figures showing the population on the first day of observation, two months before. It is, therefore, apparent that Mr. Haffkine's figures do not allow for any exodus of the population. But Mr. Haffkine admits that he had
160. ascertained that "a certain number of inoculated and uninoculated" did leave the town during the period of observation, as they did also in the case of Kirki, which will next be considered. In view of this we cannot accept as accurate the figures laid before
69. us, which have been based on computations and not on the "investigation sheets referring to each family separately" which Mr. Haffkine has in his possession.

Apart, from Mr. Haffkine's figures, we have had evidence with regard to the effect of the inoculations in Lanauli, laid before us by Major Richard Baker, I.M.S.,
18,430. who stated that he noticed that at Lanauli "in the place where inoculation had been made use of, the town was thriving and full of people," and the other part of the town
18,431. was absolutely empty. . . . . One side had plague and the other had none."

So far as we are able to judge, therefore, on a review of all the evidence laid before us, we arrive at the conclusion that inoculation exerted a distinct preventive effect for the short time during which the epidemic lasted at Lanauli.

IV.(5.) *Inoculations in Kirki.*

Inoculations at Kirki.

435. After the outbreak at Lanauli, the inoculations were tested at Kirki, a cantonment adjoining Poona. Mr. Haffkine states that "the group of people who
68. were inoculated were the native followers of the four batteries of artillery stationed there. The total number of followers, including males, females, and children at the time of the epidemic was above 1,500. They lived in 27 Government barracks on an isolated maidan, or lawn, more or less away from the rest of the population, and were subject to a strict supervision, during the whole time of the epidemic, on the part of all the military authorities and of a special medical staff. The inoculated and uninoculated belonged to an absolutely uniform class of the population. A large number of those inoculated were members of the same families as the uninoculated."

21,229-35. 21,975-21,981. When at Poona we endeavoured to obtain further information with regard to the inoculations at Kirki from Colonel Fawcett, R.A.M.C., the Principal Medical Officer at Poona. This witness, however, stated that, owing to various reasons, the Medical Officer in charge at Kirki at the time had been unable to keep a proper record of the inoculations, so that he was not in a position to bring forward any facts.
26,351-5. Mr. Haffkine, however, was himself on the spot, and personally with two assistants collected the information he has supplied to us. He was in Kirki, he says, "during nearly the whole of the epidemic, and was visiting the infected barracks and the Plague Hospital twice a day." The following are the statistics regarding Kirki, as tabulated by Mr. Haffkine:—

III. 352. OCCURRENCES of PLAGUE in the FOLLOWERS of the FOUR BATTERIES at KIRKI (put together), between the 16th August, 1897, and the end of September, 1897.

Date.	Number of uninoculated present.	Attacks.	Deaths.	Number of inoculated present.	Attacks.	Deaths.	Date.	Number of uninoculated present.	Attacks.	Deaths.	Number of inoculated present.	Attacks.	Deaths.
16-17.8.97	1,542	11	10	98	—	—	7-8.9.97 -	755	1	—	741	1	—
17-18.8.97	1,307	4	4	322	—	—	8-9.9.97 -	754	4	2	740	—	—
18-19.8.97	1,268	2	1	357	1	1	9-10.9.97 -	750	1	1	740	—	—
19-20.8.97	1,239	6	4	383	—	—	10-11.9.97	749	1	—	740	1	—
20-21.8.97	1,105	7	7	511	1	1	11-12.9.97	748	3	2	739	—	—
21-22.8.97	1,074	5	5	534	2	—	12-13.9.97	745	1	1	739	—	—
22-23 8.97	1,047	3	3	554	2	1	13-14.9.97	744	4	3	739	1	1
23-24.8.97	1,044	9	7	552	5	3	14-15.9.97	740	1	—	738	—	—
24-25.8.97	926	6	4	656	—	—	15-16.9.97	739	2	—	738	—	—
25-26.8.97	827	6	6	749	3	2	16-17.9.97	737	2	2	738	—	—
26-27.8.97	812	3	1	755	4	3	17-18.9.97	735	1	1	738	—	—
27-28.8.97	809	5	3	751	2	1	19-20.9.97	734	1	—	738	1	—
28-29.8.97	804	4	2	749	—	—	20-21.9.97	733	2	—	737	—	—
29-30.8.97	800	3	3	749	1	1	23-24.9.97	731	1	1	737	—	—
30-31.8.97	797	2	1	748	1	1	25-26.9.97	730	1	1	737	—	—
31.8.1.9.97	795	11	6	747	1	1	28-29.9.97	729	1	—	737	—	—
1-2.9.97 -	784	9	4	746	1	1							
2-3.9.97 -	775	3	2	745	1	—	Total -	—	143	98	—	32	17
3-4.9.97 -	772	3	2	744	—	—							
4-5.9.97 -	769	5	3	744	1	—	Average daily strength	859	—	—	671	—	—
5-6.9.97 -	764	6	3	743	2	—							
6-7.9.97 -	758	3	3	741	—	—							

A consideration of this table shows that in Kirki, as in Lanauli, for the greater part of the time during which the epidemic lasted after the introduction of inoculation, the value of inoculation can be calculated on absolute strengths. From the 26th of August to the end of the epidemic there were no fresh inoculations, and for this period there is no necessity to resort to a calculation on average strengths. For this period a calculation on absolute strengths gives the following results: Consideration of results at Kirki.

	Number.	Attacks.		Deaths.		Case Mortality Per cent.
		Number.	Per cent.	Number.	Per cent.	
Uninoculated -	812	84	10·3	47	5·8	56
Inoculated - -	755	18	2·4	9	1·2	50
Diminution of mortality in favour of the inoculated - -					4·6	

Mr. Haffkine, however, has calculated the number of attacks and deaths upon the average daily strength in order to compare the susceptibility to plague of the inoculated and the non-inoculated The daily strength of the inoculated reached its average on the 24th August 1897. Before this date the number of attacks among the uninoculated amounted to 4·9 of their average strength for that period, while in the subsequent phase of the epidemic, 11·8 per cent. among them were attacked. In these circumstances, after taking into account the fact that the maximum number of the inoculated was reached soon after the beginning of the second phase, we might have expected that Mr. Haffkine's calculation would have slightly underestimated the effects of inoculation, inasmuch as the phase of the outbreak during which the inoculated exceeded their average strength was severer than the preceding phase. His calculation is as follows :—

	Average daily Strength.	Attacks.		Deaths.		Case Mortality Per cent.
		Number.	Percentage.	Number.	Percentage.	
Uninoculated -	859	143	16·6	98	11·4	67·8
Inoculated - -	671	32	4·7	17	2·5	53

68.

It will be seen that this calculation gives a slightly lower value for inoculation than that obtained by the calculation on absolute strengths. The result is as close an approximation as can be expected in applying the complicated method of calculating on averages to figures of an individual epidemic which are to a large extent the result of undetermined causes.

With regard to the trustworthiness of the data which are here in question, it is to be remarked that the figures in Mr. Haffkine's statistics, representing the numbers of the inoculated and the uninoculated, are arrived at by a method of computation similar to that which we have already noticed in the case of Daman and Lanauli. The errors which we have pointed out in the case of the Daman and Lanauli statistics are repeated here. Nevertheless, we have no reason to suppose that the summary of results would have been very materially altered if the figures of the daily strength of inoculated and uninoculated had been based on an actual census, instead of being arrived at by a computation, and, in these circumstances, we do not doubt that the inoculations exerted a beneficial influence in Kirki. Paras. 433–4 of this Report.

IV.(6.) *Inoculations in the Khoja Community of Bombay City.*

436. We may now pass on to consider the case of the inoculations among the Khoja community of Bombay, regarding which Mr. Haffkine has prepared a special Report, reproduced in an Appendix to our Proceedings. The inoculations were begun on December 27th, 1897, and 33 persons were inoculated on that date, 33 on the 28th, 53 Inoculations among the Khoja community at Bombay. 69 ; App. IV. in Vol. I. of these Proceedings.

on the 29th, and so on, the inoculated on the last day of February numbering 4,719. During the following seven and a half weeks the number increased only by 465, and the total number of inoculations among the Khoja community in the first five months of 1898 was 5,184. The above were inoculated gradually, and the number includes Khojas who were re-inoculated during 1897–8, after having been previously inoculated in the outbreak of 1896–7. Khojas inoculated in 1897, but not re-inoculated in 1898, were on the other hand reckoned among the uninoculated. According to Mr. Haffkine's calculations, 8,146 remained uninoculated, and the average daily strength of inoculated people was 3,814 during the period covered by his Report, namely, from 27th December 1897 to 20th April 1898.

I. 337.

Mr. Haffkine's estimate of Bombay Khoja population and his statement of results of inoculation.

I. 337.

A census taken by the Khoja community in the beginning of 1898 showed the total community to consist of 9,350 people, but as, on account of the epidemic, a certain number of families may have been away when the census was taken, the above number, Mr. Haffkine thinks, is, perhaps, not absolutely accurate. He, therefore, estimated the number of Khojas as follows:—"The annual Khoja mortality in years free from plague, as registered in the burial-ground books of the Jamat office, was:—

In 1892	385
In 1893	301
In 1894	335
In 1895	349

giving an average of 342 deaths in a year. The mean mortality of the population of Bombay is close upon 35 per mille per annum. Counting for the Khojas a mortality per mille per annum of 25 only, the average of 342 deaths a year gives a population of 13,680, which is likely to be higher than the actual number. During the epidemic of plague which preceded the 16½ weeks under consideration, the Khojas had 350 deaths in excess of their average mortality. The above number of persons in the community should, therefore, be reduced by 350, giving a population of 13,330."

I. 337.

Mr. Haffkine states that "during the 16½ weeks between the 27th December 1897 and the 20th April 1898, 184 deaths in all occurred in the Khoja community. Of these, six, including two of plague, took place in Khojas inoculated in the epidemic of 1896–97, and not re-inoculated since; seven deaths, including three of plague, occurred in the 5,184 Khojas inoculated or re-inoculated in 1898; and 171 deaths occurred in the uninoculated Khojas. In the years 1892 to 1896, while there was no plague in Bombay, the mortality among the Khojas during the same part of the year, between the 27th December and 20th April, was 131, 96, 100, 106, and 96 respectively, or on the average 105, in a normal population of 13,680 individuals. In the reduced population of 13,330, as it stood at the beginning of the period under consideration, it should have been proportionately 102. The advent of the plague may have altered the course of the mortality, viz., the liability to deaths from other causes than plague, may have been influenced by the fact that, during the plague year preceding the period under consideration, double the usual number of individuals had been carried away by death. The normal mortality must have been also reduced by a part of the community having left the city. Admitting, however, that, of the total number of 184 deaths (during the spring of 1898), fully the usual proportion of 102 died of other causes than plague, there remain 82 deaths in excess of that number which are attributable to plague alone; 64 of this number were actually acknowledged as plague deaths by the relatives of the deceased, or certified as such by the Khoja Jamaat books. Of the 102 deaths attributable to other causes 4 occurred in inoculated this year (1898), 4 in inoculated last year (1897), and the rest, 94, in uninoculated. Of the 82 deaths attributable to plague, 3 occurred in inoculated this year, 2 in inoculated last year, and 77 in uninoculated." After making such allowances as he thinks necessary for the correction of the figures, Mr. Haffkine himself claims "a difference of 89·7 per cent. of deaths from plague in favour of the inoculated part of the community, and of 73·3 per cent. of deaths from what has been returned as 'other causes' in favour of the same part of the community."

I. 338.

Critical examination of Mr. Haffkine's estimate of number of Bombay Khojas.

With regard to the figures which Mr. Haffkine has adopted as representing the total number of Khojas present when inoculations began, we note that he has designedly under-estimated the rate of mortality used for his calculation, by assuming it to be only 25 per mille per annum, and that he has thus over-estimated the Khoja population

and the number of uninoculated people comprised in it. This exaggeration of the uninoculated number brings out the percentage of plague incidence among them at an unduly low figure, and, therefore, operates to the disadvantage of inoculation in the comparison which Mr. Haffkine has made. Mr. Ismail Jan Mahomad, a Khoja medical practitioner in Bombay, who appeared before us as a witness, thought that there were in the City about 10,000 Khojas, of whom half had been inoculated. Moreover, a calculation upon the average mortality rate of the City, 35 per mille per annum, gives us an estimate that the Khoja community amounted to 9,770 persons. This figure closely approaches the result of the census of 1898. 17,799–800.

Critical examination of statistics of mortality among Bombay Khojas.

In view of all the considerations which we have had before us, we have in the following calculations adopted as representing the total population of the Khoja community, the figure of 9,770 persons. Assuming, therefore, that the average strength of the inoculated Khojas during the period of observation was, as Mr. Haffkine puts it, 3,814, we arrive at an average strength of 5,956 for the uninoculated. If we distribute upon these strengths the deaths which are recorded in the books of the Khoja community, as having occurred during the period of observation from plague and from general causes, we arrive at the following results :—

	Average Strength.	Deaths from Plague.		Deaths from General Causes.	
		Number.	Percentage.	Number.	Percentage.
Uninoculated - -	5,956	61	1	116	1·9
Inoculated - - - -	3,814	3	·08	4	·1

This table shows that among the inoculated deaths from plague were $12\frac{1}{2}$ times less numerous, and deaths from general causes were 19 times less numerous, than among the uninoculated. *Primâ facie*, therefore, it would appear that Mr. Haffkine's anti-plague inoculation protects against plague, but that it protects more against ordinary diseases. This result is so striking and so difficult to accept that we first addressed ourselves to the task of inquiring whether the inoculated were a picked body, and whether the uninoculated contained a large proportion of the sick and feeble, and of the very young and very old. Surgeon-General Harvey, I.M.S., who made a special personal inquiry into the results of the inoculations performed in the Khoja community, was of opinion that, in the main, the explanation of the disproportion between the deaths from general causes among inoculated and uninoculated Khojas must be sought in the assumption that in many families the sick, the weak, the elderly people, and the children, did not present themselves for inoculation, and that only the strong and healthy undergo the operation. In view of this opinion, we have tested the proportions in which the different age groups were represented among the inoculated and the uninoculated Khojas respectively. For this purpose we have made use of the figures given in Appendix I. in Mr. Haffkine's Report, which we may presume to give with approximate accuracy the relative numbers of persons of different ages among the inoculated and the uninoculated Khojas. We find that on the total number of Khojas present in March the percentages of children under seven, of old people of 61 years of age and upwards, and of persons of intermediate ages respectively were 19, $3\frac{1}{2}$, and $77\frac{1}{2}$ among the uninoculated, and were 16, $1\frac{1}{2}$, and $82\frac{1}{2}$ among the inoculated. These percentages indicate that, in point of age at least, the two communities were not sensibly incomparable. In view of these percentages the next point to determine was whether the excessive mortality from general causes assigned to the uninoculated was really due to excessive deaths in any particular class of the uninoculated community. To determine this we have made an examination of the details contained in Mr. Haffkine's Report. We have computed the average strength among the inoculated and uninoculated, of children under seven and of old persons of 61 years of age and upwards, upon the basis of the percentages of these persons that we have already noted as having been present in the whole Khoja community in March. Further, we have determined from the nominal rolls appended to Mr. Haffkine's Report, the numbers of deaths from general causes that were attributed to each of these classes of the community. 26,438. I. 338. I. 339-44.

69. Moreover, basing ourselves on Mr. Haffkine's statement that the rate of mortality from general causes for the period of the year at present under consideration has in a series of years closely approximated to the rate of mortality for the whole year, we have expressed the death rates in the different classes in the form of a rate per mille per annum. Finally, on the basis of a death-rate of 35 per mille per annum, which is the average death-rate of Bombay, we have, for the purposes of comparison, added an estimate of the number of deaths which might have been expected on the average strength of the inoculated and uninoculated respectively, if they had been in all respects comparable communities. The results of our calculations are set forth in the following table:—

	Uninoculated.			Inoculated.		
		Recorded deaths from general causes.			Recorded deaths from general causes.	
	Average strength.	Number.	Rate per mille per annum calculated on the total average strength.	Average strength.	Number.	Rate per mille per annum calculated on the total average strength.
Children under seven - -	1,120	33	18	610	1	·8
Persons of intermediate ages -	4,622	62	34	3,151	3	2·4
Old people of 61 and over - -	214	21	11	53	0	0
Total - - - - - - -	5,956	116	63	3,814	4	3·2
Numbers of deaths that might have been expected at a death-rate of 35 per mille per annum - -	—	*61*	—	—	*41*	—

From this table it will be apparent that excessive mortality from general causes occurred in all three classes of the uninoculated community. The difference between the mortality assigned to the inoculated and the uninoculated will appear very clearly if we contrast the death-rates, which the mortality recorded in the different age-groups of the inoculated and uninoculated gives on the average strength of these classes. While among uninoculated persons of 61 years of age and over, whose average strength was 214, the number of recorded deaths amounted to 21, giving a rate of 313 per mille per annum, no death whatever is recorded as having occurred among an estimated strength of 53 inoculated persons of similar age. The uninoculated children, whose average strength was 1,120, are reported to have lost 33, at a rate of 95 per mille per annum, while 610 inoculated children are reported to have lost only one, at the rate of 5 per mille per annum. In the case of the adults, 4,622 uninoculated are reported to have lost 62 by death, at the rate of 43 mille per annum, while 3,151 inoculated are reported to have lost 3, at the rate of 3 per mille per annum.

Conclusions from figures regarding inoculations among Bombay Khojas. I. 338.

In view of the facts that are thus summarised here, Mr. Haffkine sums up the case of the Khojas in his Report, as follows:—"After making all allowances for inaccurate certification of deaths in the uninoculated group, with which the inoculated is being compared, and admitting that a part of the causes of deaths in the uninoculated may be due to a certain number of sickly people having abstained from inoculation, the result still contains an indication that besides the protection against plague, this inoculation influences also favourably the resistance to certain other diseases than plague." We find ourselves in agreement with Mr. Haffkine in holding that the difference in mortality among the inoculated and uninoculated cannot be fully accounted for, either by the excess mortality of the uninoculated children and old people, or by the incorrect assignment of plague deaths among the uninoculated to general causes. Such incorrect assignment, if it occurred unwittingly among the uninoculated, would have occurred also among the inoculated, and there was no room for it to have occurred among the inoculated. Therefore, there remains to be considered of the explanations offered by Mr. Haffkine, only the suggestion that his anti-plague inoculation exercises a protective influence against diseases other than plague. This question is discussed elsewhere in this Report. We cannot, however, accept Mr. Haffkine's view that the low mortality among the inoculated can be accounted

Section IX. of this Chapter.

for on this hypothesis. It seems to us very probable, on consideration of all the circumstances, that the figures of mortality, which have been given above, must be accounted for by assuming that deaths which occurred among the inoculated were wrongly assigned to the uninoculated.

Results obtained in the Bombay Khoja community after period comprised in Mr. Haffkine's Report.

The period referred to in Mr. Haffkine's Report extends over 16½ weeks, namely, to the 20th April 1898, and Mr. Ismail Jan Mahomad has endeavoured to collect the figures of the mortality in the Khoja community from the 20th April 1898 to December 21st of the same year. He states that in the period in question out of 5,000 inoculated persons, 22 got plague, and 26 died of other diseases, while of a similar number of non-inoculated, 44 died of plague, and 218 of other diseases. In the books of the Khoja community, however, only four persons are entered as having died from plague. These books Surgeon-General Harvey told us "are extremely inaccurate as regards causes of death;" referring to these, he said: "some of the entries are absolutely ridiculous." Mr. Ismail Jan Mahomad estimates at 22 the number of inoculated persons who died of plague, because, from his investigations, he is of opinion that some persons entered in the death register by the practitioners as having died of "brain-fever" had really died of plague. Further, he believes, that of the 218 uninoculated Khojas entered as having died from other diseases, 90, at least, had died from plague. It will be plain that no accurate conclusion can be drawn from statements like these, which are founded merely upon impressions. Asked how he accounted for the enormous mortality from general causes among non-inoculated people, as compared with the inoculated, Mr. Ismail Jan Mahomad advanced the same explanations as have been dealt with in reference to the period covered by Mr. Haffkine's figures, that sick and elderly people, as well as children, did not get inoculated. The criticisms made above regarding Mr. Haffkine's figures apply equally to the figures put before us by Mr. Ismail Jan Mahomad, and we need not discuss these in detail.

17,798. 17,804. 17,806. 26,435. 17,806. 17,808–9, 17,816.

Feeling of Bombay Khoja community regarding inoculation.

We may note that the feeling of the Khoja community towards inoculation was the reverse of hostile. Surgeon-General Harvey told us that the Khojas have a most extraordinary belief in the efficiency of inoculation, so that many of them have been re-inoculated, some as often as five times. Inoculation among them was encouraged by their Chief, His Highness the Aga Khan. It is not impossible that religious belief and loyalty to their leader may have had a powerful influence in prepossessing the Khojas in favour of inoculation. A bias in favour of inoculation, like a bias opposed to it, is, especially in an Indian community, not unlikely to result in the intentional or unintentional production of one-sided statistics.

26,435. 12,965. II. 423.

Nevertheless, quite apart from the statistics put before us, which we think inaccurate, we do not doubt that inoculations had a good effect, inasmuch as much weight must be allowed to the opinions of a community so intelligent as that of the Khojas regarding the practical effects of Mr. Haffkine's prophylactic fluid as tested by them in the presence of plague.

IV.(7.) *Inoculations in the Umarkhadi Common Jail, Bombay.*

Inoculations in Umarkhadi Common Jail

437. We may proceed now to discuss the test to which inoculation was submitted in the Umarkhadi Common Jail at Bombay. Before inoculation was offered to the prisoners, three cases, all fatal, occurred in this jail, namely, two on the 31st December 1897, and one on January 1st, 1898. On January 1st the 402 prisoners present in the jail were questioned as to their willingness to be inoculated. It was understood that all consented to undergo the operation. They were then paraded, and every second man or woman, without distinction, was picked out for inoculation. When their turn came, two persons refused to be inoculated. Thus out of the 201 to whom it was offered 199 prisoners were inoculated. The conditions of life of the inoculated and uninoculated prisoners were so arranged that there should not, in Mr. Haffkine's words, "be a single circumstance constituting a difference in the condition of the inoculated and uninoculated."

69. III. 660 (*a*). 20,311–6.

Results of inoculations.

In considering this outbreak, we may confine our attention to the cases of plague which occurred among the prisoners present in the jail at the time of inoculation, though some other cases occurred among prisoners subsequently admitted. The course of the outbreak, so far as it concerns us, is thus stated in Mr. Haffkine's words:— "The plague continued for about a month; and on 11 different days there occurred 13 cases. During that month a certain number of prisoners were discharged, and others

69.

admitted to the jail. Taking day by day the number of inoculated and uninoculated who remained in the jail during that month of plague, from the original group of 402 prisoners, the average daily strength of the inoculated appears 147, and of the uninoculated 127. The number of inoculated who stayed in the jail all through the epidemic was therefore larger by about one-seventh than that of the uninoculated. All the 13 cases of plague occurred among the prisoners of *the original group* who were present in the jail on the day of inoculation. . . . Therefore the distribution of the cases of plague among the inoculated and the non-inoculated represents the distribution of the two original absolutely comparable groups of people which I have described. Ten of the 13 cases were among the smaller number of the uninoculated, and six of these ten died. There were three cases amongst the larger number of 147 inoculated, and all three recovered." These statements are borne out by the official figures with which we have been supplied, except on two points, which do not affect the general results of the statistics. The first of these is that plague continued for more than a month, as will be apparent from the dates of attack given below. The second point is, that in the official figures the number of cases among the uninoculated is given as nine (with five deaths) instead of ten (with six deaths). Mr. Haffkine has explained that the extra man included in his figures was one who was present when the inoculations were performed, but was attacked with plague immediately afterwards, and we think that, in view of this fact, it would be well to exclude this case from consideration, as he had already contracted plague before the inoculations were done.

III 441–4.

III. 660–1 (*b*).

III. 441–4. The dates on which plague cases occurred in the jail after inoculation are of interest. Among the groups of inoculated and uninocculated prisoners with whom we are dealing, no cases at all occurred in the interval from the 1st to the 16th January. On the 7th January the whole of the convicts had been turned out of the buildings, in which the original three cases had occurred, and were made to sleep in the open for from 10 to 15 days. In the 14 days dating from the 17th January to the end of the month there occurred among the uninoculated 8 of the 9 cases we have referred to above, while no cases occurred among the inoculated. On the 2nd February there occurred the last of the cases among the uninoculated. The three cases among the inoculated then occurred, the first on the 10th February, the second on the 28th February, and the third and last on the 18th March. We find that the average monthly strength of the inoculated and uninoculated prisoners of the original groups was 92 and 99 respectively during the months of February and March. Thus the incidence of plague in these two months was three times as great in the inoculated as it was in the uninoculated. This, however, must not be allowed to obscure the important fact that, during the whole period of the outbreak, the number of attacks among the inoculated was only one-third of the number among the uninoculated, and that the disease among the inoculated was remarkably mild, resembling mumps more than plague, though the cases among the uninoculated were of average severity.

69; 7236 (I.) and (II.).

IV.(8.) *Inoculations in Undhera.*

Surgeon-General Harvey's account of the inoculations at Undhera.

438. The last of the examples of the effects of inoculation which Mr. Haffkine personally investigated, was that of Undhera. Surgeon-General Harvey, the Director-General of the Indian Medical Service, submitted to the Government of India a report upon this case, from which we may quote the following narrative of the course of events:

69. —"Undhera, a village about six miles from Baroda, was attacked by plague in January 1898. On the 5th February a careful census was taken and showed a population of 1,029. Up to and inclusive of 14th February, 79 plague deaths occurred, leaving 950 people to be dealt with. Of these 513 were inoculated, leaving 437 uninoculated. As far as possible an equal number of each sex, age, and family, was done, and as all were living under precisely similar conditions as to sanitary surroundings, food, drink, clothing, etc., the case is the best and most conclusive example yet available of the result of inoculation. Except for the inoculations, all were on the same footing, and the disease had got a thorough hold of the place. The usual sanitary precautions as to segregation and disinfection were carried out, all plague cases being removed to hospital, and every effort made to combat the disease in the usual way. The inoculations were done on the 12th (February), but the following figures are taken from the 15th, so as to eliminate cases incubating plague at the time of the inoculations. Three deaths occurred among the uninoculated between the 12th and 14th inclusive, none among the inoculated. These three deaths, together with two others which might possibly have been due to diseases other than plague, have been eliminated, so that no exaggeration as to the effect of the inoculation may be possible. The results up to

the 2nd April (1898) are as follows, but no case occurred after the 28th March (1898), so that we are probably dealing with a finished epidemic. Between 15th February and the cessation of the disease plague cases occurred in 29 families, living together as already said under exactly similar conditions, save that some were, and others were not, inoculated. These 29 families comprised 135 individuals of all ages, 71 of whom had been inoculated and 64 not. The 71 inoculated had eight cases with three deaths, while the uninoculated had 28 cases with 26 deaths. Had the inoculated been as susceptible as the uninoculated, they should have had 29 deaths instead of three, and the inference seems irresistible that the inoculation saved 26 lives out of this small number, or 89·65 per cent. Taking the whole number inoculated, 513 had eight cases or 1·56 per cent., and three deaths, or ·58 per cent., while the 437 uninoculated had 28 cases or 6·4 per cent., and 26 deaths or 5·9 per cent., just 10 times as many. The protective influence of the inoculations is brought out still more strongly in some particular instances. Thus, in hut 84, ward 4, five persons were inoculated and five uninoculated in a family of 10. The five inoculated remained healthy, while two of the five uninoculated got plague and died. In hut 18 of the same ward three inoculated persons remained healthy, two uninoculated died out of a family of five. In hut 26, also in ward 4, one inoculated person escaped, two uninoculated died out of a family of three. In hut 8, ward 1, four inoculated persons escaped, while the one who remained uninoculated contracted the disease and died. In hut 24, ward 2, out of a family of two, the inoculated member escaped, the uninoculated died. In hut 20, ward 3, one of the three inoculated contracted plague, but recovered, while one of the four uninoculated got it and died. In two out of the three huts where fatal cases occurred among the inoculated, a death also occurred among the uninoculated, and in only one instance in the whole did a fatal case occur among the inoculated, while the uninoculated went free. This was in hut 31, ward 4, where one of four inoculated contracted and died of plague, while two uninoculated escaped. These figures have been verified by Surgeon-Major Bannerman, Madras Medical Service, Surgeon-Captain Dyson, Bombay Medical Service, Mr. Haffkine, and myself, case by case and family by family, and seem to me to prove that while inoculation as at present practiced is not an absolute protective either against seizure or death, it is of immense value both as a prophylactic and as modifying the severity of the disease and reducing the case mortality. This was 37·5 per cent. among the inoculated, against 92·85 per cent. in the uninoculated."

We add here, for convenience of reference, a tabular statement of the results recorded in Surgeon-General Harvey's Report:— Statistics relating to Undhera.

—	—	Strength.	Attacks.		Deaths.		Case Mortality per cent.
			Number.	Percentage.	Number.	Percentage.	
Whole Village.	Uninoculated -	437	28	6·2	26	5·9	93
	Inoculated - -	513	8	1·5	3	0·6	38
Houses in which plague occurred	Uninoculated -	64	28	43·8	26	40·6	93
	Inoculated - -	71	8	11·5	3	4·2	38
Diminution of mortality in population of whole village - - -						5·3	
„ „ plague infected houses - -						36·4	

With regard to the methods by which the above statistics were compiled, we may note, in the first place, that the strength of the uninoculated appears to have been ascertained not by a direct enumeration, but by a computation. The census of the village, according to Mr. Mehta, the medical officer who was in charge of plague operations in the Baroda State, was taken on the 5th January 1898, not on the 5th February. Mr. Mehta has made two statements of the population ascertained, both of which differ slightly from the figure given by Surgeon-General Harvey; the difference is not important. The census population was brought up to date when the inoculations were performed by a deduction for persons who had died or left the village, and an addition for births. Different statements have been laid before us as to the number by which the population had actually declined between the date of the census and the date of the inoculations; but the differences are small, and may be neglected. With regard to 14,694. 14,883–97.

4046–8. the mortality which occurred after inoculation, we may note that the keeping of the records, and the diagnosis of the causes of death, was the work of the native Hospital Assistant or Apothecary who was present in the village. This work was checked, as has been indicated above, by a house to house inquiry by Mr. Haffkine, Surgeon-General Harvey, and other officers. These officers came to Undhera on April 2nd, 1898, went into all the houses, and, with the help of a census paper, controlled the statistics.

69, 4049–51, 7872–8. I. 331.

Criticism of Undhera experiment. App. III. in Vol. I. of these Proceedings. I. 333–6.

Attached to Mr. Haffkine's Report on Undhera will be found a series of investigation sheets which give in detail the composition of each of the 28 families, (not 29 as stated by Surgeon-General Harvey), in which plague occurred, and the occurrences of plague in it. We have scrutinised these investigation sheets to control the statements made regarding the comparability of the inoculated and uninoculated in Undhera, and we find that the proportions of the various ages and sexes were evenly distributed among the two classes. The correctness of Mr. Haffkine's figures is further confirmed, after making allowance for the deaths excluded by him from his calculations,

4043–5, 14,912. by figures separately given to us by Major Bannerman and Mr. H. D. Mehta. The experiment at Undhera appears to us to be the most important experiment regarding the effect of anti-plague inoculations that has as yet been carried out. It is important, because firstly the inoculated and uninoculated persons under observation were fairly numerous, while their numbers were determined with a considerable degree of accuracy; secondly, because the communities of the inoculated and the uninoculated were comparable, and lived under similar conditions; thirdly, because the communities were equally exposed for a period of six weeks to an outbreak of plague which carried off 6·2 per cent. of the uninoculated; and, lastly, because the results set down by the Native Hospital Assistant were checked in the manner described above. The only point which seems calculated to arouse a doubt as to the trustworthiness of the results recorded is the fact that 27 out of 28 persons attacked are said to have died. This certainly is an astonishing mortality, and might be taken as indicating that the village records are seriously inaccurate. If, however, we assume—and, with the exception of Dr. Ruffer, who thinks that the records cannot be accepted as accurate, we do make the assumption —that the checks applied on the spot were sufficient to ensure the detection of really gross errors, then we may deduce two lessons from the Undhera experiment. Firstly, we learn that the number of plague attacks among the uninoculated is much larger than among the inoculated; in this particular case, as the table given above will show, plague attacks were proportionately four times more numerous among the uninoculated than among the inoculated. The second lesson we learn is that the percentage of deaths among the attacked is very much higher among the uninoculated than among the inoculated; in this particular case, as the table again shows, the percentage of deaths among the uninoculated was proportionately 10 times as great as among the inoculated. An indication which is afforded by the figures for Undhera with respect to the duration of the immunity conferred by inoculation, will be referred to in a subsequent portion of this Report. A fact which invites notice in the table given above is the large percentage of the inoculated who suffered in the houses brought into the investigation sheets, as compared with the percentage who suffered in the village as a whole. We cannot, however, in this case, as in the case of some of the houses in Daman, conclude that this high percentage is referable to the greater liability of inoculated persons to contract plague when they are living in infected houses. It would appear that only one of the eight inoculated persons attacked was attacked in a house in which there had already been a case of plague. Therefore the percentage of attacks among the inoculated residents of the infected houses in Undhera is not a trustworthy index of the liability of inoculated persons to contract plague in infected houses.

V. History of Cases in which Inoculation was resorted to on a Large Scale, with Criticism of Statistical Results put forward in these Cases, and Conclusions to be drawn from them.

V.(1.) *The Town of Hubli.*

Inoculations at Hubli. 439. The preliminary experiments which we have discussed above had afforded sufficient proof of the efficacy of inoculation to induce the authorities to take advantage of a serious outbreak of plague at Hubli in May 1898 for testing the value of inoculation on a large scale as a plague measure. We had an opportunity of examining many of the persons employed in this work, more especially Mr. Cappel, the Collector of the district, and Captain Leumann, I.M.S., who had been in charge of the operations.

1955, 2428–34, 2471–2, 3055, 3158–9. Hubli was one of the first towns in India where an attempt was made to combat plague by the inoculation of the whole population of an infected place, and we have therefore to carefully consider the results obtained. Every effort was made to

encourage inoculation, and, in particular, re-inoculation, both by the application of a great deal of moral persuasion and also by the grant of certain material advantages to inoculated persons. The policy adopted was to relax in favour of the inoculated the severe plague measures which were enforced with regard to the non-inoculated. The uninoculated inmates of houses in which plague cases occurred were taken to segregation camps, and were detained there for 10 to 15, and at one time 18, days; uninoculated persons living in an infected quarter were liable to removal to health camps; uninoculated persons, whether removed originally to segregation or to health camps, were not permitted to return to live in the town unless they became twice inoculated, even though their houses and effects had been disinfected; uninoculated persons could not leave Hubli by rail, and an attempt was made to prevent them leaving by road, and such as got away were liable to detention at other places. Persons once inoculated were treated in these respects like the uninoculated, and a single inoculation was regarded merely as a step to securing the advantages of double inoculation, without which the district authorities were not satisfied that protection was conferred. The twice inoculated, on the other hand, were not segregated except during the time necessary for the disinfection of their property and houses, and were sometimes allowed to re-occupy their houses on the same day as that on which they were removed from them for their disinfection; and the twice inoculated were freely granted passes which enabled them to leave Hubli, and to a large extent guaranteed them against restrictions on travelling in other districts. The result to be expected was attained, and a very large number of the people became inoculated, all but a small part of the inoculated submitting to the operation twice. As the work proceeded statistics regarding its progress and effect were collected, which have been put before us by Mr. Cappel in his evidence, and by Captain Leumann in a Report which he wrote upon the inoculations at Hubli. The object of these statistics is presumably to permit of a calculation of the exact measure of advantage which was gained from inoculation, and Captain Leumann in his Report does, as a matter of fact, work out numerical statements on which he bases a claim that in respect of plague mortality the advantage in favour of the twice inoculated over a period of seven successive weeks amounted to more than 95 per cent.

19,826–8, 19,888, 23,375. I. 398 (III.). 3164, 3187. App. XV. in Vol. I. of these Proceedings. I. 404–5.

The table which was furnished to us by Mr. Cappel as a corrected record of the statistics contains figures for each week commencing with that ending on the 17th June 1898. Details for weeks preceding that week are not given, as inoculations were commenced on a large scale only in the middle of May, when the outbreak of plague began to assume serious proportions. The statement is as follows:—

Mr. Cappel's statistics regarding inoculations at Hubli. 25,151.

Weeks ending	Population for the Week.	Inoculated.			Uninoculated.	Attacks in					Deaths in				
		Once.	Twice.	Total.		Once inoculated.	Twice inoculated.	Total inoculated.	Uninoculated.	Total.	Once inoculated.	Twice inoculated.	Total inoculated.	Uninoculated.	Total.
Up to June 17	47,427	2,323	531	2,854	44,573	Details of these corrected figures cannot be found.		1	123	124	—	1	1	106	107
„ „ 24	47,082	3,368	2,220	5,588	41,494			2	28	30	2	1	3	22	25
„ July 1	47,485	4,487	3,956	8,443	39,042			1	37	38	1	—	1	29	30
„ „ 8	46,537	5,057	5,420	10,477	36,020			4	52	56	3	3	6	55	61
„ „ 15	46,518	5,974	7,289	13,263	33,255			8	58	66	4	2	6	34	40
„ „ 22	45,240	6,565	8,959	15,524	29,716			13	100	113	1	6	7	82	89
„ „ 29	43,809	9,386	10,311	19,697	24,112			21	145	166	6	9	15	100	115
„ August 5	43,707	10,016	12,660	22,676	21,031			22	169	191	7	9	16	140	156
„ „ 12	42,768	11,339	15,845	27,184	15,584			37	317	354	5	14	19	272	291
„ „ 19	40,441	10,265	19,491	29,756	10,685			69	350	419	30	31	61	386	447
„ „ 26	39,400	9,671	23,362	33,033	6,367			66	446	512	21	20	41	371	412
„ September 2	38,210	7,569	26,547	34,116	4,094			32	393	425	8	20	28	328	356
„ „ 9	38,382	6,798	28,671	35,469	2,731			47	261	308	11	23	34	227	261
„ „ 16	38,408	6,381	30,911	37,292	1,116			55	153	208	7	40	47	133	185
„ „ 23	38,408	6,567	31,638	38,205	937			35	106	141	11	44	55	106	161
„ „ 30	39,315	6,820	31,872	38,692	603			28	76	104	4	16	20	58	78
„ October 7	39,500	5,258	33,963	39,221	279	4	11	15	41	56	4	10	14	40	54
„ „ 14	39,600	5,217	34,527	39,774	200	3	17	20	14	34	3	15	18	18	36
„ „ 21	39,953	5,286	34,763	40,049	200	—	6	6	11	17	—	6	6	11	17
„ „ 28	40,453	4,196	35,040	40,236	217*	1	4	5	3	8	1	4	5	3	8
„ November 4	40,693	5,100	35,229	40,329	364	—	2	2	7	9	—	2	2	6	8
„ „ 11	41,164	5,116	35,348	40,464	700	—	2	2	2	4	—	2	2	2	4
„ „ 18	41,643	5,117	35,397	40,514	1,129	—	—	—	2	2	—	—	—	2	2
„ „ 25	42,206	5,214	35,558	40,772	1,434	—	—	—	—	—	—	—	—	—	—
„ December 2	42,640	5,157	35,959	41,116	1,524	—	—	—	3	3	—	—	—	3	3
Average - - -	42,040	—	—	—	—	—	—	—	—	—	—	—	—	—	—
Total -	—	—	—	—	—	—	—	491	2,897	3,388	129	278	407	2,539	2,946

* The rising figures represent new comers.

25,151. These figures are summed up by Mr. Cappel in a statement that the percentage of the total plague-deaths in Hubli was 7 per cent. on a population taken, for the period covered by the statistics, as 42,000. Of the total plague deaths $\frac{3}{70}$ths are assigned to the once inoculated, $\frac{6}{70}$ths to the twice inoculated, and $\frac{61}{70}$ths to the uninoculated.

3056–65, 23,143–50. The figures in this statement are not the same as those given by Captain Leumann in his Report, but this is to be expected from the fact that Captain Leumann's Report refers to weeks that ended three days before the weeks adopted by Mr. Cappel. Mr. Cappel, however, has informed us that his figures have been reconciled with those of Captain Leumann, and it will therefore suffice for us now to confine our consideration mainly to Mr. Cappel's corrected table which we have printed above.

Criticism of Hubli figures. Four sets of figures are essential in every case in which a numerical estimate is to be formed of the relative immunity of the inoculated and the uninoculated. These are the total numbers respectively of the inoculated and the uninoculated comprising the population under observation, and the totals of attacks, or, as the case may be, of deaths, in each of the two categories of inoculated and uninoculated respectively. Mr. Cappel's statement purports to give accurately the four sets of figures required, and we have therefore now to consider the extent to which the figures are reliable and capable of use in calculating the exact advantage obtained by inoculation in Hubli.

Method of collecting information in Hubli. 25,157-60. A census of the town was taken under the directions of the Collector some time after the plague had commenced in October 1897. This census forms the basis of the statistics which were collected during the subsequent course of the epidemic. A procedure similar to that adopted for taking the Imperial Census of 1891 was followed, and, though Europeans were not especially employed to supervise it, we have no reason to doubt that the census of 1897 gives a fairly accurate estimate of the number of people present in Hubli at the time when it was taken. The population was found to be near that ascertained in 1891, but we are not in possession of exact information regarding this point. The census was thoroughly revised when plague began to get bad, and from that time onwards an attempt was made to keep it up to date, and to check, by a weekly revision, the fluctuation in the population which was caused by the movements of the people. To facilitate the revision, the town which contained about 12,000 houses, was divided into sub-divisions and wards, each of the latter having from 90 to 120 houses in it, and being under the charge of a supervisor. On a fixed day every week it was the duty of the supervisor to visit every house in his charge, and to check and correct for each house the entries which stood in his census books regarding it. In addition to his weekly visit the supervisor was instructed to make daily inquiries in his ward, and to note the names of all persons arriving in or leaving the ward, and all information which could be gathered regarding the distribution of the people between inoculated and uninoculated, and regarding the occurrence of plague attacks and deaths among these classes. The weekly visit was intended for a thorough revision of the information collected day by day, and the supervisor was instructed to call out and actually verify the presence of all persons living in the house at the time of his visit, and, after inquiry, to make appropriate entries in his census book against the names of all previously recorded inmates who were found to be absent. The weekly revisions of the ward lists finally reached the Chief Plague Officer, and were used by him for a compilation of the population of the whole town.

20,172–6. 1665–6, 3174–8. 19,870–3, 20,811–3. 2523–7, 3123–30, 3166–7.

Criticism of method of collecting statistics at Hubli, and notice of certain discrepancies of minor importance. 1589, 3174–8. Before passing on to criticism of the statistics thus collected, it will be well to note certain points with regard to the agency employed in collecting them. Most of the supervisors at one time were volunteers; the work of revising the census was rendered difficult at the most important time by panic due to the fear of the plague; some of them ran away and had to be replaced; during the rains it was difficult to keep them up to their duty; and only one European officer was available to supervise and control the proper working of the organisation.

In some weeks the statistics compiled are obviously wrong as the figures stand in Mr. Cappel's table. Thus, the numbers of the inoculated and uninoculated population, added together do not in the case of four weeks—those ending on the 23rd and 30th of September, and on the 14th and 21st of October—correspond with the total population of the town. Indeed, in the last of the weeks just referred to, the number of the inoculated alone is stated to be 40,049, though this is in excess of the total population, which is given as 39,953 only. We are, however, not disposed to lay much stress on points like this as arguments to show that the figures actually returned by

the supervisors were wrong. The discrepancies we have pointed out may be due to simple clerical errors, as is perhaps indicated by the repetition of the number for the preceding week in the case of the total population on the 23rd September, and by a similar repetition in the case of the uninoculated on the 21st October. It is sufficient simply to mention these discrepancies.

Criticism of estimate of uninoculated population of Hubli.

Much graver doubts regarding the accuracy of Mr. Cappel's statistics are awakened by the fact that in the table he originally gave us the plague mortality of the uninoculated, calculated on the census figures, amounted to 20 per cent. in a period of eight successive weeks,—a plague mortality unprecedented in any urban community in British India. Though in the revised table put before us the plague mortality among the uninoculated has been brought down to 20 per cent. in a period of 30 successive weeks, still we think that this mortality, in a small community of uninoculated persons, interspersed among a large number of protected persons, is so large as to make it incumbent upon us to examine the figures very carefully. The first important point to be considered is the accuracy of the figures representing the respective numbers of inoculated and uninoculated people in the population. The latter had every reason to avoid getting themselves registered on the supervisors' census lists. We have already described the policy which was adopted towards them, and a reference to what we have said on the subject will explain that concealment on their part was only to be expected. That they did endeavour to hide themselves is proved by the evidence of Lieutenant Keelan, who directly supervised the census operations; he allowed that when a plague officer appeared, uninoculated persons escaped through the back door. That the error among the uninoculated may have been considerable, and is in reality indeterminable, is indicated by the estimates formed of the maximum number of uninoculated people in the town by various witnesses. The minimum number given in the table is 200, while for 14 weeks the table shows that less than 5,000 uninoculated people were in the town, their numbers being given as under 2,000 from the week ending on the 16th September to the last week for which we have a record. Mr. Cardoz, a medical officer employed in Hubli during the outbreak, gave us his opinion that the minimum number of uninoculated never fell below 6,000 or 8,000. Lieutenant Thomas, who was engaged in disinfection duties in the town, thought that the uninoculated population was about 5,000, and that it did not go below 5,000. His statement, however, like that of Mr. Cardoz', can only be accepted as showing that the general impression left on the minds of officers on the spot was that the census gave an incorrect idea of the number of the uninoculated. Lieutenant Keelan, who was directly responsible for the control of the census operations, was in an exceptionally good position to judge. He, speaking of the middle of September, when Mr. Cappel's figures put the numbers of the uninoculated at 1,116, stated, "I should have said by the figures I had then that there were about 3,000 or 4,000 who had not been inoculated." In answer to another question, however, Lieutenant Keelan said that "the census which was sent in was really correct for each division to within 50 or 100." Inasmuch as there were, we understand, about 10 divisions, this, we may note, would give us a much lower estimate of the number of the uninoculated present in the town than that of 3,000 to 4,000 quoted above. But inasmuch as the error would be proportionately greater among the uninoculated than among the inoculated, the statistics regarding them would be seriously vitiated even by an error of 50 to 100 in the enumeration of each division.

I. 65. 2521, 3169–71. 19,890. 2488–92, 2518–9. 23,461–2. 19,849–51. 19,891. 19,796.

Criticism of estimate of inoculated population of Hubli. Para. 472 of this Report.

The case of the inoculated differed materially from that of the uninoculated, for they had every reason to get themselves recognised as inoculated, and doubtless took care to be registered upon the census. The doubt in their case is that the census rather exaggerated than understated their numbers. As we shall show later on, the Indian public has generally resorted to inoculation to escape other plague measures. The chief value of inoculation, from their point of view, is not the protection it affords. They have, therefore, been anxious to use devices to get the privileges of inoculation, without undergoing the pain and inconvenience of the operation. With a subordinate agency like that usually available in India (and we have evidence that in connection with disinfection the subordinate agency in Hubli itself was actually found to be open to bribery), we cannot exclude the possibility of persons being able to induce census supervisors to enter them improperly as inoculated. A few instances came to the notice of the authorities in which personation was resorted to for the purpose of

23,366. 23,399. 1965, 25,504.

1966–7, 2482–7, 2513. 2647, 3089, 23,872, I. 408.

obtaining the desired privileges. Certificates of inoculation had, it is believed, a marketable value. On the one hand, certificates of single inoculation were occasionally transferred to assist the persons who became possessed of them in obtaining the privileges of double inoculation after undergoing the process only once. On the other hand certificates of double inoculation were occasionally transferred in order that the privileges desired might be thus obtained without any personal inconvenience. We have, however, no reason to suppose that the census figures of the inoculated were to any considerable extent inflated by the addition of persons who improperly obtained certificates of inoculation; the evidence, in fact, points the other way.

Criticism of statistics relating to plague occurrences in Hubli.

1656, 1944–5. 2404.

So far we have dealt with the statistics regarding the distribution of the population into the classes of inoculated and uninoculated. In passing on to a consideration of the information supplied to us regarding plague attacks and deaths, we may repeat the fact, which has been already noted above, that the authorities depended for this information upon the same inefficient agency as we have described in criticising the general accuracy of the census. Moreover, difficulty in the collection of accurate information was experienced because the discovery of plague in a house was necessarily followed up by the application of the prescribed plague measures. The patient, if alive, was removed to hospital; the inmates of the house were segregated; the house was disinfected and unroofed. The milder measures adopted in the case of inoculated persons were still such as to cause considerable inconvenience. Hence there was great inducement to concealment of plague. That the agency employed to collect information regarding the occurrence of plague was unable to successfully defeat this concealment is proved by the fact that, during the most important period of the outbreak which is covered by these statistics, it was only in rare instances that plague cases were discovered during life. To conceal the occurrence of deaths, however, appears to have been a matter of considerable difficulty, inasmuch as the removal of all dead bodies was prohibited until they had been inspected by medical officers appointed for the purpose and a death certificate had been issued. But in spite of these rules, deaths also were concealed. An instance of this is the case incidentally referred to by Lieutenant Thomas, the Chief Plague Officer at Hubli, in connection with his evidence regarding disinfection. He referred to the case of a house in which, out of 13 inmates, nine, having died of plague, had been buried surreptitiously before information reached the authorities. In such a case it may be doubted whether the people who had died were on the census at all, and whether the deaths when discovered would be correctly classified as inoculated or uninoculated. The principles on which classification was ordinarily made were as follows. When a death came to the knowledge of the authorities, the medical officer appointed for the purpose proceeded to make an inspection of the body. He had first to determine whether the death was or was not due to plague. Very considerable difficulty appears to have been experienced in many cases in arriving at a correct conclusion, even when the inspection was made by experienced European medical officers, and in the earlier part of the epidemic, when time was available, it was found necessary to have resort to post mortems and bacteriological examinations. Later on, however, when the epidemic was at its height, the task of examining bodies was committed to Assistant Surgeons and Hospital Assistants, and we cannot doubt that many erroneous diagnoses may have been made. A decision as to the cause of death, whether right or wrong, having been arrived at, the next point to determine was whether the deceased belonged to the class of the inoculated or of the uninoculated. Inquiries were made from the relatives, and a search was instituted for a certificate of inoculation. When a certificate was found, or the body of a person identified showed signs of inoculation by the existence of a nodule on the left arm, the deceased was classed as inoculated. When the body of a known person showed no signs of inoculation, and no certificate was forthcoming, the statements of the relatives were accepted, unless on consulting the register of inoculations there appeared to be grounds for doubting these. When the body was that of a person not identified, and nothing could be found out about it, the classification of the death as that of an inoculated or uninoculated person, depended upon the proportion in which the inoculated and uninoculated were represented in the population. This is the statement of Captain Leumann, I.M.S.; but, on the other hand, the statement of Assistant-Surgeon Cardoz, who was temporarily employed on this particular duty, is that unless a certificate of inoculation was found on a body or was produced by the friends, the deceased was considered to have been uninoculated. Mr. Cardoz added that there was no inducement for the friends to produce certificates. This we can well

1590, 1981, 19,802–3, 19,836.

3076–7.

23,383.

20,007, 20,014.

3077–8.

3082–4, 3131–7, 3181–2.

2473–7. 2515–6, 2530.

2480

believe, for as we have already pointed out, certificates were occasionally transferred by sale to uninoculated persons, and relatives may have sometimes been unwilling to part with certificates on account of the importance which is attached in India to the mere possession of such documents. In cases where certificates were not forthcoming, it seems to us to have been practically impossible to get evidence of inoculation, as the local induration resulting from inoculation would rapidly pass off, and, even if the patient's name was known—which from Captain Leumann's Report, appears to have been the case with reference to all but 5 per cent. of the dead examined in Hubli—it would be an extremely difficult and laborious task to trace a particular case of inoculation in unindexed registers, to which from 500 to 800 entries were added daily by an inadequate staff. We see, therefore, that the statistics of deaths are liable to a considerable error.

I. 400 (xviii.), I. 409 (E.).

2528.

To sum up, then, we have seen that each of the four data which are essential to a correct estimation of relative immunity may be inaccurate. It is possible that the number of the inoculated has been over-estimated, and it is almost certain that the number of the uninoculated has been understated. Moreover, the number of deaths—to say nothing of the number of attacks—assigned to plague is inaccurate, and in the attribution to the inoculated and the uninoculated of deaths assigned to plague there is almost certainly further error. We, therefore, cannot but conclude that it is absolutely impossible to arrive at any exact arithmetical estimate of the relative immunity of the inoculated and uninoculated, and that the estimate put forward by Captain Leumann cannot be accepted as correct.

No exact arithmetical estimate of amount of protection conferred by inoculation can be obtained from Hubli figures.

We have, however, to consider whether it would not be possible to work out from the figures some approximate expression for the amount of protection afforded by inoculation at Hubli. It would be possible to do this if the errors in the statistics which we have referred to above were not such as to seriously affect the results when expressed in percentages, and if the communities of the inoculated and uninoculated could be regarded as comparable from the point of view of general health. To enable us to come to a conclusion regarding the comparability of the communities we can make use of figures with which we have been supplied by Dr. Winter. These figures relate to the number of deaths from general causes which took place among the inoculated and uninoculated respectively. We learn from them that in the period of 30 weeks, from the 12th May to the 29th November, the uninoculated lost 646 by deaths due to general causes, while the inoculated lost 74 only. We cannot work out from these figures the exact annual death-rate, for we do not know the census figures of the population for the first four weeks included in Dr. Winter's period. But Dr. Winter's figures, taken upon the average strength of the inoculated and uninoculated for the rest of his period, give us a mortality rate from general causes of over 80 per mille per annum among the uninoculated and of only four per mille per annum among the inoculated. It is not open to us to assume that deaths among the uninoculated which were assigned in Dr. Winter's figures to general causes were in reality unrecognised plague deaths. We are excluded from making this assumption, because, as we have pointed out above, the number of plague deaths already assigned to the uninoculated is so large as to awaken serious doubts as to its accuracy. We must therefore take Dr. Winter's figures as foreclosing the question of the comparability of the two communities from the point of view of general health. The conclusion that the communities were incomparable is borne out by a consideration of the process of selection which operated in the direction of excluding certain classes of unfit persons from the inoculated community. Candidates for inoculation were often rejected on account of age, disease, and advanced pregnancy. In a town which just before the outbreak of plague had 58,000 inhabitants, the whole body of the uninoculated, when their number was at or near its recorded minimum of 200, must have consisted entirely of persons of the rejected classes. It is precisely during this period that the errors pointed out above in the enumeration of the uninoculated population and in the assignment of plague deaths to the uninoculated must have most seriously vitiated the statistics. On the other hand, a consideration of Mr. Cappel's table shows that during a considerable portion of the period covered by his statistics the numbers of the uninoculated were large. During this period the effect of the errors due to the incomparability of the inoculated and the uninoculated, and to defects in the collection of the statistics cannot, we think, have been large enough to vitiate deductions from the figures expressed in the form of percentages. From this opinion one of us, Dr. Ruffer, dissents, and he thinks that no useful arithmetical estimate of the value of inoculation at Hubli can be made.

Comparability of inoculated and uninoculated in Hubli.

1819, 1823–4.

1689–95, 1759–62, 3066–70.

Approximate estimate of effects of inoculation at Hubli, and general remarks

We think that it may be possible to draw some approximate inference from the figures in Mr. Cappel's table, provided we exclude the figures after those for the week ending on the 19th August, after which time the numbers of the uninoculated were small. Moreover, we must exclude also the figures for the earlier weeks included in Mr. Cappel's table. This will be manifest when we consider that the population, which we should here have under consideration, would consist first of two groups of 5,588 and 10,685 persons respectively, who are continuously included (during the period from the 18th June to the 19th August inclusive) in the respective classes of the inoculated and uninoculated, and secondly of a group of some 24,200 persons who are included in the first weeks of the period among the uninoculated and who gradually pass over to the inoculated and are at the end of the period all included among the inoculated. Now, inasmuch as plague was only beginning to gather strength in the earlier part of the period, it will be manifest that this group of 24,200 persons must have been brought more frequently in contact with infection in the latter weeks than in the earlier weeks. Hence the number of cases might have been expected to be greater among these 24,200 people in the later than in the earlier period. Therefore the number of cases which did actually occur among this group in the second period, when they are included in the inoculated, will not, when compared with the number of cases that occurred among them in the first period, when they were included among the uninoculated, give a full measure of the value of inoculation. With a view to eliminating the source of error that would thus be introduced, we have taken as the basis of our comparison only the figures which have been supplied for the period from the 16th July to the 19th August. The comparison thus worked out covers the period which begins when plague became severe, and which ends when the comparability of the inoculated and uninoculated begins to be seriously doubtful. The comparison is as follows :—

—	Average Strength.	Number of Plague Deaths.	Percentage of Deaths on Average Strength.
Uninoculated - - - - -	20,225	980	4·8
Inoculated - - - - -	22,967	118	·5
Diminution of mortality in favour of inoculated - - - -			4·3

According to this table it would appear that 987 lives were saved in Hubli during five weeks. Even if the saving is considerably exaggerated by this calculation, it is still quite clear that a very large number of lives must have been saved in Hubli by inoculations during the whole course of the epidemic there. Moreover, we may note that an arithmetical estimate is not the only criterion by which we can appreciate the value of inoculations. And in Hubli their value is approved by the consensus of opinions of officers who have seen probably far more of this process and its results in practice than any other persons in India, and who, having every facility for forming a sound judgment as to its effect where plague was really virulent, are satisfied as to its great value.

1956–7.
I. 407 (xxx.).

In addition to the results of endeavours made to obtain an exact estimate of the benefits accruing from inoculation to the whole town, two series of data are before us from which it might appear possible to form such an estimate for parts of the town separately. These data relate to the results obtained by inoculation in certain selected houses in Hubli and among the employés of the Southern Mahratta Spinning Mills.

Captain Leumann's investigation sheets for Hubli.

I. 417–28.

The former series is accessible to us in a set of 69 investigation sheets which Capt. Leumann has appended to his report on Hubli. The results are striking, for out of 336 inoculated persons inhabiting the houses to which the investigation sheets related, only 11 were attacked, and 4 died, whereas of 144 uninoculated persons 84 got the plague, and 80 died. The mortality among the inoculated was 1 per cent. and the attacks 3 per cent., giving a case mortality of 33 per cent.; whereas among the uninoculated inhabitants the percentage of attacks was 61 per cent. and the mortality 57 per cent., giving a case mortality 94 per cent. The uninoculated people in these plague houses, therefore, suffered far more severely than the inoculated. But only a very small minority of the plague deaths among the inoculated in the town occurred in the houses in question. For Lieutenant Keelan, who collected 200 cases from which most of the 69 published in the Appendix were selected by Captain Leumann, stated that his 200 selected houses did not contain all the houses in which

725, 19,829–35. 9,877–88, 9,899–906.

inoculated and non-inoculated people lived together, and in which plague cases had occurred. His instructions were, he said, "to get good cases, houses where several had been inoculated, and one or two had not been inoculated. There were a lot of houses where, perhaps one person had not been inoculated, and the others had, and those I should call good cases where only one of the uninoculated got the disease." It is to be understood, however, that Lieutenant Keelan did not reject houses because of the result not being so favourable, but simply that he kept a special look-out for what he called good cases. We are unable to attach any importance to any numerical results deduced from these investigation sheets, for the reason that it is admitted that the houses were subjected to a double process of selection, first by Lieutenant Keelan, and then by Captain Leumann. While they have no statistical value, they are of interest as quite special examples of successful inoculation. 19,878.

Captain Leumann states that in the Southern Mahratta Spinning Mills a series of inoculations was carried out at the express wish of the Manager, Mr. Narayenrao, who kept careful records and carefully checked the results. On the 21st June there were 1,173 mill hands on the muster roll. We are told that after that date 1,040 were twice inoculated with a death-rate from plague of 22 altogether, or 2·11 per cent.; 58 were inoculated once only, and show a plague death-rate of 13·79 per cent. (8 altogether); while the remaining 75, who refused inoculation lost 20 out of their number from plague, or a percentage of 26·6 per cent. These figures are of no value for computing the relative immunity of inoculated and uninoculated, because we are not informed whether the inoculations were performed simultaneously or at what stage of the outbreak the average strength of the inoculated was reached. But the fact that the mortality among the inoculated is said to have amounted to 3·3 per cent., shows, if the figures are accurate, that the inoculated living among uninoculated inhabitants during an outbreak may suffer considerably from plague. Inoculations in the Southern Mahratta Spinning Mills, Hubli. I. 407 (xxviii.).

V.(2.) *The Town of Dharwar.*

440. Contemporaneously with the decline of the outbreak at Hubli, a serious outbreak of plague occurred in Dharwar. The population of the town in August 1898 amounted to 38,325, which is in excess of the population ascertained by the Imperial Census of 1891. The policy adopted on the outbreak of plague was the same as in Hubli, that is to say, stringent measures were enforced against the uninoculated, and these were, to a large extent, relaxed in the case of twice inoculated persons. When the plague got a foothold in the town in August, inoculations were pushed to the utmost extent by the energetic Collector of Dharwar, Mr. Cappel. By the week ending on the 14th October, the first for which we have figures, 4,926 people had been inoculated once, and 3,387 persons twice. The inoculations were proceeded with during the outbreak, and by the 4th January 1899, as we learn from a report which has been supplied by Dr. Hornabrook, who was continuously on plague duty at Dharwar, 17,604 persons had been inoculated, 11,457 twice, and 6,147 once only. A considerable number of inoculations were done in the town in the case of villagers who came in specially for the purpose, but in this place we need take no account of these. Inoculations in Dharwar town. 1571, I. 67. 1610, 1943–5, 2328–37, 23,747. 1974. I. 67. Not reprinted with these Proceedings 1958.

Owing to the fear of segregation and inoculation, Dharwar was, to a large extent, emptied of its inhabitants. Influenced by this fear, 18,000 people left the town in one week at the commencement of October, and by the beginning of November the census figures show that the population was reduced to about 9,700. The large majority of those who left the town camped in the fields round Dharwar. The greater part of those who remained in the town submitted to inoculation, and the condition of things thus brought about was, apparently, the following: Dharwar itself contained a population largely inoculated, while in huts in the fields round about the town there was a larger population mostly uninoculated. The importance of this will be realised when we come to deal with the statistics of the results of inoculation. 23,746. 1956, 1974–9. 1991–4, 2351.

Two series of statistics have been furnished to us. The first is that placed before us by Mr. Cappel, and based on a weekly revision of the census which was carried out after October 19th, in the same manner as at Hubli. The second series, that put in by Miss Corthorn, M.B., who worked at Dharwar during the whole time of the outbreak there, covers the period for the five weeks ending on the 16th November. Mr. Cappel's table does not distribute the population in such a manner as to permit of our basing upon it calculations to determine the relative immunity of the inoculated and the uninoculated. Neither are we able to make such calculations upon the figures for plague attacks and deaths which he gave us up to the 2nd December 1898, and according to which the Statistics of inoculation in Dharwar town. I. 67, 1763. 1771

uninoculated had had 1,172 attacks, with 916 deaths; the once inoculated had had 115 cases and 42 deaths, and the twice inoculated had had 29 attacks, with 7 deaths. On the other hand, Miss Corthorn's table gives us the number of the inoculated and uninoculated present in the town, and the number of plague attacks and deaths distributed between inoculated and uninoculated persons. The numbers of the population she took from the census, and she made special inquiries to obtain information of attacks and deaths, but none the less her figures in many cases agree with those given by Mr. Cappel for weeks ending two days later than the weeks adopted by her. Miss Corthorn said:—

1763. "I have kept a register as accurately as I could of all the people inoculated. I have kept in touch with the medical officer of the hospital, and I have examined the returns of all the patients going into the hospital who were returned as being inoculated, and I have also got the authorities where I have been at work at different centres to tell me of any people they know whom I have inoculated and who have got plague. I have also got, where possible, the inoculation checks returned and entered in the register of the people that I could get hold of whom I have inoculated and who have developed plague afterwards. Of course, I have missed a good number, but I think I have obtained very fair results. I have got hold of the larger part of the people who have developed plague. I have got hold of all the cases that I could each week, and I have worked out week by week the percentage of morbidity and mortality on the weekly census. I have done it also on my register and compared it with the average population, and I have also done it week by week on the numbers given in the census of the inoculated and uninoculated in the town." Upon the figures she collected she based calculations showing in respect of mortality a percentage in favour of the twice inoculated of from 93 to 100 per cent. The method of calculation employed by Miss Corthorn is that which has been adopted by Mr. Haffkine, and which is criticised in paragraph 454 below. The table compiled by her is as follows:—

Period. Week ending	Population.			Plague Attacks among			Plague Deaths among			Percentage of Reduction in Favour of			
										Once inoculated of		Twice inoculated of	
	Uninoculated.	Once inoculated.	Twice inoculated.	Uninoculated.	Once inoculated.	Twice inoculated.	Uninoculated.	Once inoculated.	Twice inoculated.	Morbidity	Mortality	Morbidity	Mortality
19th October 1898 -	5,748	3,542	2,112	168	17	1	124	7	1	83·5	90·	98·	99·
26th „ „ -	4,200	3,535	2,428	100	20	8	71	6	1	74·3	89·9	86·	97·5
2nd November „ -	3,089	3,074	3,557	61	15	2	52	3	1	75·2	94·2	97·	98·5
9th „ „ -	2,276	3,184	4,290	25	1	5	24	1	3	97·7	97·7	89·4	93·4
16th „ „ -	2,132	3,018	4,931	16	4	5	16	2	0	82·	82·3	86·4	100·

Criticism of statistics for Dharwar town.

1976, 1988, 1991, 1996, 2352–8.

Certain sources of fallacy which detract from the accuracy of Miss Corthorn's figures have been pointed out by herself in her statement quoted above. It seems to us, however, in consideration of the evidence of Mr. Cappel and Major Hardy, Chief Plague Officer at Dharwar, that a further serious source of fallacy, and a fallacy which Miss Corthorn has not attended to, is that in the table the population figures refer to the town alone, while the figures relating to plague attacks and deaths include also cases of persons who found their way back to their houses, or were brought into hospitals in the town from the camps around Dharwar. We have not been supplied with the numbers of these cases, but we were informed by Major Hardy that in the last week of November and the first week of December (*i.e.*, in the period almost

2356. immediately subsequent to the period covered by Miss Corthorn's statistics) nearly all the attacks and deaths among uninoculated which came to the cognizance of the authorities, were imported from among the body of uninoculated who were encamped in the fields. It will thus be seen that the statistics must have been seriously vitiated by this source of error. Our attention has, moreover, been drawn by Dr. Hornabrook to another source of error. He, in his report referred to in para. 440 above, speaking of Dharwar, stated that "It is impossible to properly ascertain the number of plague attacks that may occur among inoculated persons, for so many of the people are removed into the jungles directly they are taken ill." If this occurred, and we have no doubt that it did, inasmuch as the stringent plague measures which were adopted must have engendered concealment, we have here again a serious source of fallacy. For the number of plague attacks given for the inoculated

was so small that the removal of even a few of these cases from the cognizance of the authorities would have introduced an important error into the figures.

In view of the many serious sources of error adverted to above, we are of opinion that no reliance can be placed on the arithmetical estimate of the relative immunity of inoculated and uninoculated put forward by Miss Corthorn. But here again, as everywhere where we show reason for discrediting the arithmetical estimates which have been put forward as evidence of the benefits which have been derived from inoculation, we are desirous of making it plain that we are satisfied with the weighty evidence in favour of inoculation, which is afforded by the opinions of those who were on the spot, and had under their constant observation its beneficial effect in warding off plague and diminishing the fatality of the disease.

Lastly, the inoculations which were performed in the Dharwar Jail may here be briefly referred to, for the outbreaks of epidemic disease occurring in jails are of particular value, as all circumstances connected with them can be accurately controlled. Plague broke out in the jail in October, the first case occurring on the 19th of the month. Between that date and the 9th November, when the outbreak ceased, six attacks took place, of which five proved fatal. After the occurrence of three cases, inoculations were resorted to in addition to other sanitary measures. They were commenced on the 23rd October, and all the prisoners were inoculated by the 4th November. Only one case occurred among the inoculated, in a prisoner who was inoculated on the 6th November, was attacked on the 9th November, and was the only person attacked who recovered. Inoculations in Dharwar Jail. 2236. 2259. 2263–4.

V.(3.)—*The Town of Gadag-Betigeri.*

441. Next after the inoculations in Hubli and Dharwar, it will be convenient to consider the inoculations in Gadag-Betigeri, a third town in the Dharwar district to which the policy of extensive inoculations was applied. The town is not far from Hubli, and is divided into two parts, about one mile distant from each other, each of which had a population of about 13,000 in the beginning of October 1898. The total population for the whole town was thus somewhat greater than that, 23,000, ascertained in 1891. One part, known as Gadag, is inhabited principally by agriculturists who have their fields to move into when they vacate their houses, while the other part, Betigeri, contains chiefly residents of the commercial and industrial classes who do not find it so easy to move out of their houses. Plague established itself in the two parts at different times, but though it broke out in Gadag about the middle of October 1898, and did not become serious in Betigeri till it had reached its climax in Gadag, about two months later, we must deal with the figures for the town as a whole, for they were so collected for the purpose of compiling the statistics supplied to us. Inoculations at Gadag-Betigeri. 23,680.

The plague measures applied were practically the same as in Hubli and Dharwar, and inoculations were pushed by the grant of special privileges to the twice inoculated. A special census of the town had been taken in December 1897, and after the outbreak of plague, from 18th November 1898, arrangements were made to register the people as inoculated and uninoculated, and to keep the census up to date by a weekly revision on the same system as in Hubli. Evacuation appears, as in Dharwar, to have been practically insisted on in the case of the uninoculated. There were, however, many inoculated persons among those who left their houses, and as the result of evacuation, which was to a large extent voluntary, Gadag was practically emptied. We are told by Mr. Foy, M.B., who was on plague duty in the town, that "Betigeri, on the other hand, learned by the experience of Gadag. . . . The people of Betigeri made a great rush for inoculation, and thus avoided the general evacuation of the town by getting all the inmates of their houses inoculated." We find, however, from estimates put before us by Mr. Foy, that a large exodus, amounting to that of 5,800 people out of the population present in October, did take place from Betigeri, and that the proportion of the inhabitants of Gadag inoculated was not very much less than in Betigeri. Mr. Foy informed us that even before the plague obtained a hold of the place, the numbers of once and twice inoculated persons in Gadag-Betigeri were 2,135 and 365 respectively, of whom about 2,000 lived in Gadag. The subsequent progress of the inoculations and the course of the plague among the inoculated and uninoculated respectively is illustrated by the sets of statistics put before us by Mr. Foy and by Miss Corthorn, M.B., who also was on plague duty in the place. Miss Corthorn's statistics cover a slightly longer period and are arranged by weeks, Statistics regarding inoculations in Gadag-Betigeri. 23,832–6, 23,853–62. 23,680. 23,727. 23,831.

while Mr. Foy's are arranged by months. From both these series of statistics we have compiled the following table :—

—	Population as taken from the Census.			Total.	Plague Deaths according to Miss Corthorn.				Plague Deaths according to Mr. Foy.					Remarks.
	Uninoculated.	Once inoculated.	Twice inoculated.		Uninoculated.	Once inoculated.	Twice inoculated.	Total.	—	Uninoculated.	Once inoculated.	Twice inoculated.	Total.	
1898. Before week ending Nov. 18	—	—	—	—	11	0	0	11						
Week ending Nov. 18	20,813	2,560	1,696	25,069	5	0	0	5	Up to Nov. 18	37	0	0	37	
„ 25	17,659	2,916	2,799	23,374	9	0	0	9						
Dec. 2	15,473	3,456	3,660	22,592	13	1	0	14	*(difference*	*+21*	*0*	*0*	*+21)*	The figures given in italics represent the difference between Miss Corthorn's figures and Mr. Foy's.
„ 9	13,835	3,546	4,275	21,656	7	1	0	8	18 Nov. to					
„ 16	11,772	3,608	5,186	20,566	14	1	0	15	16 Dec.	52	2	0	54	
„ 23	8,726	3,676	6,706	19,108	15	1	2	18	*(difference*	*+9*	*−1*	*0*	*+8)*	
„ 30	6,145	3,168	7,173	16,486	27	3	4	34						
1899. Jan. 6	3,281	2,360	8,757	14,498	35	1	5	41	16 Dec. to					
„ 13	2,635	1,905	9,454	13,994	23	2	5	30	13 Jan.	105	7	16	128	
„ 20	1,766	1,292	10,390	13,448	10	2	5	17	*(difference*	*+5*	*0*	*0*	*+5)*	
„ 27	1,135	1,071	10,977	13,183	13	2	4	19						
Feb. 3	907	774	11,149	12,830	8	0	7	15	13 Jan. to					
„ 10	723	709	10,994	12,426	8	0	7	15	10 Feb.	41	5	30	76	
„ 17	732	585	10,940	12,257	6	0	8	14	*(difference*	*+2*	*+1*	*+7*	*+10)*	
Total from beginning of outbreak to 17th Feb.	—	—	—	—	204	14	47	265	Total from beginning of outbreak to 10th Feb.	235	14	46	295	

Criticism of above statistics. Para. 439 of this Report.

Miss Corthorn's and Mr. Foy's figures for the population are taken from the weekl census; for reasons similar to those discussed in connection with the Hubli census their absolute accuracy must be doubted. Further, the figures given contain som internal evidence of error, in that the totals of the average weekly strength of th uninoculated, the once inoculated, and the twice inoculated, as calculated from th figures given under these headings, do not together amount to the average weekl strength of the total population. The figures for plague deaths, moreover, do no agree in the two sets of returns which have been given us. Mr. Foy was himsel

23,837, 23,863. responsible for classifying as inoculated or uninoculated the cases of patients wh died in the hospital, and he was the ultimate authority for the classification, in case of doubt, regarding patients who died outside the hospital. Mr. Foy's are, therefore the official figures. Miss Corthorn's figures were, to some extent, obtained by inde

23,837-9. pendent inquiry. She tells us that she went every day to the hospital to look at all th cases that came in, and cross-examine the patients and their attendants, as t whether they had been inoculated or not. Further, where such cross-examinatio yielded no reliable information, she went to the census supervisors to consult their list and obtain their assistance, as they had got to know all the people fairly well. I these circumstances, we might have expected that Miss Corthorn's figures would no be less than the official figures, and would differ, if at all, only in the distribution o the deaths between inoculated and uninoculated. But, as a matter of fact, Mis Corthorn's figures, from the commencement of plague up to the 10th February 189 for which period a comparison can be instituted, give us a smaller total number o plague deaths than Mr. Foy's figures, as will be seen in the following table :—

Plague Occurrences from Commencement of Plague to 10th February 1899.	Among Uninoculated.		Among Once inoculated.		Among Twice inoculated.		Total.	
	Attacks.	Deaths.	Attacks.	Deaths.	Attacks.	Deaths.	Attacks.	Deaths.
Miss Corthorn's Figures -	266	198	28	14	86	39	380	251
Mr. Foy's Figures - -	287	235	35	14	104	46	426	295

In view of the fact that there is a discrepancy between the totals given us for plagu deaths in these two estimates, it is manifest that both cannot be correct. But inasmuc as the discrepancy does not affect the results when they are expressed in percentage

we have thought it of interest to calculate the relative incidence of plague cases and plague deaths on the inoculated and the uninoculated on the two sets of figures supplied to us. Miss Corthorn's statement allows us to make such a calculation for a period extending from the 18th November to the 17th February, while from Mr. Foy's figures the calculation can be made from the 18th November to the 10th February only. The results which we obtain are as follows :—

	From Miss Corthorn's Figures. From 18th November to 17th February.					From Mr. Foy's Figures. From 18th November to 10th February.				
	Average Population.	Attacks. No.	Attacks. Per cent.	Deaths. No.	Deaths. Per cent.	Case Mortality per cent.	Average Population.	Attacks. No.	Attacks. Per cent.	Deaths. No.	Deaths. Per cent.	Case Mortality per cent.
Uninoculated -	8,075	240	3·	188	2·3	78·2	8,687	239	2·7	198	2·3	83·
Once Inoculated -	2,388	29	1·2	14	·6	48·2	2,528	33	1·3	14	·5	42·5
Twice Inoculated	7,170	107	1·4	47	·6	44·0	6,868	104	1·5	46	·7	44·
Total Inoculated	9,558	136	1·4	61	·6	45·0	9,396	137	1·5	60	·6	44·
Diminution of mortality in favour of the inoculated -		-	-		1·7	-		-	-	-	1·	

Quite apart from the question of the accuracy of the data on which these calculations are based there are two points which must be noted. It must be noted, in the first place, that the comparison thus instituted is unduly to the disadvantage of inoculation, inasmuch as plague was relatively less severe in the earlier phase of the epidemic, when the number of the inoculated was below their average strength, than in the later phase when the number of the inoculated was above their average strength. The second point to be noted is that in the later portion of the period of observation the uninoculated population declined to such an extent as to suggest doubts as to whether it was comparable with the inoculated. An incomparability of this sort would, in the comparison which has been made above, operate unduly to the advantage of inoculation. The deduction to be made, if we may set off the error which would be introduced by the inclusion of the earlier period against the error which would be introduced by the inclusion of the later period, is that the uninoculated proved themselves in Gadag-Betigeri to be four times as liable to plague as the inoculated.

V.(4.)—*Villages in the Dharwar District.*

Inoculation in villages of the Dharwar district.

442. In order to complete the consideration of facts relating to the Dharwar district, it will be convenient to notice here two instances of severe village outbreaks in the district, in the case of which the plague measures ordinarily applied to villages were supplemented by the use of inoculation. The outbreaks are those that took place in Lingdhal and Huilgol. We have been supplied with particulars regarding them by Rao Bahadur R. R. Bhardi, who dealt with the plague in the villages in question in his capacity as an officer on the staff of the district.

Lingdhal. III, 225.

Infection was brought into Lingdhal some time in July 1898, but the existence of the disease there did not come to the notice of the authorities until the 21st August. Steps were then taken to number the people and to evacuate the village, and inoculations were also at once commenced by Dr. Foy. Evacuation was completed in ten days, and by that time 117 cases with 85 deaths had occurred. The inoculations were continued periodically, and a large number of people were operated on on the 7th and 8th, and on the 27th and 28th, of September. The population of the village in 1891 was 1,469, and the census taken after plague was detected showed that 1,372 people were then present. Of these 678 were inoculated, many twice, before the 14th October, after which date no further primary inoculations took place. After complete evacuation, the plague still continued severe, and 318 cases with 287 deaths took place. Of the plague occurrences reported after complete evacuation, 34 cases and 31 deaths were assigned to the inoculated. If this is correct, the balance of 284 cases with 254 deaths must have occurred among the uninoculated.

The numbers of the inoculated and uninoculated as given to us have been arrived at in the following way. The number of the inoculated was got by taking the total number of persons inoculated, without deductions for deaths and emigration. The number of the uninoculated was obtained by deducting the number of the inoculated, arrived at as above, from the population of the village ascertained by the census taken after plague was detected. Here again no allowance was made for diminution of numbers by deaths and exodus. These figures do not even approximately represent the average strengths of the inoculated and uninoculated present during the period of

observation. Therefore, they do not afford any basis for a correct arithmetical estimate of the amount of protection conferred by inoculation. None the less, while we appreciate the existence of serious errors in the figures, it seems to us impossible to draw any other conclusion from them than that inoculations must have done much good. The outbreak in Lingdhal was of extraordinary severity, evacuation did not check it, the proportion of the population who submitted to inoculation before the outbreak began to decline was certainly large, and yet the inoculated are reported to have suffered only a little more than one-eighth of the loss which befell the uninoculated after the completion of the evacuation.

Huilgol. III. 225-6.

The history of the second village which is to be noticed here may be given as follows in the words of Mr. Bhardi. "In the village of Huilgol, with a population of 1,866, both cholera and plague prevailed simultaneously. The first case of plague occurred on the 7th September 1898. Between the 7th and 18th September only eight cases of plague occurred. Between the 19th and 22nd September there were no attacks or deaths from plague. On the 24th September, Dr. Foy examined 11 cases of sickness, but was able to discover plague in only four cases. The other cases were found more or less suspicious and had all the symptoms of cholera. Complete evacuation could not be promptly effected owing to rain and inclement weather. By the 10th October 1898, a greater portion of the village was evacuated, and by the end of October it was completely evacuated. Only a small number of twice inoculated people were allowed to stay in the village. But a large number of cases of plague occurred among the people residing in the village before complete evacuation. Up to the 10th October 170 cases occurred, and between 10th and 31st October 228 cases occurred. Altogether, 398 cases occurred before complete evacuation, out of which 350 proved fatal. After complete evacuation only 45 cases occurred within about a fortnight. About 300 persons were inoculated by Dr. Foy about the 10th September, of whom 35 only were inoculated a second time. Forty-five attacks occurred among the inoculated, of which 31 proved fatal. The records do not show how many cases and deaths occurred among the twice inoculated. But I think that they were few, if not nil."

Here, as in the case of Lingdhal, it is obvious that percentages of incidence worked out on the numbers given as those of the uninoculated and inoculated will be fallacious, but, taking such percentages as a rough guide, we find that the inoculated, in comparison with uninoculated persons living under the same conditions, appear to have enjoyed a considerable amount of protection against attack, though inoculation did not prevent the plague from affecting them severely.

V.(5.)—*Bangalore City.*

Inoculations in Bangalore City.

443. We may now pass on to deal with inoculations in the City and the Civil and Military Station of Bangalore.

Para 76 of this Report.

Plague broke out in Bangalore City in the middle of August, 1898. By the 31st March 1899, as we have already stated, it had carried off an estimated number of 6,370 persons, amounting to 7·5 per cent of the normal population. Inoculation on a large scale was made a prominent feature of the plague policy, and the Mysore

3261, 3482.

Government on October 4th, issued an order making it practically compulsory for all its servants and those attending schools and colleges to be inoculated. The

3162.

authorities, however, did not insist on double inoculations. Contacts who had undergone inoculation once were not removed to segregation camps, and permission

3292, 3298-9, 3315-6.

was given for the home treatment of sick persons living in good houses of which all the inmates had been inoculated. The restrictions imposed on travelling were also

3482.

remitted in the case of the once inoculated. Inoculations were commenced in the first week of September directly the City was declared infected. Among the first to have themselves and their families inoculated were the majority of the officers serving under the Mysore Government. It was not, however, until the third week of September that the people came forward readily for inoculation. From the 1st September, to the 23rd September 523, and between the 23rd and the 30th no less than 4,163 operations were performed. By the 1st November 12,100 persons had been inoculated, and about this time the supply of fluid from Mr. Haffkine's laboratory ran short. The mortality from plague in the City continued to increase until the middle of November, and there had then already begun the enormous exodus of the population which, by the middle of

3505.

December, reduced the numbers in the town from 84,000 to an estimated number of

25,034, *and* Weekly Statements relating to the course of the epidemic.

45,000. By the end of December, when the epidemic had practically ceased, 25,163 persons had been inoculated, including 1,279 twice inoculated, but even after the plague had practically died out inoculations continued, and about 10,000 more persons were operated upon. No definite data can be obtained regarding the effects of the inoculations, because no periodical census was taken, and the population was reduced, as

3251, 25,111.

we have seen, by nearly 50 per cent. The inoculated left the town in large numbers

and we have thus no knowledge of the actual numbers of the inoculated and uninoculated present in the town at different times during the course of the epidemic. Inasmuch as we have seen that the recorded plague deaths in the City amounted to about two-fifths only of the presumptive number of such deaths, not much reliance can be placed on the figures which have been given as representing the incidence of the plague among the inoculated. Among the inoculated in Bangalore City 194 attacks were reported by the third week in December, but of these only 109 cases, of which 66 were fatal, were verified as actual plague cases among inoculates. The number of reported attacks among inoculates rose to 215 by the end of December, and to 233 by the 31st March; of these 139 and 158 respectively occurred in persons whose inoculation could be actually verified. Colonel Benson, I.M.S., Senior Surgeon and Sanitary Commissioner to the Mysore Government, stated that there may have been a very large number more of attacks and deaths among the inoculated than have been recorded, and that he was unable to say that either the number of attacks or the number of deaths was in any way complete. It was also pointed out that it was very difficult to ascertain, in the case of the large numbers of dead bodies thrown out into the streets, whether these had or had not been inoculated. Mr. Madhava Rao, the Plague Commissioner for the State of Mysore, stated that in some places he found that there had been attacks in inoculated persons which were concealed from the authorities. Such cases, he said, were never put on the register, in which therefore there must have been errors. There are thus no statistical results to be considered in connexion with the effect of inoculations in Bangalore City.

Para. 76 of this Report. 3516–20. 3522. 3287.

V.(6.)—*Bangalore City and Military Station.*

444. As has been already stated, the plague spread from Bangalore City to the adjoining Civil and Military Station at the end of September 1898. The policy adopted in the Civil and Military Station was similar to that adopted in the City. The first case of plague occurred on 15th September, but the epidemic did not become really severe until the second week in October. On the 14th October, 11 persons died of plague, and the highest mortality was reached on 22nd November, when in one day 75 persons died of plague. From that time the epidemic gradually decreased, though it was still high in December when we reached Bangalore. In February 1899 very few cases occurred, and the outbreak was practically over by the end of March. In the Civil and Military Station 1,075 persons, among the civil population, had been inoculated during the month of September; 12,378 had been inoculated by the 28th October; 20,591 by the 25th November; 27,419 by the 30th December; 29,928 by the 27th January 1899; 30,091 by the 24th February; and 31,794 by the 31st March. Of the ultimate total, 30,823 were single inoculations, and 971 double inoculations. The policy adopted towards inoculated persons was similar to that adopted in the City, and, as in the City, there was no proper periodical census or check upon the number of inoculated and uninoculated persons actually present in the Station from time to time, and the absolute necessity of accurate information on this point is obvious, in view of the large exodus which took place. With regard to the number of plague deaths, and also to the distribution of these among the inoculated and uninoculated, the information which is available is, as in the case of Bangalore City, very inaccurate. In many cases dead bodies were secretly buried or placed in the open, so as to prevent identification of the house in which death had occurred. In other cases bodies were found deserted in houses. These came to the notice of the medical men who were appointed to visit all cases of sickness that occurred and to find out particulars regarding all deaths. When asked concerning the correctness of the information that reached the authorities as to the number of deaths among the inoculated, Colonel Robertson, the Resident in Mysore, said, "I am not prepared to say that information would be reliable. . . . If a man has died, the municipal officer makes an attempt to get an inoculation certificate. He is supposed under the orders to get a certificate and send it to the inoculating officer. If there is any doubt as to whether the man has been inoculated or not, he sends it to the medical officer, who tries to trace it. The matter, however, is of great difficulty. I do not think the system is working well." Asked whether the people would readily give up the certificate of inoculation, the Resident replied, "I should think the tendency on the part of the deceased's relatives would be to stick to the certificate," and it follows that such a case would probably be classed as having occurred among the uninoculated. Captain Roe, R.E., who was in charge of plague measures in Bangalore, corroborated Colonel Robertson's opinion and said, "I think it is quite possible that these certificates may be passed on," and he admitted that there would be an undoubted inducement to retain them. The following table which refers in particular to cases certified to be plague by Government medical officers and unqualified native

Inoculations in Bangalore Civil and Military Station. Paras. 74–5 of this Report. 25,034 *and* Weekly Statements relating to the course of the epidemic. 2576–7, 2637. 2661–8. 2639–41. 3105. 2593–4. 2604. 2645. 2546. 2647. 2676. 2714–5.

25,037. medical practitioners between the 15th September 1898 and the 28th February 1899, has to be read in the light of the observations above quoted:—

—	Total number of Cases diagnosed as plague.		Number found to be inoculated.		Number found uninoculated.		Number doubtful or unknown.		Number of bodies buried without permit and counted as plague.
	Attacks.	Deaths.	Attacks.	Deaths.	Attacks.	Deaths.	Attacks.	Deaths.	
Seen by Government Medical Officers.	3,473	2,772	374	260	2,961	2,374	138	138	516
Seen by unqualified Native practitioners.	9	9	2	2	3	3	4	4	
Percentage of Total seen -	—	—	10·8	9·4	85·1	85·5	4·1	5·1	

In short, the figures with which we have been supplied regarding the civil population of the Civil and Military Station afford us no better basis for a correct estimate of the amount of protection conferred by inoculation upon the inoculated than do the figures which relate to the City of Bangalore.

Inoculations among the military population of Bangalore.

I. 114–5; I. 165–75.

III. 281–7.

Plague broke out among the Native troops of the Madras Army, the native followers of British Regiments, and the Transport and Commissariat followers stationed at Bangalore. We have been supplied with two sets of figures relating to the course of plague and the progress of inoculations among the military population affected. The first set of figures, put in by witnesses who appeared before us at Bangalore, carries us down to the 2nd December 1898 only, and will be considered later. The other set put in by Mr. Cadell covers the whole period during which there was plague in the Cantonments. Mr. Cadell's statistics supply us with details regarding each Regiment and body of followers among whom plague broke out, and are summarised by him in the two following statements:—

III. 286.

Corps.	Plague Seizures.					Plague Deaths.			
	Strength.	Among uninoculated.	Among once inoculated.	Among twice inoculated.	Total.	Among uninoculated.	Among once inoculated.	Among twice inoculated.	Total.
A.—Fighting Men.									
Queen's Own Sappers and Miners.	769	2	9	—	11	2	4	—	6
2nd Madras Lancers -	554	3	12	—	15	2	7	—	9
1st Madras Pioners -	797	1	—	2	3	1	—	2	3
17th Madras Infantry -	780	2	7	—	9	2	5	—	7
B.—Followers and Families of Native Troops.									
Queen's Own Sappers and Miners.	1,651	6	9	—	15	4	3	—	7
2nd Madras Lancers -	1,298	7	15	1	23	5	10	1	16
1st Madras Pioneers -	786	—	5	1	6	—	—	1	1
17th Madras Infantry -	959	—	7	—	7	—	3	—	3
Commissariat and Transport.	1,272	5	10	—	15	3	6	—	9
Total A. and B. -	8,866	26	74	4	104	19	38	4	61
C.—Followers of British Troops.									
4th Queen's Own Hussars	1,105	33	17	—	50	30	15	—	45
J. Battery, R.H.A. -	505	1	3	—	4	1	3	—	4
21st Field Battery, R.A.	595	3	11	—	14	3	9	—	12
25th Field Battery, R.A.	667	6	12	—	18	6	11	—	17
Total C. - -	2,872	43	43	—	86	40	38	—	78

III. 285.

Period.	Total of Native Troops and Followers.															
	Fighting Men.		Followers and Families.		Plague Attacks											
					Fighting Men.						Followers and Families.					
					Among uninoculated.		Among once inoculated.		Among twice inoculated.		Among uninoculated.		Among once inoculated.		Among twice inoculated.	
	Number once inoculated.	Number twice inoculated.	Number once inoculated.	Number twice inoculated.	Seizures.	Deaths.	Seizures.	Deaths.	Seizures.	Deaths.	Seizures.	Deaths.	Seizures.	Deaths.	Seizures.	Deaths.
Week ending 14th October 1898	637	–	328	–	–	–	–	–	–	–	–	–	–	–	–	–
„ 21st „ „	1,012	–	828	–	1	–	–	–	–	–	2	2	–	–	–	–
„ 28th „ „	1,728	–	2,603	–	6	6	–	–	–	–	14	12	2	1	–	–
„ 4th November „	2,548	–	6,553	–	1	1	4	3	–	–	4	3	11	10	–	–
„ 11th „ „	2,854	–	8,112	–	–	–	8	5	–	–	18	16	12	9	–	–
„ 18th „ „	2,839	28	8,403	38	–	–	2	–	–	–	9	9	20	13	–	–
„ 25th „ „	2,134	739	8,056	730	–	–	8	6	–	–	5	3	3	2	–	–
„ 2nd December 1898	1,476	1,401	7,769	1,062	–	–	3	1	1	1	1	–	5	4	–	–
„ 9th „ „	944	1,933	5,739	3,098	–	–	1	1	1	1	6	6	12	10	1	1
„ 16th „ „	691	2,209	5,508	3,330	–	–	1	–	–	–	2	1	14	9	–	–
„ 23rd „ „	678	2,222	5,421	3,417	–	–	–	–	–	–	–	–	2	1	1	1
„ 30th „ „	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–
„ 6th January 1899	–	–	–	–	–	–	1	–	–	–	–	–	3	–	–	–
„ 13th „ „	–	–	–	–	–	–	–	–	–	–	–	–	3	1	–	–
„ 20th „ „	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–
„ 27th „ „	–	–	–	–	–	–	–	–	–	–	–	–	1	–	–	–
„ 3rd February „	–	–	–	–	–	–	–	–	–	–	–	–	1	–	–	–
Total - - -	8,962	8,532	–	–	8	7	28	16	2	2	61	52	89	60	2	2

	Seizures.	Deaths.
Total among uninoculated - - -	69	59
„ „ once inoculated - - -	117	76
„ „ twice „ - - -	4	4
Total - - -	190	139

Criticism of figures regarding inoculations in military population of Bangalore.

The figures given above are summarised from detailed statements which set forth the the strength of the troops, the number of the followers as far as it was known, the numbers inoculated week by week from the commencement of inoculation to the date when plague ceased, and the numbers of attacks and deaths that occurred among the uninoculated, the once inoculated, and the twice inoculated respectively. Postponing for the moment the consideration of the accuracy of this statistical material, we may in the first place proceed to consider whether it is such as to be of use for the deduction of an estimate of the amount of protection which inoculation confers. We must therefore inquire whether, in addition to data with regard to the inoculated, the detailed statistics, which have been summarised above, supply us with data regarding the incidence of plague in uninoculated persons, whom we may justifiably compare with the inoculated soldiers and followers. Firstly, then, we must inquire whether there remained over in the separate military units any group of uninoculated persons who could serve as controls. Failing a control of the kind just mentioned, which would enable us to calculate the incidence of plague on the absolute number of inoculated and uninoculated, we must inquire whether we can obtain from the figures an average weekly strength of inoculated and uninoculated which we may justifiably use for the purpose of instituting the comparison which we have in view. Lastly, if we find that the statistics do not supply any figures on which we can base a comparison between the inoculated and uninoculated among the troops and followers themselves, we must inquire whether any of the facts with which we have been furnished with regard to the civil population of Bangalore supply us with the data for the required comparison. A scrutiny of the statistics of the military population reveals the fact that in each case the whole of the fighting men and of the followers who formed the communities under consideration were inoculated. There is thus no control group available in these communities themselves, in the sense in which control groups were present in the inoculation experiments in the Byculla Jail and at Undhera. Further, we cannot from the facts and figures regarding the civil population at Bangalore gather any idea as to what would have been the plague incidence among the military population if it had remained uninoculated. We have already pointed out that the number of

Paras. 432 and 438 of this Report.

the civil population is uncertain, that its distribution into inoculated and uninoculated is unknown, and that the record of attacks and deaths is incorrect. In order to determine whether we can find in the figures for the separate military units any material for a comparison between the average number of inoculated and the average number of uninoculated, during the period of the outbreak which elapsed between the commencement and end of inoculations, it will be convenient to
II. 281-5. examine the data supplied us regarding the different bodies of troops and followers in the order given by Mr. Cadell in his detailed statistics. In the 2nd Madras Lancers, it appears possible to compare the incidence of plague on the average strength of the inoculated and uninoculated, among the troops from the 15th October to the 18th November, and among the followers from the 29th October to the 18th November. Before the dates given there was no plague among the troops and followers, and subsequently there were no uninoculated persons to be treated as controls. Among the Queen's Own Sappers and Miners, the only period in which a comparison seems possible, either among the men or the followers, is in the week ending on the 28th October, but it is doubtful whether conclusions of any value can be drawn from the figures of an isolated week. Previous to the week ending on the 28th October, there was no plague in the corps now in question, and afterwards all the troops and followers were inoculated in the interval which elapsed before plague reappeared. No deductions of any value can be made from the figures relating to the 1st Madras Pioneers. One case of plague occurred among the uninoculated men of this corps in the week ending on the 28th October, when the inoculated men were somewhat less in numbers than the uninoculated. No further cases occurred till the week ending on the 2nd December, by which time no men remained uninoculated. Among the followers no plague occurred till all had undergone inoculation. The figures for the 17th Madras Infantry up to the week ending on the 21st October relate to a period when there was no plague, but a comparison may be possible from the figures which relate to plague among the men from the 22nd October to the 25th November as cases then occurred while part of the regiment was still uninoculated. After the 25th November all the men were inoculated, and the followers did not get plague at all till practically all had undergone the process. The Commissariat and Transport followers, we find, had cases in the week ending on the 21st October, before inoculation began among them, and then no further cases till the week ending on the 2nd December by which time all had been inoculated. If correct figures for the followers of the 4th Hussars were available, they would have afforded a very useful and interesting comparison between the inoculated and the uninoculated, as the number of attacks recorded among both classes was fairly considerable for some time after the inoculated reached their average strength; but as will be shown later on, the figures are not trustworthy, and therefore cannot be used to any good purpose. All the followers, both of J. Battery, R.H.A., and of the 21st Field Battery, had been inoculated before plague appeared among them. Among the followers of the 25th Field Battery, a comparison between the inoculated and uninoculated is possible for the period from the 29th October to the 25th November, but after that no uninoculated control remained.

Figures referring to followers incorrect and supply no basis for satisfactory comparison.

We have thus seen that a comparison could be instituted by making use of the figures relating to the men and followers of the 2nd Madras Lancers and of the Queen's Own Sappers and Miners, the men of the 17th Madras Infantry, and the followers of the 25th Field Battery; but when we proceed to inquire into the correctness of the figures we find that those relating to the followers cannot be accepted as accurate enough for our purpose. In one case the exact facts regarding the date of inoculation do not seem to be known. This is the case of the followers of the 4th Hussars. Inoculation
I. 115, III. 283. was said to have commenced on the 31st October 1898, but the returns show two
4622, 4626. inoculated cases even before the 28th October, and Lieutenant Clements, R.A.M.C. informed us that he commenced the inoculations on the 26th of October and finished them in four or five days. In the case of the 21st Field Battery, Lieutenant Clements,
4630. R.A.M.C., said he had seen a re-inoculated person sick with plague on the 15th
III. 284. December, but no re-inoculated person is set down in the returns as attacked. In several instances, cases among the uninoculated were reported to have occurred after it was supposed that the whole of the followers had been inoculated. Thus, the
III. 283-6. 25,139. followers of the Commissariat and Transport were, it was supposed, all inoculated by the 25th November, but after that date three cases in uninoculated persons were reported among them. The reported strength of the followers of J. Battery, R.H.A. had been inoculated by the 4th November, yet one uninoculated follower is set down as having died from plague in the week ending on the 9th December. In the 21st Field Battery, the number of inoculated followers was equal to the supposed total

number of followers by the 11th November, yet three cases among uninoculated followers occurred later. Similarly in the 25th Field Battery, where all followers are said to have been inoculated by the 25th November, four seizures and deaths among uninoculated followers nevertheless occurred between that date and the 9th December. Some idea of the amount of error that may exist can be formed from the case of the followers of the 4th Hussars. When he went to inoculate them, Lieutenant Clements was informed that they numbered 800, yet after he had operated on 1,068, he estimated that a balance of 50 still remained, and the number inoculated ultimately rose to 1,105 by the 25th November. It would appear that the people from the town were in the habit of coming into the lines of the followers to sleep, and may have been attacked and died there. Thus, as there can be no certainty about the number of the followers—which was in no case known exactly and may have been recorded with a considerable degree of error—it would be unprofitable to further deal with the figures for the followers of the 2nd Madras Lancers, the Queen's Own Sappers and Miners, and the 25th Field Battery, or to draw any conclusions from the figures for the followers as a whole. 4619-20.

In the case of the troops, however, though we may point out that their total strength as given by Colonel McGann and Mr. Cadell differs slightly, the figures given us may be accepted as fairly accurate and as, in a body of men who like these are under regular supervision, the facts regarding inoculation and attacks and deaths must have been correctly recorded, we may proceed to ascertain the results which we can gather from the figures regarding the men of the 2nd Madras Lancers, the Queen's Own Sappers and Miners, and the 17th Madras Infantry, for the periods which we have stated above to be those for which comparisons in these corps can be fairly made. These results may be summarised as follows:— Comparison between inoculated and uninoculated troops in different corps. 2832, III. 281-2.

Corps.	Period.	Average Strength.		Attacks.		Percentage of Attacks.	
		Inoculated.	Uninoculated.	Inoculated.	Uninoculated.	Inoculated.	Uninoculated.
2nd Madras Lancers.	15th Oct. to 18th Nov.	426	128	9	3	2·1	2·3
Queen's Own Sappers and Miners.	Week ending 28th Oct.	749	20	0	2	0	10
17th Madras Infantry.	22nd Oct. to 25th Nov.	698	82	5	2	·7	2·4

The figures are so small and agree with each other so little, that they do not give us any trustworthy estimate of the protection afforded by inoculation.

As then we have seen that the returns for individual regiments and bodies of followers are of little help to us for the purpose of making the comparison which we have in view, it seems necessary to summarise the results so as to ascertain what the average effect of inoculation was in the whole of the troops who were subjected to inoculation. We can take the figures for the troops after the week ending on the 21st October, when plague appeared among them, and up to the week ending on the 9th December, when the whole of the troops had been inoculated and the disease practically died out among them, and, if we assume, as we think we may, that all the troops were throughout exposed to some risk of infection, then, inasmuch as the inoculations in the different corps were done at different times, we may treat the uninoculated men of the later inoculated corps as controls for the inoculated men of the earlier inoculated corps. If we take the strength of the troops present to have been 2,900, we find that between the dates named we get the following results:— Results among troops as a whole. III. 285.

—	Average Strength.	Attacks.		Deaths.		Percentage of Case-Mortality.
		No.	Percentage.	No.	Percentage.	
Uninoculated - -	446	8	1·7	7	1·5	87·5
Inoculated - -	2,454	28	1·1	18	·7	62
Diminution of mortality in favour of inoculated - - - -						·8

This is the only comparison which we think legitimate, and though it does not tell much in favour of inoculation as a means of warding off plague, we may point out that the vaccine was not severely tested on account of the efficiency with which sanitary

measures generally were applied to the military community. The results of another comparison have been placed before us by Mr. Cadell, in which the percentage of estimated deaths among the population of the Civil and Military Station is reckoned as 5·4, and is contrasted with a mortality of only 0·9 among a total military population of 15,183. But this cannot be accepted as even approximate, for while on the one hand Mr. Cadell's comparison is unduly to the disadvantage of inoculation so far as it fails to take account of the exodus from the Civil and Military Station which was not parallelled by any scattering of the military population, on the other hand it is unduly to the advantage of inoculation in that no allowance is made for the effects of the strict supervision of the military, the impossibility of the spread of infection from concealed cases, and the segregation and other sanitary measures which were effectively applied to them.

III. 287.

Statistics put forward by witnesses at Bangalore. I. 114–5; I. 165–75.

We have postponed up to this point the consideration of the figures given to us by Colonel Esmond White, R.A.M.C., and other witnesses when we were at Bangalore because we find that they afford us no help in arriving at a correct estimate of the advantages of inoculation. The comparison which is instituted in them is one based on average strengths, and the average strengths have been incorrectly taken as, in calculating them out, figures have been included which refer to a period antecedent to the outbreak of plague.

We have thus seen that, owing to the fact that all the soldiers of the Native Regiments affected in Bangalore were inoculated, and that all necessary sanitary measures were effectively applied to them on the outbreak of plague, results which can be deduced from the figures relating to inoculations among them are not so valuable as they might have otherwise been.

V.(7.)—*Mysore City.*

Inoculations in Mysore City.

Para. 79 of this Report.

Para. 84 of this Report.

445. It will be convenient here to complete the consideration of inoculations in the Mysore State by dealing with the case of Mysore City, where also inoculations on a large scale were resorted to. We have already stated that though plague was imported into the City from Bangalore in October 1898, it did not become indigenous till the end of November, and was not severe till January 1899. To the end of June 1899, as narrated in the historical portion of this Report, a number estimated to amount to about 3·5 per cent. out of 80,000, the normal population of the City, died of plague. The measures adopted to deal with the outbreak were much less stringent than those generally adopted elsewhere. They did not confer any material advantages upon the inoculated, except, perhaps, in the matter of facilities for railway travelling. Inoculations were performed in Mysore City under conditions more favourable for the production of the greatest possible advantage than in most places in India, for a very large number of persons were inoculated before plague become virulent. The number of people inoculated in October was 3,745; 6,926 more inoculations were done in November; and the number rose to 25,922 by the end of December. In January 1899, 3,149 inoculations were performed, and in February, though the climax of the outbreak was not reached till that month, less than 500 people submitted themselves for the operation. By the end of March the total number of inoculations amounted to 29,978.

25,034 *and* Weekly Statements relating to the course of the epidemic.

Consideration of statistics for Mysore City. 24,977.

There was an exodus from Mysore, and a periodical census of the inoculated and uninoculated was not regularly taken from the commencement of the outbreak. Therefore, we have no means for the certain determination of the number of people present in the city from time to time, and their distribution among the inoculated and uninoculated. Moreover, the total recorded mortality exceeded the normal mortality by more than the recorded number of plague deaths, and the exact number of plague deaths, and their incidence on the inoculated and uninoculated parts of the population, is, therefore, unknown. In putting before us statistics regarding Mysore City, Mr. Cadell stated that the figures referring to attacks and deaths among the inoculated were ascertained by inquiries made in the households concerned, and that, if possible, a certificate was recovered, but it appears that out of 118 attacks assigned to the inoculated for the period up to the 28th February, 1899, only 98 attacks were verified as those of inoculates. Up to the end of March, out of 2,076 reported plague deaths, about 111 only are said to have occurred among the inoculated portion of the community. Only 158 inoculated persons are reported to have been attacked. *Primâ facie* it is not likely that, in Mysore, relatives or others would represent a dead man to have been inoculated when he was not so, and the medical officers stated that they only found rare instances in which transference of certificates was suspected. But, on the other hand, there was nothing to induce people to report plague, inasmuch as omissions to report usually passed undetected and unpunished. Therefore we cannot assume that the number of reported cases correctly represents the number of cases that actually occurred. Moreover, in consideration of the large proportion

25,038.

25,034 (2). Weekly Statements relating to the course of the epidemic.

25,040.

of the population which was inoculated, and the fact that the numbers of inoculated must have lived in close connection with plague in their houses, we cannot believe that the number of attacks and deaths attributed to the inoculated actually represents the number that occurred. Indeed, Mr. Cadell in putting the figures before us, pointed out that they were inaccurate. He said with regard to these figures: "I have put them here for what they are worth, but from Mysore City they are inaccurate as far as regards the examination of the bodies, and I would not vouch for them." Nevertheless, Mr. Cadell believed that there was no great number of hidden cases and deaths among the inoculated, though he thinks that a certain number of people did get inoculated in order to get away, as facilities for travel were given to the inoculated. 25,037. 25,112-5. 25,108.

We have, therefore, no data for any calculation, even approximate, of the effects of inoculation in Mysore City, and any attempt at such a calculation would fail. But we do not doubt that inoculations did much good in the City.

V.(8.)—*Karachi City.*

446. The most elaborate attempts to collect statistics regarding inoculations, in places other than those we have already dealt with, were made in Karachi, Bulsar, and Baroda, and we will now proceed to consider the figures laid before us for these places in the order given. Inoculations in Karachi City.

With reference to inoculations in Karachi we have to consider only the second outbreak of plague in the City. When this outbreak commenced in March 1898, Mr. Kaka, the Health Officer to the Municipality, suggested that inoculation should be introduced. On the 13th April he commenced to inoculate those who offered themselves. The Plague Committee, however, did not at once take up the matter, and it was not till the 2nd of May that they resolved to encourage inoculation by relaxing the ordinary plague measures in favour of the inoculated. The privileges that the Committee resolved to grant to the inoculated were not great, and consisted in permitting once-inoculated contacts to go to health camps instead of to segregation camps, provided that they had been inoculated at least four days before the occurrence of the case on account of which, if uninoculated, they would have been segregated. No additional privileges were accorded to the twice-inoculated. The privileges that were accorded were not sufficient to induce many people to come forward for inoculation, and manifestly, there was no inducement to undergo double inoculation. Up to the 9th July, when inoculation ceased because the outbreak was well on the decline, only 6,106 persons had undergone inoculation; only 287 of these had been inoculated twice. Of the total of 6,106 about one-third were Khojas, who will be particularly dealt with later on, and who had special reasons for being inoculated. II. 423.

The materials contained in the evidence of Mr. Kaka, and in his report on the Karachi inoculations which we have before us, provide us with information regarding the progress of the inoculations and the occurrence of plague among inoculated persons. This information may be tabulated as follows:— Statistics for Karachi City.

Week ending	Number of inoculations performed among			Occurrences of Plague among inoculated.		Remarks.
	Residents of Karachi other than Punjabhai Khojas.	Punjabhai Khojas.	Total.	Cases.	Deaths.	
April 15th, 1898	19	—	19	—	—	The second outbreak in Karachi reached its climax in the beginning of May and declined to small proportions in July, with a continuation of cases in small numbers in the succeeding months
„ 22nd „	25	—	25	—	—	
„ 29th „	108	16	124	—	—	
May 6th „	295	616	911	—	—	
„ 13th „	555	690	1,245	6	2	
„ 20th „	667	304	971	11	5	
„ 27th „	857	137	994	2	3	
June 3rd „	411	46	457	7	4	
„ 10th „	580	51	631	1	2	
„ 17th „	360	29	389	5	—	
„ 24th „	188	2	190	2	1	
July 1st „	62	7	69	1	—	
„ 8th „	23	—	23	—	—	
„ 15th „	41	—	41	—	—	
„ 22nd „	17	—	17	4	2	
„ 29th „	—	—	—	2	1	
August 5th „	—	—	—	2	4	
„ 12th „	—	—	—	1	1	
October 21st „	—	—	—	—	3	
Total	4,208	1,898	6,106	44	28	

II. 423, 426.

NOTE.—Mr. Kaka has counted among the uninoculated the three cases which occurred in the week ending on 21st October the patients were attacked more than five months after inoculation, and as he was of opinion that inoculation did not protect more than five months. This opinion he founded upon the dictum of a newspaper. 11,760-3.

Criticism of figures for Karachi City. 11,765; II. 424–5.

Mr. Kaka, in summing up his remarks on the subject of inoculations in Karachi as a whole, states that the figures show a difference of 84 per cent. in plague mortality in favour of the inoculated, but he himself says: "These results must be accepted with reservation, as the actual population of Karachi during the plague is not accurately known." Not only was the population not accurately known, but Mr. Kaka's estimates may have been very wide of the mark. No attempt was made to determine the respective numbers of those who remained in the City and those who left for the camps outside. Neither in the City nor in the camps outside was any census taken to ascertain the respective numbers of the inoculated and the uninoculated. Inasmuch as both classes left the City, the basis of Mr. Kaka's calculations with respect to the relative incidence of plague among the uninoculated and inoculated must be wrong, inasmuch as he made a deduction from the uninoculated on account of persons who left the City while he made no such deduction in the case of the inoculated. A further error has been admitted into Mr. Kaka's calculations, inasmuch as he omitted to notice that the incidence of plague among the inoculated and the uninoculated respectively ought to have been calculated on their average strength, and not, as he has calculated it, on the respective total numbers of those who were inoculated and uninoculated at the end of his period of observation. In the particular case of Karachi, the error which must thus have been imported into the calculations is very considerable, inasmuch as only 2,324 of the 6,106 inoculations were done before the middle of May, when the outbreak had begun to decline.

11,374, 11,713–7, 11,756, 11,804.

For the above reasons it will be apparent that no good purpose can be served by a detailed consideration of the figures relating to the City as a whole. We may therefore, turn to the consideration of certain further statistics which are included in the totals for the City, and relate to narrower spheres of observation. We have been furnished with two sets of statistics, one relating to the Khoja community, and the other to a group of inoculated and uninoculated persons living in a number of plague-infected houses in the City.

Inoculations among the Khojas of Karachi.

In dealing first with the figures relating to the Khojas, it must be pointed out that Khoja community of Karachi is divided into two sects, the Pirai and the Punjabhai Khojas who lived chiefly in the badly affected Machi Miani quarter, and were not like the other inhabitants of the quarter evacuated from their houses. The Pirai Khojas who are not followers of H.H. the Aga Khan, were practically uninoculated; of 21 members, 4 only were inoculated. On the other hand, the Punjabhai Khojas, like their co-religionists in Bombay, resorted to inoculation, in accordance with the recommendations and directions of H.H. the Aga Khan, who is recognised as spiritual leader by this sect. Under the Aga Khan's encouragement, it is not surprising to find that 81 per cent. of the Punjabhais were inoculated.

II. 426–7, 11,718.

Statistics regarding Punjabhai Khojas and method of compilation.

The statistics at our disposal, which relate to the Punjabhai Khojas, fix the number of the community at 2,326. This estimate is based upon a census taken by the Khojas in August 1898, at a time when they had ceased to suffer from plague. Mr. Kaka has checked the accuracy of the census by calculating out the numbers of the community on the number of deaths recorded among them in ordinary years, taking as the basis of his calculation the assumption that their average rate of mortality was similar to that of the City of Karachi as a whole. Mr. Kaka comes to the conclusion that the census is approximately correct, and we see no reason for doubting the correctness of his conclusion. Mr. Kaka computed the number of Khojas present at the beginning of the outbreak by adding, to the number found present at the census, a number corresponding to the number of reported plague deaths. Having obtained the initial population in this manner, he obtained his number of inoculated by going through his registers and totalling up the number of persons who had undergone inoculation during the whole period of the outbreak. He arrived at the number for the uninoculated by deducting from the assumed initial population the total number of inoculated persons. The total strength of the Khojas during the second outbreak of plague in Karachi was thus computed to consist of 1,898 inoculated and 428 uninoculated persons. These figures are not absolutely correct, because in estimating the initial population no allowance was made for deaths from general causes. We have, however, satisfied ourselves that the error which has been introduced into the statistics by leaving out of consideration the deaths from general causes does not materially affect the results.

II. 426–7.

11,720.

It is apparent that for his data regarding plague cases and deaths and for the distribution between the inoculated and uninoculated, Mr. Kaka has relied principa

13,227–39.

pon a comparison of the names of persons reported by the plague officers to have ıffered from plague, with the names recorded in the inoculation registers. He states, owever, that he attempted to supplement the information which he thus obtained by rivate inquiries. He says :—" I took the Headman of these Punjabhai Khojas into ıy confidence. I put a special man of my own in charge. He knows all these ouses where the Khojas are, and he went round from house to house and enume- ıted them." We cannot lose sight of the possibility that the information thus btained may have been insufficient or inaccurate information, but Mr. Kaka himself laims that in so far as they relate to the inoculated, at least, his statistics are quite orrect. 13,233.

On the basis of the information thus obtained, Mr. Kaka institutes a comparison etween the plague incidence among the inoculated and that among the uninoculated espectively, and arrives at the conclusion that there was " a difference of 77 per cent." ı plague mortality in favour of the inoculated.

Mr. Kaka's estimate of protection afforded not based on average strength; results of calculation on that basis. II. 427.

We cannot accept this estimate, because, since the numbers of the inoculated and ninoculated were constantly altering, the incidence of plague ought, we think, to be alculated upon their respective average strengths. The statement of the progress of ıoculation which was supplied to us by Mr. Kaka allows us to ascertain the average trength of the inoculated, and, from this, we are able to obtain an approximate stimate of the average strength of the uninoculated by deducting the average of the ıoculated from the total estimated population, which, as we have already stated, can e accepted as sufficiently accurate for this purpose.

We find that the large majority (1,809) of the total inoculations (1,898) was done etween the 25th April and the 3rd June. This interval corresponds with the period uring which plague was most severe in the City, and also to that in which all the ecorded cases among the Punjabhai Khojas occurred. We, therefore, cannot go rong if we limit our consideration to that part of the statistics which relates to the .x weeks between the 23rd April and the 3rd of June. We may work out a comparison or the period in question as follows :—

—	Average Strength.	Reported Plague Deaths.	
		Number.	Percentage.
Uninoculated - -	1,132	28	2·4
Inoculated - -	1,149	9	·75
Diminution of mortality due to inoculation - - -			1·6

It is to be noted that the comparison given above holds true only if the inoculated nd uninoculated are comparable communities, and if the deaths are correctly distri- uted among the inoculated and uninoculated and between plague and general causes. Ve now proceed to consider these points.

The figures which Mr. Kaka has given us, of the deaths recorded in the Khoja ommunity from causes other than plague, supply us with a means of determining hether, from the point of view of general health, the inoculated and uninoculated re comparable. We propose to set forth the data, first, for the period in which ıoculations were in progress and the community suffered from plague; secondly, for he period from the 1st July to the 30th September; and, thirdly, for the additional eriod from the 1st October to 21st January 1899, for which Mr. Kaka prepared figures ; our request and after his examination. For the first period we can base our calcula- ions on the average strength of the inoculated and uninoculated deduced from the gures in Mr. Kaka's report; for the last two periods, the only basis of calculation hich is available are the figures of the inoculated and uninoculated respectively, as etermined in the census made in the month of August. In making use of the census gures we have abstained from any attempt to apply corrections for exodus or for eaths from general causes, for, as we have already explained, we give these figures only

Question of comparability of inoculated and uninoculated Khojas at Karachi. 11,740; 11,780.

as approximately correct. The data for each of the periods may be summarised as follows:—

Period.	Number of		Deaths from General Causes among							
			Inoculated.				Uninoculated.			
	Inoculated.	Uninoculated.	Total.		Children under 5 (amounting to 8·9 per cent. of Total Inoculated).		Total		Children under 5 (amounting to 24·7 per cent. of Total Uninoculated).	
			No. of Deaths.	Rate per Mille per Annum.	No. of Deaths.	Rate per Mille per Annum.	No. of Deaths.	Rate per Mille per Annum.	No. of Deaths.	Rate per Mill per Annum, calculated upon the tota strength of th uninoculated
11,740. 23 April 1898 to 30 June (Period of plague) 10 weeks	Average Strength. 1,470	856	0	0	0	0	27	163	10	60
11,780. 1 July to 30 Sept. 13 weeks -	Absolute Strength. 1,898	428	0	0	0	0	15	140	11	106
1 Oct. to 21 Jan. 1899 16 weeks -	1,898	428	2	·5	0	0	3	22	2	15
Number and rate per mille per annum of deaths recorded in 39 weeks -			2	1·1	0	0	45	112	23	57

The results tabulated above are so remarkable, indicating as they do such an extraordinary difference in respect of general health between the inoculated and the uninoculated, that attempts have been made in various ways to account for the differences in the mortality of the two groups.

Excess of deaths in uninoculated not due to children.

It has been confidently suggested that the excessive number of deaths from general causes among the uninoculated must have been due to the excess of deaths among children, who formed a larger proportion of the uninoculated community than of the inoculated community. With a view of testing this suggestion, we have worked out from the data which have been supplied to us, the rate of mortality from general causes which deaths among children under five give upon the total strength of the uninoculated, and we have included the results in the table given above. It is shown by the table that no death occurred in 39 weeks among children under five 11,734. who were inoculated (these amounted to 168), while in the same period uninoculated children of similar age (these numbered 106) lost by deaths from general causes 23, or one-fifth of their number. It is also clear that, after deducting the percentage of deaths from general causes in children, there still remains to be accounted for an extraordinary difference in mortality from general causes between the inoculated and the uninoculated. Excluding the children, we find that during the period of plague the inoculated had not a single death from general causes, while the uninoculated suffered at the rate of 103 deaths per mille per annum, and that during the 39 weeks for which we have figures, the inoculated died at the rate of 1·1 only per mille per annum, while the death-rate among the uninoculated was 55 per mille per annum.

Excess of mortality in uninoculated not explained by wrong assignment of plague deaths

We must, therefore, look further for an explanation. Consideration shows that we should have an explanation of the excessive mortality among the uninoculated if we assumed that deaths have been wrongly attributed to general causes which in reality are due to plague. That this might have been so is evident from the fact that the causes of death were, at least in some cases, set down on no other evidence than the statement of the relatives of the deceased. Verification by corpse inspection 13,259. was not always made. Moreover, the suggestion that a large number of plague deaths may have been assigned among the uninoculated to general causes, might be supported by the figures given in the table above, which show that the period when the largest number of deaths from general causes was assigned to the uninoculated is the period in which plague cases were occurring among the Khojas. On the other hand, this suggestion would appear to be inconsistent with the fact that the number set down for the mortality from general causes among the uninoculated remained

abnormally high during the succeeding period up to the 21st January. In the 29 weeks beginning from the 1st July, the death-rate according to the figures of the mortality from general causes assigned to the uninoculated is equivalent to an annual death-rate among them of 76 per mille. Therefore, this suggestion does not explain the above normally high mortality among the uninoculated. Moreover, it leaves unexplained the extraordinary absence of mortality from general causes among the inoculated.

Wrong assignment to uninoculated of deaths in inoculated only explanation of excess of general causes mortality in uninoculated. 11,752–7.

We cannot believe that, in a community, like that of the inoculated, whose average strength was nearly 1,800, and which included among it 168 children under five and 73 persons of 60 years of age and upwards, there can have been no deaths from general causes during a period of 33 weeks, and that only two deaths occurred in the whole period of 39 weeks covered by the statistics supplied to us. We can see no way out of the difficulty except in assuming that a certain number of deaths which are set down as having occurred among the uninoculated ought in reality to have been assigned to the inoculated. This conclusion wins support from the fact that the total number of deaths which are assigned to general causes, set down as having occurred among the Khojas during the whole period of observation, would, if calculated upon the total number of the members of the community, both inoculated and uninoculated, give us a death-rate from general causes of about 28 per mille per annum, which, allowing for exodus and other causes of error, is an approximately normal death-rate such as we might have expected to have occurred.

Explanation that inoculation protects against diseases other than plague must be rejected. Para. 436 of this Report. 11,743.

We see no alternative to accepting this explanation, that deaths which occurred among the inoculated were erroneously assigned to the uninoculated, unless we can here fall back upon the suggestion which we have already dealt with in considering the case of the Khojas of Bombay, that anti-plague inoculation affords protection not only against plague but also against other diseases. We are unable to accept this suggestion as an explanation of the statistical data which we are considering, for we could not explain these data in accordance with this suggestion unless we assumed that the plague prophylactic conferred an even greater protection against diseases in general than that which it confers against plague.

Accurate deductions impossible from statistics of Khojas in Karachi.

On a consideration of all the circumstances, therefore, our conclusion is that the statistics compiled by Mr. Kaka cannot be accepted as giving even an approximately correct indication of the course of plague and the results of inoculation among the members of the Punjabhai branch of the Khoja community in Karachi, and it is not improbable that the statistics relating to this community have been seriously vitiated on account of the existence of some such tendency in Karachi, as that which we indicated to have been in operation among the Khojas in Bombay, to do more than justice to inoculation. At any rate, the statistics are so untrustworthy that they are useless for the purposes of accurate scientific deductions.

Mr. Kaka's investigation sheets. II. 428–42.

We have now to consider a group of figures which relates to the effect of inoculation in 56 plague-infected houses. The particulars concerning these have been collected by Mr. Kaka and put before us in the form of investigation sheets. These investigation sheets will be found in the appendix to Mr. Kaka's report on inoculation. Before proceeding to consider whether any deductions can be based on these, it is necessary to note a few points regarding them which have attracted our attention and which we think indicate that success has not been attained in collecting, for all the houses in question, all the data which are essential to enable us to arrive at a correct judgment as to the incidence of plague among the inoculated and the uninoculated. We notice, for instance, that in the case of one house—that to which the investigation sheet bearing the number 38 in Mr. Kaka's appendix refers—the only residents of the house would appear to have been a girl of eight years of age who died of plague and a boy of five years of age. It is obvious that there must have been other people in this house, and, indeed, we learn from a note on the investigation sheet that there was, at least, their mother in the house with the two children. The investigation sheets numbered 2, 11, 15, 16 and 24 give us to understand that in each of the houses in question there was only one resident. In one case the sole resident was a young woman. In view of our knowledge of the customs of the natives of India, with regard to family life and the coverture of women, we think it probable that these statements cannot be taken as accurately representing the condition of things in these houses. Moreover, we find that in the case of each of these houses the resident who appears

in the records was an inoculated person who died. We think that we may from this fact infer that Mr. Kaka's investigation sheets were, in the first instance, compiled by checking from the register of inoculation the names of those who were reported to have been attacked or to have died of plague. We do not doubt that Mr. Kaka then proceeded to verify and complete the data thus obtained by actual visits to the houses concerned. But we think that, at the time of the inquiry, the rest of the inhabitants—and this relates in particular to the uninoculated inhabitants who were liable to be segregated—may very probably in many cases have run away, so that it was impossible to ascertain how many people had really lived in each house, and whether other cases of plague had or had not taken place there. Mr. Kaka himself, in the case of one or two houses, adds a note to the effect that full particulars could not be ascertained. Further, we find on going over the investigation sheets that, although there are six houses in which more than one case is recorded among the inoculated, there is no house in which more than one case is recorded as having occurred among the uninoculated. This can, we think, be explained only on the assumption that the uninoculated left their houses directly after plague appeared in them.

Investigation Sheets, Nos. 38 and 90.

Mr. Kaka's investigation sheets appear to us to have little value; first, because as has been shown above, we have reason to believe that they are not complete records, and, secondly, because the inoculated and uninoculated were not living under comparable conditions, inasmuch as the former remained in the infected houses, while the latter were removed from them as soon as a plague case occurred. Nevertheless, it will be of interest to summarise the data which are set down by Mr. Kaka. If we take into consideration the facts which are recorded regarding the whole of the 56 houses mentioned in Mr. Kaka's appendix, the results are as follows:—

	No.	Attacks.		Deaths.		Case Mortality per cent.
		No.	Per cent.	No.	Per cent.	
Uninoculated -	76	17	22·5	12	15·7	70
Inoculated - -	285	47	16·7	25	8·9	53
Diminution of mortality due to inoculation - - - -					6·8	

If, on the other hand, we exclude 21 houses in which plague cases occurred only among the inoculated, and in which there is no record of the presence of uninoculated residents, we shall, on account of the elimination of houses in which there are no uninoculated controls, obtain from such materials as Mr. Kaka has furnished results which will have somewhat greater objective value. The comparison thus instituted will then read as follows:—

	No.	Attacks.		Deaths.		Case Mortality per cent.
		No.	Per cent.	No.	Per cent.	
Uninoculated - -	76	17	22·5	12	15·7	70
Inoculated - -	213	23	10·6	11	5·1	45
Diminution of mortality due to inoculation - - - -					10·6	

V.(9.)—*The Town of Bulsar.*

Inoculations at Bulsar 16,256. 16,291.

447. Inoculations were introduced into Bulsar, a town in the neighbourhood of Surat, in the early part of 1898. They were limited in their application to four wards of the town. Considerable inducements were offered to encourage inoculations, inasmuch as, in contrast with what was done in the case of the uninoculated, the inoculated were not segregated and were allowed at first to treat their sick in their own houses, and afterwards in special houses set apart in their quarters. Out of a total population of 1,190 in the four quarters concerned 1,018 were inoculated. The

results of the inoculations are set forth in the following table compiled from particulars given to us by Assistant Surgeon Dadachanji, who did the inoculations :—

Quarter.	Population taken into account.	Number inoculated.	Number uninoculated.	Percentage of plague attacks among		Percentage of plague deaths among		Case mortality (deaths to attacks) among	
				Inoculated.	Uninoculated.	Inoculated.	Uninoculated.	Inoculated.	Uninoculated.
Dheberwad -	180	170	10	2·4	0	0·6	0	1 in 4	0 in 0
Taiwad -	499	490	9	13·3	44·4	4·7	22·2	23 in 65	2 in 4
Machiwad -	105	97	8	8·2	12·5	3·1	12·5	3 in 8	1 in 1
Ghanchiwad	296	261	35	2·7	11·4	1·5	11·4	4 in 7	4 in 4
Total -	1,080	1,018	62	8·2	14·5	3·0	11·3	31 in 84	7 in 9

16,256.

It is to be noted that the plague occurrences entered in this table all occurred after the completion of inoculations. It is further to be noted that the population under observation does not include the whole of the population of the wards. Mr. Dadachanji has in his figures for the population under observation left out of account 110 people out of the total population of the four wards, as 62 of these were children under two, 30 had had plague during a previous outbreak, and 18 were attacked by plague in the outbreak under consideration before inoculations were completed. Lastly it is to be noted that the number of uninoculated persons who came under observation amounted to no more than 62. This number is so small that we may assume that it consisted in large part of persons who remained uninoculated because they were unfit for inoculation. The mortality among these might naturally be expected to be higher than in a normal community. As a matter of fact, we find that from May to December 1898, the 62 uninoculated lost 8 persons on account of deaths from general causes, while the 1,018 inoculated lost 10.

16,259. II 320. II. 320.

As far as we have been able to judge, Mr. Dadachanji's figures have been carefully collected. A census of the town was taken at the end of 1897, and a complete census of the inoculated wards was taken again at the time of inoculations. A list of the inoculated and uninoculated inhabitants was posted up on the door of each house, and each house is said to have been visited daily by a medical officer who had orders to satisfy himself regarding every person living in it, and to call out the inhabitants daily for inspection. In this manner the number of plague cases and deaths, and their distribution between the inoculated and the uninoculated, were ascertained.

16,288. 16,298–306.

It will be seen from the table that the effect of anti-plague inoculation manifested itself here especially in the reduction of case mortality. When we take into consideration the fact that the uninoculated were a sickly community, we see that it is probable that inoculation exerted little, if any, effect in warding off plague attacks.

In addition to the comparison which has been set forth above between inoculated and uninoculated persons living in the same wards, we have, in Bulsar, in two instances data for comparing the course of plague in a ward which was almost completely inoculated "with the object," as Mr. Haffkine told us, "of seeing to what extent plague could be arrested by that," with the course of plague in a similar ward to which inoculations were not applied. The wards in question are two wards inhabited by Ghanchis, and two wards inhabited by Tais. In each case, in order to obtain the total population of the inoculated wards for comparison with the total population of the uninoculated wards, we may add together, in the case of the former, the whole of the inoculated and uninoculated residents.

Comparisons between inoculated and uninoculated wards in Bulsar.

75. 16,280, 16,287.

We may consider first the case of the two Ghanchi wards. Our information concerning these was obtained from Mr. Dadachanji. The following table shows the incidence of plague among the Ghanchis in the inoculated and the uninoculated wards respectively :—

Two Ghanchi wards in Bulsar.

16,287.

Ward.		Population.	Plague Cases.		Plague Deaths.	
			Number.	Percentage on population.	Number.	Percentage on population.
Ghanchis	Uninoculated Ward -	154	23	14·9	16	10·4
	Inoculated Ward -	296	11	3·7	8	2·7

We have no details which will enable us to arrive at a judgment as to how far the two Ghanchi wards contained comparable communities. None the less, it is worthy of note that the above table shows a diminution in plague attacks of about 11 per cent. and in plague deaths of about 8 per cent. in favour of the inoculated ward. This estimate is based upon the census population of the two wards. We were, however, told by M. Dadachanji that there was a considerable exodus from the uninoculated ward and that at least half the population left; on the other hand, it is said that no similar exodus occurred from the inoculated ward. In view of this, the advantage in favour of the inoculated ward would be practically double that which is set forth in the table.

16,287-91.

Two Tai wards in Bulsar.

The next case we have to consider, is that of the two Tai wards. The information at our disposal as to the inoculated Tai ward is derived from Mr. Dadachanji's evidence; the data relative to the uninoculated Tai ward were supplied by Major T. E. Dyson, I.M.S., who was in the Surat district in 1897 and 1898. We were assured by Major Dyson that the two wards were absolutely comparable. On the other hand, it would appear that both wards did not suffer from plague at one and the same time, so that the figures of the two wards may relate to outbreaks of different severity. A comparison, however, can be made as follows:—

16,280-1.
19,970-94.

Ward.		Population.	Plague Cases.		Plague Deaths.	
			Number.	Percentage on population.	Number.	Percentage on population.
19,991. Tais -	Uninoculated Ward -	492	65	13·2	47	9·5
16,281.	Inoculated Ward -	490	65	13	23	4·6

In spite of the fact that the prophylactic fluid was operative in reducing plague mortality by about 5 per cent., it appears that it did not sensibly reduce liability to attack by plague.

V.(10.)—*Baroda State.*

Inoculations in the Baroda State.

448. In the State of Baroda inoculations were introduced into a number of villages. Our information with regard to them is drawn from data supplied by Mr. Mehta, who was the medical officer in charge of plague operations in the State. This information is contained in Mr. Mehta's evidence and in a report which is published as an Appendix to our Proceedings. In 13 of the 31 villages into which inoculation was introduced there was no plague after inoculation. In them we have no materials for a determination of the effects of inoculation on plague. In 18 villages, cases of plague did occur after the inoculations. One of these villages was Undhera, where Mr. Haffkine personally superintended the inoculations, the results of which have already been considered in this Report. Excluding Undhera, there remain only 17 villages which come into consideration here. With a view to testing the accuracy of the figures which have been supplied to us with regard to the course of plague in these villages, we propose to consider only the figures which relate to the six villages in which plague cases occurred among both the inoculated and the uninoculated. The figures relating to these six villages may be collected together as follows:—

App. LII. in Vol. II. of these Proceedings.

Para. 438 of this Report.

Village.	Total.	Population. App. R. II. 517.		Total Plague Cases. App. U. II. 519.	Total Plague Deaths. App. U. II. 519.	Plague Cases after Commencement of Inoculations among. App. R. II. 517.	
		Uninoculated.	Inoculated.			Uninoculated.	Inoculated.
Savali - -	6,551	6,535	16	446	321	465	4
Billimora - -	5,350	4,918	432	277	217	228	26
Koili - - -	3,221	2,162	1,159	144	129	123	13
Jarod - - -	2,137	2,057	80	61	44	24	2
Bajwa - -	609	453	156	47	37	34	5
Kantharia - -	364	254	110	22	22	4	2

Mr. Mehta has, after adding in the figures which relate to the village of Undhera, summed up the results of inoculation which are tabulated above as follows; "The number of attacks per mille of population is 24·3 in inoculated, against 53·8 in the others, and of deaths, 55·4* against 46·9.* The results of Undhera, Koili, and

II. 508.

* We presume these figures have been inadvertently transposed by Mr. Mehta.

Bajwa are very good, and of Billimora and Kantharia very fair. The effect in case mortality at Savali is also fairly good."

Criticism of statistics of villages in Baroda

Before we can accept Mr. Mehta's conclusions we must check against each other the figures given by Mr. Mehta in the various tables with which he has supplied us, and further we must check Mr. Mehta's figures by comparing them with figures given by other witnesses. It will suffice to notice a few of the points which have arrested our attention in applying these checks. First then as to the total population. In the case of Billimora we find that Mr. Mehta, on pp. 263, 265, 504, 517, and 520 of the Second Volume of our Proceedings, has given five different estimates of the population which vary from 5,215 to 5,577. Further there are differences in the estimates of population furnished by Mr. Mehta and those furnished by other witnesses in the cases of Koili and Bajwa. Dewan Bahadur V. M. Samarth, Subah of the Baroda Division, and Mr. Shamsuddin J. Sulemani, the Chief Medical Officer of the State, stated that the population of Koili and Bajwa was 3,695 and 703 respectively, instead of 3,321 and 609, the figures given by Mr. Mehta. With regard to plague cases we may notice in the first place that Mr. Mehta is in conflict with himself in respect of the figures he gives for Savali. It will be seen that he gives the total number of plague cases during the epidemic as 446, whereas he gives the number that occurred after inoculation as 469. Similarly he is conflict with himself in respect of the number of plague cases that occurred after the introduction of inoculations in Billimora: in one statement he gives the gives the number as 254 and in another as 156. Dewan Bahadur Samarth has given us the number of plague attacks that occurred in each of the villages entered in the table above, excepting Billimora. His figures in every instance disagree with those given us by Mr. Mehta. Thus Mr. Mehta's totals of plague attacks for Savali, Jarod, and Kantharia are 446, 61 and 22 respectively, while the totals of attacks in these villages according to Dewan Bahadur Samarth's figures are 507, 24 and 15 respectively. Moreover, Mr. Shamsuddin J. Sulemani gives us the number of attacks in Koili and Bajwa, and in the case of the former village he states that there were 117 attacks, in this differing from both Mr. Mehta and Dewan Bahadur Samarth, though in the case of Bajwa, his figures agree with those of Mr. Mehta.

II. 496–7.
14,794.
II. 509 (A. 1).
II. 517 (B.).
II. 501, 500, 498.
14,794.

After these criticisms it is obvious that we shall not be able upon Mr. Mehta's general statistics to base any accurate estimate of the amount of protection conferred by Mr. Haffkine's inoculation throughout the State of Baroda or in any particular Baroda village considered as a whole.

Mr. Mehta's investigation sheets.

We have, however, further to consider certain statistics which Mr. Mehta has prepared and which deal with the incidence of plague in particular groups of infected houses situated in certain of the villages of the State. The data which are furnished regarding these are in the form of a synopsis of investigation sheets for each of the three villages of Billimora, Koili, and Kantharia. They may be tabulated as follows:—

Village.	Number of houses to which data apply.	Number of inoculated.	Number of uninoculated.	Percentage of attacks among		Percentage of deaths among		Percentage of case mortality among		
				Inoculated.	Uninoculated.	Inoculated.	Uninoculated.	Inoculated.	Uninoculated	
				Per cent.	Per cent.	Per cent.	Per cent.	Per cent.	Per cent.	
Billimora	64	105	249	25	30·4	15·2	24·8	60·6	81·7	14,848.
Koili	26	48	73	27	24·7	18·8	23·3	69·4	94·5	14,937.
Kantharia	3	4	14	50	28·6	50·0	28·6	100·0	100·0	II. 506.
Total	93	157	336	26	29·4	17·2	28·0	66·0	95·0	

We have not been supplied with any materials for effectively criticising these investigation sheets, for we have not been informed how they were originally prepared, how the numbers of the inoculated and the uninoculated in the various houses were ascertained, and what steps were taken to secure that entries were properly made. We learned however that the facts set down on record were not always elicited by personal enquiry, and our general impression is that the inoculation sheets were not actually prepared on the spot and therefore cannot be trustworthy. Nevertheless we may note two points regarding the results set down. The first of these is that the inoculated residents of the plague-infected houses under investigation appear to be have been almost as liable to attack as their uninoculated fellows. The second point is that,

15,040–54.

inasmuch as inoculation considerably reduced the severity of attacks, there was a diminution of mortality amounting to 11 per cent. in the number of the inoculated as compared with the uninoculated.

Inoculations in Baroda City.

The figures which have been supplied to us regarding inoculations in the City of Baroda afford us no help in coming to a conclusion as to the value of inoculations in general. Two thousand seven hundred and twenty-five people in all were inoculated in the City. Of these 2,146 were residents of the City, which has a population of

14,988. 112,471; the balance of 579 inoculated persons consisted of prisoners. These prisoners may be left out of account, inasmuch as there was no case of plague in the Jail or anywhere near it. As to the other persons inoculated, it is to be noted that no plague

14,986. occurrences were recorded among them, but we have nothing to show that they were kept under observation. So little was known of them that in the case of 822 persons no information of any kind is available as to their caste or social position. Plague occurrences may have taken place among them without being brought to the notice of the authorities as having occurred among inoculated persons.

Inoculations in Baroda Cantonments.

In concluding the consideration of information collected by us at Baroda, we have to deal with the facts which Major Sarkies, I.M.S. Residency Surgeon at Baroda, gave us with regard to inoculations in the Baroda Cantonment. Plague appears to have

15,401, 15,431, 15,436. been severe in the Cantonment in March, and to have then declined until September and October, when it again became severe, and though we have not been informed of the exact dates of the inoculations, we understand that a large number were done in March and another large number in September and October. Major Sarkies told

15,401-2. us that a total of 2,048 persons were inoculated between November 1897 and December 1898, and that of these 330 were soldiers and their families, while the remainder were

15,403. civilians. As to the uninoculated, Major Sarkies told us that "there were about 800 not inoculated in the regiment, including the families, and about 1,000 in the bazar." The

15,475. strength of the inoculated and uninoculated respectively was computed by simply subtracting the total number of inoculated persons from the total population of the Cantonments, which was apparently known to have been about 4,000. One hundred

15,426. and nineteen cases with 94 deaths were reported to have occurred among the uninoculated, while the inoculated were reported to have had only 13 cases with

15,424. 4 deaths, and Major Sarkies thinks that "that is a marked difference, as over half the people were inoculated." He, therefore, formed a strong opinion that inoculation was beneficial.

Examination of these figures shows that they make no pretence to accuracy. Moreover, as the period covered by the inoculations was clearly a prolonged one, no comparison between the inoculated and uninoculated is possible, except by comparing the incidence of plague upon the average strength of each class. But we do not know the period of observation during which the cases and deaths occurred. Further, we are unable to determine the average strength either of the inoculated or of the uninoculated, and this is a matter of considerable importance, as about a quarter of the whole number of the inoculated are said to have left the Cantonments, and there was probably an exodus among the uninoculated also. We can, therefore, make no accurate comparison between the inoculated and the uninoculated, but we see no reason to reject the opinion offered to us by Major Sarkies in favour of inoculation after an extended observation of its results in the Baroda Cantonment.

V.(11.) *The Punjab.*

Inoculations in the Punjab.

Para. 101 of this Report.

449. Inoculation was not an important feature of the plague policy applied to Punjab. The population of the villages in which plague appeared, up to the end of September 1898, was about 92,000, but the number of people inoculated in these villages was small. As is stated by Captain James, I.M.S., in his Report on the Punjab, "in the two infected areas there were 3,918 people inoculated. Among these there were eight attacked with plague in the Jullundur district and only one died, and that woman who was proved to have had an insufficient dose of prophylactic." In the Jullundur district the number of the inoculated amounted to 2,408, while in the Hoshiarpur district the number was 1,510. The people were unwilling to submit to a second inoculation, and we are told that only 103 were inoculated twice.

Captain James' Report. 10,167.

In considering the details which have been placed before us regarding these inoculations we find that the prophylactic fluid was not much used in the Punjab under conditions which put it to any considerable test of its efficacy. Thus we find that of the inoculations done in the Jullundur district, 244 were done in the village of Thandian and 372 in the village of Dasanjh, where there was no plague. Moreover, 209 were done in Khatkar Kalan in a camp of 600 people in which no plague occurred after the inoculation except in the case of one inoculated person, who probably had been infected before

is inoculation. Similarly, although a number of people—variously stated as 801 nd as 826—were inoculated in Garhshankar, out of a population of 5,000 to 6,000, we nd that the inoculations were not done till the 11th May, and the plague outbreak a the place had by that time practically ceased, for out of a total of 152 plague cases hich occurred among the people of the town, only five occurred after the inoculations ere begun. 10,674-8. 10,861.

Banga Town is the only place in the Punjab regarding which we need go into any etails of the progress of inoculation and the course of plague. The first case in this own occurred on the 7th March 1898, though the authorities did not hear of the existence f the disease until the 11th of that month. The last case took place on the 20th June, nd the total number of attacks was 103, with 65 deaths. A census of the population as taken in March, and showed that there were 4,917 residents of the village. Evacuation, disinfection, and inoculation were all commenced on the day when plague as discovered and were completed about the 29th of April. As the people did not at rst show themselves very willing to come forward for inoculation, the inoculations roceeded gradually. On April 30th, the number of the inoculated amounted in all o 865. It is remarkable that among the inoculated in Banga a very large number of ildren under 10 were included. Such children numbered 292, or more than 33 per nt. of the total number of the inoculated, a proportion which is very different from at noticed in other places. Inoculations at Banga, in the Punjab. 10,172. 10,235. 10,178.

Captain James, in commenting upon the results obtained in this village, remarks that 18 per cent. of the whole population were attacked, while the attacks among the oculated amounted to ·69 per cent., and all the inoculated had the disease very mildly. n a case like this, however, more correct results could have been arrived at by lculating percentages upon the average strength of the inoculated and uninoculated spectively. We have not before us materials which enable us to do this. 10,172.

In addition to summing up the results for the village as a whole, Captain James has ven us a list of all families in Banga in which plague cases occurred. This list ows the number of inoculated and uninoculated persons in the families and of e plague occurrences among them. In the case of houses in which inoculated rsons were attacked, Captain James has supplemented his list by fuller details the form of investigation sheets. An examination of the list shows that while tails are given with reference to 81 families, the 103 attacks which took place the town were distributed over 70 families only. A summary of particulars regarding l the families entered in the list is given below in tabular form, but we may point out at a statement of this sort is of no value unless we can assume that the cases among e uninoculated all occurred after the members of their households, who are set down inoculated, had already undergone the operation:— List of plague infected families in Banga and investigation sheets put in by Capt. James, I.M.S. 10,173-7.

—	No. of Houses.	Inoculated Residents.		Percentage of Attacks on Total Number.	Uninoculated Residents.		Percentage of Attacks on Total Number.
		Total Number.	Number attacked.		Total Number.	Number attacked.	
	7	10	0	—	19	0	—
	2	7	0	—	0	0	—
	2	0	0	—	12	0	—
	1	3	1	—	0	0	—
	43	0	0	—	143	55	38·5 p.c.
	26	54	5	9 p.c.	107	42	39·2 p.c.
Total	81	74	6	—	281	97	—

When we collate with data set down in the general list of affected families the rticulars which Captain James has given in his investigation sheets for the six houses which inoculated persons were attacked, we find that the particulars in the cases of houses referred to in the investigation sheets numbered 1, 3, and 5, do not agree ith the details which have been given us in the general list. For instance, the vestigation sheet numbered 1 states that the house to which it relates contained two oculated persons, of whom one was attacked, and three uninoculated persons, of whom o were attacked; but the general list of families shows that in the family in question ere were three inoculated persons, of whom one was attacked, and two uninoculated rsons, who were both attacked. It is unnecessary to set forth here the similar crepancies which occur between the details given in the investigation sheets and in general list of families in the case of the other two houses of which the particulars

are not the same in these two statements. It appears, in view of the discrepancies just referred to, either that one or the other of the two statements sets forth the facts of the case incorrectly, or, perhaps, that one statement refers to the number of people who occupied the houses in the town before evacuation, and the second to the number of persons who occupied various huts in camp after evacuation. Whatever the explanation of the discrepancies may be, these discrepancies are such that we cannot use the investigation sheets laid before us as the basis for accurate conclusions as to the amount of protection conferred against attack. But the fact that all the six cases of plague which occurred among the inoculated ended in recovery is of itself evidence of the beneficial effect of inoculation.

Inoculation of disinfecting gangs in the Punjab.

In the Punjab special attempts were made to protect by inoculation persons who were engaged in working on disinfection gangs, and were thus subject to great risks of contracting plague. But the exact numbers of the disinfecting staff who were employed, and their distribution among inoculated and uninoculated, are unknown. Regarding the disinfectors employed in the Jullundur District, we have no full

10,006-8. particulars. We are, however, told that the inoculated had one genuine case of plague and one case regarding which it was doubtful whether it was plague or not. As to Hoshiarpur, we are told that cases among the disinfecting gangs occurred in every

10,685-6. village that was being disinfected, but that after inoculation had been introduced into the gangs, cases occurred only among the uninoculated disinfectors. After the inoculations there were only three cases, one in each of the gangs employed at Garhshankar,

10,690. Garhi, and Bilron. In the Garhshankar gang there were about 800 coolies, of whom

10,693-4. 700 were inoculated; in Garhi, out of 80 coolies, 60 were inoculated; and in Bilron

10,691. out of about 60 coolies, nine were inoculated. The total number of the uninoculated was, therefore, 170 as compared with 770 inoculated coolies, and the fact that the 170 uninoculated had three cases, while the 770 inoculated had no cases, is some index of the efficacy of inoculations.

V.(12.) *Belgaum Town and Cantonments.*

Inoculations in Belgaum town.

450. We may now return to the consideration of the results obtained by inoculation in Southern India, and in this connection we may consider in particular the facts put before us regarding Belgaum, Satara, and Alibag.

Inoculations were introduced into the town of Belgaum in December 1897, and the period of observation covered by the figures which have been supplied to us regarding inoculated persons extends over one year, to the end of December 1898. This covers

23,127. the two periods of very severe plague mortality. Plague did not reach the town till the end of November 1897. It was most severe during January 1898, and died out in the second week in May. Another outbreak commenced very shortly afterwards in July 1898, and this, after attaining its period of greatest severity in October, ceased in January 1899. The officers personally responsible for the work connected with the inoculations and the maintenance of the records of inoculated persons did not appear before us as witnesses. The information with which we were supplied was given to us by Mr. Carmichael, the Collector of the district, Lieut.-Colonel Peters, I.M.S., Civil Surgeon, and Mr. Uchgaonkar, the Chairman of the Belgaum Municipality.

Statistics regarding inoculations in Belgaum.

23,131, 23,152-3.

32,133.

We learn that inoculations began on 26th December, 1897, but we are left in some doubt as to the exact number of persons who were inoculated. Mr. Uchgaonkar put in a statement showing this number to be 3,438, but he withdrew that statement and supplied us with another, printed in his evidence. In this statement the number of persons operated upon in Belgaum is represented by the total of 3,447, of whom a total of 2,565 are shown as belonging to the town itself, while 292 are entered as residents of the Cantonment, and the rest as residents of other places. We find however, that the details given in the statement of the number of inoculated residents

23,132. of Belgaum town total up to 2,466. Between the date of the commencement of inoculations and the 15th January, 1898, inoculated persons were not liable to segregation or removal to hospital, but after the 15th January the ordinary plague measures were applied to the inoculated in the same way as to the uninoculated. The result of the withdrawal of the privileges at first conferred was, says Mr. Uchgaonkar, "to put almost a complete check to further progress of inoculation." The statement put in shows that of the total operated upon, 1,466 persons were inoculated between the 26th December 1897, and the 16th January 1898, and that only 1,000 persons submitted themselves to the process in the next 11½ months, of whom about 450 were done in the last fortnight of September. The number of inoculations performed in the town, which has a normal population of about 30,700, was thus very small, but it is apparent that the majority of those inoculated were operated upon before the climax of the first outbreak, and that practically the whole of the inoculated were operated upon before the second outbreak became severe.

Para. 53 of this Report.

No attempt has been made to work out estimates purporting to be accurate with regard to the relative amount of protection which inured to the inoculated when compared with the uninoculated. But we have been informed that of the people inoculated 61 or 62 were attacked and 33 died. We do not, however, lay any stress on these figures, for we have it in evidence that the reporting of cases during the first outbreak left much to be desired, and though the people, it is said, "gave information" during the second outbreak because they were not removed to hospital, our general experience of plague statistics in India leads us to believe that this information can hardly have been complete. Moreover, even if it were the case that occurrences of plague were not concealed, we have good reason to believe that, when they occurred in inoculated persons, they were not always correctly assigned to the inoculated. The information which has been prepared and supplied to us regarding the number of the inoculated who were attacked was not, it would appear, compiled from inquiries made at the time of the attacks. We understand that the register of plague attacks and deaths was, sometime before our visit to Belgaum, collated with the inoculation register. When a name which appeared in the inoculation register appeared also in the register of attacks, the case in question was entered up as occurring among the inoculated. When, on the contrary, a name which appeared in the inoculation register was not found in the register of attacks, the person in question was, unless other evidence to the contrary was forthcoming, assumed to have escaped infection. It is clear from the evidence which we have recorded that the statistics thus collected were far from correct. Mr. Carmichael said:—"It is very difficult to identify the inoculated people in several cases, because there was often a single name entered, and very little else. A certain proportion is like that. In other cases the name, the number of the house, and the occupation of the person are entered in our inoculation register. They are arranged alphabetically; and we went over our attack register and death register and checked the names. That is the way we did it. . . . The attack and death register gives the streets and houses; and it also is supposed to say whether a man was inoculated or not. That, however, has not always been given." Mr. Carmichael added, that in a few cases nothing but the name of the person inoculated had been entered in the register of inoculated persons. Lieut.-Colonel Peters, after an inspection of the registers relating both to the town and the Cantonment, told us that, in the case of the town, entries in the register regarding the residence of the inoculated were made "in a general way . . . the street had been entered, and sometimes the number of the houses also"; but he had not examined the books closely enough to say whether in all cases the residence was noted in the town registers. The town registers, however, were very similar to those of the Cantonment and similarly kept up. Therefore it is probable that mistakes found by Lieut.-Colonel Peters in the Cantonment registers appeared also in the town registers. He informed us that "in the Cantonment some names were not put down at all, but it was simply stated that a certificate was given to a sepoy of the 26th Madras Infantry. . . . I should like to state that in one case a woman left the camp with a certificate of Major Minchin that she had been inoculated, and it was found on inquiry that her name was not on the register. In other instances I have recently (in the beginning of 1899) found that transport mule drivers and dooly-bearers, of the Commissariat, who had been inoculated, are not recorded in the Cantonment registers," although their inoculation papers were dated in September, October, and November, 1898. He added that "In the nominal roll of inoculated persons in the Belgaum Cantonment who have got plague, I have a number of names which have been recorded who came to the Civil Hospital after having plague, and from inquiries I found out that they had been inoculated, but the names cannot be found in the inoculation register." That names were omitted from the town as well as from the Cantonment registers is clear from the following cases, of which Lieut.-Colonel Peters told us. In the special inquiries which he made in the case of four houses in the town, which we shall presently consider, he found that the inoculation of two out of 10 persons said to be inoculated was incapable of verification. Further, he stated that 11 inoculated persons from the town and 49 from the Cantonment came to the hospital suffering from plague. Three only out of the 49 who came from the Cantonment could be traced in the registers as inoculated, and only eight out of the 11 who came from the town could be similarly traced. We are, however, without exact information as to the actual extent to which the accuracy of the figures was affected by the causes of error which existed, and in these circumstances it is impossible to accept the statistics with which we have been supplied as even approximately correct. Any attempt to draw accurate deductions from them would be fruitless.

Criticism of statistics for Belgaum town. 23,134, 23,143.

23,136–8.

23,142, 22,929–36.

23,079–92.

23,108.

23,093–102.

In addition to the facts and figures which have been given us regarding inoculations in the town of Belgaum, as a whole, we have been supplied with particulars regarding inoculations in several different groups of persons living in Belgaum. These particulars relate firstly to the troops stationed in the Cantonment; secondly, to two groups of persons who came under the observation of Major Forman, R.A.M.C.; and thirdly, to the residents of four houses regarding whom special inquiries were made by Lieut.-Colonel Peters, I.M.S.

Inoculations in the 26th Madras Infantry at Belgaum. 25,930.

We may consider first the case of the troops. The regiment regarding which we have particulars is the 26th Madras Infantry. The first recognised case of plague in this regiment occurred on the 12th November, 1897, and the patient died on that day. Between this date and the 21st November, a period in which the regiment remained in its lines, there were 13 attacks of plague among the troops, families, and followers. On the 22nd November steps were taken to evacuate the lines, and evacuation was completed on the 28th November. During the period in which evacuation was proceeding, 15 attacks of plague took place. The whole regiment with its families and followers was moved into camps outside the town, and in the 10 days following this removal to camp, 13 sepoys and 21 members of their families and followers were attacked. After this the cases fell off in number from the 8th December, and only one or two attacks were occurring weekly by the time when Major Bannerman, I.M.S., arrived and proceeded to commence inoculations on the 23rd December, 1897. By this time there had been altogether 78 cases and 49 deaths from plague in the regiment. Out of the total population of 1,746, 1,665 were inoculated, and 81 only remained uninoculated. Immediately after inoculations were completed on the 6th January, the regiment went back to the disinfected lines, and during the rest of the period of the first outbreak in Belgaum, including the period in which that outbreak reached its height, there were only two attacks in the regiment, one in a sepoy and one in a European officer, both of whom had been inoculated. The regiment remained free from plague until the second outbreak took place in Belgaum. On the reappearance of plague in the town, the troops, families, and followers were re-inoculated at their own request, and at the end of the re-inoculations 1,801 persons had been inoculated. As men came back from time to time from furlough or recruiting duty, or as the families which had been absent during the first inoculations returned to the lines, they were inoculated, and after completion of the second series of inoculations there can have been very few inhabitants of the lines who had not been operated upon. The number of these is indeed stated to have been less than 81. It is probable that the real number of uninoculated persons living in the lines was unknown, and that, as in the case of the troops at Bangalore, the uninoculated cases may have occurred among persons who visited the lines and were discovered to have plague there. Whether this was so or not, it is obvious, from the fact that the bulk of the uninoculated were people who had just joined the regiment, that the uninoculated cases must have occurred among persons who contracted plague while they were not living under the same conditions as the inoculated, and who may have been more exposed to risks of infection. The regiment therefore does not contain satisfactory controls enabling us to compare the occurrences of plague among the inoculated and the uninoculated respectively, as a method of gauging the advantages of inoculation. Nevertheless it will be of interest to consider the course of plague during the second outbreak in the regiment.

25,927.

25,928.

25,932.

25,932-3.

25,939-40.

Para. 444 of this Report.

Course of plague in second outbreak among 26th Madras Infantry at Belgaum. 5621.

5625-6, 25,940.

25,934-8.

We have before us two statements of what occurred during the second outbreak. One of these is that of Major Bannerman, who has dealt with the whole period of the outbreak; and the second is that of Lieutenant Lethbridge, I.M.S., who was employed in inoculating the regiment the second time, and was able to observe what occurred during a period from the 5th September to the 15th October while he was at Belgaum. Major Bannerman's account differs from that of Lieutenant Lethbridge with regard to the date on which the re-inoculations were done, for whereas the former states that they were begun on 18th August and completed by the beginning of September, Lieutenant Lethbridge, who performed the operations, informed us that they were commenced on the 10th September and completed in about ten days. Moreover, Major Bannerman informed us that the second outbreak lasted from July to December, and that the inoculated had nine cases with three deaths; while the uninoculated had three cases all fatal. The number of cases among the inoculated was, he said, one only in each of the months of July, September, November, and December, with two cases in August and three in October; while the number of cases among the uninoculated occurred one in each of the months of September, October, and November. According to this account, therefore, during the whole of the months of September and October

here were altogether four cases among the inoculated, and two among the uninoculated. But Lieutenant Lethbridge has told us that in the five weeks of September and October during which the regiment was under his observation, eight cases of plague with five eaths occurred among the inoculated, and that about the same number of suspicious ases also occurred, in which "they did not exactly know whether it was plague or ot." Of these latter four died. Lieutenant Lethbridge also says that, in the period o which he refers, no cases occurred among the uninoculated, as there were no ninoculated. Major Bannerman was not with the regiment during the second utbreak. We therefore found some difficulty in accepting his figures in face of the vidence given before us by Lieutenant Lethbridge who had the regiment under his nmediate observation while he was at Belgaum. This difficulty has been to some xtent removed by the fact that the figures produced by Major Bannerman are ouched for both by him and by Lieutenant-Colonel Bennett, I.M.S., who was in charge of he regiment. But nevertheless, the method of the compilation of the figures given by Iajor Bannerman would, in face of Lieutenant Lethbridge's evidence, require to be ore fully investigated before those figures could be accepted as absolutely correct. As hey stand, in view of the possibility that they do not include every case that occurred, nd in view of the probable incomparability of the inoculated and uninoculated ommunities in which the recorded cases occurred, and in view, further, of the fact hat the numbers of the uninoculated are not accurately known, we are unable to ccept the comparison based on them by Major Bannerman to show the relative ability to plague of the inoculated and uninoculated respectively within the regiment self. Another comparison, however, has been made by Colonel Bennett and Major Bannerman in a paper published in the "Indian Medical Gazette" for June 1899. his comparison is between the number of cases that occurred in the regiment in the rst outbreak and the number that occurred in the second outbreak. In the first utbreak there were in all 80 cases; in the second outbreak there were only 12. In view f the fact that the sanitary measures adopted in both outbreaks were similar, except nly in respect of inoculation, and that the plague in the town of Belgaum was, during he second outbreak in the regiment, much more severe than the plague which prevailed n the town during the first outbreak in the regiment, it would hardly appear to be pen to doubt that the practical immunity of the regiment during the second outbreak as due to inoculation.

5621-3 5642. 5644. 5646.

The groups of persons concerning whom Major Forman, R.A.M.C., gave us vidence, were his private servants, and the hospital attendants of the Belgaum tation Hospital with their wives and children. He inoculated these groups when lague first broke out in the town, and was able to keep in touch with them con-inuously after that time. Regarding the first group, he says, bringing down heir history to the 3rd March 1899:—"Of my private servants there were in all, ncluding their wives and children, 28 people inoculated. There have been no cases f plague, and no deaths up to date. There were three uninoculated. One was a child f nine years of age, whose father refused to allow it to be inoculated. It died of lague 12 days after the other people were inoculated. I diagnosed the case myself as lague. The other two cases that were not inoculated were not so distinctly under my wn observation. One was a sweeper employed in the Cantonment as a municipal weeper, the husband of my sweeper woman, and sleeping in my compound. He, I m told, died of plague some months afterwards. The other was my bhisti (water-arrier). He threw himself into a well. I was informed that he had buboes and ever, and ran away to escape segregation. Of the 28 inoculated, none died of plague; nd of three uninoculated, two are said to have died of plague, and one undoubtedly ied of plague."

Groups of persons observed by Major Forman, R.A.M.C.

23,797-9.

Regarding the second group of which he gave us particulars, Major Forman said hat, out of 90 hospital servants, 87 were inoculated. Of the inoculated persons ne died from fever and endocarditis, and one died of plague. Excepting these wo, the rest of the inoculated were alive and well in March 1899. Only three persons emained uninoculated. Of these, one was not operated upon, because she had ecently been delivered, another was not operated upon because she was pregnant, nd the third was a boy of 16 years of age, whose father refused to let him be noculated. The boy died of plague less than two months after the inoculation f the rest of the hospital servants had been done. One of the two uninoculated omen died of plague two days after the boy, she having been in attendance upon im while he was sick with plague. The other uninoculated woman remained well.

23,797-9.

These particulars which Major Forman has given us regarding persons who came under his special observation are interesting, but we cannot attribute as much importance to them as to statistics collected over a larger area. While it is clear that isolated instances, like these, tell in favour of inoculation, it is equally clear that other isolated instances might tell against inoculation and that these could be obtained with little difficulty. A large number of such instances could, we may note, have been selected from the investigation sheets which Mr. Kaka put before us at Karachi. While, therefore, we have set forth the facts observed by Major Forman, we cannot consider that isolated cases of this kind are useful as a basis for deductions of general application. These cases, however, were not in the least cases which were selected to demonstrate the advantages of inoculation, and they are, for this reason, worthy of some weight in our consideration.

II. 428–42.

Groups of persons observed by Lt.-Col. Peters, I.M.S. 23,108–15.

The third group of cases which we have to consider in connection with Belgaum is that which relates to four houses inhabited by inoculated and uninoculated persons, for which Lieut.-Colonel Peters collected special information. These four houses contained 10 inoculated persons, of whom four got the plague and all recovered. Five persons who were uninoculated were residents of the houses, and there was no case of plague among them, but it is to be remarked that one of these uninoculated people was a child aged only three years, and another was a baby eight months old.

V.(13.) *The Town of Satara.*

Inoculations in Satara town. 22,393.

451. The inoculations that were done at Satara do not require more than a brief notice, and it will be convenient to consider them here. Intelligent members of the native community co-operated with the district authorities in recommending inoculations; public meetings were held to bring its advantages to the notice of the people, and the police were inoculated; people from infected places were removed to segregation or health camps unless they submitted to inoculation. Captain Thompson, I.M.S., who was in medical charge of the district, says of inoculation that "the people of Satara only resorted to it when we brought statistics before them to prove its great virtue," but the total number of people inoculated amounted to only 708 out of a population of nearly 26,000, and Lieut.-Colonel Hay, the Chief Plague Authority in Satara, told us that, in spite of inducements, the people showed no desire to be inoculated. We are informed that of the inoculated only three died, all on the first day after inoculation, and Captain Thompson has given us a table in which he compares the incidence of plague among the uninoculated people who were segregated with the incidence among the inoculated people in certain wards which were infected and had to be evacuated. The non-inoculated were removed, and the inoculated were allowed to remain in or return to their houses. The table which Captain Thompson furnishes is as follows:—

22,393.
22,390.
22,155.
22,180.

Name of Ward.	No. of Houses.	No. of Infected Houses.	Population.	No. of Persons inoculated.	After Segregation among the Non-inoculated.		After Inoculation among Inoculated.		Remarks.
					No. of Plague Cases.	No. of Plague Deaths.	No. of Plague Cases.	No. of Plague Deaths.	
Durga -	78	2	333	60	3	2	—	—	Evacuation commenced at the beginning of Nov. and inoculations were done about the same time. No case since 17th Jan.
Raviwar -	68	13	292	107	13	10	1	1	Inoculations were done about the beginning of Dec., when the ward was evacuated. No case since 6th Dec.
Pantachakote -	75	12	160	83	10	7	2	2	Evacuated on the 10th Nov. Inoculations were done between 11th Nov. and 11th Dec. No case since 24th Nov.
Totals -	221	27	785	250	26	19	3	3	

In all these wards the period of observation extends to the 31st March 1899. The data, however, are not complete enough to supply a basis for any estimation of the effects of inoculation, inasmuch as the table does not give the total number of attacks among the uninoculated in the wards, but only the number of attacks that took place among such uninoculated persons as were removed to segregation camps. Colonel Hay stated his opinion that the uninoculated people who went away from Raviwar Ward had plague among them, but he is unable to give the number of attacks or deaths, and there was probably more plague among the uninoculated inhabitants of the wards than would appear from Captain Thompson's figures. 22,160.

V.(14.) *The Town of Alibag.*

452. The last of the inoculations that we have specially considered in our survey of the results of inoculation, are the inoculations done in Alibag, near Bombay. The evidence which has been laid before us regarding these inoculations relates in the first place to general results in the town, and in the second place to the results of inoculation in certain households concerning which Mr. Arthur, the Collector of the district, has furnished special information. Inoculations in Alibag.

The outbreak, during which inoculations were applied to the town of Alibag, commenced on the 24th December 1898 and had not ceased by the 20th March 1899, when Mr. Arthur gave his evidence before us. Inoculations were commenced on the 2nd February 1899, and though the people were at first afraid of it, a certain number were afterwards induced to submit to the operation. The plague policy which was adopted was similar to that adopted in many other districts. The uninoculated were evacuated from their houses; the inoculated were allowed to remain. A peculiar feature of the plague policy adopted was that at the outset inoculation was withheld from the inhabitants of infected houses. Mr. Arthur says: "When infection was first introduced in Alibag I did not allow the members of infected families to be inoculated, lest the plague should be incubating in them, and lest their death from plague should be wrongfully assigned to the effect of inoculation; but latterly, inoculation having become more popular, I have revoked this order and have allowed the families to be inoculated, and then allowed them to go to their homes instead of being segregated." The result of this policy was that in a short time after plague broke out the town was occupied only by inoculated people, and by a few uninoculated people who were about to be inoculated. The number of inoculated persons who, according to Mr. Arthur's estimate, were living in the town at the end of March, was about 700. The other inoculated persons were, we presume, living outside the town. Mr. Arthur informs us that there were only 40 uninoculated residents in the middle of February, and only 20 at the time of his examination, but that the number of uninoculated was not definitely known. It would seem probable that there cannot have been many more uninoculated in the town, for orders were issued that "no one uninoculated should sleep in the town, and that those who did so would be prosecuted if discovered." These orders seem to have been strictly enforced and prosecutions were actually resorted to. Before proceeding to consider the results of these inoculations we may briefly summarise the conditions under which the inoculated and the uninoculated were living. Inside the town, and presumably under conditions that were favourable to correct observation, was living a community of 700 inoculated persons among whom were interspersed a few uninoculated persons. Outside the town there was a large community consisting chiefly of uninoculated persons over whom the supervision of the authorities was probably not very strict. The community inside the town was, by the nature of the case, more exposed to plague infection than the community outside; but on the other hand, there had been excluded from the former community, as we have seen above, most of the contacts who, of course, were most liable to develop plague. 25,876–91.

Up to the date of Mr. Arthur's examination the inoculated were reported to have had only two deaths from plague. Up to the same date, reckoning from the 31st January, which practically corresponds with the date of the commencement of inoculations, 54 attacks from plague had occurred among the uninoculated. We can arrive at no conclusion from these statistics, for we have not been informed what was the average strength of the inoculated and the uninoculated during the period of Criticism of result for Alibag as a whole. 25,884.

observation, and further we are left in doubt as to whether the figures regarding plague occurrences are to be referred to the numbers of inoculated and uninoculated living inside the town or to the total number of inoculated and uninoculated, both inside the town and in camps outside it.

Special cases showing effects of inoculation in Alibag.

In these circumstances it is unnecessary to dwell further upon the statistics which Mr. Arthur has given us for the town generally, and we may pass on to notice the special instances which he has laid before us of households in which inoculated members escaped while uninoculated members got plague. These may be quoted in

25,878. Mr. Arthur's own words. Mr. Arthur says: "The first instance I can give is this: On 21st February there were in the police lines about 130 inoculated persons, and about 21 uninoculated. They were being gradually inoculated, but these 21 had remained till the end. One of these, Rakmi, wife of Gangnak, Head Constable, was attacked with plague on the above date and died on the 22nd. Another, Rama Shripatrao Sinda, was attacked on the 18th and died on the 24th—two cases. The remaining persons then uninoculated were inoculated within the two or three days following, and though 150 persons have since been living in the lines, and are living there now, there has only been one other case of plague there. This was Parbati, wife of Ragnak Rannak, who, having been inoculated a fortnight previously, was attacked with plague on the 8th of March. She had high fever one day, was better the second day, and on third day she was almost well; and within a very few days was restored to perfect health. Beyond that there has not been another case in the police lines.

25,879. . . . . The second instance I wish to bring to notice was also a striking one. A Koli family of seven members was living in the town, of which six members had been inoculated in February. The remaining one, Hasu, wife of Nathu, having been delivered of a child only two months ago, was uninoculated. She caught the infection about the 8th instant, and died on the 14th in hospital. None of the other members of the family were affected, and the child was weaned from her, inoculated, and is now thriving. The third instance is also that of a Koli family of three members living together in the town. Of these, two were inoculated on the 9th and 17th February respectively. The third, a little girl of 12 years of age, who was to be inoculated later, was attacked with plague on the 1st instant, and died on the 3rd. The other two members were not affected. The fourth instance is that of a family of goldsmiths living in the town. Govindshet was inoculated in February. His daughter, Rembai (uninoculated), was attacked on the 7th instant, and died on the 8th. His wife, Bhagirthi, was inoculated the same day, and has not been affected. The fifth instance is that of a Musalman family of eight; of these, three were inoculated in February. Ismail, the son of one of them, 11 years of age (uninoculated), left Alibag on the 2nd and showed symptoms of plague on the 4th. He died on the 14th. The remaining members of the family were inoculated on the 4th, and none of the seven members now remaining have been attacked. . . . .

25,880. Three were inoculated before the first case occurred and the remainder were inoculated on the case occurring."

As in similar cases considered in connexion of the effects of inoculation in Belgaum, we think it would be unsafe to found on special instances of this kind any general conclusions as to the amount of protection conferred by inoculation.

VI.—General Synopsis of Statistics on which Conclusions can be based regarding the Value of Inoculation with Haffkine's Vaccine, and Observations regarding the Form in which the Results are set forth in the Synopsis and by Mr. Haffkine.

Synopsis of more trustworthy statistics criticised above. App. LIII. in Vol. II. of these Proceedings. 18,344-61, 19,261 *seq.*, 20,529-31.

453. We have now brought to a conclusion our critical survey of the data laid before us regarding the effects of inoculation. We have omitted from our survey only data such as those relating to inoculation among the Parsees of Broach, and among various groups of persons in Bombay, the detailed criticism of which would not, in our opinion, further the object we have in view. We will therefore now set forth the general conclusions with regard to inoculation which can be drawn from the body of evidence that has been submitted to us. It will facilitate matters if before proceeding to undertake this task we collect here in tabular form the statistical

results which seem to us to be specially worthy of consideration. These results are as follows:—

First Synoptical Table.

Name.	Population under Observation.	Percentage of Plague Attacks on Population.	Percentage of Plague Deaths on Population.	Case Mortality (expressed in Percentages in Cases where the Number of Attacks exceeds 100).	Advantages accruing from Inoculation.						Remarks.
					Diminution of Plague Attacks in every 100 of the Inoculated Population.	Ratio of Plague Attacks among the Uninoculated to Plague Attacks among an equal Number of Inoculated.	Diminution of Plague Deaths in every 100 of the Inoculated Population.	Diminution of Mortality among Inoculated as calculated according to Mr. Haffkine's Method of Calculation.	Ratio of Plague Deaths among the Uninoculated to Plague Deaths among an equal Number of Inoculated.	Ratio of Fatality in Plague Attacks among the Uninoculated to Fatality in an equal Number of Plague Attacks among the Inoculated.	
1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	12.
Byculla Jail (Para. 432 of this Report.)	Uninoculated - 172	4·	1·1	2 in 7	3·4	6·6 to 1	1·1	100	—	—	Only cases that occurred after the 4th February are included in this calculation.
	Inoculated - 152	·6	—	0 in 1							
Umarkhadi Jail (Para. 437.)	Uninoculated - 203	4·4	2·5	5 in 9	2·9	3 to 1	2·5	100	—	—	The percentages are taken on the original numbers of the inoculated and uninoculated.
	Inoculated - 190	1·5	—	0 in 3							
Undhera (Para. 438.)	Uninoculated - 437	6·4	5·9	26 in 28	4·9	4·2 to 1	5·3	89·6	10 to 1	2·5 to 1	One of us, Dr. Ruffer, is of opinion that these figures are not trustworthy.
	Inoculated - 513	1·5	·6	3 in 8							
The troops at Bangalore (Para. 444.)	Uninoculated 2,454	1·7	1·5	7 in 8	·6	1·5 to 1	·8	53·3	2·1 to 1	1·4 to 1	
	Inoculated - 446	1·1	·7	18 in 28							
Bulsar (4 Wards) (Para. 447.)	Uninoculated - 62	14·5	11·3	7 in 9	6·3	1·8 to 1	8·3	73·5	3·7 to 1	2·1 to 1	
	Inoculated 1,018	8·2	3·	31 in 84							
Lanauli (C. and D. Wards) (Para 434.)	Uninoculated - 330	8·8	5·4	46 in 69	7·5	6·8 to 1	4·6	85·2	6·7 to 1	1·5 to 1	The figures relate to the period from the 5th August to the 23rd September 1897.
	Inoculated - 370	1·3	·8	6 in 13							
Kirki (followers of 4 batteries) (Para. 435.)	Uninoculated - 812	10·3	5·8	67·8 p.c.	7·9	4·3 to 1	4·6	79·5	4·8 to 1	1·3 to 1	The figures relate to the period from the 26th August to the 28th September 1897.
	Inoculated - 755	2·4	1·2	53 p.c.							
Hubli (16th July to 19th August) (Para. 439.)	Uninoculated 20,225	—	4·8	—	—	—	4·3	89·5	9·6 to 1	—	We think the figures for attacks untrustworthy. Dr. Ruffer thinks the figures for deaths also quite untrustworthy.
	Inoculated 22,967	—	·5	—							
Gadag (Mr. Foy's figures) (Para. 441.)	Uninoculated 8,687	2·7	2·3	83 p.c.	1·7	1·8 to 1	1·7	74·0	3·8 to 1	1·9 to 1	
	Inoculated 9,396	1·5	·6	44 p.c.							
Daman (62 houses) (Para. 433.)	Uninoculated - 123	—	—	38 in 55	—	2·2 to 1	—	—	4 to 1	1·7 to 1	
	Inoculated - 255	—	—	20 in 50							

454. For the administrator the measure of success attained by the use of inoculations is represented by the actual number of attacks and deaths averted from the inoculated. This number can be ascertained for any particular place by multiplying the figures shown in columns 6 and 8 of the table given above by the number of hundreds composing the inoculated population.

Form adopted by us in stating results in above table and criticism of form of statement adopted by Mr. Haffkine.

In addition to the figures which show the diminution of plague attacks and deaths among the inoculated population, we have set down in the table figures showing the diminution in mortality, as calculated out according to the method adopted by Mr. Haffkine. This method, though it affords information which is of value to those who desire to ascertain how far the effects of the vaccine have approximated to the ideal standard which would have been attained if all cases of plague had been averted from the inoculated, gives results which are likely to convey a wrong impression to the administrator. It will be seen that, whereas the diminution of mortality which was achieved by the introduction of inoculations among the troops at Bangalore, amounted actually to ·8 per cent., it figures, when calculated out according to Mr. Haffkine's method, as 53·3 per cent. Similarly, in Undhera, the diminution in mortality which was actually effected among the inoculated was 5·3 per cent. Mr. Haffkine's statement of the result is, that there was "a reduction of 89·6 per cent. of mortality attributable to inoculation." His method of calculation correctly represents the percentage which I. 332.

the number of lives saved by inoculation bears to the maximum number of lives that might have been saved, and he takes, as the maximum number of lives that might have been saved, a number corresponding to the number of lives actually lost on account of plague among the uninoculated. The method, however, is likely to give rise to misunderstanding, especially when it is made, as it has been by Mr. Haffkine, the basis for such a general statement as the following;—"One can say in a general manner that the reduction in the mortality produced by inoculation is between 80 and 90 per cent." Such a statement, taken apart from the actual figures, might be taken to imply either that inoculation had averted death from 80 to 90 persons (*a*) in every 100 of the total population, or (*b*) in every 100 of the inoculated population, or (*c*) in every 100 inoculated persons attacked, whereas the statement really means that death was averted from 80 to 90 in every 100 of those who, without inoculation, would, judging by the mortality among the uninoculated, have died of plague. The misconception which might arise from the employment of this method of calculation will be most serious wherever the severity of the plague among the uninoculated is lightest; as, for instance, in the case of the light epidemic in the Byculla Jail. It will be seen that in that jail the diminution of mortality calculated by Mr. Haffkine's method would, on the figures given by us, amount to 100 per cent., whereas the actual saving in life achieved by inoculation would, on the same figures, be represented by the fact that death was averted from two of the inoculated persons.

78.

With a view to avoiding such fallacies we have expressed our results in a form that is not open to these objections. In column 10 of our table, we have set forth the ratio in which the deaths among the uninoculated stand to the deaths which actually occurred among equal numbers of the inoculated.

Synopsis of additional figures regarding effect of inoculation on case mortality.

455. A further point regarding the table we have given above relates to the character of the statistical material which it contains. It is to be noted that the table, while it includes all the more important figures regarding the diminution of deaths and attacks achieved by inoculation, does not include the whole of the figures worthy of consideration in connexion with the effect of inoculation on case mortality. Regarding this we have some additional data derived from observations made in hospitals, and in order to complete the synopsis of the statistical material from which our conclusions will be drawn, these additional data may be set forth as follows:—

SECOND SYNOPTICAL TABLE.

Hospital at	Once and twice inoculated Patients grouped together.			Uninoculated Patients.			Ratio of Fatality among Uninoculated to Fatality among equal Numbers of Inoculated Persons.
	Admitted.	Died.	Case Mortality.	Admitted.	Died.	Case Mortality.	
			Per cent.			Per cent.	
Dharwar* - - -	104	30	29·0	653	404	62	2·1 to 1.
23,694-5. Gadag - - -	107	56	52·2	184	130	70	2·3 to 1.
25,025-7. Bangalore City - -	57	31	54·4	2,074	1,391	67	1·2 to 1.
25,025-7. Bangalore Civil and Military Station, North Camp.	87	24	27·6	853	572	67	2·8 to 1.
25,025-7. Bangalore Civil and Military Station, South Camp.	41	12	29·3	727	450	62	2·1 to 1.
III. 286. Bangalore Civil and Military Station, Military Hospitals.	121	80	66·0	69	59	78	1·2 to 1.
25,025-7. Mysore City - -	26	9	34·0	180	92	51	1·5 to 1.

* These figures are taken from a report on the Dharwar Plague Hospital from the 28th August to 18th December 1898, by Mr. Hornabrook, M.B., the Medical Officer in charge of the Hospital.

General statement of the protective effects of inoculation.

456. This and the foregoing table show that in every case, except that of Bulsar, inoculations had a considerable effect in warding off plague attacks from the inoculated, and that in every case without exception they rendered attacks among the inoculated less fatal than attacks among the uninoculated. The protection afforded by inoculation seems, however, never to be absolute, either in the matter of warding off attacks, or of warding off a fatal issue to an attack, and we have instances, in the statement of cases submitted to us with reference to patients treated in the Khoja Hospital at Bombay, which show that plague attacks may occur in persons inoculated as many as three or four times within 18 months of attack, and that such attacks may end in death. The facts thus established by actual experience in the case of man are

App. LV.(1) in Vol. III. of these Proceedings.

precisely such as might have been anticipated from the experimental results obtained in the case of animals subjected to anti-plague inoculation.

VII.—Question of the Measure of Protection against Plague afforded by Inoculation with Haffkine's Vaccine.

457. When we consult the tables with a view to determining what measure of protection is actually afforded by inoculation, we see great differences in the results which are set down for various places. These differences are noticeable in the results set down with regard to the diminution both of attacks and of deaths among the inoculated as compared with the uninoculated population. They are noticeable also in the figures for case mortality, which represent the diminution of the death rate among the inoculated as compared with the uninoculated patients who were admitted to hospital, or whose attacks came otherwise under the observation of the authorities.

Great differences in results set forth in the synoptical tables above.

In the case of the Byculla Jail the ratio which the attacks in the uninoculated bore to the attacks in equal numbers of the inoculated was 6·6 to 1. On the other hand, in the case of the troops at Bangalore the corresponding ratio was 1·5 to 1. The corresponding ratios in other places vary between the extreme limits which are represented on the one hand by the ratio in the Byculla Jail, and, on the other hand, by the ratio among the troops at Bangalore.

The ratios of deaths among the uninoculated to deaths among similar numbers of inoculated persons vary between 10 to 1 in the case of Undhera and 2·1 to 1 in the case of the troops at Bangalore.

The diminution of fatality in cases among the inoculated as compared with cases among the uninoculated was in the North Camp Hospital at the Civil and Military Station of Bangalore represented by the ratio of 2·8 to 1, the highest ratio observed; in the case of the Military Hospitals at Bangalore the diminution of fatality was represented by the ratio of only 1·2 to 1, which is the lowest recorded.

458. The variations in the results may conceivably be explained in one of four ways. They may be due to errors in the statistics sufficiently serious to falsify the ratios; secondly, they may be due to the assignment to the inoculated in certain of the statistics of attacks and deaths which were imputable to infection contracted shortly before or shortly after inoculation; thirdly, they may be due to differences in the virulence of the plague epidemics in the various places where inoculation was employed; and, fourthly, they may be due to differences in the strength and amount of the vaccine employed in different places.

Possible explanations of differences in results.

VII.(1.) *Inaccuracies in Statistics an Explanation of the Differences in Results obtained in different places.*

459. With regard to the suggestion that the discrepancies in the results may be conditioned by serious errors in the statistics, it is to be noted that this question, in so far as it applies to the first of the two synoptical tables now under consideration, has been already discussed in our detailed review of the statistical materials supplied to us. It will be sufficient to remark here that the statistics set forth in the first table are not all equally trustworthy. While, for instance, we may take all the figures given us for the Byculla and Umarkhadi Jails, and for the troops at Bangalore, to be as accurate as such figures can be, on the other hand we may notice that the figures given for Lanauli, Kirki, Hubli, and Gadag, cannot be compared with those just mentioned in point of accuracy. We may further note that the comparisons based on the figures for Bulsar are, as we have already said, unduly to the advantage of inoculation; while, in view of this fact, it would be illegitimate to use these figures as an indication of the advantages of inoculation, it will be legitimate to use them in guaging the liability of the inoculated to contract plague.

Brief discussion of the accuracy of the figures in first synoptical table. Sections IV. and V. of this Chapter.

460. With reference to the figures for case mortality, however, we have not yet entered into a discussion of them, and we have, therefore, here to consider whether the differences of the effect of inoculation on case mortality, shown in different places, can be accounted for by differences in the conditions under which the various statistics were collected. We may begin by pointing out that patients who are really inoculated may, even in a hospital, occasionally be misclassified as uninoculated. This may readily occur in the absence of friends or relatives, or of a certificate of inoculation, in the case of patients who succumb without being able to render any account of

Discussion of accuracy of hospital figures for case mortality.

Para. 604 of this Report.

themselves. We have no means of determining the amount of error which might thus be introduced into hospital statistics. But in view of the fact that in many cases patients were moribund when brought into hospital, the point must be kept in mind. To the extent to which it occurred the error must have operated to the advantage of inoculation in increasing the difference between the case mortality set down for the inoculated and that set down for the uninoculated. The trustworthiness of the hospital figures for case mortality may, however, be impugned on more serious grounds. They may be impugned on the ground that the severity of the cases in inoculated and uninoculated patients respectively which are treated in hospital cannot be taken as representing, with a fair degree of accuracy, the average severity of the total plague cases among these two classes of the community. Consideration will show that the mildest cases and the severest cases of plague will not be adequately represented in hospitals, in as much as the mildest cases of plague will be able to avoid being taken to the hospital, and the severest cases will die before they can be brought in. It might be assumed *à priori* that this process of selection would influence the case mortality recorded for the inoculated and the uninoculated in opposite directions. In the case of the uninoculated it might be assumed that there would be excluded from observation in hospital a preponderating number of severe cases of plague, and only a very few mild cases of plague. In the case of the inoculated, on the other hand, in as much as the course of plague is favourably modified by inoculation, we might expect that there would be excluded from observation in hospital a large proportion of mild cases of plague, and only a comparatively small proportion of the severest cases of plague.

23,694-5, 23,727.

We find that the figures for Gadag harmonise with these inferences. We find that while the case mortality in the inoculated in the Plague Hospital at Gadag is set down as 52 per cent., the case mortality for the inoculated of the whole town is set down as 44 per cent., the reduction of case mortality in this instance being brought about by the inclusion, within the figures for the town as a whole, of 32 attacks of which only four were fatal. If we may make the assumption that in Gadag all the deaths are correctly assigned to the classes of the inoculated and the uninoculated respectively, and that all deaths from plague came to the knowledge of the authorities, it will be obvious that the figure of 44 per cent. cannot exceed the actual case mortality among the total inoculated population of Gadag. On the other hand, inasmuch as many mild cases must have escaped the notice of the authorities, we may assume that the actual case mortality may have been even below 44 per cent. In any case, it will be manifest that the case mortality of 52 per cent. obtained in the hospital, in all probability gives us an exaggerated estimate of the case mortality in the total number of the inoculated attacked. Precisely the contrary of this seems to hold true with regard to the uninoculated in Gadag. The case mortality for the uninoculated in the Plague Hospital at Gadag works out at 70 per cent., while the case mortality which is set down for the total uninoculated population of the town is 83 per cent. This increase in case mortality is obtained by the inclusion, in the figures for the town as a whole, of a large number of cases which came to the notice of the authorities only after the patients had died. We do not know the precise number of these cases and deaths. There appears, in fact, to be a serious inaccuracy in Mr. Foy's figures, inasmuch as when we subtract the figures which he has given us for cases and deaths in the hospital from the figures which he has given us for cases and deaths in the whole of the town, we arrive at the figure of 103 cases with 105 deaths.

Report on the Dharwar Plague Hospital.

We, however, cannot generalize from the case of Gadag. The figures obtained in other places do not tend always in the same direction. In the case of Dharwar, from the figures supplied to us from Mr. Hornabrook, we can institute a comparison between the case mortality of the inoculated in the hospital and the case mortality of the inoculated outside the hospital. Mr. Hornabrook's returns are divided into three parts. The first two parts bring the figures up to the 8th December 1898. The first part shows that, up to that date, out of 99 inoculated patients treated in the hospital, 27 died. The second part refers to inoculated persons attacked who were not brought into hospital. The result of the attacks is noted in 32 only of the cases included in this part. Of these 32 cases, 16 ended fatally. The third part of Mr. Hornabrook's returns brings the figures down to the 9th January 1899, but does not distribute them into cases that were treated in the hospital and cases that were not so treated. These figures show that in Dharwar the case mortality for the inoculated in the hospital was 2·74 per cent., while the case mortality for the 32 inoculated outside

he hospital, expressed in percentages, amounted to 50 per cent. We may thus note iat the case mortality for the inoculated who came under observation outside the ospital was greater than that of those who came under observation in hospital. The ıference that in Dharwar plague was more severe in inoculated patients who did not ɔme into hospital, in comparison with those who came into hospital, is borne out by consideration of the details given in the third part of Mr. Hornabrook's returns. Ve find there 43 deaths set down as occurring in the cases of 69 persons, who appear, s we infer from internal evidence, almost all to have been seen outside the hospital. 'his result is quite other than that which might have been expected, both on *à priori* rounds and from the consideration of the figures for Gadag.

The different results obtained in Gadag and Dharwar respectively show that the case ıortality for the inoculated in hospitals stands in no constant relation to the case ıortality among the total inoculated attacked who have come under observation. *A ɔrtiori* it can stand in no constant relation to the real case mortality of the total body f the inoculated attacked.

The above considerations throw some light upon the figures for case mortality 'hich are set down for the various hospitals in the City, and Civil and Military Station, t Bangalore. In the case of the North Camp Hospital, we have a case mortality of 7·6 per cent. for the inoculated, and in the South Camp Hospital a case mortality of 9·3 per cent. for the same class. The case mortalities just referred to have been taken n 128 cases, two-thirds of which were treated in the North Camp Hospital and ne-third in the South Camp Hospital. In contrast to these, we have the figures of the ospital in Bangalore City, giving a case mortality of 54·4 per cent., taken on 57 cases mong the inoculated, and the figures for the Military Hospitals at Bangalore, giving a ıse mortality for the inoculated of 66 per cent., taken on 121 cases. It is to be noted iat these very different results refer to one and the same plague epidemic. It is ossible that an explanation of them, so far as they relate to the case of the Military Iospitals, may be found in the assumption that, while all the inoculated attacked in ıe military community were brought into hospital, many of the more severe cases mong the inoculated in the civil population were not brought into hospital. Similar ɔnsiderations apply to the case mortality among the uninoculated in the various 3angalore hospitals. It will be seen that while this amounted to 78 per cent. in the Iilitary Hospitals, where the figures are likely to be least erroneous, it was only 67, 7, and 62 for the City, North Camp, and South Camp Hospitals respectively.

In conformity with the above considerations, the case mortalities among the ıoculated and uninoculated in the hospitals cannot, except in hospitals such as Iilitary and Jail Hospitals, be taken as representing the actual case mortalities of the ıoculated and uninoculated classes respectively. Inasmuch as the case mortality of ne of these classes may be affected in a different degree and a different sense to that f the other, it follows that the ratio between the case mortality of the uninoculated and iat of the inoculated patients treated in hospitals cannot be accepted as a correct ıdex of the advantage which has really accrued from inoculation. In proof of this, 'e need point only to the ratios set down in our tables for the hospitals in Bangalore ity and the Civil and Military Station. While these ratios ought to work out practi-ally the same, they vary in a most irregular way. In the Bangalore City and the Iilitary Hospitals, where the conditions under which patients were brought into ospital were entirely different, they are 1·2 to 1. In the Civil Hospitals of the Civil nd Military Station, to which patients were brought under conditions similar to those f the City Hospital, the ratios are 2·1 to 1 and 2·8 to 1. In these circumstances, ıough we have no doubt that the figures are sufficiently accurate to show that inocu-ıtion considerably reduces the case mortality of the inoculated, we shall be unable to ıake any use of the hospital figures in connexion with any exact calculations, except 'hen such figures refer to Military or Jail Hospitals.

VII.(2.) *Question as to how soon after Inoculation Protection against Plague is afforded.*

461. We have now to consider the possibility that certain of the less favourable esults shown in the tables which are now under discussion, may be due to the assign-ıent to the inoculated of attacks and deaths due to infection contracted shortly before r shortly after inoculation. In certain of the statistics, that is to say those for ?ndhera and the Umarkhadi Jail, which record the most striking instances of the

Possibility that less favourable results are in some cases due to inclusion among the inoculated of persons who contract

plague before or immediately after inoculation. Paras. 437 and 438 of this Report.

advantages of inoculation, the good results obtained may, in a certain degree, be attributable to the fact that there were either few or no attacks among the inoculated which are referable to infection contracted shortly before or shortly after inoculation In the case of the Umarkhadi Jail, plague was entirely in abeyance for a fortnight after the inoculation. In Undhera, though plague continued among the uninoculated at the rate of 10 cases in the first week after inoculation, there were among the inoculated two persons, at most, who can be regarded as having contracted plague at the time of inoculation. The experiment in the Byculla Jail is of particular interest in this connexion. If we compare the number of attacks that occurred among the uninoculated and the number among the inoculated from the date of inoculation, we find that there was a percentage of 6·9 and 3·9 cases respectively among these two classes, giving a ratio of 1·8 to 1. But if, allowing 5 days for the incubation period we exclude all cases that might have been incubating plague at the time of inoculation we arrive at the results which have been tabulated above, and which give a ratio of 6·6 to 1. The inclusion of cases among the inoculated who were incubating plague at the time of inoculation, must influence every comparison based on average strength it will be remembered that this method of comparison is resorted to only when inoculations are continued during the period of observation. This fact must be borne in mind in connexion with the less favourable results obtained in Kirki, among the troops at Bangalore, and possibly in the case of Bulsar. Regarding the dates of inoculation at this last place, however, we have no exact information.

Para. 432 of this Report.

Paras. 435, 444, and 447 of this

Criticism of Mr. Haffkine's suggestion that protection is conferred within 24 hours. 44. 45. 47.

462. The assumption that the relatively less favourable results were due to the inclusion among the inoculated of persons who were incubating plague at the time of inoculation could not be made, if there were any reason to accept Mr. Haffkine's suggestion, that, "though the prophylactic was powerless to arrest symptoms already started, or which developed a few hours after inoculation," "there is a possibility of its influencing the disease in the incubation period in an individual infected three or four days previously," and that "the time necessary for the plague prophylactic to produce a useful effect is . . . apparently less than 24 hours." The results obtained by the Pasteur system of inoculation against rabies clearly show that an inoculation may afford protection against the effects of infection already contracted Indeed, there is no reason for inferring that the period required for the achievement of protection must always exceed the incubation period of the disease protected against. Thus, theoretically, Mr. Haffkine's vaccine might quite well protect against infection already contracted. On the other hand, in view of the short incubation period of plague, and in view of the fact that our experience in the case of other diseases, both in animals and men, indicates that protection is not at all rapidly established, it seems to us unlikely that the anti-plague inoculation can exert any favourable influence on persons who are already incubating plague. The matter however, is one which can be definitely determined only by scrutinising the record of actual observation to ascertain how soon protection becomes established. We have therefore, endeavoured to gather indications regarding this matter from the evidence laid before us.

Para. 197 of this Report.

Possibility of determining by consideration of dates of attack how soon protection is acquired.

463. It would seem possible to test how soon protection is achieved by determining what percentages of the total attacks among the inoculated are attributable to each successive day or week after inoculation. We may consider first what details are necessary to apply this test. If we are dealing with a group of persons all inoculated on the same day, we should require to know the number of inoculated persons composing the group, the period for which they were kept under observation, and the number of cases that occurred among them in each day or week of the period of observation. It would, moreover, be necessary to know to what extent plague continued among the uninoculated during each day or week throughout the period of observation in order to determine whether the inoculated in the concluding days or weeks of that period were or were not exposed to less infection than those in the earlier days or weeks. We could then calculate out the daily or weekly percentage of attacks, for the first day or week on the original number of inoculates, and afterwards on the number of these remaining over unattacked on each day or week. A comparison of the percentage of the attacks which occurred shortly after inoculation with the percentages which occurred later would give the required information as to the date on which protection was achieved. When the results of a series of inoculations done on

successive days in one community are in question, the same particulars regarding each successive group of inoculates will be required. This is not the method that has been adopted in the preparation of the statistics placed before us. The method adopted has been to bring the period of observation for a number of successive groups of inoculates to a conclusion on one and the same day, to sort out the cases that have occurred before that day into groups according to the length of time after inoculation at which they occurred, and then to compare the numbers of attacks in different days or weeks with a view to drawing conclusions with regard to the protection achieved at varying periods after inoculation. This method leads to erroneous results. How erroneous these results may be will be manifest when we consider what would be the effect of applying the method to an extreme case. In such an extreme case as that of a community composed of two groups of inoculates, inoculated on two days separated by a considerable interval of time, if the period of observation was brought to a close within, let us say, a week after the second series of inoculations, it is plain that the ratios which the percentage of attacks occurring in the first week after inoculation would bear to the percentage of attacks occuring in weeks remoter from the dates of inoculation would be vitiated by the fact that the figures for the first week would include the cases that occurred in both groups of inoculates, while the figures for the subsequent weeks would exclude the attacks that might have occurred among the second group of inoculates after the date on which the period of observation was brought to a close.

Possibility of determining by consideration of case mortality how soon protection is acquired.

464. We have gone through the figures supplied to us and find that they are presented to us in such a form as to make it impossible to arrive at any conclusion as to whether the percentage of attacks among inoculated persons has been greater or less in the first few days after inoculation than in the subsequent period. In default of the data necessary for the decision of this question we have fallen back upon the only other line of inquiry which seems open to us. In other words, we take the case mortality as our index of the protection acquired by the inoculated. We have gone through various statements laid before us, with a view to determining whether there are any differences between the case mortality in inoculated persons attacked during the first few days after inoculation and the case mortality in inoculated persons attacked at a later period. The most satisfactory and most detailed statement which we have to assist us in this matter is that furnished by Mr. Hornabrook. He has given us a statement of the cases of 208 inoculated persons. Of these we can here make use of 142 cases only, as the rest are those of twice-inoculated persons or of once-inoculated persons regarding whom full details are not given. Our examination of the 142 cases has given us the following results, which show the case mortality among the inoculated attacked on the day of inoculation, among those attacked on each of the five succeeding days, and among those attacked in the period after the expiration of five days.

App. XVI. in Vol. I. of these Proceedings.

Number of Cases among Once-inoculated Persons.

On Day of Inoculation.		On 1st Day after Inoculation.		On 2nd Day after Inoculation.		On 3rd Day after Inoculation.		On 4th Day after Inoculation.		On 5th Day after Inoculation.		After 5 Days after Inoculation.	
Cases.	Fatal Cases.	Cases.	Fatal Cases.	Cases.	Fatal Cases.	Cases.	Fatal Cases.	Cases.	Fatal Cases.	Cases.	Fatal Cases.	Cases.	Fatal Cases.
11	7	9	4	8	3	7	5	8	0	6	0	93	30

This summary of observations shows that the case mortality is very much higher in patients attacked during the first few days after inoculation than it is at a later period. In view of the fact that Mr. Hornabrook's list is the most extensive series of observations which we have, and that it shows that the case mortality is severest in the first three days after inoculation, we assumed that figures of other observers would, if scrutinised, likewise show that protection was definitely established only after the expiration of a similar interval. With a view to determine this, we have, wherever the data were available, worked out the case mortality in once-inoculated persons attacked within the first three days after inoculation. Similarly, with a view to instituting a comparison, we have worked out from the statistics in question the case

mortality in persons attacked more than three days after inoculation. The result of the analysis of the figures is as follows:—

	Place.	Cases in Once-inoculated Persons attacked on Day of Inoculation and the subsequent Three Days.			Cases of Inoculated Persons attacked more than Three Days after Day of Inoculation.		
		Cases.	Fatal Cases.	Case Mortality.	Cases.	Fatal Cases.	Case Mortality.
				Per cent.			Per cent.
I. 429–38.	Dharwar (Mr. Hornabrook's figures).	35	19	54	107	30	28
II. 428–442.	Karachi (from Mr. Kaka's investigation sheets).	6	6	100	41	19	48·5
I. 310–30.	Daman (from Major Lyons' investigation sheets).	7	5	71·5	84	31	37
15,431.	Baroda (Major Sarkies' figures).	4	1	25	9	3	33·3
23,143.	Belgaum (Mr. Uchgaonkar's figures).	3	3	100	58	30	52
	Total - -	55	34	62	299	113	43·3

It is thus manifest that, while the case mortality of the first three days compares favourably with that of uninoculated patients, the protection which is achieved within the first three days after inoculation compares very unfavourably with that which is achieved later on. It is probable that this inference would be confirmed by the examination of other figures, if we had been able in other cases to distribute the plague attacks into those that occurred in the first three days after inoculation and those that occurred afterwards. We see an indication of this in the fact that when we institute, as we are able to do in certain instances, a comparison between the case mortality in persons attacked within seven days of inoculation, and the case mortality in persons attacked later, the case mortality for the former is less favourable than that for the latter. The figures to which we refer are as follows:—

	Place.	Cases of Persons attacked on Day of Inoculation, or within Seven Days after.			Cases of Persons attacked more than Seven Days from Date of Inoculation.		
		Cases.	Deaths.	Percentage.	Cases.	Deaths.	Percentage.
I. 333–6.	Undhera (investigation sheets) -	2	1	50	6	3	50
II. 428–42.	Karachi (investigation sheets) -	11	8	77·7	38	18	47·4
16,284.	Bulsar (details supplied by Mr. Dadachanji).	13	5	38·5	71	26	36
14,838, 14,940.	Billimora and Koili (details supplied by Mr. Mehta).	6	4	66·6	33	21	63
1773.	Dharwar (details supplied by Miss Corthorn).	50	16	33	45	11	24·5
		72	34	47	193	79	41

VII.(3.) *Question whether Variations in Virulence of Epidemics in Different Places account for Differences of Results obtained from Inoculation.*

Possibility that variations in results of inoculation are due to variations in virulence of plague. Para. 432 of this Report.

465. Another explanation of the discrepancies in the synoptical tables which suggests itself is that the protective effect of inoculation may be inversely to the virulence of the epidemic. Countenance would appear to be given to this explanation by the results shown for the Byculla Jail; here we have very good results, both as to the warding off of attacks and as to the reduction of fatality, in the case of an epidemic where the case mortality taken on the 26 uninoculated attacked during the whole course of the the epidemic was only 50 per cent. The explanation would also appear to be countenanced by the results of the inoculations done among the troops at Bangalore III. 285 (Stat. I.). Among them, in an epidemic whose virulence may be gauged from the fact that deaths occurred at the rate of 85 per cent. among the 69 uninoculate

attacked, we have much less favourable results for inoculation, both in reduction of attacks and in reduction of fatality. This explanation, however, is discountenanced by the fact that the results obtained in Undhera were many times better than the results obtained among the troops at Bangalore, though, since the virulence of the epidemic was slightly greater, worse results might have been expected. Moreover, the explanation would appear to be in conflict with the fact that, with regard to both attacks and deaths, the results obtained at Undhera, where the case mortality during the whole course of epidemic was 91 per cent. among the uninoculated, are much better than the results obtained in Bulsar, where, judging from the figures for the uninoculated Ghanchi Ward, we may take the case mortality among the uninoculated to have been 69·5 per cent. (16 deaths in 23 cases). The smaller effect produced in Bulsar is the more remarkable inasmuch as we have seen above that the comparison that has been instituted there between inoculated and uninoculated is unduly to the advantage of inoculation. We must therefore conclude that the explanation of the greater or less effectiveness of inoculation is not to be found in the variation in the virulence of the plague in different places.

Para. 438 of this Report.
Para. 447 of this Report.

VII.(4.) *Question whether Differences in Results of Inoculation are due to Differences in the Strength and Dosage of the Vaccine used.*

466. We may now seek for an explanation of the varying effectiveness of inoculation by inquiring whether the result of inoculation is governed by differences in the vaccinating material injected. In respect of this question we may note that Mr. Haffkine himself has suggested that the effectiveness of the vaccine is conditioned by differences in strength. In confirmation of this suggestion he has drawn attention to the results obtained from the inoculations in Kirki and Daman.

Possibility that variations in results of inoculation are due to variations in strength or character of the anti-plague vaccine.
68, 72, 119, 184–90.

First, in connexion with the results in Kirki, which Mr. Haffkine imputed to the defective strength of the vaccine, we may note that the first of our synoptical tables shows that in Kirki the protection against attacks was inferior only to the protection against attacks obtained in Lanauli. We may exclude from consideration here the figures for the Byculla Jail, where the epidemic was light. Similarly, the ratio of diminution in deaths among the inoculated as compared with the uninoculated at Kirki was, if we leave out of account the epidemics of the Byculla and Umarkhadi Jails, where there were no deaths, inferior only to the figure obtained at Lanauli and Undhera, and to that set down for Hubli. Hence, although the vaccine used at Kirki is said to have been below the average strength in its power of producing temperature reaction in the inoculated, yet, assuming always the accuracy of the data, it must be considered as being well above the average from the point of view of its efficacy.

Para. 453 of this Report.

In connexion with the relative efficacy of weak and strong vaccines and of larger and smaller doses, Mr. Haffkine has referred, in the case of Daman, first to the better results which are stated to have been obtained in the town by the inoculations performed in March with vaccine which produced a higher fever reaction, as compared with the vaccinations performed in April with vaccine which produced a lower temperature reaction; and further, Mr. Haffkine has referred to the better results stated to have been obtained by Dr. Kalapesi with large doses as compared with the results obtained by Dr. Poiares with small doses. The results as stated by Mr. Haffkine are as follows:—

	No. of Persons inoculated.	Attacks.		Deaths.		Case Mortality per cent.	
		No.	Per cent.	No.	Per cent.		
In March, with strong vaccine -	1,017	49	4·8	15	1·5	30·6	I. 309.
After March, with weaker vaccine -	628	41	6·5	20	3·2	48·8	
By Dr. Kalapesi, with full doses -	1,924	70	3·6	22	1·1	31·4	
By Dr. Poiares, with reduced doses	270	21	7·8	4	5·2	66·7	

NOTE.—Mr. Haffkine's figures include persons inoculated more than once.

"Thus," says Mr. Haffkine, "amongst the inoculated with reduced doses the total mortality was 4·5 times and the case mortality 2·1 times higher than that amongst the inoculated with larger doses." Inasmuch as we have already pointed out that figures for the town as a whole must be received with great reserve, we have endeavoured to check the results set forth by Mr. Haffkine by reference to the

I. 309.
Para. 433 of this Report.

investigation sheets prepared by Major Lyons, I.M.S. From these we have taken out all those once-inoculated persons who were inoculated in March, and all those who were inoculated after March. The name of the medical man who performed the inoculations has not been stated in the details supplied in the investigation sheets, but we presume that the inoculations detailed in the third series of investigation sheets, and spoken of as having been done with reduced doses, are inoculations done by Dr. Poiares, and that the inoculations included in the three other series of investigation sheets were done with full doses. The results of our analysis of the investigation sheets is as follows :—

	No. of Persons once inoculated.	Attacks.		Deaths.		Case Mortality.
		No.	Per cent.	No.	Per cent.	Per cent.
I. 310-30. In March with strong vaccine	87	23	26·6	4	4·6	17·4
After March with weaker vaccine	167	57	34·0	30	18·0	31·6
With full doses	199	60	30	21	10·6	35
With reduced doses	55	20	36·5	13	22·3	65

The results agree in all important points with those obtained by Mr. Haffkine for the town as a whole. They indicate, as Mr. Haffkine himself has recognised, that it is essential to the attainment of the best results that the vaccine should be carefully standardized.

VII.(5.) *The Comparative Efficiency of Single and Repeated Inoculations.*

Effect of double as compared with single inoculations. Para. 453 of this Report.

467. Closely connected with the question as to what dose of vaccine will give the best results is the question as to what is the comparative efficacy of double and single inoculations. We are able to gather only uncertain indications from the results set down in the first of our synoptical tables. We find that the best results were obtained in Undhera, where only single inoculations were employed. On the other hand, the least favourable results included in the table are those which were obtained among the troops at Bangalore, who were, for the most part, inoculated twice. In short, the figures of the table tend, on the whole, to show that a single inoculation is more effective than a double inoculation. To test this we have collected such additional information as we have been able to extract from the evidence recorded in our Proceedings. That information is summarised in the following table :—

Place.	Number of People under Observation.		Percentage of Attacks among		Percentage of Deaths among		Percentage of Case Mortality among		Ratio of Attacks among the Once Inoculated to Attacks among an equal Number of Twice Inoculated.	Ratio of Deaths among the Once Inoculated to Deaths among an equal number of Twice Inoculated.	Ratio of Fatality in Attacks among the Once Inoculated to the Fatality in an equal number of Attacks among the Twice Inoculated.
	Once Inoculated.	Twice Inoculated.	Once Inoculated.	Twice Inoculated.	Once Inoculated.	Twice Inoculated.	Once Inoculated.	Twice Inoculated.			
I. 310-30. Daman (investigation sheets).	254	128	31·5	8·6	13·4	1·5	42·5	18·2	2·3 to 1	3·6 to 1	9 to 1
23,727. Gadag (Mr. Foy's figures)	2,528	6,868	1·3	1·5	·5	·7	42·5	44·2	·9 to 1	·9 to 1	·7 to 1
1771. Dharwar (Miss Corthorn's figures).	3,270	3,463	1·7	·6	·6	·2	33·3	28·6	1·2 to 1	2·8 to 1	3 to 1
Para. 439 of this Report. Hubli (Mr. Cappel's figures for 5 weeks).	9,514	13,453	—	—	·5	·5	—	—	—	—	1 to 1
			Patients admitted.		Number of Deaths.						
III. 404-6. Khoja Plague Hospital (Mr. Khaja Adbulla's figures).	—	—	35	27	17	7	48·7	26·0	—	—	1·8 to 1
23,694-5. Gadag Plague Hospital (Mr. Foy's figures).	—	—	32	75	15	41	47·0	59·7	—	—	·8 to 1
I. 429-38. Dharwar Plague Hospital (Mr. Hornabrook's figures).	—	—	157	50	53	26	34·0	52·0	—	—	·6 to 1

A scrutiny of this table will show that the results are most divergent. It will be seen that in Daman a second inoculation conferred considerable additional protection. The same appears to hold true, though to a less extent, of patients treated in the

Khoja Hospital at Bombay. From Miss Corthorn's figures for the town it appears that a small amount of additional protection accrued to the doubly inoculated at Dharwar, but Mr. Hornabrook's figures, regarding 157 of the cases observed by him, indicate that the fatality among the twice-inoculated was one-third greater than the fatality among the once-inoculated. Mr. Foy's figures for the town and for the hospital of Gadag similarly show that the twice-inoculated as compared with the once-inoculated were less effectively protected. Lastly, in Hubli the figures regarding deaths set down for the town indicate that there was no difference between the once and the twice inoculated.

If we are to seek an explanation for these divergent results, it is manifest that we can seek it only in the varying strength of the vaccine. We have already pointed out, in the earlier portion of this Chapter, not only that plague cultures of very varying virulence were employed for the preparation of the vaccine, but also that the prescribed dose of the vaccine contained, in some cases, seven times more of the effective bacterial sediment than it did in other cases. We think that differences in the strength of the vaccine may, in addition to explaining the differences set forth in the table we have last given, also account for many of the differences set forth in the synoptical tables. There would, so far as we can see, appear to be a particular dose of vaccinating material which gives the best results, and it would appear to us to be important to secure by the adoption of an accurate method of standardization that this quantum should be contained in the prescribed dose, and that, if possible, this quantum should be administered in a single inoculation, so as to avoid the necessity for re-inoculation.

Section II.(4) of this Chapter.

VIII.—Question of the Duration of the Protection against Plague afforded by Inoculation with Haffkine's Vaccine.

468. We have now reached a point at which it is appropriate to discuss the duration of the protection afforded by inoculation. There are no data to enable us to arrive at a conclusion on this matter, unless we regard as such Major Bannerman's figures for Indhera, which seem to show that the protection had not decreased in the course of six weeks, the figures for the Umarkhadi Jail which show that the inoculated suffered more than the uninoculated in a period dating from the fourth week after inoculation, and lastly (if these can be considered sufficiently accurate to furnish a general indication) the figures for Hubli, Dharwar, Gadag, Bangalore and Mysore. The figures for the five places last mentioned show that the inoculated as a body, though numbers of them were exposed to plague for a considerable time after inoculation, suffered less than the uninoculated, and they may thus be taken as indicating that the effect of inoculation is to some extent lasting.

Duration of protection conferred by inoculation. 4045.

Paras. 437, 439, 440, 441, and 445, of this Report.

From the above it will be seen that the statistics placed before us afford us little material for a decision as to the duration of the protection conferred by inoculation, but this question could be easily determined by following up the history of a group of persons all inoculated at the same time, and by noting the number of cases and deaths from plague among them in a succession of months following upon their inoculation. In interpreting the information thus collected it would be necessary to take into consideration the variation in the amount of plague among the uninoculated population during the months included in the period of observation. While the consideration of the attacks and deaths alone would give us an indication with regard to the duration of protection, more accurate conclusions would probably be drawn if the case mortality was taken into consideration. Just as in the fact that the case mortality among the inoculated in the first days after inoculation is greater than in the succeeding period we have an indication that protection conferred by inoculation is not fully achieved during the first few days, so it is probable that we should find in an increase of case mortality an indication of the time when the protection was beginning to wear off. If two or more groups who had been inoculated with different and accurately known quantities of vaccinating material were kept under observation, valuable indications would probably be drawn from the results as to the quantity of vaccine which produced the most lasting protection.

IX.—Question whether Haffkine's Vaccine exerts any Influence on Diseases other than Plague.

469. Only one question now remains to be dealt with in connexion with the influence on the organism exerted by Mr. Haffkine's anti-plague prophylactic fluid. This question relates to the suggestion, to which we have already referred, that anti-plague inoculation

Question whether anti-plague inoculation exerts influence on other diseases.

I. 338. Paras. 436 and 446 of this Report.

protects not only against plague but also against other diseases. This suggestion emanated from Mr. Haffkine. It appears that in the course of his inoculation work a certain number of cases were brought to Mr. Haffkine's notice in which fevers of an undetermined nature were favourably influenced by the injection of his vaccine. An idea that anti-plague inoculation might possibly protect against other diseases having thus suggested itself to him, Mr. Haffkine proceeded to seek in this idea an explanation for the extraordinary absence of mortality from general causes which was noted in the case of the Khojas inoculated in Bombay. We have, however, pointed out that the exceptionally light mortality recorded for the inoculated Khojas is capable of being explained in quite a different way. What we have just said applies not only to the case of the Bombay Khojas but also to the case of the inoculated Karachi Khojas, among whom there was a similar extraordinary absence of deaths from general causes, which was similarly attributed to the effects of inoculation. The instances which have been just referred to constitute the only statistical evidence which has been brought forward in support of Mr. Haffkine's claim that his vaccine favourably influences diseases other than plague. As this statistical evidence is untrustworthy, we have to fall back on *à priori* considerations, the two isolated instances which Mr. Haffkine has adduced of cases of fever favourably influenced by inoculation, and a few other isolated instances adduced by other witnesses. According to the more or less indefinite statements of the two or three witnesses who are in question, the plague prophylactic is capable of favourably modifying every possible class of disease, from ringworm to leprosy. It is obvious that statements of this kind are not deserving of serious attention. That vaccination against one disease may influence the course of another is, however, *à priori* quite credible, inasmuch as it is known that one disease may influence the course of another. But since no trustworthy evidence has been adduced before us to show that this obtains in the case of anti-plague inoculation, the suggestion though one that might be kept in view by future observers, need here no longer engage our attention.

198–9.

4872. I. 400, 14,824-7, 17,916, 17,963, 19,055, 22,402, 25,459

X.—Summary of Conclusions regarding Efficacy of Haffkine's Vaccine.

Summary of conclusions regarding protective effect of inoculation.

470. As a preliminary to discussing the application of inoculation as a measure for combating plague, it will be convenient to summarise here the results of the preceding critical study of the nature and effect of the anti-plague vaccine. These results may be summarised briefly as follows:—

I. Inoculation sensibly diminishes the incidence of plague attacks on the inoculated population, but the protection which is afforded against attacks is not absolute. On the one hand, plague has attacked persons who have undergone inoculation as many as four times in the course of two years previous to their attack. On the other hand, as many as 8 per cent of the inoculated population may suffer from plague, as was the case in Bulsar. Many varying influences have been at work in determining the rate of attack in different places, and it is impossible to give a numerical expression for the measure of protection against attack which inoculation confers.

II. Inoculation diminishes the death-rate among the inoculated population. This is due not only to the fact that the rate of attack is diminished, but also to the fact that the fatality of attacks is diminished. Here, again, no numerical expression for the amount by which the death-rate is diminished can be given.

III. Inoculation does not appear to confer any great degree of protection within the first few days after the inoculation has been performed. This fact, we may note in passing, has an important bearing on the risk of infection which would be incurred by recently inoculated persons if they were left behind in surroundings so plague-infected as to render their evacuation by the uninoculated desirable.

IV. Inoculation confers a protection which certainly lasts for some considerable number of weeks. It is possible that the protection lasts for a number of months. The maximum duration of protection can only be determined by further observation.

V. The varying strength of the vaccine employed has apparently had a great effect upon the results which have been obtained from inoculation. There appears to be a definite quantum of vaccinating material which gives the maximum amount of protection; and provided that this quantum can be injected

one dose, and provided also that the protection turns out to be a lasting one, re-inoculation might with advantage be dispensed with. The best results of inoculation will only be obtained after an accurate method of standardization has been devised.

XI.—Consideration of the Policy of the extended use of Inoculation as a Plague Measure.

471. The utility of inoculation as a practical measure depends, above all things, upon he extent to which the people are willing to submit to the operation. In a country ke India compulsion in such a matter is not to be recommended. The attitude of he people towards inoculation is therefore deserving of very careful consideration. 'he attitude of the people towards inoculation has generally been the reverse of ncouraging. Though, at times, some of the better classes may have been ready to ubmit to it, the feeling amongst the masses, at any rate until the good results of ioculation caused influential persons to be inoculated, has generally been one f dislike and opposition. This dislike and opposition, so far as we are able to gauge he reasons for it, was based upon the following objections:—First, there was an nwillingness on the part of the people to undergo the personal inconvenience and ever caused by inoculation, in view of the doubts which they entertained regarding he reality and duration of the protection said to be conferred. Secondly, there ras, as we have been told by Colonel Weir, I.M.S., the fear of communication of isease by the syringe from one person to another. Thirdly, in one case at least, at untakal, the people were afraid to be inoculated, because a case of sudden death after ioculation occurred in a man previously to all appearances perfectly healthy. 'ourthly, we have an instance in Bulsar of a community which refused to avail itself f inoculation because they saw that plague cases continued to occur in a large number 3 per cent.) of a group of inoculated persons which was directly under their observation. astly, we have reason to believe that, particularly in Bangalore and Bombay, the rogress of inoculations was checked by a statement made in evidence before us to the ffect that the prophylactic fluid was very seriously contaminated with putrefactive, nd in some instances pathogenic, organisms.

Reason for dislike and opposition of the people towards inoculation. 7533, 9211, 9803, 15,434, 18,594, 22,155, 23,393, 23,746, I. 469, I. 476. 921. 919. 5068. Para. 412 of this Report.

472. This being the feeling of the populace towards inoculation, they have seldom ome forward to undergo it, except where they were in imminent fear of something hat they disliked still more, and which they hoped by means of it to escape. It has sually been not so much the disease itself as the removal to hospital, segregation, estrictions on travelling, evacuation, and other measures resorted to to check isease, that have induced them to resort to inoculation. Thus, where there was no mmediate prospect of measures, there was hardly any inoculation.

The Director-General of the Indian Medical Service, when asked whether he nought the people will ever consent to be inoculated on a large scale before there s an outbreak of plague among them, replied that he did not: "You have only to ook at the way in which they neglected vaccination. They admit the effectiveness f vaccination, but they will be apt to neglect it." The Sanitary Commissioner of the Iadras Presidency was of the same opinion. "The practical difficulty," he said, seems to me to be to get an operation of that sort accepted, especially by certain lasses of the people and the uneducated generally, in time to secure a prevention f the spread of the disease, that is to say, I do not think it is possible, it is not ithin the practical sphere of sanitation, to say that any area can be so prepared eforehand (by means of inoculation) as to prevent an invasion of plague." Practical xperience in various uninfected places not exposed to any immediate danger seems to rove the correctness of these views. Madras City is a case in point. It has hitherto scaped from indigenous plague altogether, and towards the close of 1898 there was o infection even near it. Major Bannerman, I.M.S., gave a number of lectures there n the subject. He was assisted by three medical officers specially deputed for the urpose; four district surgeons, and a lady doctor also aided him in attempting to opularise inoculation. The net result was that in $2\frac{1}{2}$ months about 5,600 people ere inoculated out of a population of 450,000.

Degree to which inoculation resorted to in places threatened, but not actually infected. 7881. 3872. 3,872.

Madras City was not in any immediate danger of infection. In a place which, ough still free, is practically certain to be infected at a very early date, the motives or being inoculated may be expected to be stronger. Such a case is that of Mysore ity. The motive for all the people getting themselves immunized beforehand was robably as strong in Mysore City as it is ever likely to be in any place, since it was

almost certain that the infection would come there from Bangalore. Yet, when the disease did come into Mysore City, it found about six-sevenths of the people still uninoculated.

23,501. 25,106– The one advantage to be gained by inoculation in Mysore City was that inoculated persons could travel more readily than the uninoculated. There was no special exemption that the Government could offer to the people, for there were practically no plague measures. Therefore, putting aside a certain number who were inoculated probably because they wanted to leave the town, the majority became inoculated presumably because they believed that they were being protected. The circumstances of Mysore City, however—situated as it is in a Native State—are special, and we have no exact information as to the motives which there induced people to come forward for inoculation before the outbreak was severe, and to cease to submit themselves to inoculation at about the time when plague was at its height.

Except Mysore City, the only place of which we have information that any large proportion of the people were willing to get themselves inoculated before plague broke out among them is Palewal, a small village in the Punjab. It would have been possible, Captain Clarke, I.M.S., told us, to inoculate all the inhabitants of this village. "There was a Banniah who took the thing up, and he was anxious to remain in and not to be turned out, because it was the beginning of the rains . . . . The people had seen plague all round them, and they themselves stood alone. It was the last village—a little island surrounded by plague villages—and they had seen the discomforts that the others were subjected to . . . . and they were anxious to get relief from those measures. They thought that if they were inoculated they might be allowed to stop in." Here it is obvious that the chief motive for resorting to inoculation was the desire to avoid the ordinary plague measures, and further it is probable that even that motive would not have had much effect had it not been for the influence brought to bear on the people by a prominent inhabitant of the village who was particularly interested in avoiding evacuation.

10,723–8.

In other threatened villages in the neighbourhood of towns where inoculations had been carried out on a large scale, the motives that induced the inhabitants to have themselves inoculated may have included, in addition to the motives referred to above, a conviction that inoculation really did protect against plague. The villagers in the neighbourhood of Hubli and Dharwar were, Mr. Cappel told us, very anxious to be inoculated. The inoculation measures in these two towns had been on such a large scale, and had gone on with advantage to the inoculated for so long, that the people round had been thoroughly convinced of the protection conferred by inoculation. "From one village a long way south of Hubli, I," says Mr. Cappel, "had an offer from a native banker to produce 500 people for inoculation and pay all the expenses of the English Commissioned inoculating officer and his staff. I had another application from the basket weavers of a village 22 miles south of Hubli, who offered to come in in a body to Hubli if we promised not to segregate them, or put them in health camps—a promise which I gave them. There is another village called Annigerry which has frequently applied to me by telegraph and otherwise for the assistance of inoculators, and, that village being a large one, we have treated it as a town, and given it the benefit of inoculation."

1961.

Only a few instances like the above have been brought to our notice. On the other hand, we may notice that village opinion is not always influenced by the success of inoculation in neighbouring places. We have a striking illustration of this in the case of the villages of the Baroda State. Here, in spite of the fact that the inoculation experiment carried out at Undhera proved so successful, we find that only a very small number of the inhabitants of actually infected villages accepted the offer of inoculation which was made to them. The number who did so in villages threatened but not infected was so small as to be insignificant.

II. 517(R.).

Degree to which inoculation resorted to in places actually infected with plague.

I. 469. 7532–3.

473. Even after plague has attacked a place, inoculation has not made much way with the people, unless in cases where severe plague measures were enforced in the case of the uninoculated and were relaxed in the case of the inoculated. The indifference or opposition of the people towards inoculation is illustrated by the attitude of the people in the two largest cities in India. In Calcutta, in the first epidemic, "they made things very unpleasant for persons who were supposed to be inoculators." Dr. Kailas Chandra Bose told us he tried to induce people to be inoculated, but without success. He said, "they are deadly against it, I do not know why. I do not think they would submit to that process at all." The amount of inoculation done was altogether insignificant. Operations were begun the very day that the existence of plague was notified, viz., 30th April 1898. But up to the 22nd July 1898, the

otal number of inoculations performed was only 2,490, a number that did not go ny appreciable way towards protecting the population. It is true that there were nly two inoculators at work, but if there had been more it would not, in the opinion f Dr. Nield Cook, the Health Officer, have made any appreciable difference. The eaders of the Muhammadan community did indeed come forward, and a few of the etter class people of all communities. But inoculation "did not get really taken up: he plague did not get quite bad enough." The bulk of those who offered themselves or inoculation were "persons who desired to travel by rail without suffering the nconvenience of detention at inspection camps." And during the remainder of the ear, owing to the small amount of plague, the absence of scare, and the removal of he office to another place, the number seeking inoculation rapidly became smaller till; only 31 persons presenting themselves in a space of five months. 6557, 6640. 6645–6. 6643. 6650. 6537.

In Bombay City, the number inoculated in 1898 was very small relatively to the otal population. The Municipal Commissioner endeavoured to get the influential eaders among the Hindus to co-operate. Small payments were allowed to poor eople who were prevented by the reaction from going to their ordinary work; and ncouragement to perform inoculations was given to the Medical Officers of the Municipality and to certain private practitioners. But from the beginning of the ear up to 26th November only 25,358 people were inoculated. Lieut.-Colonel Weir, .M.S., told us in December that the opposition was certainly much less than it had een; and he anticipated that during the next outbreak a very much larger number f people might come forward. From returns lately sent to us, however, we find that, lthough the people of Bombay have had experience of three outbreaks, the number ioculated up to 13th September 1899 was only 38,454 out of a population of over 00,000. Moreover, of these a considerable percentage consisted of persons inoculated 1 the Khoja community, which resorted to inoculation principally on account of the ıfluence of their Chief, His Highness the Aga Khan. 1363. 1366. 922.

The case of Karachi is precisely similar to that of Bombay. A very small proportion f the general population of Karachi submitted themselves to inoculation, while a ery large proportion of the Khojas got themselves inoculated. In Broach it would ppear that while the general population did not avail themselves to any extent of ioculation, the influence of the Parsee Panchayat was able to induce most of the 'arsee community of the place to become inoculated. In the Punjab, where inocula- ions were resorted to, they seem, as in particular at Banga, to have been resorted to hiefly on account of the advice and example of influential people who interested ıemselves in the matter. Para. 446 of this Report. 15,511, II. 522-3. 10,172.

In Satara, Belgaum, Surat, and the Central Provinces, where few inoculations were one, the attitude of the people towards inoculation may be gathered from the ollowing extracts, to some of which we have already referred, of the evidence given efore us by responsible plague officers.

At Satara, "the people," says Captain Thompson, I.M.S., "only resorted to it when 'e brought statistics before them to prove its great value." But it also appears that was only "more or less" voluntarily that the few came forward who did come rward. "Some compulsion was used, that is to say, they had privileges held out) them if they submitted to inoculation. If they were living in infected places they ere told that unless they became inoculated they would be moved out into segregation nd health camps." "I have tried every means in my power," said Colonel Hay, lso referring to the town of Satara, "to persuade them, even to the extent of romising re-admission to their houses; they have been promised that those who have een evacuated should be allowed to return if they would be inoculated, but it has had) effect on them at all." 22,393. 22,391.

The object of the few people who got themselves inoculated at Karad, in the Satara istrict, seems to have been mainly to get permission to go out into the infected llages for trading purposes. 21,118, 21,123.

At Belgaum the people who came up for inoculation came "on the assurance given) them that no inoculated person would be liable to be removed to hospital or segrega- ion camp against his wish." The withdrawal of that assurance resulted, Mr. chgaonkar says, "in almost a complete check to the further progress of inoculation." 23,132.

Most of the people inoculated at Surat were, Lieut.-Colonel Nariman, I.M.S., said, Refugees from Bombay, and they wanted to get back to Bombay." 15,879

Colonel Hutcheson, I.M.S., again, told us that at Hinganghat, in the Central Provinces, e people applied for the inoculations to be done simply in order to obtain a pass which ould permit them to go to their various employments. 6046.

9211. In the North-west Provinces, Mr. Winter said, "At Jawalapur arrangements were made for inoculation, but we were to use no pressure on the people, and they would not hear of it."

Conditions in places where inoculations largely resorted to.

474. We may now pass on from the consideration of infected places where inoculations were either not done or were done only in very small numbers, to the consideration of infected places where a considerable portion of the population was inoculated. We have already considered the case of Mysore City, which stands by itself, and we have now to consider only the cases of Hubli, Dharwar, and Gadag, all three within the Dharwar district, and of the City and Civil and Military Station of Bangalore. We have referred above to the question of the general motives which induced people to became inoculated, and we have also in our critical study of the statistics, in the cases of the particular towns now in question, dealt with the motives which were specially operative in them, and the manner in which inoculations were pushed. It may, however, be advisable here further to quote the opinion of some of the officers on the spot.

1963. "The object of the people," says the Collector of Dharwar, speaking of all the towns in his district, but more especially of Hubli, "is, in a very large proportion of cases, and the object of almost all the people at the beginning of the operations before they become convinced by bitter experience of the advantages of the process, is not to obtain an inoculation, but to obtain the certificate, which carries with it certain immunities and privileges."

The motives which influenced the people of Dharwar to get inoculated may be judged also from the following statements. On the one hand, Major Hardy, the Chief Plague
23,746. Officer of the town, stated the people "feared segregation and inoculation . . . . 18,000 went out in one week influenced by this fear." On the other hand, Surgeon-General Harvey, the Director-General of the Indian Medical Service, in addressing the Royal Society, and, we understand referring to the case of Dharwar, said "that while the mention of inoculation drove 300,000 people from the City of Calcutta, he had himself seen, in another part of the country, people coming in their hundreds to be inoculated, and paying 2 annas each for the privilege." That payment was made for inoculation does not prove that the people appreciated the value of inoculation as a protection against plague, any more than the fact that, in the neighbouring town of Hubli (and we think that this was the case at Dharwar also) payments were made by the people for the disinfection of their houses proves that they had a belief in the efficacy of disinfection. In both places inoculation and disinfection were necessary preliminaries to the return of the people to their houses.

23,680. Speaking of Gadag, Mr. Foy, M.B., told us, "With the appearance of plague there was a further rush for inoculation, and a large number were inoculated before the epidemic had taken a very strong hold, and to prevent having themselves evacuated from their homes." And the same witness also said, "The people of Betigeri made a great rush for inoculation, and thus avoided the general evacuation of the town by getting all the inmates of their houses inoculated."

When asked, with regard to the inoculations in Bangalore, whether the protection afforded to them was the only inducement for people to become inoculated there.
3184, 3170. Captain Leumann, I.M.S., said, "Partially the protection afforded, but chiefly, I think, to escape segregation."

Importance of encouragement, of inoculation, by influential members of the native communities of India.

475. It appears, then, to us to be evident that hitherto at all events the principal motive which has induced individuals to come forward to be inoculated has been the desire to obtain relief from the inconveniences of the plague measures ordinarily applied, rather than that of protecting themselves against an attack of plague. This is a somewhat discouraging conclusion; but we may, perhaps, hope that when the beneficial effects of inoculation are more widely known native opinion may be won over to its favour. Seeing what has been done in certain places by personal influence, like that exerted by H.H. the Aga Khan, we think that influential men in the native communities of India can do much to bring about this more satisfactory state of affairs. It is to be noticed that personal influence is particularly valuable wherever severe plague measures are not applied, and where, therefore, there are no material advantages in the form of a relaxation of measures which can be conferred upon the inoculated, and which can operate as an inducement for people to submit to inoculation. Moreover, the encouragement of inoculation by the personal influence of prominent members of the native communities will render it possible to dispense with such a system as that of encouraging inoculation by the grant of small compassionate allowances, which is liable to abuse.

476. In view of the fact that the policy of inoculations will give its best results only when inoculations have been undertaken before the advent of plague, or soon after it has broken out, it is important to note that experience in the past has shown that inoculation as a measure is much more easily applied in towns than in villages. A scrutiny of the figures relating to towns where inoculation has been largely resorted to will show that it takes a very considerable time to inoculate any sensible fraction of the population. In only one case, that of Mysore, has such a sensible fraction been inoculated before the outbreak of plague. In the case of villages, the time required is greater relatively to the number of the population, as we can see by a reference to the time which was occupied in the inoculations performed in the villages of the Baroda State, and at Lingdhal and Huilgol, which have been mentioned in a previous part of this Report.

Considerable time required to inoculate any sensible fraction of population of towns and villages.

Paras. 448 and 442 of this Report.

477. We have hitherto dealt with the protective effective of inoculation in preventing attacks and deaths from plague, and we must now pass on to consider whether the adoption of inoculation as a measure is likely to have the beneficial effect claimed for it in preventing panic in an infected place with its attendant exodus of the population and consequent dislocation of trade. In this connexion Mr. Cappel, referring to both Hubli and Dharwar, said:—"In Hubli trade was not seriously interfered with at any time, and in Dharwar the course of business has never been interrupted. In Hubli, owing to weather conditions, the town was never evacuated on any large scale. In Dharwar about one-third of the people went out immediately on the outbreak of plague. But these people who went out were of the agricultural class, owning land round about, and their families and dependents. The 10,000 people who remained and who were enabled to remain by inoculation alone were the Government employees, the large mass of railway employees, and the business part of the community. There was no interruption to railway business, Government business, or commercial business in Dharwar. That state of things would have been impossible without inoculation, and if inoculation had not been carried out, or if inoculation had failed, the consequence would have been very serious, both in Hubli and Dharwar."

Effect of inoculation in preventing panic.

1956.

In speaking of Betigeri, Mr. Foy claimed advantages for inoculation similar to those claimed by Mr. Cappel in the cases of Hubli and Dharwar. He said that the people freely resorted to inoculation and "the consequence is that the main street of Betigeri has always presented a busy aspect, the shops remaining open, and some amount of trade is carried on." 23,680.

Major Richard Baker, I.M.S., as we have already noted, told us with reference to Lanauli that while the uninoculated part of the town was deserted on account of the plague, the inoculated part was full of people. 18,430.

Regarding the causes of the exodus which took place from Bangalore City, Colonel Benson, I.M.S., told us that at the commencement of the outbreak the supply of vaccine ran short. "The result of sufficient vaccine not being available was that a very great number of people ran away from the town who would otherwise have remained." In Mysore City the inoculations, as we have seen, were largely done before plague got a hold on the town, and though an exodus took place, it was smaller than that from Bangalore City and the Civil and Military Station. 3482. 24,977-9.

It is quite plain that panic could only be prevented by inoculation in so far as inoculation prevents severe mortality from plague. In other words, if the people are not inoculated sufficiently early and in sufficiently large numbers, a flight from an infected locality will not be prevented. Where, however, the inoculated have confidence in the efficacy of inoculation, the exodus will be that of uninoculated persons only, and the inoculated will continue to perform their avocations. On the other hand, when people dread inoculation, and fear that it may be insisted on, the adoption of inoculation as a plague policy may, as we have seen in the case of Dharwar and Calcutta, give rise to a far more formidable panic and exodus than any which would have been produced by the fear of plague. In response to a question whether the people of Dharwar would have left the town from fear of the plague, Mr. Cappel told us: "No, none of them would have moved. They would have waited till they were nearly all dead. They would have moved out in the next epidemic undoubtedly, but they would not have learned their lesson before." Our conclusion, then, regarding this matter is that people after being inoculated have shown less disposition to fly from an infected place than the uninoculated, but that it is impossible to judge how far this result has been due to the sense of security derived from inoculation, or how far it has been due to the escape from the worries of other plague measures from which, as inoculated persons, they have been exempt. 1614.

Conclusions as to feasibility of adoption of inoculation as a plague measure.

478. With regard to the feasibility of adopting a general policy of inoculation, our conclusion, based upon the above considerations, may be set forth as follows:—

(1.) Experience gained hitherto has shown that it is very seldom possible to get a large proportion of the inhabitants of an uninfected place inoculated.

(2.) It has been possible, where the inducement of exemption from segregation and eviction has been offered, to get a large proportion of the inhabitants of an infected place inoculated quickly.

(3.) It has been possible in one place, Mysore City, even where no inducement that touched the great mass of the people could be offered, to get a considerable proportion of the inhabitants of an infected place inoculated quickly.

(4.) It has been possible also to induce a large proportion of particular communities, such as the Khojas of Bombay and Karachi, to be inoculated under the influence of their leaders.

XII.—Recommendations regarding the Adoption of Inoculation with Haffkine's Vaccine as a Plague Measure, and Conclusion.

Recommendation regarding adoption of inoculation as a plague measure.

479. Our recommendations are governed by these conclusions, as well as by the conclusions we have already drawn with regard to the protection conferred by inoculation. They are further governed by the consideration that it is necessary, as far as may be possible, to dispel the particular objections on account of which the people have hitherto refrained from inoculation. Moreover, our recommendations are based on the consideration that it is advisable to make the operation as effective as possible, as little inconvenient as possible to the person inoculated, and as easily carried out as possible by the inoculator.

I.—We have already insisted on the necessity for the accurate standardization of the vaccine, as being essential to the attainment of the best results, both as to the protection conferred and as to the duration of that protection. We have pointed out, also, that the introduction of an accurate system of standardization may possibly obviate the necessity of employing two successive inoculations.

II.—With a view to dispelling the natural fears of the people, and avoiding all unnecessary risk to the inoculated, and further, with a view to enabling the inoculations to be carried on with the least possible amount of attendant inconvenience, we would insist upon the necessity that stringent precautions should be taken to insure that all the vaccine used is quite free from contamination, and we would recommend that the volume of the necessary dose should be made as small as possible.

III.—With a view to removing any possible ground for distrust as to the conveyance of disease by inoculation from one person to another, we would recommend that the syringe should be sterilised in the presence of the person about to be inoculated.

IV.—We are of opinion that inoculations, under the safeguards and conditions stated above, should be encouraged wherever possible, and in particular, it seems to us desirable to encourage inoculation among disinfecting staffs and the attendants of Plague Hospitals.

Conclusion.

480. We cannot bring to a conclusion this consideration of anti-plague inoculation by Mr. Haffkine's prophylactic fluid without expressing our sense of the importance of the method which Mr. Haffkine has devised and of the results which have been achieved by it. The credit due to Mr. Haffkine is the greater because the difficulties with which he had to contend in this matter could only have been overcome by great zeal and endurance. Mr. Haffkine's work on anti-plague inoculation, while not based on any new scientific principle, constitutes, it seems to us, a great practical achievement in the region of preventive medicine.

CHAPTER V.

SERUM THERAPEUTICS OF PLAGUE WITH SPECIAL REFERENCE TO THE ANTI-PLAGUE SERA OF YERSIN AND LUSTIG.

I.—Introductory Discussion regarding the Production, Characters and Mode of Action, and Methods of Testing for the presence, of Anti-Toxic and Anti-Bacterial Substances.

I. (1.)—*Principles of Serum Therapathy.*

Serum treatment applied to plague.

481. The discovery (made almost simultaneously by Behring and Fraenkel) that the serum of animals which have been subjected to repeated inoculations of the specific poisons of diptheria and tetanus is capable of functioning as a specific antidote to these poisons is a discovery of the most far-reaching importance, opening up as it does a prospect of effectively treating all bacterial diseases by antidotal substances elaborated in the organism of immunised animals. Our concern here will, however, be not so much with the general therapeutic principle which takes its origin in the discovery of Behring, as with applications of this therapeutic principle which have been made by Yersin and Lustig respectively in connexion with the prophylaxis and treatment of plague. Before dealing with these we may with advantage set before us certain general considerations with regard to the pathological processes, and with regard to the serum treatment, of plague.

Nature of the pathological processes associated with plague and question as to kinds of therapeutic substances required for the treatment of these.

482. In the case of plague, we have to deal with a "combined invasion and intoxication process." In other words, we have to deal, on the one hand, with an invasion of the system by the micro-organisms of plague, and, on the other hand, with an intoxication produced by the poisons of these bacilli. In conformity with this, it is manifest that two different classes of therapeutic substances might appropriately be brought into application in the treatment of plague. In the first place, *anti-bacterial substances* might be brought into application with a view to killing, or checking the growth of, the invading bacteria. In the second place, *anti-toxic substances* might be brought into application with a view to abolishing or alleviating the symptoms which are referable to the bacterial poisons. The fundamental facts with regard to the (*a*) production, (*b*) characters and mode of action, and (*c*) methods of testing for the presence, of each of these classes of therapeutic substances may conveniently be separately discussed.

I. (2.)—*Anti-toxic Substances.*

Question whether anti-toxins are elaborated in the organism for all poisons.

483. Beginning with the anti-toxins we may consider certain questions which suggest themselves in connexion with Behring's discovery that diphtheria and tetanus anti-toxins make their appearance in the blood of animals which have been subjected to repeated inoculations of the corresponding toxins. The first of these questions is as to whether the animal organism which responds to the incorporation of diphtheric and tetanus poisons with a production of anti-toxic substances possesses the potentiality of responding in a similar manner to the incorporation of all other poisons. A moment's consideration furnishes the answer, it being manifest that, if the organism possessed the power of elaborating an anti-toxin to any and every poison, it would necessarily manifest a capacity for tolerating increasing doses of all poisons indifferently. Such a capacity, however, exists only with respect to a comparatively small minority of poisons. Even in the case of this minority of poisons, anti-toxins are not in all cases produced by the incorporation of repeated and gradually increasing doses. It would appear that toleration of alcohol and of morphia does not depend upon the elaboration of any corresponding anti-toxins.

Question whether anti-toxins are obtained exclusively in connexion with bacterial poisons and invariably in connexion with these.

484. The next question which presents itself for consideration in connexion with the production of anti-toxins is the question as to whether anti-toxins are obtained exclusively in connexion with the incorporation of bacterial poisons and invariably in connexion with these. This question may, as regards the former point, unhesitatingly be answered in the negative, for anti-toxins have been obtained, on the one hand, to the non-coagulable neurotropic constituent of snake venom, and, on the other hand, to a variety of poisonous vegetable "albumoses," in particular to ricin, croton, abrin, and robinin. The question as to whether anti-toxins can invariably be obtained by the incorporation of poisons of bacterial origin must also apparently be answered in the negative. In particular, the attempts which have been made to obtain an anti-toxin to the poisons of the tubercle bacillus have proved quite unsuccessful.

Classification of poisons into a class of antitoxi-gennetic and a class of non-antitoxi-gennetic poisons.

What has been said above with regard to the elaboration of anti-toxins in the system may be summed up in the statement that poisons may be distributed into a class of antitoxi-gennetic, or anti-toxin producing, poisons, and into a class of non-antitoxi-gennetic poisons, poisons which do not produce anti-toxin.

Toxins may, after they have been incorporated, circulate as such for a considerable period in the blood.

485. The next question which must engage our attention—for it would lead us too far afield to discuss the brilliant hypothesis with which Ehrlich has illuminated the whole question of the production of anti-toxins within the system—is the question as to the immediate and remote results exerted on the blood by the incorporation of a bacterial poison.

It is important to realise that poisons in general, and, in particular, bacterial poisons, whether these afterwards give rise to a production of anti-toxins or not, may, for quite considerable periods, continue to circulate as such in the blood of the animal into which they may have been incorporated. It was shown by Metschnikoff that this occurs in the case of tetanus toxin introduced into the system of crocodiles and other reptiles. The same observation was made by one of us (Ruffer) in the case of tuberculin introduced into the system of a horse. In the particular experiments in question serum, drawn off from a horse after the expiration of an interval of many weeks from the date of the incorporation of the tuberculin, gave origin, when introduced into the tubercular organism, to a typical tuberculin reaction.

Annales de l'Institut Pasteur November, 1897.

Sequence of events after incorporation of antitoxi-gennetic poisons.

Ztsch. f. Hyg. XIII., p. 336.

486. The course of events, however, after the incorporation of a poison which ultimately gives origin to a production of anti-toxin is more complex. It may be assumed to be identical with that registered by Ehrlich and Brieger in an experimental investigation directed to the study of the effect produced by the incorporation of tetanus toxin into a milch goat. It was shown in this investigation that in the phase which supervened directly upon the inoculation of the tetanic poison (this phase probably covers the whole period during which the animal is suffering from constitutional symptoms) the milk—and we may assume also the blood of the inoculated animal—exerted upon the animals to which it was administered an effect indistinguishable from that which supervenes upon a direct inoculation of the tetanic poison. Upon this first phase supervened, after a certain interval, a stage in which the milk and blood produced when inoculated into animals neither toxic nor anti-toxic effects. Finally, a stage supervened in which the blood was definitely anti-toxic.

It would thus appear that the blood of an animal which has been subjected to the injection of a bacterial poison may, when it is introduced into the organism of another animal, produce either toxic or anti-toxic effects, according to the period at which the blood has been drawn off, and according as the particular poison may happen to belong to the class of the antitoxi-gennetic poisons, or to the class of the non-antitoxi-gennetic poisons.

Toxic effect may be produced by sera drawn off for therapeutic uses.

487. It is obviously a corollary to the conclusion we have just stated that where a mixture of antitoxi-gennetic and non-antitoxi-gennetic poisons has been incorporated, the blood which is drawn off, while it possesses anti-toxic properties with respect to one class of poisons, may at one and the same time possess also certain toxic properties. In connexion with this particular possibility, we may refer to the fact that, in a series of experiments made by one of us (the President), it was found that animals which had been inoculated with less than the minimum lethal dose of venene succumbed when treated with an anti-toxic blood drawn off from a horse which had been inoculated with snake-venom. Again, in connexion with the general question of the possibility of toxic effect being produced by sera drawn off for therapeutic purposes, we may note that it was found by Dr. W. Bulloch, in a series of experiments of which the results have been privately communicated to us, that the serum of a particular horse which had been treated with staphylo-coccus cultures was lethal to guinea-pigs when administered in doses of over 1 c.c. Dr. Klein informed us that he had had a similar experience. He had observed that the serum of animals which had been treated with cultures of the bacillus sporogenes enteritidis exerted toxic instead of curative effects. In the case of man also definitely toxic effects following on the injection of therapeutic sera have probably come under the notice of every observer who has had any wide experience of serum-therapathy. In particular, toxic effects have come under observation in connexion with the administration of anti-strepto-coccus serum. Such toxic effects, while they are, we are aware, currently ascribed to a toxic influence exerted by foreign blood upon the human or animal organism, may, we think, in many cases, be referable to the introduction into the system of non-neutralised bacterial poisons.

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Mode of action of anti-toxins is by direct combination with their corresponding toxins, the reaction being a strictly quantitative one where the two classes of substances are brought directly in contact with each other.

488. In connexion with the question of the characters and mode of action of anti-toxic substances, there are two points in particular which we must place clearly before us. The first of these relates to the mode of action of anti-toxins, and to the quantitative relations in which anti-toxins must stand to their corresponding toxins. With regard to this, it has been definitely ascertained that it is essential to the complete achievement of the desired therapeutic effect that the amount of anti-toxin administered shall be in correspondence with the amount of toxin which is to be counteracted; given, in other words, that a unit, such as an ordinary lethal dose, or a multiple of that unit, of toxin is to be counteracted, the neutralisation of that toxin will involve the administration of a corresponding unit, or multiple of that unit, of anti-toxin. This, it will be noted, is in conformity with the assumption established by the researches of Ehrlich, C. J. Martin, and one of ourselves (the President), that anti-toxins are toxitropic substances, that is, substances which enter into direct chemical combination with their corresponding poisons. It must be clearly understood that the toxin and anti-toxin inter-act with each other in accurately determinable quantitative relations only in cases where the anti-toxin is brought directly into relation to the toxin before it is injected, or where, failing this, the anti-toxin is incorporated either before or immediately after the injection of the toxin; in other words, the proposition, that the toxin and anti-toxin inter-act strictly in accordance with the law of chemical equivalents, applies only in cases where the conditions are such that the anti-toxin can operate upon the corresponding poison before this poison has entered into chemical combination with the protoplasm of the body cells.

Where the toxins have already entered into combination with the tissues a process of dissociation must first take place.

489. A principle, quite other than that of combination in accordance with the law of chemical equivalence, comes into operation when the anti-toxic substance is introduced into the body at a period when the toxin has already entered into chemical combination with the body cells. Under these conditions the chemical, and in consequence the therapeutic, event would appear to depend entirely upon the anti-toxin being incorporated in sufficient quantity, and in such a manner, as to bring it in relation with the poisoned tissue in a sufficiently concentrated form. In other words, a favourable therapeutic result would appear to be achievable only when the mass effect of the anti-toxins which come actually into relation with the poisoned cells is sufficiently great to effect the dissociation of the toxin from the combination into which it has entered with the poisoned cellular protoplasm. Only when the dissociation in question has been effected will the cell protoplasm be again free to resume its normal functions. The conditions necessary for the achievement of the result would, as consideration will show, be realisable either by the injection of moderate doses of a concentrated anti-toxin directly into the poisoned tissue, or, failing that, by the injection of enormous doses of the concentrated anti-toxin directly into the blood stream, provided always that the anti-toxin, after it had passed out into the lymph, was afterwards led in a continuous stream through the poisoned tissue. Inconsistent with the success of the anti-toxin treatment in the cases under consideration would be, on the one hand, the employment of doses of anti-toxin too small to confer anti-toxic properties on the lymph, and, on the other hand, a stagnation of the lymph stream in the poisoned tissue.

Methods of testing serum with a view to ascertaining whether it does or does not contain anti-toxic substances.

490. We pass to the consideration of the methods by which the presence of anti-toxins in a serum can be determined in a quantitative manner.

Test-toxin to be administered in a dissolved as distinguished from particulate form.

The first point which invites attention relates to the form in which the test-toxin shall come into application. In connexion with this it must be borne in view that it is essential to the correct carrying out of the test that the poison which is employed shall in every case be incorporated into the animals experimented upon in a dissolved as distinguished from a particulate form. Metschnikoff, in particular, has pointed out that, when the toxin employed for the test is contained in the body of bacteria instead of being free in solution, fallacies in judging of the results of the action of an anti-toxin may readily arise in consequence of the intra-cellular destruction of the poison in the interior of phagocytic leucocytes.

Test-toxin to be free from admixture with extraneous poisons.

A second point relates to the possible admixture of extraneous poisons with the test-toxin. In this connexion the toxic properties of peptone broth come into consideration as possible sources of fallacy. It is obviously possible where, as in the case of filtrates from plague cultures, we may be dealing with comparatively small quantities of bacterial poisons admixed with very considerable quantities of peptone broth that the fate of the animal experimented upon may depend quite as much, if

not more, on the neutralisation of the toxic element of the broth as upon the neutralisation of the bacterial toxins.

Test-toxin to be administered in large multiples of a lethal dose.

The third point relates to the quantum of the specific poison which ought to be administered in test experiments. It will not suffice to ascertain the minimum lethal dose of the test poison, and then to employ that minimum lethal dose as the test dose. The fallacies incident to such a method will be obvious when we consider that, if a test dose of toxin consists only of a simple minimum lethal dose, the animal experimented upon will survive the test inoculation even in the case where the quantum of serum employed neutralises only a small fraction of the incorporated toxin. It is obvious that, if in such a case the survival of the test animal were taken as evidence of the efficacy of the serum, an altogether fanciful value would be ascribed to that serum. With a view to avoiding the fallacies incident to all experiments where the test dose of toxin corresponds only to the minimum lethal dose, it is now an accepted principle that the test dose of the poison shall in every case correspond to a multiple, and wherever possible, to a very large multiple, of the minimum lethal dose. It may not be superfluous to make clear the advantage which results from compliance with this last provision. This may, perhaps, best be done by means of an example. If, for instance, we take as our test dose of poison a quantum which corresponds to two minimum lethal doses, and if we then find that a particular addition of serum suffices to avert a fatal issue from our test animal, we have manifestly proof of the efficacy of the serum. At the same time we are still left in doubt as to whether the quantum of poison which has been neutralised corresponds to two complete minimum lethal doses, or whether it corresponds only to a fraction more than one minimum lethal dose. We are thus admitting into our quantitative estimation of the amount of anti-toxin present a possible error of something like 50 per cent. When, on the contrary, we take as our test dose a quantum of toxin which corresponds to 100 lethal doses, and when we then find that a particular addition of serum suffices to avert a fatal issue from our test animal, we are plainly entitled to infer that the serum must have neutralised, at least, 99 minimum lethal doses. In other words, we reduce the error in our estimation within the limit of 1 per cent.

These points relative to anti-toxins having been disposed of, we pass to the consideration of the anti-bacterial substances.

I. (3.)—*Anti-bacterial Substances.*

Definition.

491. The term "anti-bacterial substances" is often employed in bacteriological literature in a very loose sense. It is frequently there employed in such a manner as to include all sorts of stimulating and nutrient substances, which may, by indirect means, enable the organism to offer a more effectual resistance to bacterial invasions. We propose to deal here, under the heading of "anti-bacterial substances," only with substances which have direct or indirect bacteriotropic affinities, *i.e.*, only with substances of which it can be shown that they exercise a chemical influence upon bacteria.

Discrimination of anti-bacterial substances into a class of stable and specific and of unstable and non-specific substances. Ztsch. f. Hyg. IV., p. 389.

492. We may begin our consideration of the character and mode of action of anti-bacterial substances by emphasising the fact that there appear to be two distinct classes of such substances. We have, on the one hand, a class of non-specific anti-bacterial substances which apparently exert an influence indiscriminately upon all bacteria. These are characterised by their great instability. They are destroyed, as was first shown by Nuttall, working under Flügge's directions, when they are exposed to a temperature of 55° C.; further, they are destroyed by exposure to sunlight; and, lastly, they are spontaneously destroyed when the serum in which they are incorporated is allowed to stand for a period of days or, as the case may be, of weeks after it has been drawn off. We have, on the other hand, a class of anti-bacterial substances which exert a specific effect each, it would appear, only upon its corresponding species of bacteria. Unlike those which have been already considered, these substances appear to be comparatively stable bodies. In particular they are not destroyed by the exposure to a temperature of 55°; neither are they destroyed when the serum is allowed to stand for prolonged periods after it has been drawn off; lastly, they are not readily broken down under the influence of sunlight, or of ordinary putrefactive micro-organisms.

Probable mode of action of stable anti-bacterial substances.

493. Passing to the consideration of the mode of action of anti-bacterial substances, it will be convenient to take up first the question of the mode of action of the specific anti-bacterial substances. When a specific anti-bacterial substance is brought in

contact with the particular species of bacteria which it is capable of affecting, it is to be assumed that a chemical combination takes place between the anti-bacterial substance, on the one hand, and some one of the constituent elements of the bacterial protoplasm, on the other hand. The conditions under which the chemical combination in question will take place are probably analogous to those considered above in the case of the anti-toxins. We saw in connexion with these, that, when an anti-toxin is brought directly into relation with its corresponding toxin, a chemical reaction takes place, which, in the case where the toxin is free, consists, it would appear, in a simple chemical combination of the anti-toxin with its equivalent of toxin. We saw further that when anti-toxic substances come into effective operation, in the case where the cellular protoplasm already is occupied by poison, the eventual chemical reaction which takes place is apparently somewhat as follows: first, under the influence of the mass effect which is exerted by the anti-toxin, the poison is dissociated from its combination with the poisoned protoplasm; following upon this dissociation, a chemical combination takes place between the anti-toxin and the toxin which has been withdrawn from the cellular protoplasm. Manifestly it will be to this complex chemical reaction, rather than to the simpler reaction which obtains between anti-toxins and free toxins, that the chemical reaction between anti-bacterial substances and the constituent element of the bacterial protoplasm is analogous. In other words, the combination of the anti-bacterial substance with the corresponding constituent of the bacterial body must be ushered in by a disruption of the bacterial protoplasm. If that disruption is a fundamental one, the anti-bacterial substance will by virtue of that disruption exert bactericidal effects. If, on the contrary, the disruption which takes place in the bacterial protoplasm is a comparatively superficial one, such, for instance, as the disruptive effect which is exerted by an agglutinin, the anti-bacterial substance will exert on the bacteria at most an influence in restraint of growth.

A word or two may here be appropriate in connexion with the forces which oppose the disruptive forces which are brought to bear by the anti-bacterial substances on the bacterial protoplasm. Manifestly these forces are represented by the chemical forces which hold together the bacterial protoplasm. These cohesive forces are, we may assume, relatively powerful in the case of virulent bacteria—virulent bacteria being by definition bacteria which are endowed with a capacity of preserving their vitality in an organism. In like manner the chemical forces which hold together the bacterial protoplasm may be assumed to be relatively weak in the case of attenuated bacteria, these being bacteria which are ill adapted to resist the destructive forces which come into operation upon them in the animal organism.

Probable mode action of non-stable anti-bacterial substances.

494. In connexion with the mode of action of the non-specific anti-bacterial substances, it appears to be certain that these substances, which are contained in the freshly drawn blood of every normal animal, are of altogether fundamental importance in connexion with the destruction of bacteria in the organism. We have proof of this in the fact that the bactericidal and bacteriolytic effects which are obtained when bacteria are brought into contact with the stale blood of immunised animals are much less pronounced than the bactericidal and bacteriolytic effects which are obtained when the same bacteria are brought into contact either with the freshly drawn blood of immunised animals, or with the stale blood of immunised animals which has received an addition of freshly drawn blood derived from normal animals. The explanation of this fact presents many difficulties. It has been suggested, on the one hand, that it is the non-specific anti-bacterial substances which alone are really active, and that they exert an action similar to that of the digestive ferments, the specific anti-bacterial substances playing merely the part of mordants which bring the non-specific anti-bacterial substances into chemical relation with the bacteria. It is, on the other hand, possible that the added effect which is obtained when non-specific anti-bacterial substances operate in conjunction with the specific anti-bacterial substances may be attributable to the combined effect of disruptive forces coming into operation upon the bacterial protoplasm simultaneously in opposite senses.

Conditions under which an anti-bacterial action will come into operation upon bacteria in the tissues of the infected organism.

495. The conditions which must be satisfied before the disruptive forces exerted by the anti-bacterial substances can come into operation on bacteria in the tissues of the infected organism are entirely analogous to the conditions which must be satisfied before the anti-toxins can in the poisoned organism affect the dissociation of toxin from the cellular protoplasm with which it has combined. In other words, the anti-bacterial substances must, before they can affect the bacteria which are situated in the tissues, pass into the blood and thence pass out into the lymph. Further, the lymph which has thus acquired anti-bacterial properties must be led in

a continuous stream through the organs or tissues which harbour the invading bacteria. Lastly, the therapeutic efficacy of the anti-bacterial substances can make itself felt only in cases where the mass effect which is achieved in the poisoned tissues is adequate to bring about the desired disruption in the bacterial protoplasm. It may be noted that the difficulties which stand in the way of the realisation of these conditions constitute perhaps a more formidable obstacle to the effective therapeutic exploitation of anti-bacterial substances than even the difficulties which are associated with the production of potent anti-bacterial sera.

Conditions under which anti-bacterial substances make their appearance in the blood.

496. With regard to the nature of the chemical stimulus which gives rise to an increase of anti-bacterial substances in the blood, the salient fact which we have to place before us is that, so far as is known, such increase occurs only after the incorporation of either living or dead bacterial cultures. In view of the fact that the bacteria which are introduced into the system of the inoculated animals are there disintegrated, we may infer that the actual chemical stimulus which comes into operation, and which gives origin to the production of the anti-bacterial substances, is the stimulus which is exerted by the poisons which are set free on the dissolution of the bacterial protoplasm. It would thus appear that the conditions under which the specific anti-bacterial substances originate are identical with those under which anti-toxic substances originate. The anti-bacterial substances have, further, this point in common with the substances to which the term anti-toxins is usually restricted, that they make their appearance in the blood only after a not inconsiderable interval has elapsed from the time of the incorporation of the corresponding poisons.

Method of testing a serum with a view of ascertaining whether it does or does not contain any anti-bacterial substances.

497. Three different kinds of methods can be employed with a view to determining whether a particular serum contains any anti-bacterial substances.

1. The serum may be mixed in vitro either in the diluted or undiluted state with the bacteria against which it is to be tested. Microscopic observations may then be made to determine whether the morphological structure or disposition of the bacteria is in any way altered by the serum. This will be a perfectly simple matter where we are dealing with motile bacteria, and with bacteria which do not, in the ordinary course of growth, agglomerate themselves into groups; if agglutinins are contained in the serum, the presence of these will be notified by the fact that bacteria will lose their motility and will be agglomerated.

2. Further, the effect of the serum upon the bacteria may be judged of by cultivation methods. We may determine, on the one hand, whether the bacteria will grow in the serum, and, on the other hand, whether all or a large proportion of the bacteria which are brought in contact with the serum are thereby killed or rendered incapable of growing when transferred to ordinary nutrient media.

By the employment of the above methods we are enabled to form some anticipation with regard to the effect which the serum would exert if it were brought into direct contact with the bacteria in the infected organism.

3. Further, a serum may, in conformity with the method of Pfeiffer, be tested for the presence of specific anti-bacterial substances by introducing it into the peritoneal cavity of a normal animal along with a measured quantity of the corresponding bacteria. The presence or absence of specific anti-bacterial substances would then be judged of by withdrawing portions of the peritoneal fluid at different intervals and examining them, with a view to ascertaining whether the bacteria which were incorporated have been spherulated or dissolved.

By the application of this method, we obtain information as to the effect which the serum would be capable of exerting when introduced into an infected organism, which had at its disposal the same resources in the form of non-specific anti-bacterial substances as are at the disposal of the normal animal.

4. Lastly, the presence of anti-bacterial substances in a particular serum may be judged of by ascertaining whether a favourable therapeutic result is obtained by the introduction of this serum into an animal which has been infected with the disease which the serum is designed to counteract.

By the employment of this method we obtain definite information as to what may be hoped for from the therapeutic application of the serum.

II. Methods employed in the Preparation of the Anti-Plague Sera which have been used in India.

The preparation of anti-plague sera.

498. Having now completed our prefatory task, we take up at this point the consideration of anti-plague sera, which form the special subject-matter of this portion of our Report. Under the heading of "Yersin's anti-plague serum," we shall deal with all sera prepared by the incorporation into animals of living or dead cultures of the plague bacillus.* Similarly, under the heading of "Lustig's anti-plague serum," we shall deal with all anti-plague sera prepared by the process of Lustig.

Our consideration may conveniently begin with the preparation of the sera in question.

Method of preparation of Yersin's serum.

499. It will not be possible in connexion with the preparation of Yersin's serum to do more than indicate generally the procedure which was followed in the case of the serum which was placed at our disposal by the authorities of the Pasteur Institute, Paris. We have also at disposal a few particulars with regard to the serum which was prepared in Bombay by Mr. Haffkine.

Details with regard to the preparation of the particular samples of Yersin's serum furnished to us by the Pasteur Institute, Paris. 26,210.

The horses which furnished the particular sera which were placed at our disposal were, as we were informed by M. Roux, of the Pasteur Institute, Paris, subjected in the first instance to hypodermic inoculations of dead plague cultures. Following upon these hypodermic inoculations, intravenous inoculations of dead plague cultures were employed. Finally, intravenous inoculations of living plague cultures were resorted to. The sera were, with a view to ensuring their perfect sterility, heated to a temperature of 60° C. before they were sent out.

Criticisms relating to the method of preparation.

If we may assume that the blood was not drawn off from the inoculated horses until these had completely recovered from the effects of their inoculations, the only point which would seem to invite comment in connexion with the procedure which is adopted is the heating of the serum prior to sending it out. The question may well suggest itself as to whether the advantages, from the point of view of securing the asepticity of the serum which result from the heating are not counterbalanced by the disadvantages associated with the destruction of the non-specific anti-bacterial substances. It might, however, be argued that, inasmuch as the non-specific anti-bacterial substances in question would probably disappear spontaneously from the serum, the heating would not make any difference in the therapeutic efficacy of the serum.

Preparation of the serum employed by Mr. Haffkine. 131–3.

500. Only very meagre details were furnished to us by Mr. Haffkine with regard to the preparation of the serum which he obtained by the injection of plague cultures into animals. We learned that the serum was furnished by horses, goats, and sheep, that "it was taken from animals at different stages of their being treated with increased quantities of plague microbes," that the treatment was in some cases continued for as long as 12 months, and that in some cases as much as 1,600 c.c. of culture was employed for the last injection.

Method of preparation of Lustig's serum.

501. Passing to the consideration of the preparation of Lustig's serum, detailed information is afforded in a note of Professors Lustig and Galeotti published as Appendix VI. of our Proceedings and in the evidence given to us by Dr. Galeotti on the two occasions when he appeared before us. The serum is obtained from horses which have been subjected to repeated inoculations of a substance which is derived from the dead bodies of plague bacteria. The derivative substance in question is obtained in the following way. Mass cultures of plague are made on large surfaces of agar. After these have been grown for a variable period (three days or less) the plague bacteria are scraped off from the surface of the agar. This done, they are—following a method previously employed by Buchner in connexion with other bacteria—treated with a 1 per cent. solution of caustic potash. Under the influence of this re-agent, the bacterial bodies are disintegrated. When solution has been effected, the fluid in which the bacterial substances are dissolved is rendered slightly acid by the addition of hydrochloric or acetic acid. As a result of this addition, a precipitate is formed in the fluid. The precipitate in question, which apparently possesses the characters of a nucleo-albumen, is collected on a filter, and is then dried and weighed. Finally, after it has been re-dissolved in a weak solution of carbonate of soda, it is introduced either hypodermically or intravenously into the system of the horses which are to furnish the serum. The blood is withdrawn from these horses after a certain interval has elapsed from the date of inoculation.

* It is perhaps hardly necessary to point out that this method of preparation, constituting as it does merely an application to the special case of plague of a procedure already previously largely employed in the case of bacterial diseases, is one that presents no features of novelty. In like manner it will not be necessary to insist on the fact that sera have been prepared by the method by bacteriologists all over the world and not exclusively by M. Yersin.

Criticisms relating to the specificity of the substances obtained by the above method of preparation.

The first question which suggests itself for consideration in connexion with the substance obtained by the method described above, is the question as to whether it is a specific derivative of the plague bacillus. We may note the following points.

There is nothing in the nucleo-albuminous character of the substance obtained by Lustig to warrant the belief that this substance is specific to the plague bacillus. It is true that nucleo-albuminous substances have not been obtained, and possibly are not obtainable, from all species of bacterial cells, but, on the other hand, they can—as already indicated above—be obtained, by treatment with caustic soda and precipitation with dilute hydrochloric acid, from many forms of micro-organisms, bacteria as well as yeasts.

Consideration of the little that is known of bacterial poisons suggests that it is hardly likely that substances which are specific to the plague or any other bacillus would resist the disintegrating action of the strong caustic soda which comes into operation in Lustig's process.

Again, it may be urged against the specificity of the nucleo-albuminous substance employed by Lustig that the intravascular coagulation which has been observed to supervene upon the inoculation of this substance, both in the case of mice (intra-peritoneal injection) and in the case of horses (intravascular injection), notoriously comes under observation also in connexion with the injection of nucleo-albumen derived from the cells of the animal organism and also, as shown by one of us (Wright), in connexion with the nucleo-albumen derived from yeast cells.

British Medical Journal, September 16, 1891.

It might, however, seem that the question might be decided by a comparison of the toxicity assigned by Lustig to his plague nucleo-albumen with the toxicity of the nucleo-albumen which is obtained from other sources. This, however, does not help us. For Lustig's statement that the minimum lethal dose of his plague nucleo-albumen varies, according to the method of proportion adopted, between 5 · 28 and 8 · 35 milli-grammes per 100 grammes of body weight of "mice, rats, and rabbits," is a statement which does not take any cognisance (*a*) of the difference of susceptibility in the various classes of animals; (*b*) of the different methods in which poison may be introduced into the body; or (*c*) of the difference between the dose of nucleo-albumen which produces death by intravascular coagulation, and the dose which produces death apart from intravascular coagulation. The importance which attaches to this last point emerges when we consider that any ordinary nucleo-albumen would, in doses much smaller than those particularised above, if intravascularly injected into a rabbit, ordinarily produce death by intravascular coagulation.

I. 347.

Hardly more conclusive as to the specificity of the nucleo-albumen obtained by Lustig is the experimental evidence brought forward to show that immunity against plague can be established by the inoculation of this substance. It is true that a certain amount of protection against plague would appear to be afforded by the injection of Lustig's plague nucleo-albumen. But this would appear to be inconclusive as to the specificity of the material employed, firstly, because, in the case of most of Lustig's experiments, both the prophylactic and the test inoculations were made into the peritoneum, and it has been shown that a short-lived non-specific immunity can readily be produced by intraperitoneal injections of the most varied substances, and secondly because, as Wooldridge showed,—and his conclusions have been confirmed,—a certain amount of protection can be conferred upon animals by the inoculation of nucleo-albumens derived from animal cells.

Proceedings of the Royal Society 1887. British Medical Journal, September 19, 1891.

In view of all the above considerations, we are of opinion that as yet there has been no conclusive evidence afforded that the plague nucleo-albumen which is employed by Lustig for the inoculation of his horses, is a specific derivative of the plague bacillus.

III. Experimental Investigations relating to the Properties of Anti-Plague Sera employed in India.

Results of laboratory examination of anti-plague serum. 134–5.

502. Excluding from consideration the serum employed by the Russian Plague Commission, regarding which we have no information, and the serum prepared and employed by Mr. Haffkine, of which we were told only that good results were obtained upon animals with certain of the sera, the only samples of anti-plague sera whose properties have been experimentally investigated in the laboratory are two samples of Yersin's serum from the Pasteur Institute, Paris, examined by the

German Plague Commission, two further samples of that serum examined by Captain Mason, R.A.M.C., in Cutch, and two samples of Yersin's and of Lustig's serum respectively placed at our disposal by the authorities of the Pasteur Institute in Paris and by Dr. Galeotti. We may conveniently dispose, first, of the examinations made by the German Plague Commission and by Captain Mason, R.A.M.C.

Examination of Yersin's serum by the German Plague Commission.

Report of German Plague Commission, pp. 325–331.

503. The first experiments undertaken by the German Plague Commission were conducted with a serum which they characterise as "the older," to distinguish it from "the newer" sample which they subsequently examined. With a view to securing that the serum should be administered under the most favourable conditions, relatively large quantities—8·5 to 10 c.c.—were administered to monkeys 24 hours before they were inoculated with plague. Three monkeys were thus treated with "the older" serum, and two out of the three died of plague. Seven monkeys were treated on similar lines with "the newer" serum; of these only two died. The deaths occurred in the case of monkeys which had received only 1 c.c. of the serum; the surviving monkeys received respectively 10, 5, 5, 3, and 3 c.c. In addition to the brown monkeys treated in the above series of experiments, two of the less resistant species of grey monkeys were treated each with 10 c.c. of serum. Both these succumbed when they were 24 hours later inoculated with 1 c.c. of a broth culture of plague. Lastly, in two series of experiments, these being conducted again on brown monkeys, the therapeutic efficacy of the serum was tested by administering it to the monkeys, after a previous inoculation with plague. Of the 11 monkeys which were treated thus, in each case with 10 c.c. of "the newer" serum, only two died, these being monkeys which had not received the serum injection until 48 hours after they had been infected with plague: of the survivors three had received the serum injection immediately after they had been infected, two received it six hours afterwards, two received it 12 hours afterwards, and two 24 hours afterwards.

Captain Mason's experimental investigation of samples of Yersin's serum.

27,065–116.

II. 479.

504. The serum tested by Captain Mason, R.A.M.C., was derived from the Pasteur Institute at Paris, and was furnished in part by Horse No. 31, and apparently in part by Horse No. 88. Of six mice experimented on with quantities varying from $\frac{1}{50}$ to $\frac{1}{3}$ c.c. of the serum of Horse No. 31, three survived a subsequent inoculation with plague, while the life of the three others was prolonged as compared with the controls: these last, three in number, all succumbed. In the case of the serum which was, it appears, derived from Horse No. 88, one mouse, which received $\frac{1}{4}$ c.c. of the serum, recovered, while a control mouse died; further, of two rats which received in each case 2 c.c. of the serum at different periods after inoculation with plague virus, one, which received the serum 24 hours after inoculation with plague, survived, while the other, which received the serum 36 hours after inoculation with plague, succumbed, though its life was prolonged as compared with the control. Lastly, a monkey which received 2 c.c. of the serum 24 hours, and 20 c.c. 49 hours, after inoculation with plague survived, while a control monkey died. These results show that the serum which was tested by Captain Mason, and was used by him in Cutch, possessed the power of favourably influencing the course of plague in animals.

Details of samples of serum examined by us.

26,210, 26,213.

505. We ourselves examined in all four samples of anti-plague sera. Two of these were samples of Yersin's serum, prepared at the Pasteur Institute in Paris, and placed at our disposal in a most generous manner by the authorities of the Institute; the two remaining samples of serum were samples of Lustig's serum furnished by Dr. Galeotti, who was working in Bombay under the instructions of Professor Lustig and under the auspices of the Municipality. These last samples, representing some hundreds of doses, were derived from two horses treated in Bombay, denoted respectively Horse No. 1 and Horse No. 3. The details of the inoculations and the bleedings to which these horses were subjected were very kindly supplied to us by Dr. Galeotti, as follows:—

The horses "received the following inoculations of nucleo proteid which was prepared by Professor Lustig's process from cultures of plague bacilli:—

Horse No. 1.

Nov. 17, 1898.	Plague-nucleo-albumen	0·18 grammes.
Nov. 28, „	„ „	0·57 „
Dec. 12, „	„ „	1·38 „
Dec. 29, „	„ „	3·97 „
Jan. 21, 1899.	„ „	1·23 „
Feb. 15, „	Bled 2,000 c.c.	
Feb. 17, „	„ 3,500 c.c.	

Horse No. 3.

Nov. 23, 1898.	0·11 grammes.
Nov. 17, „	0·92 „
Dec. 9, „	2·85 „
Jan. 2, 1899.	1·53 „
Jan 10, „	1·59 „
Feb. 5, „	2,000 c.c. blood extracted. The serum was employed on plague patients in the Arthur Road Hospital.
Feb. 7, „	4,000 c.c. blood extracted. The serum is sent herewith to the Plague Commission."

In addition to being supplied with details as to the inoculation of these horses, two of us (Dr. Wright and Dr. Ruffer) had an opportunity, on three different occasions, of inspecting the horses—some six in number—which were under treatment by Dr. Galeotti. On each of these occasions we found the horses—we would except from this description Horse No. 1—in a condition of extreme marasmus complicated by suppuration, in absolute contrast to that condition of bodily health which is characteristic of horses who have responded to an injection of snake venom, tetanus, and diphtheria, toxins with the production of a corresponding anti-toxin. This was by itself sufficient to rebut the idea that the horses could have neutralised the poisons with which they had been injected.

Application of different methods of testing sera to special case of anti plague sera.

506. Before setting forth our own experiments on the samples of Yersin's and Lustig's anti-plague sera which were tested, it will be convenient to reconsider, from the special point of view of their application to plague, the different tests which, as we have shown above, are useful in estimating the value of sera.

Para. 490 of this Report.

It has been explained above that the presence or absence of anti-toxic substances in the serum can be ascertained with certainty only when we are in a position to administer to an animal a multiple, and preferably a large multiple, of the minimum lethal dose of the corresponding poison.

Para. 425 of this Report.

Further, facts stated elsewhere in this Report show that it has not hitherto been possible to obtain the toxin of plague in a sufficiently concentrated form to allow of even small multiples of the minimum lethal dose being administered to a test animal. It is, therefore, at present, all but impossible to determine the question as to whether a particular anti-plague serum possesses any anti-toxic properties with respect to the soluble toxins of plague.

Similar difficulties, we may note, stand in the way of determining whether an anti-plague serum is, or is not, capable of neutralising poisons which are contained in the bacterial bodies; and to these difficulties are superadded other difficulties associated with the circumstance that these bacterial poisons would have to be administered in particulate form, instead of, as we saw above was essential for the avoidance of fallacies, in solution.

Experimental investigation must therefore, in the case of anti-plague sera, for the present confine itself to ascertaining whether a serum does or does not contain specific anti-bacterial substances. To the determination of this point we directed our investigations.

Results of tests applied by us with a view to ascertaining whether anti-bacterial substances were contained in the sera furnished to us.

Section III (*f*) of Chapter III. of this Report.

507. Our results may be very briefly recorded. In the case of the samples, both of Yersin's serum and of Lustig's serum, which were examined by us, we were quite unable to detect anything in the nature of agglutination or morphological alteration in the plague bacteria which were brought in contact with the diluted or undiluted serum. These observations, however, are not conclusive as to the absence of agglutinins, for, as explained elsewhere, the presence of agglutination can hardly be demonstrated with cultures such as those we were using, in which the plague bacteria are already *ab initio* agglomerated together.

Further, we were unable to satisfy ourselves by cultivation methods of the presence of any anti-bacterial substances in the serum. Plague bacteria were cultivated on Yersin's and Lustig's serum and on mixtures of those sera and ordinary nutrient broth; and these cultures were, so far as could be judged, no less luxuriant than those which were made upon normal horse serum and nutrient broth respectively.

Further, we added the serum in different proportions to fresh cultivations of plague bacteria, and we inoculated these mixtures into the peritoneal cavities of normal guinea-pigs, with a view to determining whether the serum would, in presence of the non-specific anti-bacterial substances which are contained in the normal peritoneal fluid, exert a spherulating or bacteriolytic effect. Here, again, our results were purely negative. We saw nothing which reminded us even in the remotest degree of the striking results which were obtained when cholera bacteria were introduced into the peritoneal cavity in association with the serum which contains the corresponding specific anti-bacterial substances.

Lastly, so far as was possible with the stock of animals at our disposal, we instituted experiments with a view to ascertaining whether the sera exerted any effect in the direction either of curing plague, or of controlling the development of this

disease, in animals which received, simultaneously with an inoculation of serum, an inoculation of living plague bacteria.

508. Three series of experiments were performed by us with a view to obtaining the information in question. The results of these experiments are incorporated in the following tables brought upon our record in the evidence of Lieut. Douglas, I.M.S.

Details of our experiments to determine the therapeutic effect of Yersin's and Lustig's serum on animals.

TABLE A.—EXPERIMENTS made to TEST the PROTECTIVE POWER of YERSIN'S and LUSTIG'S ANTI-PLAGUE SERA.

26,210.

Number of Guinea-pig.	Amount of Plague Culture injected.	Amount and Kind of Serum which was injected.	Date on which foregoing Inoculations were made.	Result.	Remarks.
Number 1 (control) -	$\frac{1}{10}$th agar tube	Nil.	18.2.99	Died 22.2.99	
„ 2 (control) -	„	„	„	„ „	
„ 3 - -	„	$\frac{1}{4}$ c.c. Yersin's serum (Horse 21).	„	„ 23.2.99	
„ 4 - -	„	$\frac{1}{2}$ c.c. Yersin's serum (Horse 21).	„	„ 21.2.99	
„ 5 - -	„	1 c.c. Yersin's serum (Horse 21).	„	„ 1.3.99	Much local re-action, *vide infra Note* 1.
„ 6 - -	„	2 c.c. Yersin's serum (Horse 21).	„	„ 2.3.99	Much local re-action, *vide infra Note* 2.
„ 7 - -	„	$\frac{1}{4}$ c.c. Lustig's serum (Horse No. 3).	„	„ 22.2.99	
„ 8 - -	„	$\frac{1}{2}$ c.c. Lustig's serum (Horse No. 3).	„	„ 25.2.99	
„ 9 - -	„	1 c.c. Lustig's serum (Horse No. 3).	„	„ 22.2.99	
„ 10 - -	„	2 c.c. Lustig's serum (Horse No. 3).	„	„ 20.2.99	

In each case the serum and plague culture were injected within a few minutes of each other into opposite sides of the body. In each case the presence of plague bacilli in the body was verified after the death of the animal.

NOTE 1.—Particulars of *post-mortem* examination of Guinea-pig No. 5. Abscess at seat of inoculation on left side of abdomen, bubo in left axilla. Pleural effusion on left side. Nodules in left lung. In all of these situations plague bacilli were detected on microscopic examination. No generalised infection. No plague bacilli were detected either microscopically or by cultivation in spleen, liver, or blood.

NOTE 2.—Particulars of *post-mortem* examination of Guinea-pig No. 6. Abscess at seat of inoculation on left side of abdomen. Suppurating buboes in both axillæ and both groins. Effusions in both pleural cavities. Lungs filled with hard greyish-yellow nodules. Plague bacilli were found scattered in small clumps through these nodules. Bacilli were recovered by cultivation from the suppurating buboes. Only a single colony was obtained in the cultivation made from the spleen.

TABLE B.—Further EXPERIMENTS made to TEST the PROTECTIVE POWER of YERSIN'S and LUSTIG'S ANTI-PLAGUE SERA.

26,213.

Number of Guinea-pig.	Amount of Plague Culture injected.	Amount and Kind of Serum which was injected.	Date on which foregoing Inoculations were made.	Result.	Remarks.
Number 1 (control) -	$\frac{1}{10}$th agar tube	Nil.	27.2.99	Died 4.3.99	
„ 2 -	„	$\frac{1}{4}$ c.c. Yersin's serum, (Horse No. 31).	„	„ 5.3.99	Marked local reaction.
„ 3 - -	„	$\frac{1}{2}$ c.c. Yersin's serum (Horse No. 31).	„	„ 6.3.99	do.
„ 4 - -	„	1 c.c. Yersin's serum (Horse No. 31).	„	„ 29.2.99	No cause of death detected, no plague bacilli were detected either microscopically or by cultivation.
„ -	„	2 c.c. Yersin's serum (Horse No. 31).	„	„ 8.3.99	Marked local reaction, a few plague bacilli in spleen.
„ 6 -	„	$\frac{1}{2}$ c.c. Lustig's serum (Horse No. 1).	„	„ 3.3.99	
„ 7 - -	„	1 c.c. Lustig's serum (Horse No. 1).	„	„ 3.3.99	
„ 8 - -	„	2 c.c. Lustig's serum (Horse No. 1).	„	„ 3.3.99	
„ 9 - -	„	4 c.c. Lustig's serum (Horse No. 1).	„	„ 3.3 99	

The method of experimentation was the same as in the former series of experiments.
Plague bacilli were detected after death in all the guinea-pigs with the exception of guinea-pig No 4.

TABLE C.—EXPERIMENTS made to DETERMINE the PROTECTIVE POWER of various ANTI-PLAGUE SERA and of various COMBINATIONS of ANTI-PLAGUE SERA.

Number of Guinea-pig.	Nature and Quantity of the therapeutic Serum which was injected.	Dose of Plague Bacilli which was injected.	Date on which the foregoing Injections were made.	Result.	Remarks.
No. 1 (control)	Nil	$\frac{1}{12}$ agar tube	14.3.99	Died 18.3.99	
„ 2 (control)	Nil	„	„	„ 19.3.99	
„ 3	2 c.c. Yersin's serum (horse 21)	„	„	„ 23.3.99	Much local reaction.
„ 4	2 c.c. Lustig's serum	„	„	„ 17.3.99	
„ 5	2 c.c. serum from immunised rabbits.	„	„	„ 19.3.99	
„ 6	1 c.c. Yersin's serum + 1 c.c. Lustig's serum	„	„	„ 21.3.99	Much local reaction.
„ 7	1 c.c. Yersin's serum + 1 c.c. immunised rabbits' serum	„	„	„ 23.3.99	„ „
„ 8	1 c.c. Lustig's serum + 1 c.c. immunised rabbits' serum	„	„	„ 17.3.99	
„ 9	0·5 c.c. Yersin's serum + 0·5 c.c. Lustig's serum + 0·5 c.c. immunised rabbits' serum	„	„	„ 19.3.99	
„ 10	1 c.c. immunised rabbits' serum	„	„	„ 22.3.99	Much local reaction.
„ 11	0·5 c.c. immunised rabbits' serum.	„	„	„ 17.3.99	

In each case the serum and plague bacilli were injected into opposite sides of the body. The serum was in each case injected a few minutes before the plague bacilli. In every case plague bacilli were found after death in pure culture in the internal organs.

Each of the guinea-pigs which were employed for this experiment weighed approximately 350 grammes.

Results of our experiments on animals with Yersin's serum.

509. It will be seen by reference to the above tables that the administration of Yersin's serum did, when administered to guinea-pigs, exert a well-marked effect in the direction of protection. It is true that the injection of as much as 2 c.c. did not in any case suffice to ward off a fatal issue from the animals which were inoculated with plague; but, in practically every case, the administration of serum deferred the time of death, and in practically every case it held septicæmia in check, and it operated in the direction of limiting and localising the plague invasion. The protective effects which were observed in these experiments appear, so far as we are able to judge from information which was gathered by one of us in the course of a conversation with M. Roux, to have been much less marked than those which were observed in a series of experiments which were instituted at the Pasteur Institute, Paris, with the (presumably freshly drawn) sera of the same horses from which the sera supplied to us were derived. If this difference in the result is not imputable to the larger test dose or superior virulence of the cultures which were employed by us, it may conceivably have been due to the absence of the less stable non-specific anti-bacterial substances in the sera employed by us.

Results of our experiments on animals with Lustig's serum.

510. The results of our experiments with Lustig's serum may be briefly summarised as follows:—In the first series of experiments, in which four guinea-pigs—treated respectively with $\frac{1}{4}$, $\frac{1}{2}$, 1, and 2 c.c. of the serum—and two control animals were inoculated with exactly the same quantity of plague culture, the first to succumb was the animal which had received the largest dose of serum. This animal succumbed on the second day. Two days after this the controls succumbed, and along with them the serum animals which had received respectively a $\frac{1}{4}$ and 1 c.c. Only one serum animal survived longer than the control animals. This animal, which had received $\frac{1}{2}$ c.c. of the serum, survived till the seventh day. The serum which was employed in these experiments was the serum of Horse No. 3. The series of experiments with the serum of Horse No. 1 yielded no better results. In this series of experiments four guinea-pigs received respectively $\frac{1}{2}$, 1, 2, and 4 c.c. of Lustig's serum. All these animals succumbed 24 hours before the control guinea-pig. Precisely similar results were obtained in the further series of experiments shown in the last table. Here the administration of Lustig's serum appeared in every case to accelerate death. Finally, a similar result was obtained in an unfinished experiment on rats, the results of which (so far as they were available at the date of our departure from India) are placed on record in the evidence of Lieut. Douglas, I.M.S. Here again two of the animals which received Lustig's serum died before the first control—the rat which received the largest dose (2 c.c.) dying exactly twice as quickly as this control.

It will thus be seen that in three series of experiments with guinea-pigs, and in one (unfinished) series of experiments with rats, the introduction of Lustig's serum into the plague-infected organism hastened death. In particular the animals which

received the larger doses of serum showed much less power of resisting the plague infection than the control guinea-pigs.

511. In view of the disastrous nature of these results, we availed ourselves of the opportunity of Dr. Galeotti's second appearance before us, to inquire from him as to whether he himself had tested this serum on animals. Dr. Galeotti very kindly furnished us with the results of the experiments which he had instituted with the particular sera in question. The details of these experiments will be found incorporated in Dr. Galeotti's evidence. It appears, from the statements which are there furnished, that of three rats which were inoculated by piercing the tail with an infected needle, and which were afterwards treated with the serum from Horse No. 3, two survived, while a control rat, inoculated with the same quantity of plague, and the remaining serum rat, died. Again of three monkeys inoculated with 2 c.c. of plague culture, two, who were treated with the serum from Horse 3, survived, while the untreated control monkey died. Again in the case of the serum derived from Horse No. 1, one rat which was inoculated with a very small quantity of plague, and which was afterwards treated with serum, survived, while a control rat which was inoculated with the same quantity of plague culture died. Lastly, out of two monkeys treated with the serum last mentioned after previous inoculation with plague, one monkey survived, while the other serum monkey and a control monkey which had been injected with similar quantities of plague succumbed.

Results of experiments on animals with Lustig's serum by Dr. Galeotti. 26,011.

To sum up, of a total of eight animals treated by Dr. Galeotti with the sera of Horses Nos. 3 and 1 respectively, six survived, corresponding to a mortality of only 5 per cent., while of four control animals all died, corresponding to a case-mortality of 100 per cent.

512. We are quite unable to suggest any satisfactory explanation of the striking differences between Dr. Galeotti's results and our own. Attention may, however, be drawn to the following points. With respect to the serum derived from Horse No. 3, it is to be noted that the serum which was furnished to us was drawn off on the 7th of February 1899. Our experiments with this serum were undertaken 11 days after, that is, on the 18th of February 1899. Dr. Galeotti's experiments, if, as we surmise, they were instituted with this particular serum and not with the serum drawn off from the same horse on the 5th of February, were, on the contrary, undertaken on the very day on which the serum was drawn off. Similarly in the case of the serum from Horse No. 1. The serum furnished to us was drawn off on 17th of February 1899. Our experiments with this serum were undertaken on the 27th of February. Dr. Galeotti's experiments with the serum were undertaken a week previously, on the 20th of February. It will be seen that our experiments were conducted with sera which had been standing for some 10 days, whereas those of Dr. Galeotti were conducted with the freshly drawn sera. In view of this, it is possible that the discordant results obtained may be explicable (we would note here that the same explanation has been put forward by Dr. Cobbett to account for the discordant results which have been obtained by different observers with one and the same variety of anti-streptococcus serum examined at different times) by the assumption that the sera in question contained unneutralised poisons which were in the case of the earlier experiments masked by the presence of non-specific anti-bacterial substances. In accordance with this suggestion, the earlier and more favourable results would be explained as the result of the incorporation of the unstable anti-bacterial substances present in fresh sera, while the unfavourable results obtained in the later experiments would be explained as the result of the incorporation of an unneutralised residuum of the bacterial poison employed for the injection of the horses.

Differences in results of our experiments and Dr. Galeotti's and possible explanation.

"Lancet," 9th April 1898.

Whatever view we take of the very favourable results obtained by Dr. Galeotti in his animal experiments, it seems to us clear that the evil effects which were observed to follow in our own experiments with Lustig's serum were—as in the case of the "anti-pneumous," "anti-staphylococcus," and "anti-tuberculin," and "anti-enteriditis (Klein)," sera referred to above—in all probability due to the presence of unneutralised poisons in the serum.

Para. 487 of this Report.

513. We instituted two series of experiments with a view to determining whether the serum furnished to us contained toxic substances in quantity sufficient to produce a fatal result in normal guinea pigs. The results of the experiments are set forth in the following tables.

Experiments made to determine whether toxic substances were contained in the sera furnished to us.

TABLE A.—COMPARISON of the TOXICITY of LUSTIG'S SERUM with the TOXICITY of NORMAL HORSE SERUM.

26,277.

Number of Guinea-pig.	Weight of Guinea-pig.	Amount and Kind of Serum which was injected.	Relation of foregoing Amount to Body Weight of Animal.	Result.
Number 1	230 Grammes	Lustig's serum 11·5 c.c.	5 per cent.	No toxic symptoms observed.
„ 2	220 „	Normal horse serum 11 c.c.	5 per cent.	Do.
„ 3	260 „	Lustig's serum 19·5 c.c.	7·5 per cent.	Do.
„ 4	260 „	Normal horse serum 19·5 c.c.	7·5 per cent.	Do.

TABLE B.—RELATIVE TOXICITY of NORMAL HORSE SERUM, of YERSIN'S ANTI-PLAGUE SERUM, and of LUSTIG'S ANTI-PLAGUE SERUM.

26,433.

Number of Guinea-pig.	Amount and Kind of Serum injected.	Result.
Number 1	Normal horse serum, 20 c.c.	Survived.
„ 2	Normal horse serum, 30 c.c.	do.
„ 3	Yersin's anti-plague serum, 20 c.c.	do.
„ 4	Yersin's anti-plague serum, 30 c.c.	do.
„ 5	Lustig's anti-plague serum, 20 c.c.	do.
„ 6	Lustig's anti-plague serum, 30 c.c.	do.

Nothing in the general condition of these animals suggested that there was any difference in toxicity between these various sera.

It will be seen that in neither series of experiments was any evidence obtained of the presence of unneutralised poisons in Lustig's serum. Normal animals injected with as much as 30 c.c. (one-tenth of their body weight) of the serum remained to all appearance unaffected. This, however, is not inconsistent with the assumption that the serum may have contained small quantities of unneutralised plague poisons, such as would make themselves felt only when superadded to similar poisons elaborated in the organism of a plague-infected animal or man. Phenomena quite analogous to these have been observed by one of us (the President) in connexion with anti-venomous serum, and also by Ehrlich in connexion with milk drawn off from a goat which had a few days previously received an injection of tetanus toxin. Perhaps, however, the acceleration of death obtained in the animals treated with Lustig's serum would be most simply accounted for by the assumption that the serum contained anti-bactericidal substances similar to those recently described by one of us (Wright) in connexion with an anti-typhoid serum examined by him. It would seem possible that Lustig's serum may have exerted an anti-bactericidal effect of this kind upon the guinea-pigs experimented upon.

Zeitschrift für Hygiene, 1893.

"Lancet," 1st Dec. 1900 and 2nd March 1901.

Inferences as to propriety of applying to the treatment of plague in men the samples of Yersin's and Lustig's serum examined by us.

514. In view of the fact that the results of the experiments made with the samples of Yersin's serum which were furnished to us showed that the course of plague in animals was always modified in a favourable manner by the introduction of the serum, there could obviously be no hesitation as to the propriety of determining the effects of the therapeutic administration of this serum in the case of plague patients. At the same time, seeing that the influence which was exerted on the course of plague in animals was in no case sufficient, even when large doses were employed, to ward off a fatal issue, there was obviously nothing to warrant a very sanguine expectation as to the effect which would be realised on men.

The case of the samples of Lustig's serum at our disposal was different. Here, in view of the general tenour of the results which were obtained by us in the course of the experiments mentioned above, and in view of the deplorable effect which was exercised by the inoculation of this serum into plague-infected animals, we did not consider ourselves justified in testing the effects which would be produced by the inoculation of the serum on men. We shall, therefore, so far as Lustig's serum is concerned, confine ourselves to reviewing the results which have been obtained by others by the therapeutic application of Lustig's serum in the Bombay hospitals.

IV.—DISCUSSION OF THE METHODS APPROPRIATE FOR THE STATISTICAL ESTIMATION OF THE VALUE OF ANTI-PLAGUE SERA IN THE TREATMENT OF PLAGUE.

Preliminary considerations.

515. Our study of the effects of the therapeutic administration of Yersin's and Lustig's sera may with advantage be prefaced by setting forth certain general principles which must be kept in view in the course of our consideration of these questions.

In the first place, we must keep in view that sera issued for therapeutic use must not be regarded as substances which possess any constant composition. Not only is the proportion of the anti-toxic and anti-bacterial constituents different in different samples of serum, but the serum, as we have seen, may in exceptional cases contain toxic or "anti-bactericidal" elements. This being so, it would be impossible to form a judgment on the question of the utility of an anti-plague serum, which has been prepared by a particular process, by a study which should confine itself to the determination of the value of any one particular sample of that serum. We must, consequently, with a view to arriving at a practically useful estimate of the value of a serum as a therapeutic agent, take a general survey of the results obtained with the different samples of that serum which have been brought into application.

Sera not substances of constant composition and therefore not to be judged of by effects of any particular sample only.

We propose to do this in the case of Yersin's serum and of Lustig's serum, limiting ourselves, however, to the consideration of the samples of these sera which have been employed in India. In connexion with Yersin's serum, we have included within our purview, as already indicated in an earlier part of this Chapter, the samples of serum prepared at the Pasteur Institute, Paris, at the Imperial Institute of Preventive Medicine at St. Petersburg, and, finally, by Mr. Haffkine in Bombay. In like manner, when considering the effect of the administration of Lustig's anti-plague serum, we have studied the effects, both of the samples of that serum which were prepared in Italy, and of those which were prepared at the Municipal Laboratory, Bombay, by Dr. Galeotti, Professor Lustig's representative, and, after Dr. Galeotti's return to Italy, by Dr. A. Mayr.

Present inquiry limited to consideration of sera used in India.

516. Having defined the scope of the present inquiry, we may turn our attention to certain points in connexion with the logical methods which must be followed in adjudicating upon the utility or otherwise of a particular serum. It would be unjustifiable to base a judgment merely on the fact of a favourable or, as the case may be, an unfavourable impression being left on the mind of the clinical observer who is employing a particular serum. A judgment must in all cases be based chiefly upon a consideration of the statistical results obtained in a series of plague cases treated with the serum as compared with the results obtained in a series of cases to which the serum has not been administered.

Value of serum must be determined by comparison of statistical results in treated and untreated or control cases.

517. The statistical method adopted by us in setting forth the results obtained by the serum treatment is similar to that already employed in dealing with the results of Haffkine's anti-plague inoculation; in other words, we have set forth the case mortality in patients treated with serum and those not so treated, and the difference between these two. This method, from the point of view of the objective setting forth of the facts, seems to us to present advantages over all other methods. In particular, it seems to present advantages over the method of exhibiting the results in the form of a ratio, with 100 as a basis, between the deaths which actually occurred in the control group and the deaths which were averted in the serum group. This last method is, as it seems to us, liable to convey a false estimate of the actual saving of life achieved by a therapeutic, or, as we have already pointed out, a prophylactic agent. It may, indeed, be urged against the method adopted by us, that it is inconvenient, in so far as it does not show at a glance the degree in which the particular serum employed approaches the ideal standard of a perfectly efficacious serum. Inasmuch, however, as this can be readily ascertained by a calculation from the data supplied in our tables, the inconvenience referred to is, we think, entirely outweighed by the advantages of the method we have adopted.

The form in which statistical results are set forth. Sects. IV and V. in Chap. IV. of this Report.

Para. 454 of this Report.

518. Manifestly the soundness of comparisons between plague cases treated and not treated with serum will hinge entirely upon appropriate means having been taken to secure the selection of really comparable control cases.

Principles on which serum cases and controls should be selected

It is, in the first place, necessary, in order to secure a just comparison that the controls and the serum cases shall have been, so far as possible, comparable from the point of view of their powers of resistance, and that they shall all have been treated in similar surroundings and, except as regards the administration or non-administration of serum, under similar conditions.

This, as consideration will show, will generally involve their having all been admitted into the same hospital; for in India, where special race and caste hospitals have been established, the patients admitted into different hospitals may, as we have already seen, possess very different powers of resistance. Again, the conditions in the

Serum cases and controls to be treated in same hospital.

Para. 300 of this Report.

different hospitals may vary, especially in the matter of the exhibition of stimulants, in the arrangements for feeding the patients, and in the adoption of European or Native methods of treatment.

Serum cases and controls to come under treatment at same period of the epidemic. Chap. III., section IX. (c) of this Report.

Equally necessary is it, in view of the facts which have been considered in discussing the variation in the fatality of plague in the different phases of an epidemic, to insist that the patients who serve as control cases shall be patients admitted to hospital in the same phase of the local outbreak as the serum cases.

Necessity for avoiding all selection of serum and control cases which involve possibility of results being influenced by the personal factor of the clinician.

19,041.

The next consideration of importance relates to the manner in which the serum and control cases are chosen from among the patients. In many cases a system of selection has been adopted, by which the clinician has selected patients for serum treatment from among the patients admitted to hospital with a view to applying the treatment only to those whose cases seemed to present certain elements for hope. In some cases, as will be seen, the results obtained in the case of these selected patients have been compared with the results obtained by ordinary treatment upon the residuum of plague cases. As an indication of the extreme to which this process of selection of patients for serum treatment has been carried, we may refer to the evidence of a witness who told us that he had known a series of three cases of plague to recover in the Hindu Fever Hospital at Bombay, after they had been excluded from serum treatment on the ground that they were already moribund. While we are far from suggesting that the process of selection of suitable cases for serum treatment has often been carried to anything like this length, it will be obvious that no logically cogent judgment can be arrived at in any case where the results obtained on a body of selected serum cases is compared with the results obtained on a body of controls consisting of residuum cases. And this will hold true, even when it is claimed that "moribund patients" have been excluded from the serum and the control group of cases alike. For it is obvious that the clinician may, without conscious bias, be making use of different standards in judging of the hopelessness of the cases, according as these are to be excluded from the serum group or from the control group. Even if this were not so, the complete exclusion of the personal factor in the selection of the serum and control cases respectively is eminently desirable from the point of view of allowing the statistics of all the various observers to be summarised in a single expression.

Exact conformity between individuals in serum and control groups need not be exacted in a sufficiently long series of observations.

20,952.

Having seen the necessity for the exclusion of the disturbing influence introduced by the personal factor of the clinician, the next question that arises is whether it is necessary to demand that the plague cases which are compared together should conform with each other in the matter of their possessing in common certain objective clinical characters. Dr. Choksy, in particular, has urged that the controls should correspond with the serum cases "as regards age, sex, race, duration of disease, position of buboes, general condition, and the state of the circulation," inasmuch as these circumstances exert an important influence on the event. Waiving the question as to whether there is not room for divergence of clinical opinion as to the correspondence of different patients with regard to certain of the points specified, we think that the conformity in question cannot, under the circumstances under which sera are applied, be secured. Further, we think that, however essential it may be in the case of a short series of observations to exact that each individual control case should correspond in the respects referred to above with the individual serum cases, this point need not be specially attended to in the case of a sufficiently long series of observations. For, in every longer series of observations the accident of chance will, if the cases are referred to the serum and control groups respectively in accordance with any arbitrary rule, spontaneously bring about the desired conformity between the serum and the control cases.

Advantages secured by the employment of the "alternate system" of selecting serum and control cases.

519. The arbitrary rule which immediately suggests itself as appropriate under the circumstances of hospital treatment is obviously that the patients admitted to hospital should be referred in the order of their admission alternately to the serum group and to the control group. The advantages which are bound up with the adoption of this rule are manifestly the following:—firstly, the clinician being relieved from all responsibility for the selection of cases, this important disturbing personal factor can be eliminated from consideration; secondly, the patients necessarily come under observation in the same hospital and in the same phase of the epidemic.

Objections urged by Dr. Choksy to the "alternate system." Bombay Medical

Dr. Choksy, who has, as we shall see, conducted a series of observations on this which he conveniently calls the "alternate system" on a series of 968 plague patients admitted to the Arthur Road Hospital in Bombay, has, in a recent publication in which he details the results of the comparison in question, urged that serious fallacies

are involved in this method considered as a method for eliciting the value of the therapeutic administration of a serum. The objections of Dr. Choksy, affecting as they do the question of the principle of selection of controls now under discussion, may appropriately be considered here. and Physical Society, September 1900.

520. Dr. Choksy contends that even in so extensive a series of observations as that undertaken by him, where he dealt with two groups, each comprising 484 patients, the severe and the light cases which fell to the serum group and the control group respectively, did not, in point of fact, counterbalance each other. He points out that while the convalescents included in his group of 484 serum cases numbered only 29, the convalescents included in his 484 control cases amounted to 38. It will be noted that this, which is a difference of 9, works out, when expressed in a percentage on the number of patients in each group, as a difference of 2 per cent. Again, in the matter of the number of moribund patients included in each group, Dr. Choksy points out that the moribund patients included within the serum group numbered 139, as contrasted with 147 moribund patients included within the control group. This difference of 8 works out as a percentage difference of a little less than 2 per cent. Objection that in practice severity of cases in both groups does not equalise even in long series.

Dr. Choksy has, it would seem, failed to observe that these differences, which have arrested his attention, mutually counterbalance each other, and that the 9 additional convalescents who fell to the control group were compensated for by the 8 additional moribund patients who fell to this group. Thus, in the case of Dr. Choksy's series of 968 cases, a practical equality is, as was *à priori* anticipated, established between the serum group and the control group. This objection fails.

521. The second objection urged by Dr. Choksy is, in our opinion, both a subtle and an important one. He points out that under the "alternate system" deaths referable to patients admitted to hospital in a moribund condition are debited to both the serum group and the control group, and further, that recoveries referable to patients admitted in a convalescent condition are in a similar manner credited to both groups, and he urges that as a result of this the fact is obscured that a much larger balance inures to the credit of the serum group than inures to the credit of the control group on account of recoveries of patients admitted to hospital "in the acute stage of the disease and in a non-moribund condition," that is, of patients who when admitted are still capable of being benefited by treatment with serum. Objection that results in favour of serum are watered down by inclusion of moribund and convalescent cases.

The gravamen of Dr. Choksy's argument will appear more clearly when the figures (taking Dr. Choksy's figures for the 968 patients to whom he refers in his publication of September 1900, mentioned above) are placed before us in the following form :—

TABLE I.

Figures as they stand in the Hospital Records, Moribund and Convalescent Patients being included in both the Serum and the Control Groups.

—	Cases.	Deaths.	Recoveries.	Case-Mortality.
Control Group - - -	484	385	99	79·5 per cent.
Serum Group - - -	484	329	155	68 per cent.
Difference in favour of the serum group - -				11·5 per cent.

If, however, the number of deaths and recoveries referable to convalescent and moribund patients respectively were subtracted from both the serum and the control groups, the figures would read as follows :—

TABLE II.

*Figures as they would stand if amended in accordance with Dr. Choksy's suggestion, by subtracting from each group the Convalescent and Moribund Patients * which fall to each group.*

—	Cases.	Deaths.	Recoveries.	Case-Mortality.
Control Group - - -	299	238	61	80 per cent.
Serum Group - - -	316	190	126	60 per cent.
Difference in favour of the serum group - -				20 per cent.

* The numbers of these have been given in para. 520 supra.

It will be seen that the difference between the serum cases and the control cases which amounts, when all the patients admitted to hospital are taken into consideration to 11·5 per cent., comes out as 20 per cent. as soon as the convalescent and moribund patients are left out of consideration.

This objection sound in principle.

We are thus brought face to face with the fact that the full measure of the benefit that may accrue from a serum cannot be ascertained unless when there have been eliminated from consideration all patients who are admitted to hospital in a condition from which recovery is impossible, and all patients admitted at a stage when there is no longer any risk of a fatal issue.

Reasons why it is impracticable and inadvisable to apply the principle of excluding moribund patients in statistical investigations.

522. It is, however, one thing to recognise that it would be desirable to eliminate these classes of patients from consideration and quite another to arrive at the opinion that it is desirable to attempt to do so. As a matter of fact, the adoption of the system of eliminating unsuitable cases would involve the introduction of important sources of error into the statistics. It would manifestly be impossible by any system of examination to eliminate on admission all moribund patients, without, at the same time, eliminating all the more serious cases; in other words the elimination of the moribund patients would inevitably lead to the testing the serum only upon the less grave cases. Again, it would not be legitimate to eliminate from the records, as patients admitted in a moribund condition, all such patients as might happen to die within twenty-four hours or any other arbitrary period after admission, for this might, in the case of patients who died after the administration of serum, result in improperly eliminating from the record a certain proportion of the unsuccessful cases. Moreover, in the conceivable case of the therapeutic administration of a serum being followed by evil effects, the fact that these evil effects had followed would be obscured.

A further point relates to the question as to whether the conclusion, which is to be arrived at by the statistical investigation of the effect of the administration of the serum, shall have reference to the effect of the serum when administered under ideally favourable conditions, or to the effect of the serum when administered under the conditions under which plague patients are actually admitted to hospital in India. It seems to us that the information which a statistical investigation like the present is designed to elicit is information with regard to the effect of the serum when administered under the actual conditions of hospital practice in India. We are influenced in arriving at this opinion by the consideration that it is not possible to secure the admission of patients into hospital at an earlier a stage than that at which they are now brought in. This being so, the elimination from the statistics of the patients who die within an arbitrary period after admission would lead to an altogether wrong estimate being formed of the benefit derivable from serum-therapathy in actual hospital practice.

We may illustrate what we have just said by considering the parallel case of the therapeutic application of anti-tetanic serum to the treatment of tetanus. In the case of this particular serum, it is quite beyond doubt that we are dealing with an agent of extraordinary therapeutic potency. There are, in point of fact, very few, if any, therapeutic agents which are, from the point of view of their potency, comparable to anti-tetanic serum. Yet when an anti-tetanic serum is administered to a patient, in whom the symptoms of tetanus have already developed, instead of being administered to him immediately after the tetanic poison has been incorporated, the results which are obtained are notoriously disappointing. Now, manifestly, from the point of view of practical life, the value of the serum must be appraised, not by the effect that it is capable of exerting in ideally favourable conditions, but by the effect which it is capable of exerting under the conditions when the patients came under treatment. It seems to us that the value of serum-therapathy in plague ought to be appraised on exactly the same principle.

For the reasons given above, we feel ourselves constrained to demur to the exclusion from consideration of patients admitted in a moribund condition unless the denotation of the term "moribund" is restricted to patients admitted in a pulseless condition, or actually in articulo mortis.

The criterion of efficacy of a serum of prime importance is the larger number of survivors among the serum as contrasted with control cases.

523. Our attention must next be directed to the question of the criteria by which the effect of a therapeutic serum is to be judged. It would, at first sight, seem that the only possible criterion of the efficacy of an anti-plague serum would be the survival of a greater number of plague patients among those treated with the serum than among the untreated control cases. Reflection, however, shows that the serum and control cases might be compared also in other respects.

In the first place, a comparison might be instituted between the serum and the control ases, with a view to determining whether death was postponed, and whether recovery vas hastened, in the serum cases as compared with the control cases.

Subsidiary criteria are :—

Postponement of death or shorter convalescence;

Another means by which the efficacy of a serum could be judged, would be y comparing serum cases with control cases, with a view to determining whether any articular symptom or group of symptoms was alleviated by the administration of the erum.

Amelioration of symptoms;

Again, important indications with regard to the efficacy of a serum can be gleaned y comparing the case-mortality of patients admitted and treated with the serum in he earlier stages of their illness with the case-mortality of untreated patients admitted t the same stages of their illness. Even if the aggregate of serum cases did not show ny greatly reduced death rate in comparison with the untreated cases, it would be ustifiable to expect that patients admitted to hospital early in their attack and nmediately treated with serum would, if the serum was effective, compare in a istinctly favourable manner from the point of view of their death rate with control ases admitted in the same stage of the attack, and would, at any rate, compare more avourably with their controls than would cases treated with serum in a later stage f their illness.

Comparison of case-mortality of patients treated early and late with controls in similar stages of the disease;

Lastly, indications might be obtained with regard to the efficacy of a serum by omparing a group of serum cases among themselves, with a view to determining hether there was any difference between the case-mortality of the patients who ceived the serum in the early stages of their attack, as compared with the case-ortalty of the patients who received the serum after they had reached a more advanced age of the disease. In contradistinction to the view which recommends itself to the erman Plague Commission, Captain Childe, I.M.S., Dr. Simond, and other observers, e regard indications which can be gathered from comparisons of this nature as of o great value.

Comparison of serum cases among themselves according to their treatment, early or late in the disease.

III. 462(6). 27,045.

524. In justification of this position we would suggest that the view that an effective nti-plague serum must necessarily be more effective when administered to patients dmitted to hospital shortly after they have fallen sick, than when administered to atients admitted to hospital in the later stages of the disease, would appear to be ased upon analogy with the different results obtained, in the case of animals which ave been injected with toxins, by the early or late administration of the corresponding nti-toxic sera. We may point out certain essential points of difference between the onditions of such experiments and the conditions which we have to deal with in the ase of patients suffering from plague.

Discussion of the value of the criterion last stated.

In the case of a group of animals experimented upon with a view to determining the herapeutic efficacy of an anti-toxic serum, each animal receives a dose corresponding every case to a lethal dose, or multiple of lethal dose, of the toxin in question. Death survival of the individual animals in such a series of experiments will—given that he dose of serum contains a sufficiency of anti-toxin to neutralise the poison which has een incorporated—hinge entirely upon the introduction of the anti-toxin sufficiently on after the incorporation of the toxin to allow of its neutralising this last before he tissues which are essential to life have been poisoned.

The conditions are entirely different in the case of plague patients admitted to ospital in the earlier or later stages of the disease respectively. The conditions are ot here such that all the patients if left untreated will succumb, because it is not rtain in the case of all these patients that a lethal dose of plague poisons has been or ill be elaborated in their systems by the plague bacteria which have invaded them. n the contrary, the favourable or non-favourable issue will in each case hinge upon the uestion as to whether the bacteria succeed in or are prevented from propagating them-lves in the organism up to the point at which they shall have elaborated poisons sufficient quantity to bring about a fatal issue. In conformity with this, there ill be very considerable differences between the cases of individual patients who are, om the chronological standpoint, in the same stage of the disease. Some will have en infected with smaller, others will have been infected with larger, quantities the plague bacillus; again, there will be some who have been infected with a more rulent, others with a less virulent, bacillus; lastly, some will have succeeded better an others in inhibiting the growth of the plague bacilli which have effected an entrance to their organism. If, however, we take the average of patients admitted on particular

days of the disease, and compare among themselves the groups admitted on successive days, we arrive at the following inferences. The patients admitted in the first day of the disease may be assumed to conform to the average, both from the point of view of the general severity of the infection and also from the point of view of resisting power. The mortality among this group of patients would thus accurately agree with the average mortality from plague at the particular period of the epidemic in which they come under observation. Passing now to the patients admitted in the second day of the disease, these patients—we may for present purposes omit from consideration the patients who are admitted in a manifestly moribund condition—represent a group of patients from among whom have been removed by death a certain number of the most severely infected and of those possessing the least resisting power. Second day patients, taking them on the average, thus represent those who have been less dangerously infected, or who possess by nature a higher resistance, than the average of patients in the first day of the disease. The same will hold true of each successive group of patients, as compared with other groups of patients admitted at an earlier stage of the disease; for from each of the successive groups a larger progressive total of less resistant or more seriously infected patients will have been eliminated by death. In other words, just as in the case of the individual plague patient the prognosis becomes more favourable with the lapse of each successive day, so the prognosis is always more favourable in the case of a group of patients admitted at a later stage of the disease as compared with a group of patients admitted at an earlier stage. Dr. Simond's experience is—though he fails to see the bearing of this on the general question of the value of the criterion now under discussion—in consonance with these considerations. Speaking of the opinion that "fresh cases," viz., cases admitted to hospital in the earlier stages of the disease, "have more chances of natural recovery," he says: "This is a great mistake, and it is only necessary to consult the statistics of the hospital to see that the greatest mortality always occurs in patients admitted in the first three days. Experience has proved to me that if the serum is injected in those admitted to hospital after the period has passed, an excellent proportion of success will be obtained, but one cannot with certainty attribute it to the treatment."

II. 459.

Enough has now been said on this subject to make it plain that if a serum gives better results on patients admitted to hospital and treated when they have arrived at the later stages of the disease than on patients admitted and treated early, this will not constitute disproof of the efficacy of the serum; on the contrary, the serum may be expected to yield more satisfactory results on the residuum of patients who have arrived at the later stages of the disease whose case-mortality is naturally light, than on the plague patients admitted on the first day of the disease, whose case-mortality is naturally heavy, accurately representing as it does the actual case-mortality of plague.

Fallacies incident to criteria involving comparisons between cases treated early and late.

525. If the prognosis of cases admitted to hospital in the later stages of the disease were uniformly more favourable than that of cases admitted in the earlier stages of the disease, comparisons between early and late cases would, at any rate, always show an advantage in favour of the late cases. Other factors, however, come into consideration.

One of the most important of these is the fact that many patients are admitted to hospital in an actually moribund condition, and it would seem to depend largely on chance whether such patients would fall into the category of patients admitted to hospital on the first, second, third, fourth, fifth, or subsequent days of the disease. Since, however, death in plague takes place more often on the fourth or fifth day than on the first three days, it may be assumed that in normal circumstances the percentage of patients admitted in an actually moribund condition will be higher in the case of patients admitted on the fourth and fifth days than in the case of those admitted on the earlier days. The result of this will be that where patients admitted in a moribund condition are, equally with those whose cases present elements of hope, injected with serum, a factor will be introduced into the statistics which will operate in the direction of increasing the case-mortality of those treated later, as compared with those treated early with serum. It will be obvious that statistical proof or disproof of the superior efficacy of a serum when administered early may, when this source of error has not been specially guarded against, be quite fallacious. This fallacy will be the more serious where, as in many series of experiments, the cases treated in

the later stages of the disease are relatively fewer and more severe as compared with the cases treated with serum in the early stages.

Finally—and this, regarded as a source of fallacy, probably exceeds in importance all the others which insinuate themselves into a comparison instituted between cases treated early and cases treated late with serum—it is in India very difficult to obtain accurate information with regard to the date of the onset of the attack. It is generally absolutely impossible in the case of patients admitted in a moribund condition. Further, in many cases—and this, as we shall see, is said to have occurred in every case in the Seth Vishandas Hospital, Karachi—the native statements, which, with regard to the lapse of time, are in all cases very untrustworthy, were designedly falsified with a view to concealing the fact that their cases had not been reported to the authorities.

Para. 543 of this Report.

For all the reasons we have given above, while we shall, in the course of our examination of the serum statistics, take occasion to set forth the results arrived at by comparisons of the case-mortality of patients treated with serum in an early stage of the disease with the case-mortality of patients treated with serum in the later stages of the disease, we shall not rely to any extent upon these results in forming our judgment on the efficacy or otherwise of the sera which we shall have under consideration.

V.—Results obtained by the Therapeutic Administration of Yersin's Serum to Human Plague Patients.

V.(A.) *Results obtained in the Observations made by Ourselves.*

526. We pass now from this to consider the results obtained by the inoculation of Yersin's serum into patients suffering from plague. We may conveniently deal first with our own observations and then pass to consider those made by other observers.

Account of our observations in Bangalore with Yersin's serum.

The serum which was employed by us was, as has already been stated, very generously placed at our disposal by the authorities of the Pasteur Institute in Paris. After consultation we decided that the serum could be most advantageously employed in Bangalore, where a serious epidemic of plague was raging at the time of our visit in December 1898. With a view to avoiding the fallacies associated with the employment of any system of selection, we decided that the treatment should be applied without distinction to every plague patient not obviously convalescent who should be admitted to hospital. We selected the South Camp Hospital, one of the two larger Plague Hospitals which had been established in the Civil and Military Station of Bangalore, as the hospital which should furnish the patients for serum treatment. The North Camp Hospital, which received a similar class of patients, was selected to furnish the controls. In accordance with the above programme, the serum treatment of the patients admitted to the South Camp Hospital was undertaken by Lieutenant Stewart Douglas, I.M.S., who was specially attached to us for this purpose. The first case was treated with serum on the 25th of December 1898, each subsequent patient being treated soon after admission, provided that there was no doubt as to the diagnosis of plague, and provided further that the patient was not already convalescent, or absolutely in articulo mortis. The epidemic coming to an almost abrupt termination in January 1899, only three plague cases were admitted and treated with serum in the South Camp Hospital after the 12th of January. For the same reason there were no further admissions subsequent to this date into the North Camp Hospital.

Including the three patients referred to as admitted after the 12th January, 49 plague patients in all were treated with serum. Of these, 31 succumbed to the disease, giving a case-mortality corresponding to 63 per cent. The details of the cases and the particulars as to the amount of serum employed are printed in Appendix LXXIX. of our Proceedings and an abstract of the cases is subjoined below in tabular form. It will suffice here to remark that the serum was generally administered once a day, and the daily quantity administered in the first 28 cases varied between 10 and 20 c.c.; in the 21 remaining cases, 40 c.c. were administered on admission to hospital, and generally 20 c.c. on each of the subsequent days. With regard to the clinical type of plague treated, the cases were, with eight exceptions, well marked bubonic cases. In a considerable proportion of cases lung symptoms, in the form of bronchitis and œdema of the lungs, occurred during the progress of the disease, or were present when the patient was admitted.

III. 593–619. Para. 531 of this Report.

Statistics of gross case-mortality in Bangalore Hospitals during period of serum treatment.

527. For the purpose of comparing the gross case-mortality of the South Camp Hospital with that obtaining in the corresponding period in the North Camp Hospital, we must add to the serum cases treated up to the 12th of January the number of moribund and convalescent plague patients admitted into the South Camp Hospital and the other patients admitted in whose case the diagnosis of plague was doubtful. After making the necessary additions which come under these headings, the comparison between the Hospital where the serum was employed and the Control Hospital, where presumably all the classes of patients just specified were represented, would read as follows:—

III. 620.

Hospital.	Cases.	Deaths.	Case-Mortality.
North Camp Hospital, where the patients served as controls.	54	29	53·7 per cent.
South Camp Hospital, where the large majority of patients were treated with serum.	73	35	47·9 per cent.
Difference in favour of the hospital where the serum was employed			5·8 per cent.

Conclusions from statistics of gross case-mortality.

It will be seen there was a small balance, but only a very small balance, to the advantage of the hospital where the serum treatment was applied. No conclusion as to the efficacy of the serum treatment can, however, be drawn from this fact, for a difference in three or four fatal cases admitted into the North Camp Hospital during the period in question would have sufficed to turn the scale to the advantage of this hospital. Again, in the previous history of the two hospitals the North Camp Hospital had always shown the higher case-mortality. We find that the previous record of the North Camp Hospital reads 365 cases with a case-mortality of 75 per cent., while the record of the South Camp Hospital reads 686 cases with a case-mortality of 58 per cent. There was thus in the period previous to the period under consideration a balance of nearly 17 per cent. in favour of the South Camp Hospital, as compared with a balance of only 6 per cent. during the time that the serum was employed.

III. 620.

Effect of serum treatment in Bangalore according to date of disease on which treatment was initiated.

It was not observed that any of the symptoms were markedly alleviated by the serum treatment, or that the life of patients was prolonged. The serum treatment did not give any better results in the cases where it was inaugurated early than in the cases where it was inaugurated in the later stages of the disease. Taking the patients in the order corresponding to the stage of the disease at which they had arrived when they first received the serum, and excluding two patients in whom the duration of the disease was unknown, the record of mortality runs as follows:—

Days of Disease.	1st.	2nd.	3rd.	4th.	5th.	6th.	7th.	9th.
Cases	2	8	11	7	7	9	2	1
Deaths	2	6	7	2	4	6	1	1
Cases-mortality	1 in 1	1 in 1·3	1 in 1·6	1 in 3·5	1 in 1·7	1 in 1·5	1 in 2	1 in 1

It does not appear from these results that there was any important difference in the case-mortality of patients treated on the different days; if anything, the mortality was slightly greater in those who were treated earlier with serum, except in the case of patients treated on the fourth day of the disease. The number of cases, however, is not sufficiently large to form the basis of any conclusion.

Observations carried out in the Modi Khana Hospital, Bombay.

528. In view of the dying out of the epidemic in Bangalore, the theatre of operations was transferred to Bombay, and the observations were continued on patients admitted to the Modi Khana Hospital, which was situated conveniently near the Laboratory in the Municipal Buildings which was occupied by us. It was very kindly arranged by Dr. Turkhad, Medical Officer in charge of the Hospital, that every second patient who was admitted to hospital should be placed at our disposal for the serum treatment, the intermediate patients serving as controls. The supply of serum which was still at our disposal sufficed for the treatment of 28 cases in the Modi Khana Hospital

The actual treatment and recording of the cases was undertaken by Captain Walton and Lieutenant Stewart Douglas, I.M.S., two of us (Dr. Wright and Dr. Ruffer) watching the results. The amount of serum injected was, as a rule, 40 c.c. on admission, often with a second dose of 20 c.c. on the same day, two doses of 20 c.c. each generally being injected on each subsequent day of treatment. The details of the cases are set forth in Appendices LXXX. and LXXXI. of our Proceedings. The general results were as follows:— 26,203–7. Effect of serum treatment in the Modi Khana Hospital on case-mortality. III. 621–633, 634–43.

	Cases.	Deaths.	Case-Mortality.
Control Group	28	24	85 per cent.
Serum Group	28	23	81 per cent.
Difference in favour of the serum group			4 per cent.

It will be manifest that the advantage in favour of the serum cases was quite insignificant.

The cases submitted to treatment were closely comparable to those which served as controls. All the cases were bubonic cases, no cases of primary septicæmic plague or of primary pneumonic plague having been admitted to hospital during the period of observation. The patients were all males between the ages of ten and sixty. The patients in the early and late stages of the disease were represented in approximately equal proportions in both groups. This will appear from the following table:— Comparability of serum cases and control cases in the Modi Khana Hospital.

	3rd Day.	4th Day.	5th Day.	6th Day.	7th Day.	8th Day.	9th Day.	Undetermined.
Control Group	10	9	2	1	2	1	1	2
Serum Group	11	8	5	0	2	0	1	1

The fact that the control cases were so closely comparable with the serum cases from the point of view of the stage of the disease at which they had arrived, affords a favourable opportunity for comparing the course of the disease in patients admitted and brought under the serum treatment in the early and late stages of the disease, with the course of the disease in the untreated patients who were admitted in the same stages of the disease. The result of this comparison is shown in the following table:— Effect of serum treatment in the Modi Khana Hospital according to stage of disease at which treatment was initiated.

	3rd Day.	4th Day.	5th Day.	6th Day.	7th and later Days.
	Case-Mortality.	Case-Mortality.	Case-Mortality.	Case-Mortality.	Case-Mortality.
Control Group	1 in 1·1	1 in 1·3	1 in 1·2	—	1 in 3
Serum Group	1 in 1·1	1 in 1·3	1 in 1	1 in 1	1 in 1·3

It is clear that the serum cases admitted and treated early with serum do not show any diminished case-mortality as compared with the untreated cases admitted in the same stage. Nor, again—but we have above already pointed out that no certain inference can be drawn from such comparisons—does the comparison of the mortality of cases treated early with serum, as compared with the mortality of cases treated with serum in the later stages of the disease, show that the treatment was more efficacious in the former class of cases.

529. In connexion with the question of the comparative efficacy of larger and smaller quantities of serum, we may mention that, while the first 28 cases, which were treated at Bangalore with the smaller doses of serum, gave a case-mortality of 1 in 1·4 cases, the 21 later cases, which were treated with larger quantities of serum, gave a mortality of only 1 in 1·9. We cannot, however, infer anything from this comparison with regard to the efficacy of the larger quantities of serum, for on the one hand the Efficacy of larger and smaller quantities of serum.

figures are quite small, and on the other hand, the epidemic, and with it the case-mortality, was rapidly declining. The cases in Bombay, being of a much severer type, cannot be compared with those in Bangalore.

Effect of serum on special symptoms in cases treated in the Bangalore and Modi Khana Hospitals.

530. We may now examine the question whether the symptoms presented by the patients were modified to any considerable extent by serum treatment, for although serum treatment might have no effect in prolonging life, it might nevertheless prove useful in alleviating suffering.

Effect on temperature in cases ending in recovery.

It is an undoubted fact that, not unfrequently, the temperature rapidly sinks soon or within three to four hours after the injection of serum ; but inspection of the series of charts prepared by Captain Walton and Lieut. Douglas shows that this fall of temperature is generally of very short duration, and that a rise of temperature sometimes follows the injection. Indeed, a series of charts of cases which recover under serum treatment does not differ in any characteristic manner from that of a series of cases that have recovered under ordinary treatment ; no characteristic difference in the course of temperature can be found between them.

Effect on temperature in cases ending fatally.

Even in severe cases ending fatally a fall of temperature may supervene upon the injection of serum. This, however, is exceptional. The fall, when it occurs, is a very transient one, and often lasts but a short time. A rise of temperature, as the following examples will show, occasionally supervenes on the injection of serum.

Appendix LXXIX., III. 593.

Case No. I., inoculated with 20 c.c. of serum on the 3rd day of the disease, at 11.30 a.m. :—

Temperature at	9.0 a.m.	103·4
Do.	at 12.45 p.m.	104·2
Do.	at 3.0 p.m.	104·6

Appendix LXXIX., III. 595.

Case No. V., inoculated with 10 c.c. of serum, on the 2nd day of the disease, at 1.30 p.m. :—

Temperature at	1.30 p.m.	103·8
Do.	at 4 p.m.	105·4

Appendix LXXIX., III. 599.

Case No. XIII., inoculated with 20 c.c. of serum, on the fifth day of the disease, 9 a.m. :—

Temperature at	9 a.m.	97·4
Do.	at 11 a.m.	101
Do.	at 1 p.m.	105·4
Do.	at 2.30 p.m.	105·6
Do.	at 5 p.m.	104

Appendix LXXIX., III. 612.

Case No. XXVIII.—This, like Case V., was injected with serum within 24 hours of the onset of symptoms. At 9 a.m. in the morning, 40 c.c. of serum were injected, the temperature at the time being 100·8 F. At 2.20 p.m. the thermometer indicated 102·6° F. At 5 p.m. it was 102·2, and it stood at the same point the next morning. At 9 a.m. 20 c.c. of serum were injected. By 3 p.m. the thermometer had risen to 103·2, but in two hours fell suddenly to 99·4. At 10 a.m. the next day, the temperature was 100·2° F., and at 1 p.m., when 20 c.c. of serum were injected, it had risen to 103·2° F., but it fell to 99·2° F. by 5 p.m. The temperature therefore showed the oscillations often seen in cases of severe plague just as if no serum had been injected, and this although the treatment had commenced within 24 hours of the first symptoms.

Appendix LXXX., III. 626, 628.

Even enormous doses may be without influence on the temperature. Thus in Case Nos. XI. and XV., 160 c.c. and 135 c.c. were injected in four days with no beneficial effect on the temperature.

It seems to us possible that the rise or fall of the temperature curve after the injection of the serum may stand in connexion with the time of the day at which the serum is injected. We notice that in the case of 10 patients who were injected with serum both in the morning and afternoon, the morning injection was followed by a rise of temperature within 2 to 5½ hours in seven cases, in two cases there was a fall within 2 to 5 hours, and in one case the temperature remained the same. On the other hand, after the 10 injections made after 12 noon, in seven cases the temperature fell within 2 to 2½ hours, in two it was higher, and in one case it underwent no change.

Effect of serum injection on the pulse.

The temporary effect noticed on the temperature is also noticeable on the pulse which becomes stronger, fuller, and slower, but the effect is but transitory ; and the pulse curve of cases treated with serum shows no appreciable difference from the

curve of other cases recovering on ordinary treatment. After injection of serum, although the temperature may be going down, the pulse rate often shows no alteration. This is well illustrated by Case No. IV., as the following notes will show:— Appendix LXXX., III. 623.

	Temperature.	Pulse.
At the time of injection of serum 40 c.c. -	103·4° F.	116
4 hours afterwards - - -	99·6° F.	120
8 „ „ - - -	101·2° F.	116
12 „ „ - - -	98·0° F.	128
16 „ „ - - -	100·0° F.	116
24 „ „ - - -	101·6° F.	108

Towards the end of the illness, when the patient recovers, the pulse resumes its normal rate, but not until some time after the temperature has returned to normal.

Effect of serum injection on respiration.

A distinct temporary improvement in the respiration often follows the injection of serum, the respiration rate following the pulse rate very closely.

Effect of serum injection on the bubo.

The serum appears to exert no effect whatever on the buboes. In some cases the bubo became smaller and markedly less painful, but in many others, in spite of repeated injections of serum, the bubo enlarged, and the tissues over it became more œdematous. Out of 20 bubonic cases that recovered, suppuration occurred in seven.

Effect of serum injection on lung complications.

Since there was not either at Bangalore or in Bombay a single case of primary plague pneumonia admitted to hospital while the serum treatment was in progress, no opportunity presented itself for studying the effect of the serum on such cases. On the other hand, no less than 27 of the patients treated with serum showed lung symptoms either on admission or during the progress of the disease. Of these 21 died, and six recovered. The lung symptoms in the six cases ending in recovery were of the slightest. The symptoms in those who died varied from a few rhonchi, over one or both lungs, and prolonged expiration to well-marked lobular or lobar pneumonia. Pneumonia was present in six cases. In three cases the symptoms of pneumonia appeared some considerable time after the serum treatment had been begun; in one case they appeared after four days; in another after two days; in the last case, the pneumonia appeared when the patient was convalescent and proved fatal in a few hours,—whether it was or was not due to an invasion of the lungs by the plague bacillus could not be ascertained.

It will be seen that the serum treatment appears to exert little or no influence on the supervention or course of the lung complications which are so commonly met with in plague.

Effect of serum injection on delirium and other nervous symptoms.

In many cases the patients appeared to be rather less restless after the injection of serum, the delirium somewhat abating. Unfortunately, the improvement in the nervous symptoms is by no means constant. Many patients, in spite of repeated injections, suffered from most violent delirium and had to be tied down in bed.

Effect of serum injection on convalescence and prolongation of life.

In the cases that recovered convalescence was slow and tedious, being no more rapid than that of other patients.

Again, life did not appear to be appreciably prolonged in the patients who succumbed after treatment with serum. In the case of 51 patients regarding whom details as to the duration of the disease are available, the date on which death supervened is shown in the following table:—

2	died on 2nd day.	5	died on 8th day.
3	„ 3rd day.	3	„ 10th day.
11	„ 4th day.	1	„ 11th day.
9	„ 5th day.	1	„ 12th day.
7	„ 6th day.	1	„ 17th day.
8	„ 7th day.		

No less than 25, therefore, died before the sixth day, and the average duration of the disease was just over five days; this corresponds to what might have been expected if no serum had been injected.

Causes of death in serum patients.

In six cases death seemed to be due to the supervention of pneumonia; in the other cases it seemed to be due to exhaustion and heart failure; this last sometimes set in with great rapidity. In two cases convulsions set in immediately before death.

Abstract of cases treated with Yersin's serum in Bangalore and Modi Khana Hospitals.

531. We may conclude our Report on the observations made by us on Yersin's serum by setting forth in tabular form, firstly, an abstract of the cases of plague which were treated with serum; secondly, details regarding the serum injection resorted to in each case.

Abstract of Cases treated with Yersin's Serum, prepared at the Pasteur Institute, Paris.

Cases No. 1 to 49 treated at the South Camp Hospital, Bangalore; Cases No. 49 to 77 treated at the Modi Khana Hospital, Bombay.

Serial No.	Reference to Detailed Notes.	Sex.	Age	Date of Disease on which Serum first injected.	Situation of Bubo.	Quantity of Serum Injected.	Date of Disease, or Period after Injection, at which Death occurred in Fatal Cases.	Remarks.
1	App. LXXIX. I.	Male	45	3rd day	Left groin	35 c.c. in 2 doses	On the 4th day of disease	In spite of injection temperature rose for more than 3 hours. No lung complications.
2	II.	Male	25	2nd day	Right axilla	20 c.c. in 1 dose	On the 3rd day of disease, 20 hours after injection.	No respiratory symptoms.
3	III.	Female	25	3rd day	Right axilla	20 c.c. in 1 dose	On the 4th day of disease, 15 hours after injection.	No lung symptoms.
4	IV.	Female	20	3rd day	Right axilla	56 c.c. in 4 doses on 4 days.	On the 6th day of disease	Developed lung symptoms, probably of pneumonic type shortly before death.
5	V.	Female	8	2nd day	Left groin	20 c.c. in 2 doses	On the 2nd day of disease, about 24 hours after first injection, and about 5 hours after last.	No respiratory symptoms.
6	VI.	Female	35	3rd day	No bubo	40 c.c. in 3 days	-	Recovered. The case was complicated by an attack of malaria. No lung symptoms.
7	VII.	Female	30	2nd day	Right groin	17 c.c. in 1 dose	On the 2nd day of disease, half an hour after injection.	No lung symptoms. Patient was pregnant.
8	VIII.	Female	13	6th day	Right inguinal and right femoral.	23 c.c. in 2 doses on day of admission.	-	Recovered. Bubo suppurated. No lung symptoms.
9	IX.	Female	12	4th day	No bubo	10 c.c. in 1 dose	-	Recovered. Not injected till day after admission to hospital. Slight lung symptoms.
10	X.	Male	40	3rd day	Submaxillary region.	17 c.c. in 1 dose	On the 3rd day of disease, about 12 hours after injection and admission.	No lung symptoms.
11	XI.	Male	48	4th day	Left inguinal region.	90 c.c. in 5 doses	-	Recovered. No suppuration. No lung symptoms.
12	XII.	Male	12	3rd day	Right axilla	45 c.c. in 3 doses	On the 8th day of the disease.	The case is peculiar, in so far that the patient was so well on the morning of 6th day that no injection was given. In the afternoon he was so much worse that an injection was given. No lung symptoms.
13	XIII.	Male	25	5th day	Right groin, right axilla.	60 c.c. in 3 doses	On the 7th day of disease, and 7 hours after last injection.	Lung symptoms.
14	XIV	Male	15	3rd day	Right femoral region.	40 c.c. in 2 doses	On the 4th day of disease, about 12 hours after last injection.	Respiration went up to 148 before death. No lung symptoms.
15	XV.	Female	55	?	None	40 c.c. in 2 doses	24 hours after last injection.	Very violent delirium. No lung symptoms.
16	XVI.	Female	50	2nd day	Left axilla	40 c.c. in 2 doses	-	Recovered. Mild case, so that no serum was thought necessary after 2nd day of treatment. No lung symptoms.
17	XVII.	Female	28	2nd day	Right groin	60 c.c. in 3 doses	On the 4th day of disease.	No lung symptoms.
18	XVIII.	Female	60	7th day	Left groin	20 c.c. in 1 dose	On the 7th day of disease, about 12 hours after injection.	Practically moribund when inoculated. Congestion of both lungs at bases.
19	XIX.	Male	11	6th day	Left femoral glands.	30 c.c. in 2 doses	-	Recovered. No suppuration. No lung symptoms.
20	XX.	Male	35	6th day	Right femoral region	20 c.c. in 1 dose	On the 6th day of disease, about 3 hours after inoculation.	No lung symptoms.
21	XXI.	Female	25	9th day	Left axilla, left popliteal space.	50 c.c. in 2 doses	On the 10th day of disease, 5 hours after last injection.	Patient had aborted 4 days before admission. Secondary pneumonia (?) Injected the morning after her admission to hospital.
22	XXII	Female	11	2nd day	Left axilla	55 c.c. in 3 doses	On the 4th day of disease, about 12 hours after last injection.	The temperature in this case never went below 103° F., except once. No lung symptoms.
23	XXIII.	Female	13	5th day	Left submaxillary region.	40 c.c. in 2 doses	On the 6th day of disease, less than 24 hours after last injection.	No lung symptoms.
24	XXIV.	Female		2nd day	Right submaxillary region.	75 c.c. in 4 doses	-	Recovered. No suppuration. No lung symptoms.

Serial No.	Reference to Detailed Notes.	Sex.	Age.	Date of Disease on which Serum first Injected.	Situation of Bubo.	Quantity of Serum Injected.	Date of Disease, or Period after Injection, at which Death occurred in Fatal Cases.	Remarks.
25	App. LXXIX. XXV.	Female	25	6th day	No bubo	20 c.c. in 1 dose -	On the 10th day of disease, and 4 days after inoculation.	Doubt as to the diagnosis of plague. Patient had lung symptoms.
26	XXVI.	Male	27	2nd day	Left groin	110 c.c. in 5 doses -	On the 5th day of disease	Delirium was very marked throughout. No lung symptoms.
27	XXVIII.	Female	25	4th day	Right axilla	70 c.c. in 3 doses -	- - - -	Recovered. No lung symptoms. No suppuration.
28	XXIX.	Female	2	?	None	15 c.c. in 1 dose -	A few hours after admission and inoculation.	No lung symptoms.
29	XXX.	Male	An aged man.	5th day	Right groin and left side of neck.	100 c.c. in 4 doses -	On the 8th day of disease, 3 days after first injection and about 16½ hours after the last.	Very serious case from the first. No lung symptoms.
30	XXXI.	Male	18	6th day	Right epitrochlear gland.	60 c.c. in 2 doses -	On the 7th day of disease, and same day as last injection.	Secondary lung symptoms, first noticed on the left side, pneumonia gradually invading the right lung.
31	XXXII.	Female	35	6th day	Left femoral region.	40 c.c. in 1 dose -	On the 7th day of disease, and 20 hours after inoculation.	No lung symptoms.
32	XXXIII.	Female	55	A few hours after first symptoms set in, patient being an inmate of hospital.	No bubo	60 c.c. in 2 doses within 24 hours of onset of disease.	On the 12th day of disease, and 12th day after inoculation.	Patient died rather suddenly, probably from œdema of lungs(?) No sputum could be obtained.
33	XXXIV.	Male	14	5th day	No bubo	60 c.c. in 2 doses -	- - - -	Recovered. No lung symptoms. Was injected the morning after admission to hospital.
34	XXXV.	Male	10	3rd day	Right groin, left axilla.	20 c.c. in 1 dose -	- - - -	Recovered. Buboes had to be opened. The bubo in left axilla developed on day following injection of serum. No lung symptoms.
35	XXXVI.	Female	25	7th day	Left groin	100 c.c. in 4 doses -	- - - -	Recovered. Slight lung symptoms.
36	XXXVII.	Female	30	4th day	Left groin	160 c.c. in 7 doses on 7 consecutive days.	On the 11th day of disease, and on day following last injection.	Had a few sibilant rales in chest.
37	XXXVIII.	Male	60	1st day	Left groin	100 c.c. in 4 doses -	On the 5th day of disease, and 1¾ hours after last injection.	No lung symptoms.
38	XXXIX.	Female	15	6th day	Submaxillary region.	100 c.c. in 4 doses on 6th, 7th, 8th, and 9th day of disease.	On the 17th day of disease, and 8 days after last injection.	The patient was doing fairly well when signs of pneumonia set in, and she died in 24 hours. Bubo was opened.
39	XL.	Female	20	4th day	Left axilla	40 c.c. in 1 dose -	- - - -	Recovered. Case was never severe. No lung symptoms.
40	XLI.	Male	50	5th day	?	40 c.c. in 1 dose -	On the 6th day of disease, 14 hours after injection.	Lung symptoms, probably pneumonia. Was injected the morning after admission to hospital.
41	XLII.	Female	35	6th day	Right groin	60 c.c. in 2 doses -	On the 10th day of disease, and 3 days after last injection.	Patient was 6 months pregnant, and aborted. No lung symptoms. Was injected the morning after admission to hospital.
42	XLIII.	Female	30	3rd day	Right cervical glands.	40 c.c. in 1 dose -	On the 4th day of disease, about 12 hours after injection.	No lung symptoms. Was injected the morning after admission to hospital.
43	XLIV.	Female	?	3rd day	Right axilla	40 c.c. in 1 dose -	- - - -	Recovered. No lung symptoms. Was never a severe case.
44	Ia.	Male	3	3rd day	Right axilla	20 c.c. in 1 dose -	- - - -	Recovered. No lung symptoms. Bubo suppurated.
45	IIa.	Female	13	5th day	Left side of neck.	20 c.c. in 1 dose -	- - - -	Recovered. No lung symptoms Was never a severe case.
46	IIIa.	Female	2½	4th day	Left groin	20 c.c. in 1 dose -	- - - -	Recovered. Condition was good from the first. No lung symptoms.
47	IVa.	Male	35	5th day	Left groin	100 c.c. in 4 doses -	- - - -	Recovered. No lung symptoms. Bubo suppurated. Was injected the morning after admission to hospital.
48	Va.	Male	21	4th day	Left side of neck.	140 c.c. in 6 doses on 3 days.	On the 7th day of disease, and 19 hours after last injection.	No lung symptoms.
49	VIa.	Male	50	6th day	Left groin	40 c.c. in 1 dose -	- - - -	Recovered. No lung symptoms. Condition good from the first. Was injected the morning after admission to hospital.
50	App. LXXX. I.	Male	28	3rd day	Right femoral region.	160 c.c. in 7 doses on 4 days.	On the 7th day of disease, 5 days after first injection, and about 24 hours after the last.	No lung symptoms. Retention of urine.
51	II.	Male	35	4th day	Left groin	140 c.c. in 6 doses on 4 days.	On the 8th day of disease, 4 days after the first, and less than 24 hours after the last.	No lung symptoms.

Serial No.	Reference to Detailed Notes.	Sex.	Age.	Date of Disease on which Serum first Injected.	Situation of Bubo.	Quantity of Serum Injected.	Date of Disease or Period after Injection, at which Death occurred in Fatal Cases.	Remarks.
52	App. LXXX. III.	Male	28	3rd day	Left groin	60 c.c. in 1 dose	On the 3rd day of disease, about 12 hours after injection.	No lung symptoms.
53	IV.	Male	30	3rd day	Left groin	140 c.c. in 6 doses on 4 consecutive days.	- - - -	Recovered. Glands necrosed and had to be removed. Patient had cough, with rales in right lung.
54	V.	Male	40	3rd day	Left inguinal region.	100 c.c. in 3 doses on 3 consecutive days.	On the 5th day of disease, 2 days after 11th injection, and shortly after last.	Prolonged expiration and rhonchi over both lungs.
55	VI.	Male	30	4th day	Right submaxillary region.	100 c.c. in 4 doses on 4th and 5th day of disease.	On the 7th day of disease, 3 days after 1st, and and about 36 hours after the last injection.	Respiration extremely rapid towards the end. No lung symptoms.
56	VII.	Male	30	5th day	Left femoral region.	40 c.c. in 1 dose	On the 5th day of disease, a few hours after injection.	Lungs normal.
57	VIII.	Male	25	5th day	Right groin	115 c.c. in 5 doses on 3 consecutive days.	- - - -	Recovered. Rhonchi at base of right lung. Bubo incised.
58	IX.	Male	24	5th day	Right axilla	150 c.c. in 6 doses on 3 consecutive days.	On the 8th day of disease, 3 days after the 1st injection, and less than 24 hours after the last.	Lungs normal.
59	X.	Male	40	4th day	Left axilla	120 c.c. in 5 doses on 3 consecutive days.	On the 7th day of disease, 3 days after 1st, and on day following last injection.	Was injected on the day after admission to hospital. Rales and rhonchi over both lungs.
60	XI.	Male	25	3rd day	Right femoral region, Left groin.	160 c.c. in 7 doses on 4 consecutive days.	On the 6th day of disease, 4 days after the 1st, and a few hours after the last injection.	Lungs normal. The gland in left groin became affected on the 3rd day of serum treatment.
61	XII.	Male	24	3rd day	Left axilla	80 c.c. in 3 doses on 2 days.	On the 5th day of disease, 2 days after 1st, and a few hours after last injection.	Sibilant rhonchi over both lungs.
62	XIII.	Male	30	3rd day	Right groin	80 c.c. in 3 doses on 2 consecutive days.	On the 4th day of disease	Lungs normal.
63	XIV.	Male	25	4th day	Right groin	40 c.c. in 1 dose	On the 4th day of disease, a few hours after injection.	Bronchial breathing over the whole of the left lung.
64	XV.	Male	25	3rd day	Right femoral region.	135 c.c. in 6 doses on 4 consecutive days.	On the 6th day of disease, about 12 hours after the last injection.	Prolonged expiration at both bases of lungs with rhonchi at end of expiration.
65	App. LXXXI I.	Male	23	7th day	Left axilla	175 c.c. in 8 doses on 6 consecutive days.	- - - -	Recovered. Lungs normal. Retention of urine.
66	II.	Male	28	4th day	Right axilla	58 c.c. in 2 doses	On the 4th day of disease, the same day as the two injections.	Lungs normal. Was injected on the day after his admission to hospital.
67	III.	Male	40	3rd day	Right femoral region.	60 c.c. in 2 doses, of which 20 c.c. were injected in subcutaneous vein.	On the 4th day of disease, on day of injection.	Rales and rhonchi at bases of both lungs. Was injected the day after admission to hospital.
68	IV.	Male	45	4th day	Left groin	40 c.c. in 2 doses on the same day.	On the 5th day of disease, about 15 hours after last injection.	Lungs on both sides affected.
69	V.	Male	18	?	Right axilla	235 c.c. in 6 doses on 3 consecutive days.	Died 3 days after 1st injection, and on day of last injection.	Lungs normal.
70	VI.	Male	30	9th day	Right groin	140 c.c. in 5 doses on 3 consecutive days.	- - - -	Recovered. The bubo was already ulcerated when serum was injected. The lungs were full of rales, and patient had paroxysms of coughing.
71	VII.	Male	35	5th day	Right groin	40 c.c. in 1 dose	On the 5th day of disease, 5 hours after injection.	Lungs normal.
72	VIII.	Male	22	3rd day	Right femoral region.	135 c.c. in 6 doses on 3 consecutive days.	On the 6th day of disease, 3 days after 1st injection, and about 15 hours after last injection.	Chest full of rales. Cough troublesome.
73	IX.	Male	29	4th day	Left femoral region.	220 c.c. in 10 doses on 5 consecutive days.	- - - -	Recovered. Cough and expectoration. Rales both sides of chest.
74	X.	Male	30	4th day	Right axilla	80 c.c. in 3 doses on 2 consecutive days.	On the 5th day of disease	Patches of pneumonia at bases of both lungs. Patient was injected the day after his admission to hospital.
75	XI.	Male	14	7th day	Left groin	80 c.c. in 3 doses, on 2 consecutive days.	On the 8th day of disease	Slight lung symptoms.
76	XII.	Male	25	3rd day	Right axilla	80 c.c. in 3 doses on 2 consecutive days.	On the 4th day of disease	No lung symptoms.
77	XI.	Male	12	5th day	Left axilla	40 c.c. in 1 dose	On the 5th day of disease, about 6 hours after injection.	Lungs slightly affected on the left side.

TABLE showing the AMOUNTS of SERUM administered on each day of SERUM TREATMENT to the PATIENTS particularised in the Table above.

Serial No. (as in table immediately preceding).	Doses and Days of Treatment.							Total Amount of Serum injected.	Result.
	1st Day.	2nd Day.	3rd Day.	4th Day.	5th Day.	6th Day.	7th Day.		
1	20 c.c.	15 c.c.	—	—	—	—	—	35 c.c.	Death.
2	20 c.c.	—	—	—	—	—	—	20 c.c.	Death.
3	20 c.c.	—	—	—	—	—	—	20 c.c.	Death.
4	16 c.c.	10 c.c.	10 c.c.	20 c.c.	—	—	—	56 c.c.	Death.
5	10 c.c.	10 c.c.	—	—	—	—	—	20 c.c.	Death.
6	20 c.c.	10 c.c.	10 c.c.	—	—	—	—	40 c.c.	Recovery.
7	17 c.c.	—	—	—	—	—	—	17 c.c.	Death.
8	13 c.c. 10 c.c.	—	—	—	—	—	—	23 c.c.	Recovery.
9	10 c.c.	—	—	—	—	—	—	10 c.c.	Recovery.
10	17 c.c.	—	—	—	—	—	—	17 c.c.	Death.
11	20 c.c.	20 c.c.	10 c c.	—	20 c.c.	20 c.c.	—	90 c.c.	Recovery.
12	15 c.c.	15 c.c.	15 c.c.	—	—	—	—	45 c.c.	Death.
13	20 c.c.	20 c.c.	20 c.c.	—	—	—	—	60 c.c.	Death.
14	20 c.c.	20 c.c.	—	—	—	—	—	40 c.c.	Death.
15	20 c.c.	20 c.c.	—	—	—	—	—	40 c.c.	Death.
16	20 c.c.	20 c.c.	—	—	—	—	—	40 c.c.	Recovery.
17	20 c.c.	20 c.c.	20 c.c.	—	—	—	—	60 c.c.	Death.
18	20 c.c.	—	—	—	—	—	—	20 c.c.	Death.
19	15 c.c.	15 c.c.	—	—	—	—	—	30 c.c.	Recovery.
20	20 c.c.	—	—	—	—	—	—	20 c.c.	Death.
21	30 c.c.	20 c.c.	—	—	—	—	—	50 c.c.	Death.
22	15 c.c.	20 c.c.	20 c.c.	—	—	—	—	55 c.c.	Death.
23	20 c.c.	20 c.c.	—	—	—	—	—	40 c.c.	Death.
24	15 c.c.	20 c.c.	20 c.c.	20 c.c.	—	—	—	75 c.c.	Recovery.
25	20 c.c.	—	—	—	—	—	—	20 c.c.	Death.
26	30 c.c.	20 c.c.	20 c.c.	20 c.c.	20 c c.	—	—	110 c.c.	Death
27	30 c.c.	20 c.c.	20 c.c.	—	—	—	—	70 c.c.	Recovery.
28	15 c.c.	—	—	—	—	—	—	15 c.c.	Death.
29	40 c.c.	20 c.c. 20 c.c.	—	20 c.c.	—	—	—	100 c.c.	Death.
30	40 c.c.	20 c.c.	—	—	—	—	—	60 c.c.	Death.
31	40 c.c.	—	—	—	—	—	—	40 c.c.	Death.
32	40 c.c.	20 c.c.	—	—	—	—	—	60 c.c.	Death.
33	40 c.c.	20 c.c.	—	—	—	—	—	60 c.c.	Recovery.
34	20 c.c.	—	—	—	—	—	—	20 c.c.	Recovery.
35	40 c.c.	20 c.c.	20 c.c.	20 c.c.	—	—	—	100 c.c	Recovery.
36	40 c c.	20 c.c.	20 c.c.	20 c.c.	20 c.c.	20 c.c.	20 c.c.	160 c.c.	Death.
37	40 c.c.	20 c.c.	20 c.c.	20 c.c.	—	—	—	100 c.c.	Death.
38	40 c.c.	20 c.c.	20 c.c.	20 c.c.	—	—	—	100 c.c.	Death from pneumonia some time after serum treatment closed.
39	40 c.c.	—	—	—	—	—	—	40 c.c.	Recovery
40	40 c.c.	—	—	—	—	—	—	40 c.c.	Death.
41	40 c.c.	20 c.c.	—	—	—	—	—	60 c.c.	Death.
42	40 c.c.	—	—	—	—	—	—	40 c.c.	Death.
43	40 c.c.	—	—	—	—	—	—	40 c.c.	Recovery.
44	20 c.c.	—	—	—	—	—	—	20 c.c.	Recovery.
45	20 c.c.	—	—	—	—	—	—	20 c.c.	Recovery.
46	20 c.c.	—	—	—	—	—	—	20 c.c.	Recovery.
47	40 c.c.	20 c.c.	20 c.c.	20 c.c.	—	—	—	100 c.c.	Recovery.
48	40 c.c. 20 c.c.	20 c.c. 20 c.c.	20 c.c. 20 c.c.	—	—	—	—	140 c.c.	Death.
49	40 c.c.	—	—	—	—	—	—	40 c.c.	Recovery.
50	40 c.c. 20 c.c.	20 c.c. 20 c.c.	20 c.c. 20 c.c.	20 c.c.	—	—	—	160 c.c.	Death.
51	40 c.c.	20 c.c. 20 c.c.	20 c.c. 20 c.c.	20 c.c.	—	—	—	140 c.c.	Death.
52	60 c.c.	—	—	—	—	—	—	60 c.c.	Death.
53	40 c.c.	20 c.c. 20 c.c.	20 c.c. 20 c.c.	20 c.c.	—	—	—	140 c.c.	Recovery.

Serial No. (as in table immediately preceding).	Doses and Days of Treatment.						Total Amount of Serum injected.	Result.
	1st Day.	2nd Day.	3rd Day.	4th Day.	5th Day.	6th Day.		
54	40 c.c.	20 c.c.	40 c.c.	—	—	—	100 c.c.	Death.
55	40 c.c. 20 c.c.	20 c.c. 20 c.c.	—	—	—	—	100 c.c.	Death.
56	40 c.c.	—	—	—	—	—	40 c.c.	Death.
57	40 c.c.	20 c.c. 15 c.c.	20 c.c. 20 c.c.	—	—	—	115 c.c.	Recovery.
58	40 c.c. 30 c.c.	20 c.c. 20 c.c	20 c.c. 20 c.c.	—	—	—	150 c.c.	Death.
59	40 c.c. 20 c.c.	20 c.c. 20 c.c.	20 c.c.	—	—	—	120 c.c.	Death.
60	40 c.c. 20 c.c.	20 c.c. 20 c.c.	20 c.c. 20 c.c.	20 c.c.	—	—	160 c.c.	Death.
61	40 c.c.	20 c.c. 20 c.c.	—	—	—	—	80 c.c.	Death.
62	40 c.c. 20 c.c.	20 c.c.	—	—	—	—	80 c.c.	Death.
63	40 c.c.	—	—	—	—	—	40 c.c.	Death.
64	35 c.c. 20 c.c.	20 c.c. 20 c.c.	20 c.c.	20 c.c.	—	—	135 c.c.	Death
65	40 c.c.	20 c.c. 15 c.c.	20 c.c. 20 c.c.	20 c.c.	20 c.c.	20 c.c.	175 c.c.	Recovery.
66	40 c.c. 18 c.c.	—	—	—	—	—	58 c.c.	Death.
67	40 c.c. 20 c.c.	—	—	—	—	—	60 c.c.	Death.
68	20 c.c. 20 c.c.	—	—	—	—	—	40 c.c.	Death.
69	40 c.c. 35 c.c.	40 c.c. 40 c.c.	40 c.c. 40 c.c.	—	—	—	235 c.c.	Death.
70	40 c.c.	40 c.c. 20 c.c.	20 c.c. 20 c.c.	—	—	—	140 c.c.	Recovery.
71	40 c.c.	—	—	—	—	—	40 c.c.	Death.
72	20 c.c. 15 c.c.	40 c.c. 20 c.c.	20 c.c. 20 c.c.	—	—	—	135 c.c.	Death.
73	40 c.c. 20 c.c.	20 c.c. 20 c.c.	20 c.c. 20 c.c.	20 c.c. 20 c.c.	20 c.c. 20 c c.	—	220 c.c.	Recovery.
74	40 c.c. 20 c.c.	20 c.c.	—	—	—	—	80 c.c.	Death.
75	40 c.c. 20 c.c.	20 c.c.	—	—	—	—	80 c.c.	Death.
76	40 c.c. 20 c.c.	20 c.c.	—	—	—	—	80 c.c.	Death.
77	40 c.c.	—	—	—	—	—	40 c.c.	Death.

V.—(B.) *Results obtained by Captain Mason, R.A.M.C., by the Employment of Yersin's Serum during the Epidemic in Cutch.*

Source of the serum used in Cutch Mandvi.

532. Captain Mason, R.A.M.C., has reported to us the particulars of a series of systematic observations which he made on plague patients with Yersin's anti-plague serum obtained direct from the Pasteur Institute, Paris. In the case of one of the consignments of serum which were furnished to him, he made also a series of experiments on animals. The results of the experiments have been set forth in an earlier part of this Chapter.

Para. 503 in this Chapter.

Method of administration.

27,034.

27,055.

27,195.

The serum was administered to human plague patients in doses of from 10 to 60 c.c. The smaller doses were employed in the cases first treated, the larger doses being resorted to as experience was gained. Quantities larger than 60 c.c. were not administered, because it was found that with this last dose the patients began to suffer from painful swelling of the joints.

Principle on which the patients were selected for treatment.

27,024; 27,153.

The patients who were therapeutically treated with serum were drawn from several hospitals. All classes of patients were submitted to the treatment, with the exception of patients admitted five or more days after they had fallen ill. In a very few instances the treatment was extended also to these.

533. The results which were obtained in the case of 100 patients treated with the serum were compared by Captain Mason with the results obtained in 100 untreated patients. The results of this comparison are shown in the following table:— Statistics of cases treated by Captain Mason and of controls.

COMPARISON INSTITUTED BY CAPTAIN MASON, R.A.M.C., AS BETWEEN 100 SERUM CASES AND 100 CONTROL CASES. Comparison of 100 serum cases and 100 control cases.

—	Cases.	Deaths.	Case-Mortality.
Control Group - - -	100	83	83 per cent.
Serum Group - - -	100	59	59 per cent.
Difference in favour of the serum cases - - - -			24 per cent.

27,028.

In determining the value which is to be placed on the results of the above comparison, it is important to take into consideration the following. The plague cases that served as controls were admitted to the Brahmapuri Hospital at Cutch Mandvi during the period of eight days (from the 25th of May to the 2nd of June 1897) at the height of the epidemic, when the fatality of the disease was extreme as is evidenced by the mortality of 83 per cent. in the hospital. The plague cases, however, which were treated with serum were, with the exception of the first 5 cases treated, admitted to hospital during a period of four months of a declining epidemic, when we may presume the type of the disease to have been much milder. Again, only 31 of the 100 serum cases were admitted into the Brahmapuri Hospital, this, apparently, being in consequence of the decline of the epidemic in Cutch Mandvi. The remaining 69 cases were admitted into hospital in smaller places in the neighbourhood of Cutch Mandvi. The serum cases were thus hardly comparable with the control cases. It is, indeed, recognised by Captain Mason that the average severity of the disease in the control cases was greater than that of the serum cases. Captain Mason was, however, not in a position to put forward any other series of control cases which complied more strictly with the required conditions. Critical consideration of the above statistics.

27,146–9.

27,151–2–4.

Failing a series of serum cases treated under similar conditions with the control cases, we may compare the 31 cases which were treated with serum in the Brahmapuri Hospital with the 100 control cases which were, as we saw, admitted into that hospital. The results of this comparison are expressed in the following table:— Comparison of the serum and control cases treated in the Brahmapuri Hospital.

COMPARISON BETWEEN 31 SERUM CASES TREATED IN THE BRAHMAPURI HOSPITAL, CUTCH MANDVI, AND 100 CASES IN THE SAME HOSPITAL NOT TREATED WITH SERUM.

—	Cases.	Deaths.	Case-Mortality.
Control Cases - - - -	100	83	83 per cent.
Serum Cases - - -	31	20	63 per cent.
Difference in favour of the serum cases - - - -			20 per cent.

The comparison thus instituted is somewhat less favourable to the serum treatment than the comparison instituted by Captain Mason. Even more unfavourable to the serum treatment becomes the comparison if we compare the results obtained in the control cases with the results obtained in the serum cases treated contemporaneously,

or within a week after the date when the last control case was admitted. The comparison would then be as follows:—

COMPARISON BETWEEN 100 CONTROL CASES IN THE BRAHMAPURI HOSPITAL AND THE SERUM CASES THAT WERE TREATED IN THE SAME HOSPITAL IN OR ABOUT THE SAME PERIOD.

—	Cases.	Deaths.	Case-Mortality.
Control Group - - - -	100	83	83 per cent.
Serum Group - - -	19	14	75 per cent.
Difference in favour of the serum cases - - - -			8 per cent.

In view of the more unfavourable results arrived at by these last comparisons, it is obviously possible that the diminished mortality of the serum cases which is exhibited in the first table may be referable quite as much to the decline in the virulence of the epidemic as to an effect exerted by the serum treatment.

Effect of the serum treatment on clinical symptoms in cases treated at Cutch Mandvi.

Effect on temperature.

27,050.
27,052.

534. We pass now from considering the influence exerted on case-mortality, to consider whether the symptoms of the disease were modified by the exhibition of the serum. Captain Mason's testimony on this question is quite unequivocal. He tells us that patients became calmer on treatment with the serum, their temperature fell, and there was a general amelioration in their condition. "It is certain," says this clinical observer, "that in ordinary cases the temperature falls under the action of the first injection, and it is equally certain that in many cases it never reascends; the fall of the temperature is obtained whatever the day of the disease on which the treatment is commenced." On the other hand, Captain Mason tells us that "the improvement in the general condition, like the fall in temperature, is in the majority of the gravest cases apt to be transitory: the symptoms again become serious and the disease appears to run its course as if the serum had not been given."

No effect on development of secondary buboes.

Para. 536 below.

Do.

It does not, however, appear from the clinical notes tendered by Captain Mason that the injection of serum prevented the appearance of secondary buboes. In one of Captain Mason's cases (Case No. 85) the patient, although injected on the first day of the disease, developed a secondary left submaxillary bubo and a secondary popliteal bubo on the third day, and died on the fifth day, while in another of the cases (Case No. 98) the patient who was injected on the second day of the disease developed buboes in the right and left groins three days afterwards, though his case ended in recovery.

No effect on primary plague pneumonia.

It is clear, from Captain Mason's notes, that the serum exerted no beneficial effect on cases of primary plague pneumonia.

Effect of the serum treatment according to the date of the disease on which treatment was commenced.

Lastly, we may make a comparison between the course of the disease in patients treated with the serum with the course of the disease in control patients, with a view to determining whether the patients who were admitted and treated with serum in the early stages of the disease compare more favourably with their corresponding control patients than those admitted and treated with serum in the later stages of the disease do with their respective controls. At the same time we may compare the serum cases among themselves, with a view to determining whether the case-mortality was less in those which were treated early as compared with the cases which were treated late. The data for these comparisons are set forth in the following table:—

—	1st Day.		2nd Day.		3rd Day.		4th Day.		6th Day or later.	
	Cases.	Deaths.	Cases.	Deaths.	Cases.	Deaths.	Cases.	Deaths.	Cases.	Deaths.
Control Group - - -	19	14	41	32	21	20	15	11	3	3
Case-mortality of this group	1 in 1·3		1 in 1·3		1 in 1, nearly		1 in 1·4		1 in 1	
Serum Group - - -	27	15	47	30	17	8	8	4	1	1
Case-mortality of this group	1 in 1·8		1 in 1·6		1 in 2·1		1 in 2		1 in 1	

It will be seen that the case-mortality on each day was smaller in the cases treated with serum than in the untreated cases. On the other hand, the cases admitted and treated with serum on the first and second days do not compare more favourably with the control cases admitted on those days than do the cases admitted and treated on the third and fourth days when contrasted with the control cases admitted on those days. The figures thus supply no evidence that the serum exerts a greater beneficial influence when administered early than when administered late. The same inference can be drawn from a comparison of the serum cases among themselves. This last comparison is, however, as has been shown in a previous section of this Chapter, so complicated with fallacies as to be of comparatively small value.

Para. 525 of this Report.

535. It would seem, in view of what has been said above, that we cannot do otherwise than conclude that the statistics furnished by Captain Mason, R.A.M.C., are inconclusive as to the value of the serum employed by him. At the same time, we are disposed to think that the favourable impression which was created on the mind of Captain Mason by his observation of the clinical effects of the serum treatment furnishes a presumption in favour of the utility of Yersin's serum.

Conclusion regarding value of serum treatment in Cutch Mandvi.

536. Finally we reprint from Captain Mason's evidence a table giving details with regard to the cases of plague treated with Yersin's serum in Cutch Mandvi and the neighbourhood.

Details of cases treated with serum at Cutch Mandvi.

Hospital in which treated.	No.	Sex.	Age.	Admitted to Hospital.	How long ill when injected.	Total Quantity injected.	Result.	Bubo.	Remarks.
				1897.			1897.		
Brahmapuri Mandvi.	1	Male	9	30.5	4 days -	90 c.c.	Discharged -	Parotid, left groin, left cervical.	
„	2	„	35	„	4 „ -	105 „	Died, 4.6 -	Right axilla -	Not injected till 4th May.
„	3	Female	15	1.6	1 day -	120 „	„ 3.6 -	„ „	
„	4	Male	8	„	2 days -	210 „	„ 11.6 -	Right inguinal and femoral.	
„	5	Female	32	2.6	2 „ -	220 „	Discharged, 11.6.	Right groin, plague pustule left foot.	
„	6	„	13	„	4 „ -	190 „	Died, 6.6 -	Left femoral, right and left feet swollen, right and left cervical.	
„	7	Male	8	„	1 day -	50 „	„ 4.6 -	Left axilla.	
„	8	Female	40	3.6	2 days -	140 „	„ 5.6 -	Left femoral.	
„	9	„	45	4.6	2 „ -	1?0 „	„ 11.6 -	Right groin.	
„	10	„	29	„	2 „ -	200 „	„ 23.6 -	Left femoral (pneumonia).	
„	11	„	30	„	1 day -	160 „	Discharged, 11.6.	Left groin.	
„	12	„	20	„	1 „ -	100 „	Discharged -	Nil.	
„	13	„	40	„	1 „ -	140 „	Died, 9.6 -	Left femoral.	
„	14	Male	40	4.6	2 days -	230 „	„ 8.6 -	Nil.	
„	15	„	45	5.6	2 „ -	170 „	„ 9.6 -	Right inguinal, suppression of urine.	
„	16	Female	28	6.6	2 „ -	230 „	„ 13.6 -	Right groin (pneumonia).	
„	17	„	25	7.6	3 „ -	140 „	„ 11.6 -	Left groin.	
„	18	„	60	9.6	3 „ -	120 „	„ 12.6 -	Right axilla.	
„	19	Male	60	„	4 „ -	100 „	Discharged, 19.6.	Left groin.	
„	20	Female	21	11.6	3 „ -	40 „	Discharged, 19.6.	Right groin.	
„	21	„	30	15.6	3 „ -	90 „	Discharged -		
„	22	„	10	„	3 „ -	100 „	„ -		
„	23	„	33	„	1 day -	150 „	Died - -		
„	24	„	45	22.6	2 days -	120 „	„ - -	Left submaxillary.	
„	25	Male	55	23.6	1 day -	150 „	„ - -	Right groin.	
„	26	„	50	„	1 „ -	90 „	Discharged -	Left axilla.	
„	27	Female	18	19.6	2 days -	110 „	„ -	Left groin.	
„	28	„	17	„	2 „ -	10 „	„ -	„ „ -	Severe central intoxication. Bilious vomiting. Sputa typical.
„	29	Male	20	3.7	2 „ -	80 „	Died - -	„ „	
„	30	Female	18	5.7	2 „ -	75 „	„ - -	Right axilla.	
„	31	„	12	„	2 „ -	100 „	„ - -	Tenderness both groins.	
„	32	„	105	14.7	4 „ -	70 „	„ - -	Left groin.	
Gundiala	33	„	7	4.8	1 day -	45 „	Discharged, 6.8.	Nil - -	Swelling and tenderness in right groin.
„	34	„	10.	„	1 „ -	50 „	Discharged, 6.8.	„	
„	35	„	40	6.8	1 „ -	100 „	Died, 8.8 -	„	

Hospital in which treated.	No.	Sex.	Age.	Admitted to Hospital.	How long ill when injected.	Total Quantity injected.	Result.	Bubo.	Remarks.
				1897.			1897.		
Gundiala	36	Male	11	6.8	1 day -	30 c.c.	Died, 7.8 -	Nil - -	Passed round worms.
„	37	Female	11	„	1 „ -	120 „	„ 9.8 -	Left inguinal.	
„	38	„	25	4.8	4 days -	95 „	Discharged, 13.8.	Right axilla.	
„	39	„	7	10.8	3 „ -	70 „	Discharged, 13.8.	Left inguinal.	
„	40	Male	14	16.8	1 day -	80 „	Died, 18.8 -	Nil - -	
„	41	Female	11	„	1 „ -	100 „	„ 13.8	Left and right groin, right axilla.	
„	42	Female	9	16.8	1 „ -	30 „	„ 16.8 -	Right groin.	
„	43	„	—	17.8	2 days -	110 „	„ 18·8 -	Nil - -	Pemphigoid rash on face and back.
„	44	Male	30	19.8	2 „ -	130 „	„ 22.8 -	Left inguinal.	
„	45	„	50	„	3 „ -	180 „	„ 24.8 -	Right groin.	
„	46	Female	—	17.8	2 „ -	80 „	„ 20.8 -	„ „	
„	47	Male	28	20.8	2 „ -	130 „	„ 24.8 -	Left inguinal.	
„	48	Female	9	17.8	3 „ -	100 „	„ 22.8 -	Right axilla, right cervical, left cervical, left axilla, right and left inguinal.	
„	49	Male	—	18.8	2 „ -	140 „	„ 21.8 -	Right groin.	
„	50	„	14	13.8	1 day -	120 „	Discharged, 30.8.	Right axilla.	
„	51	Female	10	15.8	1 „ -	100 „	Discharged, 2.9.	Left groin.	
„	52	Male	11	10.8	1 „ -	100 „	Discharged, 30.8.	Right and left groin.	
„	53	„	—	15.8	1 „ -	130 „	Died, 19.8 -	Left axilla and right groin.	
„	54	Female	22	16.8	4 days -	160 „	„ 28.8 -	Nil - - -	Muscular rheumatism, secondary pneumonia.
„	55	„	10	17.8	2 „ -	130 „	Discharged, 2.9.	Right axilla, right inguinal.	Secondary, right leg, and synovitis.
„	56	Male	7	19.8	3 „ -	100 „	Discharged, 2.9.	Left groin and left cervical.	Secondary.
„	57	„	12	20.8	2 „ -	100 „	Died, 25.8 -	Left inguinal.	
„	58	„	9	23.8	1 day -	120 „	Discharged, 2.9.	Left cervical, and tenderness both groins.	
„	59	Female	9	24.8	3 days -	100 „	Discharged, 1.9.	Right inguinal -	Absorbed.
„	60	Male	4	26.8	2 „ -	40 „	Discharged, 2.9.	Nil.	
„	61	Female	13	„	2 „ -	100 „	Discharged, 2.9.	Left axilla.	
„	62	„	40	„	2 „ -	120 „	Discharged, 2.9.	Right inguinal.	
„	63	Male	7	28.8	2 „ -	50 „	Discharged, 2.9.	Right femoral.	
„	64	Female	40	„	2 „ -	130 „	Died, 31·8 -	Right inguinal.	
„	65	Male	12	28.8	2 „ -	45 „	„ 30·8 -	Left groin.	
„	66	Female	—	17.8	2 „	129 „	Discharged, 27.8.	„ „	
„	67	„	—	—	1 day -	130 „	Discharged, 28.8.	Right groin.	
„	68	Male	12	18.8	2 days -	100 „	Discharged, 5.9.	Left axilla.	
„	69	Female	—	25.8	1 day -	160 „	Died, 29.8 -	Nil.	
„	70	Male	—	28.8	3 days -	160 „	„ 29.8 -	„	
„	71	Female	20	27.8	2 „ -	120 „	„ 29.8 -	Tenderness, right groin.	
„	72	„	12	28.8	2 „ -	30 „	„ 30.8 -	Right axilla, with effusion.	
„	73	Male	35	„	2 „ -	160 „	„ 2.9 -	Nil.	
„	74	Female	45	„	2 „ -	160 „	„ 6.9 -	Left groin - -	Popliteal bubo, acute thrombosis of left femoral and popliteal and posterior tibial veins.
„	75	Male	40	2.9	2 „ -	150 „	„ 6.9 -	Left groin.	
„	76	Female	10	6.9	6 „ -	150 „	„ 6.9 -	Primary plague pneumonia.	
„	77	„	24	2.9	4 „ -	120 „	Discharged, 14.9.	Effusion, right axilla.	
„	78	„		„	2 „ -	180 „	Discharged, 13.9.	Nil.	
„	79	„	8	„	3 „	60 „	Discharged, 14.9.	Left inguinal.	
„	80	„	60	5.9	2 „ -	160 „	Discharged, 10.9.	Nil.	
„	81	„	40	„	3 „ -	150 „	Discharged, 15.9.	Left groin.	
„	82	Male	50	„	2 „ -	220 „	Died, 10.9 -	Right axilla.	

Hospital in which treated.	No.	Sex.	Age.	Admitted to Hospital.	How long ill when injected.	Total Quantity injected.	Result.	Bubo.	Remarks.
				1897.			1897.		
Gundiala	83	Female	25	5.9	3 days -	150 c.c.	Died, 8.9 -	Nil - -	Primary plague pneumonia.
„	84	„	18	6.9	1 day -	130 „	„ 8.9 -	Left inguinal.	
„	85	„	14	7.9	1 „ -	60 „	„ 11.9 -	Right groin -	Left sub-poplitea bubo developed 9.9.97.
„	86	„	10	8.9	2 days -	45 „	Discharged, 17.9.	Sub-clavicular -	Bubo, left side, developed.
„	87	„	19	„	3 „ -	90 „	Died, 9.9 -	Left groin - -	Eyes injected.
„	88	„	30	„	2 „ -	110 „	„ 13.9 -	Left axilla.	
„	89	„	40	2.9	3 „ -	120 „	„ 8.9 -	Left inguinal -	Primary plague pneumonia.
Merau	90	Female	—	5.9	1 day -	90 „	Discharged, 18.9.	Left groin	
Gundiala	91	„	35	„	2 days -	140 „	Discharged, 9.9.	Right inguinal.	
„	92	„	9	6.9	2 „ -	60 „	Died, 9.9 -	Right axilla -	With effusion.
„	93	Male	75	„	2 „ -	160 „	„ 8.9 -	Nil.	
Merau	94	„	25	„	2 „ -	150 „	„ 9.9 -	„	
„	95	Female	50	7.9	2 „ -	120 „	„ 9.9 -	Right groin.	
„	96	„	24	8·9	1 day -	170 „	„ 12.9 -	Glands parotid, left.	
„	97	Male	30	11.9	3 days -	80 „	Discharged, 20.9.	Left groin.	
Gundiala	98	„	12	4.9	2 „ -	100 „	Discharged, 23.9.	Right cervical, right inguinal.	Right and left groins developed 9.9.97.
„	99	Female	16	5.9	2 „ -	150 „	Discharged, 23.9.	Right inguinal.	
„	100	„	25	10.9	2 „ -	120 „	Discharged, 21·9.	Right abdominal.	

—(C) *Results obtained by Dr. Simond by the Administration of Yersin's Serum in Karad and Karachi and elsewhere.*

537. The next observations which we have to consider with regard to the effect produced the administration of Yersin's anti-plague serum are those of Dr. Simond. The ra employed were again sera which were prepared at the Pasteur Institute, Paris. vo series of observations were made—the first of these at Karad in September 97, the second at Karachi between the 9th of May and the 6th of June 1898. Particulars regarding Dr. Simond's operations with Yersin's serum.

538. With regard to the observations made at Karad, we learnt from Captain Smith, M.S., particulars concerning 32 hospital patients treated by Dr. Simond. 27 of ese 32 were bubonic cases. Only early cases—that is, cases in which the disease d not lasted more than four days—were chosen for serum treatment. So far as could learn, the results of the serum cases were not compared with those obtained any comparable series of control cases. The general results of the serum cases re as follows :— Observations of Dr. Simond in Karad. 21,106–17.

Cases.	Deaths.	Case-Mortality.
32	21	65·5 per cent.

Failing information as to the case-mortality of a series of comparable control cases, with respect to the gross case-mortality of the hospital for the period during ich the serum was used, we are thrown back on the uncertain method of com-rison between the serum cases treated respectively in the early and the later stages the disease. The results of this comparison, when we take into consideration only bubonic cases, were as follows :—

	1st Day.		2nd Day.		3rd Day.		4th Day.	
	Cases.	Deaths.	Cases.	Deaths.	Cases.	Deaths.	Cases.	Deaths.
uber of cases and deaths	2	1	17	12	5	4	3	2
e-mortality - -	1 in 2		1 in 1·4		1 in 1·2		1 in 1·5	

It is obvious that the case-mortalities, except possibly in the case of patient admitted on the 2nd day, can hardly be regarded as representative, inasmuch a they have been calculated on a very small number of cases. Again, examination o the records shows that the cases treated on the several days were, as far as we ca judge, not comparable among themselves from the point of view of severity.

Details of cases treated with serum at Karad.

539. Finally we reproduce from the evidence of Captain Smith, I.M.S., a tabl showing the details of cases treated by Dr. Simond at Karad :—

21,109.

No.	Age.	Sex.	Nature of Disease.	Day of Disease on Admission.	Date of Admission.	Date of Death.	Amount of Serum injected.	Duration of Illness in Fatal Cases.	Date of Discharge in Cases that recovered
1	9	Female	Right inguinal bubo and pneumonia. Very serious case.	2nd day	5.9.97	7.9.97	40 c.c. in 2 doses -	4 days.	—
2	7	Male	Axillary bubo and pneumonia on right side. Very serious case.	2nd day	5.9.97	5.9.97	15 c.c. 6 hours before death.	2 days.	—
3	32	„	Right inguinal bubo and swelling of parotid gland. Very serious case.	2nd day	5.9.97	6.9.97	60 c.c. in 2 doses -	3 days.	—
4	25	Female	Axillary bubo and cervical bubo. Very serious case.	3rd day	6.9.97	8.9.97	60 c.c. in 2 doses -	5 days.	—
5	50	Male	Double pneumonia. No buboes. Very serious case.	2nd day	6.9.97	9.9.97	90 c.c. in 4 doses -	5 days.	—
6	30	Female	Pneumonia on right side and small inguinal bubo. Aphasia. Very serious case.	2nd day	6.9.97	7.9.97	40 c.c. in 2 doses -	3 days.	—
7	39	Male	Right inguinal bubo. Mild case.	4th day	6.9.97	—	20 c.c. injected at patient's request, without there being any necessity for it.	—	3.10.97
8	50	Female	Cervical inguinal and axillary buboes. Very serious case.	3rd day	6.9.97	13.9.97	50 c.c. in 3 doses -	10 days.	—
9	14	„	Right inguinal bubo. Coma. Very serious case.	2nd day	7.9.97	13.9.97	30 c.c. in 2 doses -	8 days.	—
10	50	„	Cervical buboes and pneumonia. Very serious case.	3rd day	7.9.97	7.9.97	30 c.c. in 1 dose, 7 hours before death.	3 days.	—
11	21	„	Left inguinal bubo. Stupor. Aphonia. Very serious case.	2nd day	7.9.97	—	60 c.c. in 3 doses -	—	15.9.97
12	20	„	Cervical and inguinal bubo. Coma. Very serious case.	2nd day	7.9.97	10.9.97	80 c.c. in 3 doses -	5 days.	—
13	38	Male	Intestinal plague. No buboes or pneumonia. Aphasia. Very serious case.	3rd day	8.9.97	10.9.97	20 c.c. - -	5 days.	—
14	30	„	No bubo or pneumonia. High fever. Doubtful case.	3rd day	8.9.97	—	40 c.c. in 2 doses -	—	6.10.97
15	24	Female	Axillary bubo and deep stupor. Very serious case.	2nd day	9.9.97	11.9.97	40 c.c. - -	4 days.	—
16	18	„	Inguinal bubo and absolute stupor. Very serious case.	2nd day	9.9.97	9.9.97	30 c.c. - -	2 days.	—
17	20	Male	Inguinal bubo. Delirium. Very serious case.	4th day	10.9.97	15.9.97	? - - -	9 days.	—
18	25	Female	Inguinal bubo. Case of average severity.	2nd day	10.9.97	—	40 c.c. in 2 doses -	—	5.10.97
19	20	„	Inguinal bubo. Very serious case.	2nd day	11.9.97	15.9.97	30 c.c. - -	6 days.	—
20	60	Male	Right inguinal bubo. Case of average severity.	2nd day	12.9.97	—	40 c.c. in 2 doses -	—	7.10.97
21	20	Female	Right and left inguinal buboes. Very serious case. Stupor and aphonia.	2nd day	14.9.97	—	80 c.c. in 3 doses -	—	17.11.97
22	18	„	Left inguinal bubo. Coma. Very serious case.	4th day	14.9.97	15.9.97	30 c.c. - -	5 days.	—
23	30	Male	Inguinal bubo. Serious case.	2nd day	14.9.97	—	90 c.c. in 4 doses	—	12.10.97
24	50	„	No bubo. No pneumonia. High fever (105°). Doubtful case, possibly mild intestinal plague.	?	15.9.97	—	40 c.c. in 2 doses -	—	17.10.97

No.	Age.	Sex.	Nature of Disease.	Day of Disease on Admission.	Date of Admission.	Date of Death.	Amount of Serum injected.	Duration of Illness in Fatal Cases.	Date of Discharge in Cases that recovered.
25	23	Male	Intestinal plague, without buboes or pneumonia. Very serious case, with stupor and delirium.	2nd day	16.9.97	22.9.97	70 c.c. in 3 doses -	8 days.	—
26	25	Female	Left inguinal bubo. Very serious case. Stupor and aphonia. Convulsions.	1st day	17.9.97	—	80 c.c. in 3 doses -	—	6.11.97
27	28	Male	Right and left cervical buboes. Very serious case.	1st day	18.9.97	21.9.97	60 c.c. in 3n doses -	4 days.	—
28	55	Female	Axillary bubo. Very serious case, with extreme heart weakness.	4th or 5th day.	19.9.97	19.9.97	30 c.c. a few hours before death.	4 or 5 days.	—
29	44	„	Small, painless, left inguinal bubo. Case of slight severity.	3rd day	21.9.97	—	40 c.c. in 2 doses -	—	22.10.97
30	40	„	Right inguinal bubo. Case of average severity.	2nd day	22.9.97	30.9.97	40 c.c.	10 days.	—
31	32	„	Left axillary bubo. Mild case.	2nd day	22.9.97	—	10 c.c. injected without necessity, at patient's request.	—	24 9.97
32	14	Male	Right inguinal bubo. Coma. Very serious case.	3rd day	24.9.97	24·9.97	30 c.c. a few hours before death.	3 days.	—

Dr. Simond's summary of his results at Karad and elsewhere, up to February 1898.

540. At this point, before we pass to consider the observations which were made at Karachi in May and June 1898, it will be convenient briefly to consider Dr. Simond's results up to February 1898, the date at which this observer furnished to the Surgeon-General with the Government of Bombay a report which is printed as Appendix VII. of our Proceedings.

From the report in question we learn that, up to the date when it was made, in all some 300 patients had been treated by Dr. Simond. These cases were drawn from Karad, Bombay, Cutch Mandvi, and Mumbra. The case-mortality of these 300 patients is given as 58 per cent. It is contrasted with the mean mortality—75 per cent.—"which I" (the quotation is taken from Dr. Simond's translated report) "registered in the same hospital" (hospitals, we presume is intended) "for the cases not treated with serum." Recognising that this standard of comparison is hardly an appropriate one, Dr. Simond institutes a further comparison between (*a*) "a class of patients treated from the first to the fourth day," and (*b*) 90 patients "at the same time and in the same hospital (sic)—of whom a part were treated at Cutch Mandvi and a part at Bombay." Dr. Simond states that there was a case-mortality of 66 per cent. in the former, that is in the serum-treated cases, as compared with a case-mortality of 82 per cent. in the latter, that is in the untreated cases. A further comparison is instituted between the Musalmans admitted to hospital at Karad and Mumbra on the first and second days of the disease and treated with serum, and control cases admitted on the corresponding days. Lastly, a comparison is made between Hindus admitted and treated with serum on the first two days of their illness with Hindus admitted in the same stages of the disease and untreated.

I. 349.

The results of these comparisons are summarised by Dr. Simond in the statement that the difference in favour of the sero-therapic treatment is not very great, it oscillates round 15 per cent., but that is not a quantity to be despised, if one remembers that no other treatment has produced up to date any result whatsoever." With last commentary we find ourselves in sympathetic agreement. At the same time, seeing that the difference which is claimed in favour of the serum treatment is "not very great," we think that this difference ought, at least, to have been established by more convincing statistical methods.

Loc. cit.

Observations made by Dr. Simond on serum treatment at Karachi. Appendix XLIII. in Vol. II. of these Proceedings.

541. We pass from this to the consideration of the observations which Dr. Simond made in Karachi in May and June 1898. With regard to the results of the serum treatment in this City, our sources of information are, on the one hand, a report of Dr. Simond to the Surgeon-General with the Government of Bombay, and, on the other hand, the evidence of Dr. Nazareth, who was in charge of the Seth Vishandas Plague Hospital in Karachi, where the majority of Dr. Simond's patients were treated.

We have carefully compared these two sources of information and find that they agree in all essential particulars, though there are a few obvious clerical errors in the records as they have reached us. We learn from Dr. Simond's figures—which may be taken as the basis of our study—that 70 cases—we omit from consideration four cases (the cases numbered 32, 36, 37, 38 in the lists attached to his report) the event of which has been left unrecorded—were treated by Yersin's serum in Karachi. Most of these cases were treated in the Seth Vishandas Hospital.

II., 461–3.

System on which patients were selected for treatment.

The patients were, as Dr. Nazareth informed us, taken for serum treatment "purely on the fact of their being recent." If we go by the record furnished, it would appear that the preponderating majority of the patients treated had only arrived at the second day of the disease. Only three patients are recorded as having reached the fifth day of the disease at the time when the treatment was inaugurated.

13,280.

Types of the disease in the patients treated with serum.

Of the cases tabulated by Dr. Simond, 51 were cases of uncomplicated bubonic plague, 9 others were cases of bubonic plague complicated with secondary pneumonic symptoms, 7 being cases of primary pneumonia, and the remaining 3 being apparently cases of septicæmic plague. Many of the cases treated were, when the treatment commenced, in a very grave condition, not a few being comatose, and some already at the point of death.

Statistics regarding the effect of serum treatment in Karachi.

542. The general results obtained are set forth in the following table:—

Cases.	Deaths.	Case Mortality.
70	37	53 per cent.

The case-mortality of these patients is not compared by Dr. Simond with the case-mortality of any series of controls. When the fact is taken into consideration that a large number of pneumonic cases were included in the group, and that all convalescent patients were excluded, it would seem reasonable to conclude that the mortality was below the average of that usually met with in plague.

With a view to obtaining more definite data upon which to base a conclusion with regard to the effect of the serum cases we obtained from Dr. Nazareth, on the one hand, the figures which set forth the comparative case-mortality for the treated and untreated patients in the Seth Vishandas Hospital, and, on the other hand, the gross case-mortality for this hospital during the period of serum treatment, and in the periods which preceded and followed the period in question. Both sets of figures have been incorporated in the following table:—

Period anterior to Adoption of Serum Treatment (*from beginning of the Epidemic to the 9th of May* 1898).			Period of Serum Treatment (*from the 9th of May to the 6th of June* 1898).				Period subsequent to Discontinuation of Serum Treatment (*from the 6th of June* 1898 *to the end of the Epidemic*).		
Cases.	Deaths.	Case-Mortality.	——	Cases.	Deaths.	Case-Mortality.	Cases.	Deaths.	Case-Mortality.
—	—	—	Ordinary treatment	74	55	74 %	—	—	—
—	—	—	Serum treatment	47	22	47 %	—	—	—
288	202	70 %		121	77	63 %	38	21	55 %

13,272–9.

Consideration of the above statistics.

Taking first the figures referring to the gross mortality in the hospital, we notice that the case-mortality declined considerably in the period of serum treatment as compared with the period before the serum treatment. Moreover, this downward movement in case-mortality continued in the subsequent period when the serum was no longer available. The general decline in the case-mortality must, therefore, have been due to an alteration in the type of the disease, and, indeed (as will be seen on reference to the curve of the epidemic in Karachi in the chart printed in paragraph 329 of this Report), the epidemic was very rapidly declining in the latter half of May and the beginning of June 1898.

Turning now to the other aspect of the table, it will be seen on consideration of the figures relating to the comparison between the case-mortality of the serum cases with that of the residuum of cases, that the former compares very favourably with the latter. In the absence, however, of detailed information, we cannot arrive at a conclusion as to whether the average severity of the residuum of cases treated in the hospital can be taken as comparable to the average severity of the serum cases. While the fact that all the convalescent cases were included in the residuum of cases would seem to suggest that the serum cases were cases of greater average severity than the residuum of cases, the fact that the mortality of the residuum of cases during the serum period is somewhat higher than the case-mortality of the previous period, and the fact that it was disproportionately higher than the case-mortality of the succeeding period, would seem to suggest that the residuum of cases may have included a large number of moribund or hopeless cases. We are thus, as has been said before, precluded from forming a definite conclusion as to the effect exerted by the serum on the case-mortality.

543. Having been frustrated in this direction, we may turn and employ the only other statistical methods still open to us with a view to gauging the efficacy of the serum. In other words, we may compare, as has been done by Dr. Simond, the serum cases among themselves with a view to determining whether the case-mortality was reduced among those treated early as compared with those treated late. The results of this comparison are set forth in the following table:—

Effect of serum treatment at Karachi according to the date of the disease on which it was commenced.

I. 460.

	1st Day.		2nd Day.		3rd Day.		4th Day.		5th Day.		6th Day.	
	Cases.	Deaths.	Cases.	Deaths.	Cases.	Deaths.	Cases.	Deaths.	Cases.	Deaths.	Cases.	Deaths.
Number of cases and deaths	9	4	28	14	19	8	10	8	3	2	1	1
Case-Mortality - -	1 in 2·2		1 in 2		1 in 2·4		1 in 1·2		1 in 1·5		1 in 1	

In this list there are, however, included five cases who were almost in articulo mortis when they received the serum besides 10 cases of septicæmic and primary pneumonic plague, who all died. If we eliminate these, as we perhaps should, the case-mortality of the patients treated on sucessive days reads as follows:—

	1st Day.		2nd Day.		3rd Day.		4th Day.		5th Day.		6th Day.	
	Cases.	Deaths.	Cases.	Deaths.	Cases.	Deaths.	Cases.	Deaths.	Cases.	Deaths.	Cases.	Deaths.
	6	1	23	9	16	5	6	4	3	2	1	1
Case-Mortality - -	1 in 6		1 in 2·5		1 in 3·2		1 in 1·5		1 in 1·5		1 in 1	

The comparison is even more favourable than the comparison set forth in the previous table to Dr. Simond's contention that the serum was more effective when administered in the early stages of the disease than when administered in the later stages. We cannot, however, overlook the fact that we were told by Dr. Nazareth that "everyone admitted to hospital stated that they were recently infected. They never admitted that they had been infected more than one day for fear of prosecution by the authorities." In view of this statement of Dr. Nazareth, it unfortunately becomes impossible to attribute much importance to the statistical comparison made by Dr. Simond between the case-mortality of patients treated early and of patients treated late with serum.

13,287.

544. Having given the statistical results of Dr. Simond's observations at Karachi, we may now pass on to consider the effect of the serum on the various symptoms of the disease.

Effect of serum injection at Karachi on the symptoms of the disease.

On a study of the temperature records contained in notes furnished to us by Dr. Nazareth, we find that in fatal cases the treatment had not the slightest permanent effect on the temperature. This oscillated during the treatment, almost without exception, between 102° and 104° F. In certain cases, however, the injection of serum

Effect of serum treatment on temperature.

I. 457–9.

Paragraph 530 in this Chapter.

was followed by a fall of one or two degrees in the temperature. It is noticeable that, as was the case also in our own observations, the fall occurred nearly always at night after the evening injection.

The action of serum on the temperature was not more pronounced in the cases which ended in recovery. In these the temperature, apparently, did not return to the normal any sooner than in many cases treated by ordinary methods. Indeed, a careful examination of the notes of 11 cases of patients who recovered after serum treatment shows no essential difference between these cases and those who had recovered under ordinary methods of treatment.

Effect of serum on pulse and other symptoms. 13,318, 13,319, 13,323.

The pulse, on the other hand, often became less rapid after an injection of serum. The tongue also often became clean and moist. A general improvement in the symptoms also manifested itself in many cases, this not being confined to cases which terminated in recovery.

Effect of serum on septicæmic cases and cases of primary and secondary plague pneumonia.

The serum does not appear to have the slightest influence on septicæmic or pneumonic cases. Almost all of these succumbed. Hardly more successful was the serum treatment in cases of secondary pneumonia. Again, the serum does not appear to prevent the appearance of secondary pneumonia; in some of the cases pneumonia appeared two to four days after the beginning of serum treatment.

Effect of serum treatment on convalescence.

Lastly, convalescence does not appear to have been in any way shortened or affected by the serum treatment. In several cases it is distinctly noticed that it was lingering and slow, and in four cases it was complicated by rheumatic articular pains. Suppuration occurred in several of the bubonic cases that recovered.

V.—(D.) *Observations made by the German Plague Commission.*

Details regarding number of cases treated by the German Plague Commission and the results.

Report of the German Plague Commission, pp. 226–31.

545. The last observations which we have to consider in connexion with the effect of the Yersin's serum as prepared at the Pasteur Institute in Paris, are the observations of the German Plague Commission. We would note that these observations all referred to cases treated with "the older" samples of anti-plague serum received by the German Plague Commission, which tests on animals, as reported in a previous paragraph of this Chapter, had shown to be much less efficacious than the later samples of serum received by the Commission. The observations in question relate to 26 cases of plague—we are not told upon what principle these cases were selected—which were treated with the serum in the Bombay hospitals. Of the 26 cases, 13 died and 13 recovered. These results are not compared by the German Plague Commission with those of any parallel series of controls, but we would note that the case-mortality is below the average usually met with in plague.

Comparison between cases treated early and late by the German Plague Commission

The comparison between the cases treated early with serum and those treated late is that shown in the following table:—

	1st Day.		2nd Day.		3rd Day.	
	Cases.	Deaths.	Cases.	Deaths.	Cases.	Deaths.
	11	6	9	5	6	2
Case-Mortality - - - -	1 in 1·8		1 in 1·8		1 in 3	

Conclusions arrived at by the German Plague Commission.

Loc. cit.

546. The conclusion which is arrived at by the German Plague Commission, by comparing the course and character of plague in the serum cases with that in cases not thus treated is summed up by them in the statement that "the only certain inference which can be drawn is that the injections of serum have had no perceptible immediate or remote harmful results. One finds," write the Commissioners, "that the character and duration of the course of the disease and the character and the duration of the convalescence varies within the widest limits in the case of those treated with serum. The duration of convalescence, as judged by the stay in hospital, in the case of patients treated with serum, averaged between seven and 51 days, with an average of 26 days, and is not materially different from the duration which we have calculated in 90 patients who were admitted to the Parel Hospital on the first and second day of the disease, and who recovered without treatment with serum. In them the limits were three to 110 days, the stay of the patients in the hospital averaging 20½ days."

V.—(E.) *Preventive Inoculation with Yersin's Serum.*

Preventive inoculation with Yersin's serum.

547. So far we have dealt with the results obtained by the administration of Yersin's serum to persons actually suffering from plague. The inoculation of this serum has been recommended also as a preventive measure in the case of healthy persons who are likely to come in contact with plague, though it is not claimed that it is likely to afford protection for more than 10 days.

Injection of serum as a preventive. 27,130.

It was thus employed in Cutch Mandvi, where one thousand and forty-four persons of all ages were inoculated with 10 c.c. of the serum as a prophylactic during the epidemic of plague and of these five are known to have contracted plague and two died. In the two cases which were fatal the disease appeared 12 days after inoculation, in two of the other cases the disease appeared 32 days after inoculation, and in the remaining case it appeared 45 days after inoculation.

App. VII., I., 349–50.

A similar application of the serum was made by Dr. Simond in the places where he used the serum for curative purposes. In the report, from which we have already quoted in a previous section, Dr. Simond records that he had up to February 1898 used the serum as a prophylactic in the case of 1,160 persons. It was found impracticable to follow up the subsequent history of all these patients and to determine whether they were afterwards brought into contact with infection and successfully resisted it. Dr. Simond had, however, heard of nine of his patients who contracted plague, the first of them contracting it 14 days after inoculation with the serum. He reports that the case-mortality in these attacks was not in any way reduced. Captain Smith, I.M.S., III. 137. has supplied us with certain further information with regard to the subsequent history of Dr. Simond's patients at Karad and Masur. Twenty-nine out of the inoculated persons, whose number is not accurately known, were known to Captain Smith as having been afterwards attacked by plague. The following table gives particulars with regard to the event of the disease and the date after inoculation at which 21 of these patients were attacked :—

1	patient	was	attacked	with plague	6	days after inoculation	and died.
1	„	„	„	„	12	„ „	„
2	patients	were	„	„	16	„ „	„
7	„	„	„	from	20 to 30	„ „	6 died.
7	„	„	„	„	30 to 40	„ „	all died.
1	patient	was	„	plague	40	„ „	and died.
1	„	„	„	„	53	„ „	„
1	„	„	„	„	70	„ „	„

Vol. I., p. 350.

With regard to the effects produced by the serum Dr. Simond makes the following note :—"In 21 cases I have noticed transient symptoms, such as fever, pain in the joints, and swelling of glands. I have been able to satisfy myself that these symptoms, which have been of short duration, and have never obliged the persons vaccinated to remain in bed, arise from the fact that the horse which had furnished the serum had been bled too soon after an injection of virus, at a time that his blood still had feebly toxic properties."

V.—(F.) *Observations of the Russian Plague Commission on the Effect of the Administration of an Anti-Plague Serum prepared in St. Petersburg.*

Observations by the Russian Plague Commission on serum prepared in St. Petersburg.

App. XXII., I., 497–511. 7222–32. 1322–33

548. We pass now from the consideration of the results obtained by the administration of Yersin's serum as prepared at the Pasteur Institute, Paris, to consider a series of systematic observations made by the Russian Plague Commission in March and April 1897, with a view to determining the therapeutic effect of serum that was prepared in the Imperial Institute of Preventive Medicine, St. Petersburg, on similar lines to those followed in Paris. The results of the Russian observers have already been placed on record in both Russian and English Medical Journals and evidence regarding them was given before us by Dr. Clemow and Captain Jennings, I.M.S.

Selection of cases and mode of administration of serum.

549. The cases on which the utility of the serum treatment was tested were not in any way selected. Every alternate patient who was admitted during the period of observation into the Parel Hospital, Bombay, was treated with serum, the treatment being

withheld only when the diagnosis of plague was uncertain or when the patient was actually on the point of death. The series of observations embraced 50 patients
1323. treated with serum and the 50 alternate patients who served as controls. "At first," as we learn from Captain Jennings, I.M.S., "the quantity of serum injected was 20 c.c. daily in one dose. Later on 10 c.c. were injected in the evening as a second dose, and still later the morning dose was increased to 30 c.c. These doses,c not having any effect, they were further increased to 40 c.c. in the morning and 20 c.c. at night. More than half the cases were treated with the latter doses."

Consideration of cases treated by the Russian Plague Commission.

550. The following table synopsises the results of treatment, and affords a comparison between cases treated with serum and cases which received only ordinary treatment, the figures being furnished to us by Dr. Clemow, who has supplied us with detailed notes regarding each of the serum cases:—

Results of serum injection on case-mortality.

7224.

—	Cases.	Deaths.	Case-Mortality.
Serum treatment - -	50	40	80 per cent.
Ordinary treatment - -	50	40	80 per cent.

It will be seen that the serum had no effect whatever on the case-mortality.

Effects of serum injection according to the date of the disease when injection commenced.

A comparison may also be instituted between the cases admitted and treated with serum in the early stage of plague with the cases admitted and treated with serum in the later stage of the disease. The comparison in question—we omit from consideration three cases whose history could not be ascertained—is as follows:—

—	1st Day.		2nd Day.		3rd Day.		4th Day.		5th Day.		After 5th Day, and undetermined.	
	Cases.	Deaths	Cases.	Deaths.	Cases.	Deaths.	Cases.	Deaths.	Cases.	Deaths.	Cases.	Deaths.
	6	5	19	15	5	4	6	5	5	5	9	6
Case-Mortality - -	1 in 1·2		1 in 1·3		1 in 1·2		1 in 1·2		1 in 1		1 in 1·5	

It will be seen that the comparison furnishes no evidence that the serum was more effective when administered early than when administered late.

Effect of serum injection on symptoms.

7231.

With regard to the effect of the serum on the symptoms of the patients treated, the following points may be noticed. In a few instances a fall of temperature followed the injections; such falls of temperature are, however, far from uncommon in plague. Dr. Clemow, the Medical Officer in charge of the hospital, was of opinion that they could not be attributed with any degree of certainty to the serum injections. Albuminuria, delirium, nervous symptoms, convulsions, vomiting, hiccough, diarrhœa, and all other symptoms, occurred indifferently in the serum patients and the controls. The serum thus appeared to have no effect upon the ordinary complications of plague. Secondary pneumonia occurred in two cases which had received serum; these ended fatally. A small patch of pneumonia also developed in a third serum patient who recovered. In two cases acute keratitis, passing on to general inflammation of the eye-ball, developed. Erysipelas occurred in two cases. Suppuration appeared to be of slightly more common occurrence in the buboes of the serum cases than in the control cases; the figures, however, being small, very little weight can be attached to them.

Conclusions regarding the value of the serum used by the Russian Plague Commission.

1323–4; 7223.

551. It is impossible, in view of the above, to arrive at any other conclusion with regard to the effect of the serum employed than that which was arrived at by Dr. Jassenski, of the Russian Plague Commission, who conducted the test of the serum, and which was concurred in by Captain Jennings and Dr. Clemow, namely, that the serum was "an absolutely indifferent substance, with no influence either for good or evil, on the course of acute plague."

Abstract of cases treated with serum by the Russian Plague Commission.

552. We subjoin an abstract of Dr. Clemow's notes of the cases treated with the serum.

Cases of Plague treated at the Parel Plague Hospital, Bombay, with Anti-Plague Serum prepared in the Imperial Institute of Preventive Medicine at St. Petersburg.

I. 497–511.

No. of Case.	Sex.	Age.	Nature of Illness.	Day of Disease on which first Injection of Serum was made.	Amount of Serum injected.	Length of Illness, in Days, in Fatal Cases.	Remarks.
I.	Male	21	Bubo in right groin	5th day	100 c.c. in 5 doses of 20 c.c. per diem.	16 days	Admitted to hospital on the 3rd day of illness, but not treated with serum till 2 days after.
II.	Male	22	Bubo in right groin, and bronchitis.	2nd day	160 c.c. in doses of 20 c.c. per diem.	—	Recovered. The temperature did not come down till the 10th day of the disease. The bubo had to be incised.
III.	Male	21	Bubo in left femoral region.	15 hours after onset.	40 c.c. in 2 doses of 20 c.c. per diem.	Less after 4 days after onset of symptoms.	The patient was, apparently, a mild case at the beginning.
IV.	Female	65	No bubo at first; left femoral bubo appeared on day after injection of serum.	2nd day	40 c.c. in 2 doses of 20 c.c. per diem.	4 days	
V.	Male	30	Right inguinal bubo	5th day	60 c.c. in doses of 20 c.c. per diem.	8 days	
VI.	Male	25	Bubo in right axilla, and bronchitis.	?	60 c.c. in doses of 20 c.c. per diem.	?	The patient died 52 hours after the commencement of serum treatment.
VII.	Male	32	Bubo in inguinal region	2nd day	90 c.c. in doses of 30 c.c. on the 3rd day of treatment, and 20 c.c. per diem on the other days.	7 days	
VIII.	Male	19	Right femoral bubo	2nd day	290 c.c. (*See* Remarks.)	—	Recovered. Patient's illness lasted 15 days. He had 50 c.c. in 2 doses of 30 c.c. and 20 c.c. on the 2nd day of illness. The bubo nevertheless increased, and 30 c.c., were injected on the 4th day. 20 c.c. on the 5th day, 30 c.c. on the 6th day, 20 c.c. on the 7th day, and 40 c.c. on the 8th day. The bubo had to be incised. An urticarial rash appeared on the 8th day, and disappeared on the 9th.
IX.	Female	12	Right inguinal bubo	6th day	20 c.c. in 1 dose 35 minutes before death.	6 days	
X.	Male	22	Left inguinal bubo	2nd day	80 c.c. in doses of 30 c.c. and 20 c.c. on the 1st day of treatment, and of 30 c.c. on the 2nd day.	3 days	
XI.	Male	9	Left axillary bubo	20 hours after onset.	50 c.c. in doses of 20 c.c. and 10 c.c. on the 1st day of treatment, and of 20 c.c. on the 2nd day.	2 days	
XII.	Male	25	Bubo in right groin and right side of neck.	2nd day	80 c.c. in doses of 30 c.c. on the 1st day of treatment, and of 20 c.c. and 30 c.c. on the 2nd day.	4 days	
XIII.	Male	23	Admitted with left axillary bubo; on the 7th day of illness a bubo appeared in the left groin.	2nd day	340 c.c. in doses of 30 c.c. and 20 c.c. on the 1st day of treatment, 20 c.c. on the 2nd day, 30 c.c. on each of the 3rd, 4th, and 5th days, 40 c.c. on each of the 6th, 7th, 8th, and 9th days, and 20 c.c on the 10th day of treatment.	11 days	
XIV.	Female	19	Bubo in left groin	2nd day	150 c.c. in doses of 30 c.c. on the 1st day of treatment, 60 c.c. (in equal doses) on the 2nd day, and 20 c.c. on each of the 3 succeeding days of treatment.	7 days.	
XV.	Female	35	Bubo in left groin	4th day	60 c.c. in one dose of 30 c.c. on the 1st day of treatment, and doses of 20 c.c. and 10 c.c. on the 2nd day.	6 days.	Had been confined four months beforehand.
XVI.	Female	12	Bubo in right groin	1st day	30 c.c. in one dose	2 days	
XVII.	Female	20	Left cervical bubo	7th day	110 c.c. in doses of 30 c.c. and 20 c.c. on the 1st day of treatment, and of 30 c.c. on each of the next 2 days.	10 days.	
XVIII.	Female	30	Right cervical bubo, with lung symptoms.	15th day after appearance of bubo, and 4th day after onset of fever.	60 c.c. in doses of 30 c.c. per diem.	5 days after onset of fever.	
XIX.	Female	9	Right femoral bubo	2nd day	60 c.c. in doses of 20 c.c. per diem.	—	Recovered. The bubo had to be opened
XX.	Male	40	Bubo in left groin	2nd day	90 c.c. in doses of 30 c.c. and 20 c.c. on the 1st day, and of 40 c.c. on the 2nd day of treatment.	3 days.	
XXI.	Male	13	Bubo in left groin	2nd day	30 c.c. in one dose	2 days.	

No. of Case.	Sex.	Age.	Nature of Illness.	Day of Disease on which first Injection of Serum was made.	Amount of Serum injected.	Length of Illness in Days, in Fatal Cases.	Remarks.
XXII.	Male	45	Left inguinal bubo. A bubo appeared in the right groin about a fortnight after serum treatment commenced.	4th day	160 c.c. in doses of 30 c.c. and 20 c.c. on the 1st day of treatment, 30 c.c. on the 2nd day, and 40 c.c. on each of the next two days.	—	Recovered. Patient was over 40 days in hospital. The bubo did not suppurate.
XXIII.	Male	20	Left inguinal bubo	1st day	340 c.c. in doses of 40 c.c. on each of the first 8 days of treatment except the 5th, when no injection was made, and of 20 c.c. on the 9th, and of 40 c.c. on the 11th day of treatment.	—	Recovered. The bubo suppurated. Patient was nearly two months in hospital.
XXIV.	Male	25	Right femoral bubo	2nd day	80 c.c. in two doses of 40 c.c. per diem.	4 days.	
XXV.	Male	14	Right femoral bubo and lung symptoms.	8th day	200 c.c. in doses of 40 c.c. and 20 c.c. on the 1st day of treatment, of 40 c.c. on each of the 2nd, 3rd and 5th days, and of 20 c.c. on the 8th day of treatment.	—	Recovered. Patient was nearly two months in hospital. The bubo was opened.
XXVI.	Male	30	Bubo in left groin	4th day	220 c.c. in doses of 40 c.c. on each of the first 5 days of treatment and of 20 c.c. on the 6th day.	13 days.	
XXVII.	Male	25	Right inguinal bubo	4th day	40 c.c. in one dose	5 days.	
XXVIII.	Male	25	Bubo in left groin	2nd day	80 cc. in two doses of 40 c.c. per diem.	3 days.	
XXIX.	Male	16	Left inguinal bubo	?	120 c.c. in doses of 40 c.c. and 20 c.c. on each of the days of treatment.	?	Patient died on the 2nd day after beginning of the serum treatment. He had been inoculated five days before death with Haffkine's prophylactic fluid.
XXX.	Male	27	Right axillary bubo	3rd day	140 c.c. in doses of 40 c.c. and 20 c.c. on each of the first 2 days and of 20 c.c. on the 3rd day of treatment.	—	Recovered. Patient had a severe accident while in hospital. He left hospital 23 days after entering it.
XXXI.	Male	25	Bubo in left groin	3rd day	60 c.c. in doses of 40 c.c. and 20 c.c. on the 1st day of treatment.	4 days	
XXXII.	Male	19	Bubo in right groin	3rd day	180 c.c. in doses, on each of the 3 days of treatment, of 40 c.c. and 20 c.c.	6 days	
XXXIII.	Male	67	Bubo in right groin	2nd day	250 c.c. in doses of 30 c.c. and 20 c.c. on each of the first 3 days of treatment, of 40 c.c. and 20 c.c. on the 4th day, and of 40 c.c on the 5th day.	8 days	
XXXIV.	Male	23	No bubo, sibilant rhonchi.	4th day	100 c.c. in doses of 40 c.c. and 20 c.c. on the 1st day, and of 40 c.c. on the 2nd day of treatment.	6 days	
XXXV.	Male	50	Enlargement of left supratrochlear gland and swelling of upper arm.	3rd day	100 c.c. in doses of 40 c.c. on the 1st day, and of 40 c.c. and 20 c.c. on the 2nd day of treatment.	6 days	
XXXVI.	Male	35	Left femoral bubo	2nd day	140 c.c. in doses of 40 c.c. and 20 c.c. on the 1st day, and of 40 c.c. on each of the next 2 days of treatment.	—	Recovered. Patient was fit for discharge 6 weeks after his admission to hospital.
XXXVII.	Male	22	Bubo in left groin	5th day	100 c.c. in doses of 40 c.c. on the 1st day, and of 40 c.c. and 20 c.c. on the 2nd day of treatment.	7 days	
XXXVIII.	Female	7½	Right cervical bubo	9th day	20 c.c. in two doses of 5 c.c. on each of the 1st and 2nd days of treatment.	—	Recovered. The bubo suppurated. Patient was 33 days in hospital.
XXXIX.	Male	25	Bubo in right groin	2nd day	100 c.c. in doses of 40 c.c. and 20 c.c. on the 1st, and of 40 c.c. on the 2nd day of treatment.	3 days	
XL.	Male	22	Right femoral bubo	6th day	240 c.c. in doses of 40 c.c. and 20 c.c. on each of the first 4 days of treatment.	—	Recovered. Patient was 36 days in hospital.
XLI.	Male	12	Lung symptoms on admission but no bubo; a left axillary bubo appeared the day after treatment commenced.	1st day	60 c.c. in doses of 20 c.c. on the first day, of 20 c.c. and 10 c.c. on the 2nd day, and of 10 cc. on the 3rd day of treatment.	3 days	
XLII.	Male	30	Bubo in left groin	2nd day	40 c.c. in one dose of 40 c.c. 3½ hours before death.	2 days	
XLIII.	Male	21	Bubo in right groin	3rd day	120 c.c. in doses of 40 c.c. and 20 c.c. on each of the days of treatment.	6 days	
XLIV.	Male	28	Bubo in left groin	1st day	60 c.c. in doses of 40 c.c. and 20 c.c. on the day of admission to hospital.	4 days	
XLV.	Male	28	Right femoral bubo	5th day	300 c.c. in doses of 40 c.c. and 20 c.c. on each of the 5 days of treatment.	10 days	
XLVI.	Male	23	Right femoral bubo	5th day	120 c.c. in doses of 40 c.c. and 20 c.c. on each of the 2 days of treatment.	7 days	

No. of Case.	Sex.	Age.	Nature of Illness.	Day of Disease on which first Injection of Serum was made.	Amount of Serum injected.	Length of Illness, in Days, in Fatal Cases.	Remarks.
XLVII.	Male	33	Left femoral bubo -	2nd day	60 c.c. in doses of 40 c.c. and 20 c.c. on day of admission to hospital.	2 days	
XLVIII.	Female	25	No bubo at time of admission; a bubo appeared the day after admission.	?	100 c.c. in doses of 40 c.c. and 20 c.c. on the 1st day, and of 40 c.c. on the 2nd day of treatment.	?	Patient died two days after treatment commenced.
XLIX.	Male	22	Bubo in left groin -	4th day	120 c.c. in doses of 40 c.c. and 20 c.c. on each of the 1st and 2nd days of treatment.	6 days	
L.	Male	40	Bubo in right groin -	2nd day	160 c.c. in doses of 40 c.c. and 20 c.c. on the 1st and 2nd, and of 40 c.c. on the 3rd day of treatment.	6 days	

V.—(G.) *Observations made by Mr. Haffkine on the Effect of an Anti-Plague Serum prepared by himself.*

Observations by Mr. Haffkine on the effects of serum prepared by himself.

553. Mr. Haffkine made several series of experiments to test the utility of anti-plague sera prepared by him by the inoculation of plague cultures into various animals. His first experiments were made upon six patients in the Byculla Jail, Bombay. At his request, the cases which seemed to be most hopeless were placed at his disposal for the trial of the serum. Of the six patients inoculated with the serum, three died. 133–141.

Mr. Haffkine's next experiments were done under somewhat similar conditions. He chose for inoculation with his serum the gravest cases which came under observation in the Arthur Road Hospital, Bombay. "I visited," said Mr. Haffkine, "the Arthur Road Hospital . . . and left it to the Medical Officer in charge to point me out those cases which, in his opinion, were hopeless, that is, those which he thought could not recover without the assistance of some new additional treatment. He told me, for instance, at once that he had a patient who the day before had a temperature of 107°, and that up to that time not a single patient had passed through the hospital who who recovered after having had that temperature. In the same manner he pointed me out a certain number of other patients who, in his opinion, had no chance of recovery under the ordinary treatment. I reasoned that if a proportion of such patients recovered under the specific treatment, I would have an indication that the serum treatment produced a greater effect than the ordinary treatment adopted in the hospital. Very soon I had to abandon that method, as no such indication was obtained." 140.

With a view to more completely testing the possibilities of the serum treatment, and with a view to testing it on a series of cases of average severity, Mr. Haffkine, working in collaboration with Major Bannerman, I.M.S., made a series of systematic observations on a large number of plague patients admitted to hospital in Poona. Every alternate patient was taken for serum treatment, the intermediate cases serving as controls. Mr. Haffkine describes the results he observed as follows. 140.
"With the exception of those who died within one hour, or a few hours, that is, before we could see them, we treated with plague anti-toxic serum every second patient admitted during the hours we were at the hospital, irrespective of the information as to the serious or promising condition of the patient or the duration of the disease before admission. . . . After a couple of hundred patients had passed through our hands, we compared the mortality statistics among the treated and among the non-treated. I expect it was an accident, but the mortality amongst the treated was higher than among the non-treated. The moment that became clear we suspended further treatment."

"Further attempts," said Mr. Haffkine, "were made in Bombay, but exceedingly cautiously, with some homœopathic doses, injecting 1 to 5 c.c. of the serum or so. . . . We varied the treatment in many ways. For instance, a patient would receive only 1 c.c. or he would receive that amount repeated every five hours; or, again, he would receive a dose of 10 c.c. once in two days. The patients were always observed comparatively with others admitted at the same time. In no case did we observe a noticeable advantage on the side of the patients treated." 141.

The serum prepared by Mr. Haffkine had evidently no beneficial effect. Conclusion.

VI.—The Results obtained by the Inoculation of Lustig's Serum.

VI.—(1.) *General Account of Observations made in the Arthur Road Hospital, Bombay.*

Observations of the effect of treatment with Lustig's serum.

Para. 514 of this Report.

554. We now pass to the results of the therapeutic administration of Lustig's anti-plague serum to human patients. As already explained above, we did not ourselves test the effects of Lustig's anti-plague serum upon human plague patients, inasmuch as we were satisfied that the animals which were treated with the particular sample of serum with which we were furnished succumbed to plague more readily than untreated animals. The results of the treatment with Lustig's serum which was undertaken at the Arthur Road Hospital in Bombay by Dr. Choksy may, however, be considered.

Observations made by Dr. Choksy in the Arthur Road Hospital, Bombay.

555. Dr. Choksy's observations fall naturally into three series. One of these series of observations had already been completed when we arrived in India. The results of this series of observations, which covered the period from March to October 1898, were communicated to us by Dr. Choksy when he came before us. The second series of observations was in progress during the latter part of our stay in Bombay, and we had the advantage of seeing certain of the patients. This series of observations covered the period from February to April 1899. Lastly, the third series of observations was commenced in May 1899. We have learned of the results of these from the communications made by Dr. Choksy and Dr. Mayr to the "Bombay Medical and Physical Society" in September 1900, and the "Bombay Medical Union" on the 21st of April 1900, respectively. Reprints of these communications were kindly furnished to us by the authors.

We propose to deal seriatim with the three series of observations particularised above.

VI.—(2.) *First Series of Observations in the Arthur Road Hospital, Bombay.*

First series of observations made by Dr. Choksy in March to October 1898.

20,952.

556. In the first series of observations, 257 cases of plague were treated with the serum, all at the Arthur Road Hospital. The serum which was employed was prepared under Professor Lustig's direction in Italy. The serum was usually administered night and morning, and it was employed in doses of from 10 to 30 c.c.; these were also the doses employed in the case of the sera made in Bombay and used in subsequent series of observations.

Principle upon which the serum cases and controls were selected, and statistics of results.

20,945–50.

The patients to whom the serum was therapeutically administered were to some extent selected, all moribund patients being excluded, except in March and part of the month of April. The case-mortality of the selected patients who were submitted to the serum treatment was compared with the case-mortality in the remainder of the hospital patients, including, as this did, the moribund patients who were excluded from the serum group. The results as set forth by Dr. Choksy were as follows:—

20,952.

—	Cases.	Deaths.	Case-Mortality.
Ordinary treatment	752	595	79·1 per cent.
Serum treatment	257	145	56·4 per cent.
Difference in favour of the serum cases			22·7 per cent.

Critical consideration of these results.

557. Before accepting these results as proof of a benefit accruing from the serum treatment, we must go somewhat more closely into the question of the comparability of the two contrasted groups of cases. The selected cases to whom the treatment was applied included, as Dr. Choksy told us, 30 moribund patients. We may presume that a proportionate number of convalescent patients was also included. We shall see in the next paragraph that for every 30 moribund patients admitted to the Arthur Road Hospital 7 convalescent patients were on the average admitted. Thus, during the period in which selection was applied and in which the 30 moribund patients referred to above were included in the serum group, there would in all probability have been 7 convalescent

patients included in that group. This would leave in the serum group a balance of 220 patients admitted in the acute stage of the disease and in a non-moribund condition. Now, it is obvious that during the latter period when the process of selection described by Dr. Choksy was in force an unduly large proportion of moribund and convalescent cases must have fallen to the group of those cases which were not treated with serum. Therefore, no value will attach to the comparison set forth in the table, unless we can apply such corrections to the figures as to ensure that the number of convalescent and moribund patients included in the respective groups of control and serum patients, shall stand in each case in the same proportion to the number of patients in each group admitted in the acute stages and in a non-moribund condition.

Corrections which appear applicable to Dr. Choksy's figures. III. 457(A).

558. Data for making a rough estimate of the kind here required are incidentally supplied in Appendix LXI. of our Proceedings, and in Dr. Choksy's communication to the Bombay Medical and Physical Society, already referred to above. We find that in 1898, close on 30 per cent. of the patients admitted to the Arthur Road Hospital died within 24 hours of admission. These would come under Dr. Choksy's definition of moribund patients. Further, we find from the analysis of nearly 1,000 hospital cases, given by Dr. Choksy in the communication referred to, that 7 per cent. of patients were admitted to hospital in a convalescent or semi-convalescent condition. To every 63 patients admitted in the acute stages of the disease and in a non-moribund condition, there would thus fall 30 moribund patients and 7 convalescent patients.

Calculating on the basis of these data, there would, in the absence of a system of selection, have fallen to the serum group—consisting, as it did, of 220 patients in the acute stages of the disease and in a non-moribund condition—105 moribund cases in lieu of the 30 actually included, and 24 convalescent cases in lieu of 7, the estimated number of such cases included.

If we make the adjustments which are indicated by these considerations; if, in other words, we transfer cases and deaths from the control group to the serum group, so as to establish a proportionate number of convalescent and moribund cases and deaths in each group; we arrive at the following results:—*

—	Cases	Deaths.	Case-Mortality.
Control Group	660	520	79 per cent.
Serum Group	349	220	63 per cent.
Difference in favour of the serum group			16 per cent.

Comparability of patients in serum and control groups.

559. A redistribution of the moribund and convalescent patients having been effected in the manner and with the results indicated above, the further question arises as to whether patients in each group who are classed as patients in the acute stage of the disease and in a non-moribund condition were comparable with each other from the point of view of the severity of their cases. We are not satisfied that they were. Rather, we think that when the desire to test the serum only on cases that seemed capable of benefiting from the treatment, and in particular upon patients who seemed likely to live more than 24 hours after admission, came to be a factor in the selection of cases for serum treatment, an influence was introduced which must, no doubt unconsciously, have operated in the direction of including within the serum group the milder, to the exclusion of the severer, cases of plague.

Conclusion regarding first series of observations.

560. In view of this consideration, we are of opinion that the statistics adduced by Dr. Choksy—even after they have been corrected as set forth above—do not afford any conclusive evidence of the efficacy of Lustig's serum.

* The arithmetical processes by which the figures in this table are arrived at are as follows. We add to the serum group, in addition to the 30 moribund patients already included, 75 in order to make the full number of 105 moribund patients which should have fallen to that group if no selection had been applied. In the same way, to the 7 convalescent cases, which we have assumed to be included in the serum group, we add 17 additional convalescent cases. We thus add 92 cases to the serum group and the same number of cases must naturally be subtracted from the control group. In readjusting the figures relating to deaths, we have to add to the number of deaths already debited to the serum group 75 deaths on account of the moribund patients added to the group, and the same number of deaths has of course to be subtracted from the number of deaths debited to the control group.

VI.—(3). *Second Series of Observations made in the Arthur Road Hospital, Bombay.*

Second series of observations made by Dr. Choksy from February to April 1899.

561. We pass to the second series of comparative observations conducted by Dr. Choksy. As before mentioned, the observations in question cover the period from February to April 1899. The serum employed in this series of observations was prepared in Bombay, under the auspices of the Municipality, by Dr. Galeotti, working under Professor Lustig's instructions. In this series of observations only patients admitted in the acute stages of the disease and in a non-moribund condition were treated with the serum. The residuum of patients admitted to the hospital during the corresponding period was, as in the series of observations that have just been considered, taken as controls.

The results of the second series of observations are set forth by Dr. Choksy in his communication to the Bombay Medical and Physical Society as follows :—

—	Cases.	Deaths.	Case-Mortality.
Control Group - - -	1,190	957	80·5 per cent.
Serum Group - -	403	249	61·8 per cent.
Difference in favour of the serum cases -			18·7 per cent.

Dr. Mayr's argument in support of the method of selection adopted in Dr. Choksy's observations. Communication to Bombay Medical Union.

562. Dr. A. Mayr, who is at present entrusted with the preparation of Lustig's serum, has endeavoured to vindicate the principle of selection adopted by Dr. Choksy, and in particular to establish that the system of selection employed did not result in a selection of the milder type of cases for treatment with the serum.

With this object Dr. Mayr contends, in the first place, that the "exclusion of moribund patients was irrelevant, their place being taken not by mild cases, but by such as would have died later on under the ordinary treatment." This particular argument is obviously fallacious, inasmuch as the exclusion from the serum cases of a group of patients whose mortality is *ex hypothesi* 100 per cent., is not counterbalanced by the inclusion among the serum cases of a group of patients whose mortality works out, after the moribund patients have been deducted, as between 64 and 65 per cent.

Dr. Mayr's second argument is more subtle. He contends that the fact that the case-mortality in the cases in the Arthur Road Hospital not treated with serum was higher only by a fraction than the mortality for the corresponding period in the Mahratta and Modi Khana Hospitals,* where the serum was not used, is a fact which constitutes conclusive proof that the control cases in the Arthur Road Hospital, and in correspondence with these the selected serum cases, represented equally severe types of the disease. Reflection, however, shows that no weight can attach to this argument, inasmuch as only 25 per cent. of the total patients admitted into the Arthur Road Hospital were selected for serum treatment, and hence there was no opportunity afforded for any considerable artificial inflation of the figures representing the case-mortality of the large residuum of cases at the Arthur Road Hospital who were not treated with serum. It will be seen on comparing the figures of the mortality for the cases treated and not treated with serum in the table above, with those given in the table below, that even though the selection of cases for serum treatment may not have brought about any great artificial increase of the recorded case-mortality of the patients not treated with serum, it may quite well have brought about an appreciable artificial reduction in the recorded case-mortality of the serum group.

Corrections which appear applicable to Dr. Choksy's figures.

563. With a view to measuring the effect which was probably produced by the system of selection applied to the serum cases, we may, as in the previous set of statistics, transfer moribund and convalescent patients from the control group, to which they are credited in Dr. Choksy's statistics, to the serum group, in which they would have been classed if each alternate patient had been referred to the serum or control group in accordance with the order in which he was admitted to the hospital. The

* The case-mortality in the cases not treated with serum in the Arthur Road Hospital was 80·5 per cent. The case-mortality of the total plague cases in the Modi Khana and Mahratta Hospitals was 80·3 per cent.

results which would have been arrived at, if this procedure had been adopted, would (reckoning* here as before) have been as follows :—

——	Cases.	Deaths.	Case-Mortality.
Control Group -	953 (1190 as recorded in table above — 227 cases transferred to serum group).	765 (957 — 192)	80 per cent.
Serum Group -	639 (403 as recorded in table above + 192 moribund patients + 45 convalescent patients).	441 (249 + 192)	69 per cent.
Difference in favour of serum group - - - - -			11 per cent.

After making the adjustments that would seem indicated, there would appear to remain only a comparatively small advantage to the credit of the serum cases. Moreover, against this balance must be set off the fact that the acute cases included in the serum group were, as has been pointed out in the case of the first series of statistics, probably less grave than the corresponding class of cases which is included within the control group. In conformity with this we can hardly accept the result of the second series of observations as conclusive evidence of an advantage accruing from serum treatment.

VI.—(4.) *Third Series of Observations made in the Arthur Road Hospital, Bombay.*

564. We now pass to the consideration of the third series of systematic observations which were made by Dr. Choksy on the effects of Lustig's serum. In this series of observations we learn that no system of selection came into operation. Every patient, when once the diagnosis of plague was clear, was referred, in the order of his admission, in an alternate manner either to the serum or the control group. The whole series consisted of 968 patients, 484 being included in each group. The results of this comparative series of observations were, as, indeed, we have already incidentally seen above in the course of our discussion of the method of selecting controls, as follows :—

Dr. Choksy's third series of observations from May 1899 to August 1900.

——	Cases.	Deaths.	Case-Mortality.
Control Group - -	484	385	79·5 per cent.
Serum Group - - -	484	329	68 per cent.
Difference in favour of the serum group -			11·5 per cent.

565. We have here at last a result obtained by a method which satisfies the demands formulated in a previous section. The diminution of plague mortality by 11·5 per cent. in such an extensive series of cases treated with the serum seems to us—assuming, as we presume we may do, the correctness of the records—conclusive as to benefit having been derived from the administration of the serum.

Consideration of the statistics relating to the third series of observations.

566. The question as to whether the difference of 11·5 per cent. in the case-mortality which is shown in the above table is to be regarded as the true measure of the advantage accruing from the serum treatment has already been considered above in connexion with the consideration of Dr. Choksy's contention that the record should be amended by the exclusion of the moribund and convalescent patients, and that the result of the serum treatment should be gauged only upon the patients admitted in a non-moribund condition in the acute stages of the disease. We have already expressed our opinion that, even though the full measure of the benefit received does not emerge from the comparison of all the serum cases with all the control cases, the omission of the convalescents and semi-convalescents, and of the moribund patients, from both groups of patients would none the less be inadmissible. We may here restate and somewhat amplify what we have already said on this question.

Question whether these statistics show the full extent of the benefit derived from treatment with Lustig's serum.

Paras. 521–2 in this Chapter.

* While, as in the case of the previous tables, the adjustments have been calculated out on the basis of 0 moribund and 7 convalescent patients admitted to 63 acute cases admitted in a non-moribund condition, attention is drawn to the fact that it seems possible, in view of an incidental statement in Dr. Mayr's communication to the Bombay Medical Union to the effect that the patients who were selected for serum treatment showed a mortality of 15 per cent. within 24 hours of admission, that the deductions from the control group and the additions to the serum group on account of moribund cases ought to be made on a considerably smaller scale.

Para. 557 in this Chapter.

Restatement of the arguments for rigidly adhering to the "alternate system" in observations.

567. If the statistical principle that patients who are deemed to be moribund o admission should be omitted from the record were accepted, it would, in the first plac never be certain that the clinician, actuated by the fear that the patients had alread lapsed into a condition from which recovery was impossible, had not withheld th serum from cases which, though serious, were not beyond therapeutic aid.

Again, if the principle were accepted that every patient who succumbed within certain period after he had been received into hospital might be classed as a patie admitted in a moribund condition; if, in other words, a proportion of the unsuccessfu cases were to be deleted from the record on the assumption that they were, when take in hand, unsuited for serum treatment; it would be impossible to repose any confidenc in the records.

Lastly, it will be manifest to consideration that it would be impossible to define th term "moribund" in any satisfactory way. While some clinicians might restri the term to those admitted in a pulseless condition, or actually in articulo mortis others, like Dr. Choksy, would class as moribund all patients who died within 24 to 2 hours after admission; and others, again, might, like Dr. Mayr, be of opinion that a patients who succumb within 48 hours after admission ought to be classed as moribun *i.e.*, as patients admitted in a condition beyond all possible therapeutic aid. I connexion with the views held on this question by Dr. Choksy and Dr. Mayr, we wou point out that in the case of snake bite death would appear to be preventible by th exhibition of anti-venomous serum, even at a time when, under ordinary circumstance death would have occurred in much less than 24 hours. We, therefore, think that, i the case of plague, patients ought not to be classed as beyond all conceivable therapeut aid merely on the ground that such patients have not been benefited by the anti-plag sera which have up to the present been available.

Bombay Medical and Physical Society's Transactions, Vol. IV., No. 5, September 1900.

Bombay Medical Union, 21st April 1900.

VI.—(5.) *Subordinate Questions arising in connexion with Lustig's Serum.*

Subordinate questions in connexion with Lustig's serum.

568. Before passing on to summarise the conclusions we have arrived at wi regard to the general question of the serum therapathy of plague, and the prospects improving upon the sera at present available, we may briefly glance at a few subordina questions which arise in connexion with Lustig's serum.

Longer survival of serum cases discussed.

The first of these relates to the question of the longer survival of the seru patients as compared with the control patients. In connexion with this question v have examined such records as are accessible to us without finding any evidence of longer period of survival or a more rapid convalescence after treatment with the seru Dr. Mayr, on the other hand, states that during the period covered by the employme of the serum treatment at the Arthur Road Hospital—he has in view the peri from March 1899 onwards—the percentage of deaths occurring within 48 hours aft admission, calculated on the total admissions, diminished by 8 per cent. as compar with the percentage of similar deaths during the winter months of 1898–99, when serum was available. We do not think that any certain inference can be based up the fact animadverted to by Dr. Mayr, for the number of patients who die shortly aft arrival at hospital will obviously depend in large measure upon the clinical type of t disease and upon the policy that is adopted in connexion with the removal of patie to hospital. In connexion with the last, we cannot ignore the fact that directions we issued in Bombay in the early part of 1899 that the practice of removing moribu patients to hospital should be abandoned.

Bombay Medical Union, 21 April 1900.

25,560–2.

Effect of serum treatment according to the stage of the disease in which the treatment is commenced.

Passing to the question of the relative effect of the administration of the seru in the early and late stages of the disease, we may note that Captain Childe, I.M. after studying the results of the treatment in the case of 148 patients, gives figu to show that the serum was not more efficacious in the cases treated in the earl stages of the disease than in the cases treated in the later stages. On the contra he found "that the mortality of patients treated on the first day is higher than t of all the patients who underwent the treatment, the respective figures being 73 and 69·7 per cent."

Vol. III., App. LXII., p. 462.

VII.—Summary regarding the Serum Therapeutics of Plague.

Summary.

569. We have seen in the section of this Chapter in which we deal with the resu obtained in India by the experimental administration of Yersin's serum to anim artificially infected with plague that, so far as can be judged from the results the case of the particular samples which were tested in this manner, Yersin's seru

Section III. of this Chapter.

as sent out from the Pasteur Institute, Paris, contains therapeutically useful substances in greater or smaller quantity. At the same time we saw that in the case of certain samples of serum,—we refer in particular to the samples of serum which were furnished to us, and to that which was employed on human patients by the German Plague Commission,—the dose of serum required to avert death from a plague-infected animal was so considerable as to make it *à priori* doubtful whether any considerable therapeutic effect could reasonably be expected from the exhibition of these sera to human plague patients.

In a subsequent section, in which we studied the results obtained upon human plague patients by the administration of Yersin's serum, we saw that, except in the series of observations made by ourselves and by the Russian Plague Commission, nothing in the nature of a strict statistical comparison could be made between the case-mortality of cases treated with serum and the case-mortality of cases not treated with serum. Section V. of this Chapter.

There appeared, however, in the case of the patients treated by Dr. Simond in Karachi and the patients treated by Captain Mason, R.A.M.C., in Cutch, to be clinical evidence sufficient to raise a strong presumption that the course of the disease was favourably influenced by the exhibition of the serum.

In the case of our own experiments, conducted with a serum derived from the Pasteur Institute, Paris, further, in the experiments of the German Plague Commission undertaken with a serum of similar derivation, and lastly, in the case of the observations made by the Russian Plague Commission on the effects of the administration of a serum prepared in St. Petersburg, the patients appear not to have benefited in any way from the treatment. At the same time there was not in any of these three series of observations any indication that the serum had in any way aggravated the disease.

In the case of the observations made by Mr. Haffkine with a serum prepared by himself, there appeared, on the contrary, to be a balance of advantage in favour of the patients who were not treated with serum.

Lastly, to complete our summary of the effects of the administration of Yersin's serum, we may recall the fact that in the case of a quite considerable number of healthy persons inoculated prophylactically with Yersin's serum, such statistical results as were obtained were quite inconclusive on the question as to whether the meeting protective effect which was expected was actually realised. It was, however, noted that in certain cases toxic phenomena, in the form of "fever, pain in the joints, and swelling of the glands," supervened upon the incorporation of the serum.

Passing now in our recapitulation to the effects of Lustig's serum, we saw, in the section of this Chapter which dealt with the results obtained by the incorporation of Lustig's serum into animals, that the samples of serum with which we were furnished apparently exerted an effect in the direction of diminishing the resistance to plague in the animals experimented upon. Section III. of this Chapter.

When we passed to consider the average result obtained by the incorporation of Lustig's serum into plague patients, we saw that, of the three successive series of observations brought forward by Dr. Choksy, the first two gave results which were, owing to the system of selection adopted, inconclusive. In the case, however, of the third series of systematic observations, where all possibility of selection was avoided by subjecting to the serum treatment every second patient who was admitted to the hospital, proof was afforded—always assuming the accuracy of the hospital record which, since we had already left India, did not come before us—evidence of a diminished mortality on the part of the patients who were treated with serum. The diminution which was achieved amounted, as we saw, to a reduction of at least 11 per cent. in the case-mortality of the serum cases as compared with that of the control cases treated under precisely similar circumstances. We did not come across any evidence that any unfavourable influence had been exerted when these sera were therapeutically administered to man. It is to be noted that the samples of serum tested by us were not employed upon men. Section VI. of this Chapter.

570. To sum up, we think we may, not only in view of the facts summarised above, but also of the unequivocal opinion expressed by Dr. Simond, Captain Mason, R.A.M.C., Dr. Choksy, and Dr. Nazareth, as to an improvement having been witnessed by them in the clinical symptoms of patients treated with serum, conclude that, on the whole, a certain amount of advantage in all probability accrued to the patients, both in the case of those injected with Yersin's serum and of those injected with Lustig's serum. It seems to us, however, not impossible, that in exceptional cases the serum treatment— Conclusions.

and this statement, be it noted, could be made also in connexion with every other treatment which has been adopted—may have done positive harm. We can hardly doubt, in view of the experimental results which we obtained with Lustig's serum, and in view of the toxic symptoms observed by Dr. Simond in the case of healthy persons inoculated with Yersin's serum, that the serum may, especially in cases where it was drawn off prematurely from the inoculated horses, have contained either toxic or anti-bactericidal elements. With a view to preventing such deleterious sera coming into use, it seems us to to be absolutely essential that all sera should, before they are brought into therapeutic application on man, be exhaustively tested upon animals.

Recommendations.

571. With regard to the future preparation and therapeutic employment of anti-plague sera in India, we may, perhaps, be permitted to offer the following suggestions:—

(1.) We desire to record our opinion that though the method of serum-therapathy as applied to plague has not been crowned with a therapeutic success in any way comparable to that obtained by the application of the serum method to the treatment of diphtheria, none the less the method of serum therapathy is in plague, as in other infectious diseases, the only method which holds forth a prospect of ultimate success.

(2.) We would suggest that the serum treatment has not as yet been sufficiently successful to make it desirable to extend the treatment, under present conditions, as a general measure, over all the districts affected with plague. Rather, it appears to us, that the imperfections of the present methods of preparation and application should be fully recognised, and that it should be realised that the line of progress lies, not in the direction of applying the sera at present available to the largest number of patients, but in the direction of studying in the case of the animals who furnish the serum the blood changes which are associated with the incorporation of the plague toxins and with the elaboration of antidotal and bactericidal substances. In like manner, we think that the condition of the blood in the human plague patient, and the changes brought about by the administration of the serum, should be carefully studied.

(3.) With regard to the procedure followed in the preparation of the sera, we do not think that the facts, which have come before us, are in any way conclusive as to the superiority of the procedure advocated by Professor Lustig over the procedure ordinarily employed. It is true that in the case of Lustig's serum statistical proof has been afforded of a diminished mortality in the cases treated with serum, while no such statistical proof is forthcoming as to the mortality in plague having been diminished by the use of Yersin's serum. But this does not foreclose the question. On the one hand, the absence of the required statistical demonstration of the diminution of case-mortality by the use of Yersin's serum may, so far as the observations of Dr. Simond and Captain Mason, R.A.M.C., are concerned, have depended upon the employment of faulty statistical methods. Again, quite apart from considerations such as those just referred to, the difference in the result obtained in the case of the two classes of serum, may quite well be attributable to the fact that Lustig's serum was, at least, in the case of the observations which have given a conclusively favourable result, prepared in India, and was thus presumably comparatively fresh serum, while Yersin's serum, at least in the case of the serum which was derived from Paris, was applied only after it had been altered by heating and lapse of time. We, therefore, think that it would be wise not to neglect, in the future preparation of therapeutic sera in India, the simpler methods of preparation employed by Yersin and others.

CHAPTER VI.

MEASURES FOR THE SUPPRESSION OF PLAGUE.

I.—Measures adopted in India.

Plague measures.

572. In addition to the steps taken for the discovery of cases of plague, the measures undertaken in India to suppress the disease were directed towards the isolation in hospital of the sick, the segregation of those who had been in immediate contact with the sick, the removal of the healthy from plague-infected places, inoculation with Mr. Haffkine's prophylactic, and the disinfection of infected houses. The measures taken to prevent the disease from spreading were directed to the supervision of travellers by railway and sea, and occasionally to interference with the movement of people from place to place by road. We have already referred to the value of inoculation as a measure for dealing with the plague. We shall now endeavour to indicate how far the other measures enumerated above have individually contributed to prevent the spread of plague.

II.—Methods of obtaining Information regarding the Occurrence of Plague Cases and Plague Deaths.

Importance of early information of sickness.

573. The most important and the most difficult matter in all operations against the plague is to obtain early information regarding the cases that occur. The prompt discovery of an imported case may enable the authorities to prevent a local outbreak. Even where a few indigenous cases have occurred, early information regarding each new case may make it possible to prevent the outbreak from spreading beyond a confined area. What is required is information of each case of plague that occurs, as soon as possible after the patient has been attacked. Information that a person has died of plague may be useful to a certain extent, but is less useful than a prompt report of his having fallen sick, since between the time of his being taken ill and his death the damage which it is desired to prevent may already have been caused.

Rules regarding reports concerning infectious diseases in force before the outbreak of plague.

574. Before the outbreak of plague in 1896 there was no machinery for the discovery of cases of infectious disease generally. There were, indeed, special orders of an executive character which required reports from Municipal and village officers regarding outbreaks of cholera, a disease which is readily recognized by the people in rural as well as urban areas. The Municipal law, also, in the towns of Bombay and Calcutta imposed on every medical practitioner who treated or became cognizant of the existence of infectious disease in any private or public dwelling, other than a public hospital, the duty of giving information without delay to the executive Health Officer. This obligation was not imposed in Municipalities generally, and it did not extend to any of the relatives, or to any of the attendants of the sick person, but only to the medical practitioner in charge of the case.

Thus, when plague made its appearance in India, special means had to be devised for obtaining early information regarding cases of plague, if possible during life, or, at any rate, after death. Some of the methods adopted for finding cases during life have been also utilized for the discovery of cases after death.

II.(1.)—*Measures Adopted with a view to obtaining Information of Plague Cases before the Death of the Patients.*

Methods adopted for finding plague cases during life.

575. In some cases private individuals were required during an outbreak of plague to notify sickness generally or plague in particular. Arrangements were made to place people arriving from infected areas under surveillance. Rewards were paid to persons giving information of cases of plague, and paid informers were utilized to discover cases. In infected towns volunteer agencies were used to assist the official establishments in collecting information regarding the occurrence of cases of plague. Special establishments were engaged on searching villages in the neighbourhood of infected places. In some places there was house to house visitation, and the search of houses in order to discover cases.

II.(1.)(*a.*)—*Notification of Sickness.*

Notification of sickness generally or of plague in particular.

576. The village headmen have generally been required to report cases of plague occurring in their villages.

The obligation of reporting sickness generally, or plague in particular, has been imposed in several towns on private individuals. We were given details regarding the working of this measure in Poona and Surat.

Notification of sickness in Poona.

In Poona, in a period of about eleven months between January 25th and December 31st, 1898, 2,954 cases of sickness were reported, of which 972 resulted in death. The

21,380, 21,986, 21,991, 22,005. 21,416. 21,887.

number of dead bodies examined at Poona in the calendar year 1898 was 3,035, excluding the bodies of persons who died in hospital, so that it is estimated that during this period of eleven months there should have been 9,000 cases of sickness. The penalty for not reporting a case of sickness was not enforced, and this probably had more than anything to do with the comparatively small number of reports made. Relatives or friends reported 402 cases and native practitioners 1,117. Very few of the latter cases were seen by the medical officers responsible for visiting those reported to be sick. From March to the end of December about 25 cases of plague were discovered among the persons whose sickness was reported. There were, in the calendar year, 106 deaths from plague among persons whose sickness was not reported either by relatives or medical practitioners. The cases of plague found by means of reports of sickness were, many of them, in the early stages.

21,883, 21,991, 22,005. 21,380. 21,379. 21,386.

Notification of sickness in Surat. 15,836. 16,004.

In Surat the system of reporting cases of sickness was developed and improved during the course of the second outbreak. At a late period of the outbreak a special inducement was given, at first in two wards, and subsequently elsewhere, to the people to report plague by notifying that "if the inhabitants of a house or the neighbours brought information of a plague case, the houses on each side of the plague case would not be disinfected nor would the inhabitants be segregated." A considerable number of cases of plague evidently escaped detection during the outbreak, but it is claimed that the system was successful during the later months of the outbreak.

16,005, 16,032.

Conclusions regarding notification of sickness by private individuals.

We think that it is not practicable to gain much, except at places well supplied with official medical establishments, by requiring reports of sickness to be made by private individuals during outbreaks of plague. There is the danger that medical attendants may omit to report cases, owing to the fear that reporting them may injure their practice, and there is the danger that the relatives, friends, and neighbours of the sick will not report owing to the fear of consequences unpleasant to themselves. We think that the measure will be found useful only where the medical establishments employed are sufficiently strong to take advantage of it, and where the obligation can be efficiently enforced: and, when it is enforced, we think that a fee might well be paid to medical practitioners in recognition of their services in making a report.

II.(1.)(*b.*)—*Surveillance of Persons coming from Infected Areas.*

Surveillance. Nathan's "Plague in India, 1896–7." Vol. III., App. VIII., Nos. 69–89.

577. The above remarks refer to the notification of sickness among the residents of a place. Special arrangements have been made, mainly in places not attacked by plague, to detect cases of plague among arrivals from infected areas. As early as March 1897, arrangements were made throughout the greater part of India to have the names and destinations of persons arriving from the infected areas recorded at the railway stations and forwarded to the towns or villages to which they were going, in order that a watch might be kept on them in their homes and any suspicious illness or death promptly reported and inquired into. In some Provinces there was a special observation of the health of all the people, whether they had been away anywhere or not, in places known to be in close communication, commercially or otherwise, with Bombay and other infected localities.

The surveillance system in Bombay.

The system has acquired fresh importance as the impracticability of detaining in observation camps all persons on their arrival from infected places has come to be universally recognised. It has naturally been resorted to more within the Bombay Presidency than elsewhere. The Bombay Government in October 1898 issued elaborate and detailed instructions, of general application, both for urban and rural areas. The system had already been working for some time in all the chief towns of that Presidency whilst they were uninfected. The arrangements differed to some extent in different places, but the general principle of them was that no person arriving by rail was allowed to enter a town unless he was vouched for by some respectable native gentleman, and unless his place of residence was intimated to the authorities. To meet the case of persons who succeeded in entering the town either by road or by evading observation at the railway stations, householders were required to make a report regarding every person arriving in the town. All new arrivals were required to present themselves for observation at stated intervals for a period of 10 days.

21,528.

The surveillance system in Madras. 3865–70. 3929–35. 3942–61.

In the Madras Presidency also elaborate arrangements have been made for the observation of people entering it from infected areas. Along the frontier adjoining the Bombay Presidency and the States of Hyderabad and Mysore, there is a double row of circles with a diameter of 10 miles, so that a strip 20 miles broad all along the border is under observation. In each circle there is a special staff for the observation of persons coming from beyond the border. People who appear suspicious or are not able to give trustworthy addresses are detained. On the main roads there are frontier road stations, and similar arrangements are made at the railway stations. Every person crossing the frontier is liable to be examined. If healthy he is given a

passport requiring him to present himself for 10 days to the head of the village to which he is going or other authority named in the passport. The counterfoil of the passport is sent to the latter. Lieut.-Colonel King, I.M.S., stated that this system had worked well. It had not indeed been possible to prevent everybody from coming in without a passport, but the instances where people succeeded in coming in without passports were rare. A certain number of persons had also evaded the duty of reporting themselves. The number who failed to report themselves, though only 2 per cent. in Madras City, was as high as 12·6 and 10·5 respectively, in the frontier districts of Bellary and North Arcot. It was said that many of those who failed to report themselves in these two districts were people who had come across the frontier to attend a market, or for some other temporary purpose, and then returned.

Some form of surveillance over persons arriving from infected areas is desirable at places in the neighbourhood of such infected areas, or in frequent communication with them. We think that there is no doubt that the system in the towns in the Bombay Presidency may often have been of use in deferring, though it may not have been able to prevent, an outbreak. A system of surveillance depends for its success on the active and intelligent co-operation of the people themselves. It may be expected to be most effective in places the inhabitants of which have experienced one outbreak, and, knowing the miseries of it, are anxious to avoid another. It is impossible to take absolutely effective measures to prevent strangers from entering an Indian town surreptitiously, and, even where the people co-operate most thoroughly in working a system of surveillance, there is a danger that the defence may be broken down in this way. Still we think it is one of the most useful means of preventing a healthy place from becoming infected. Conclusions regarding surveillance.

In rural areas also the surveillance of persons coming from infected places may, we think, be of service. It is not so easy for outsiders to enter a village as to enter a town without being observed, and in tracts where the people are familiar with plague, a stranger, who is not related to any influential resident, will generally have difficulty in settling himself even temporarily in a village.

II.(1.)(*c.*)—*Rewards for Information of Plague Cases.*

578. The system of granting rewards, or of employing paid informers, for the discovery of plague cases was not adopted extensively, but it was resorted to in certain places. In the villages in the Hyderabad State, a reward of Rs. 10 was always given to anyone who reported a *bonâ fide* infected village which had not already been reported by the officials. The system was said by Mr. Stevens to have acted admirably, and to have resulted in the discovery of a number of infected places. In the beginning of March 1898, a system of giving a reward of Rs. 50 for each authenticated report of the first case in a newly infected village was adopted in the Punjab. Captain James, I.M.S., who had great opportunities for forming a judgment on the manner in which the system worked, considered that it had worked very well indeed, and that no drawbacks had been found to it. Captain Clarke, I.M.S., who had not had so much experience of its working, considered that the results secured from it were encouraging. In the Cutch State rewards were given to the poorer classes, of 4 or 8 annas, for reporting a case among themselves. In Surat City rewards were given for each case reported. At first the reward was from Re. 1 to Rs. 2, and afterwards it was increased to Rs. 10 for each *bonâ fide* case of plague. Only 33 persons obtained a reward. In addition to the rewards paid to private individuals, rewards were paid in Surat to police constables and sanitary inspectors who reported cases. In Sukkur rewards of from 8 annas to Re. 1 were paid for information. Rewards for information regarding plague cases 5439. 5440-5442 9934. 9936. 10,829. 13,436. 15,575-9. 18,386.

There are, undoubtedly, disadvantages in a system of granting rewards or paying informers for the discovery of plague. It is possible, though very improbable, that a person suffering from plague might be taken into a place merely with the object of procuring a reward. The grant of a reward, too, may cause ill-feeling between the village in which plague is reported and that in which the person making the report resides. It is even possible that it may lead to misconception as to the motives of the Government, since at one time in certain parts of the country the people believed that deliberate efforts were being made by the authorities to spread the plague. But we think that these considerations may be ignored. The advantage of obtaining prompt information of early cases, even in a few instances, may be very great, and experience of the system of granting rewards is generally strongly in its favour. It appears to us that it will be found a useful measure, provided that the reward offered is substantial enough to be attractive, and that it is paid with as little delay as possible after the information has been given and ascertained to be true. The employment of paid spies or informers is a measure to which there are obvious objections. Conclusions regarding system of rewards.

It may, however, be necessary to utilize this means of getting information as to plague cases so long as the people do not come forward readily themselves to give it.

II.(1.)(*d.*)—*Employment of Volunteer Agencies for the Discovery of Plague.*

Volunteer agencies for the discovery of plague.

579. As a matter of course, Government establishments of every sort have been utilized on plague duties of all kinds. They have been assisted by private individuals in most of the infected towns in the Bombay Presidency and elsewhere; some of these were Europeans, but the large majority were natives of India. The chief function of these volunteers has been to assist in the discovery of cases. In some towns, as, for instance, in Poona in 1898, an organisation of volunteer committees was in existence prior to the occurrence of indigenous cases of plague. Where no such organisation existed before the outbreak of plague, steps were generally taken to create one on its occurrence. The first step was to divide the town into a number of wards or circles, each under a ward officer or superintendent. Besides a certain paid staff there were associated with each ward officer a number of residents of the town who assisted him in making inquiries and discovering new cases. The ward again was divided into a number of blocks, of about 100 houses each, under supervisors. A rough census was taken as soon as possible and kept up to date by the entry of all new arrivals and the striking out of all departures. The duty of the supervisors was to make inquiries from house to house and obtain information of fresh attacks from the friends of the patients, from the neighbours, from paid informers, from detectives, and any other possible source, and to communicate it to the ward officers, who were responsible in the first instance for the disposal of all matters connected with the administration of plague measures within their wards.

Necessity for official assistance and control of volunteers.

2553.
3273.

Where in a system of this sort the ward officers were themselves volunteers, and there was no strong official element in the background to give support, it was liable to break down under the stress of a severe epidemic. At Bangalore, for instance, the ward superintendents were at first all non-official, both in the City and the Civil and Military Station. As soon as the epidemic began to press severely, the voluntary agency failed entirely; some resigned and some ran away; there was a panic, and the whole machinery was disorganised. The authorities had then to resort to a paid agency. At Surat, on the other hand, there was from the time when the ward system was introduced, at the head of eight, at any rate, out of the nine wards a European official; and the position of the large number of native gentlemen who volunteered for plague work was that of honorary assistants to these officials. Mr. Moore, the Collector, considered that this system worked very successfully.

24,417.

Employment of volunteers in Bombay City.
1344-9.

In the City of Bombay at the present time the volunteer element holds, perhaps, so far as the discovery of fresh cases goes, an intermediate position between that which it held in Bangalore and that which it held in Surat. The City is divided into nine divisions, each under a district officer, seven of the district officers being European military officers, one a Muhammadan gentleman from Sind, and one a native medical officer. Besides a staff of doctors, sub-inspectors and coolies, the district officer is assisted also by committees of volunteer native gentlemen. There are throughout the City about 900 gentlemen serving on these committees. They distribute the section of the town in which they are employed among themselves and visit it daily. They endeavour to find out cases of plague, and communicate the information which they collect to the district plague officers. Some of the committees have been very successful; others, judging from a comparison between the deaths reported from all causes and the number of plague cases discovered, have been less so.

Conclusions regarding volunteer agencies.

The employment of influential and trustworthy residents of a town to collect information regarding the occurrence of cases of plague must be of material assistance to the authorities. It would be impossible to provide official agencies of sufficient strength to undertake this duty in cities like Bombay and Calcutta, and even in towns of much smaller size when a number of them are simultaneously infected. Moreover, unless there be an intermediary between the representative of Government and the people, there is constant danger that the requirements of the authorities may be misunderstood. The risk of concealment of cases is lessened where people see that the influential residents of the place have ranged themselves on the side of the authorities.

II.(1.)(*e.*)—*Special Observation of Health of Persons in places exposed to Infection.*

Inspection of villages in the neighbourhood of infected places.
13,128.
19,479-80.
9669.

580. In consequence of the special danger to which places in the immediate neighbourhood of an infected place are exposed, the authorities have endeavoured to maintain a watch over the health of the inhabitants of villages close to infected places. In the Bombay Presidency, officials—agricultural inspectors or vaccinators, relieved of their ordinary duties, or men specially employed for the purpose—have been sent to visit villages which appeared to be threatened by plague to ascertain by

he examination of the death returns, and in other ways, whether any of these villages were infected. Similar precautions have been taken in other provinces. Since there has been plague in the Hyderabad State, the villages on each side of all the roads leading from the infected areas to the city of Hyderabad have been periodically inspected. Each village within the observation circles of the Madras Presidency is inspected, usually every seven days, by the Sanitary Inspector. The most elaborate arrangement of this kind was, however, that adopted in the North-West Provinces, where about 400 square miles in the neighbourhood of Hardwar were brought under a special reporting agency. This area was divided into five divisions, each under a European officer, with a native medical subordinate. Each division was again subdivided into three or four circles in the immediate charge of a native official of the Revenue Department. Two European medical officers were also employed in travelling from village to village. A register was made of the inhabitants of every house. Each village within the area in close communication with Hardwar was visited every three days, and every inhabitant of the area was examined every eight days. In the villages further removed from Hardwar the examination of the people was less frequent. All cases of sickness were reported, and special inspections were made of the sick. As a result of these arrangements, combined with the other measures taken in the infected area in the North-Western Provinces, the spread of the infection beyond a very limited area close to Hardwar was effectually prevented. An organisation based on this model was also employed in the infected tracts in the Punjab. 5153. 25,921. 3954. 8868–7. 9205–9. 10,272.

The advantage of keeping a special watch on the health of the people of places most exposed to infection is obvious. It may not be possible, except as an emergent measure, to entertain an establishment so costly as that employed in the North-Western Provinces, but, where the object aimed at is the protection of a tract of country as yet only slightly infected, it would be worth while to incur considerable expenditure on an organisation of this nature in the hope of preventing the spread of plague. Conclusions regarding system of special observation of health of people in places most exposed to infection.

III.(1.)(*f.*)—*House-to-house Visitation and House Searches for the Discovery of Plague.*

581. Search parties have been used at different places with the object of detecting cases of plague which people are attempting to conceal. Sometimes they have consisted of soldiers or of police, or of other Government or Municipal establishments; sometimes mainly of volunteers supported by the presence of soldiers or police. In cases in which soldiers have been employed in the actual conduct of searches, they have generally been accompanied by respectable native gentlemen in order to give confidence to the people, and ensure the search being carried out with as little interference with their customs and injury to their feelings as possible. House visitation and house searches.

There were many reasons why the search of their houses was likely to excite distrust and alarm among the people. The idea of such a search in order to discover cases of sickness was novel to the native of India, who is at least as averse from the invasion of the privacy of his house as an inhabitant of Western Europe. The females of the better classes of Muhammadans everywhere, and of Hindus in many parts of the country, do not appear in public. Some parts of a Hindu's house, such as the god-room and cook-room, are, by religious custom, closed to people of other races. And, although the attitude of the people towards the search parties is reported to have been sometimes friendly, house-to-house visitation, followed by searching, has not unnaturally been an unpopular measure almost everywhere. Speaking of Bombay, Mr. Du Boulay said that it was "intolerable to the people." House searches were found at Surat to be viewed by the people with "unreasoning terror." The authorities of the Mysore State determined not to adopt house-to-house visitation, because they anticipated that the people would resent it. The objections of the people were apparently increased by the employment of soldiers at house searches. It is not surprising that they should associate the presence of soldiers at the search of a house with a possible use of force, and it is not unnatural that it should have enhanced the dislike entertained by them to the measure. But the actual conduct of the troops, British and native, on the work of searching, did not warrant this attitude on the part of the people. The behaviour of the soldiers in carrying out this disagreeable duty is reported to have been exemplary. Attitude of the people towards house searches. 25,554. Para. 6 of Bombay Govt. Resoln., No. 4,108. P. of 23th June 1899. 3279. 18,388.

Where, as in the areas which were infected in the North-Western Provinces and the Punjab, the establishments at the disposal of the authorities were very strong, the one thorough search which was generally carried out as soon as it was discovered that a place had been infected, was, it appears, usually successful. In the Cantonment of Poona also practically all the cases were discovered during life by means of house-to-house searches. The staff was strong enough to visit almost every house each day. The population to be dealt with numbered only 15,000, was more or less habituated to military discipline, and free from the unreasoning fear of the soldiers that displayed itself at other places. The result of employing search parties is also said System of house searches successful in some places. 10,908–13. 21,216. 21,259. 21,269.

14,218-9
11,370-2.

to have been good at Ankleshwar, and in the Trans-Lyari portion of Karachi. Bu Mr. Giles said, that without a military cordon it was utterly mischievous in th town of Karachi itself, because it scattered the people all over the place. H guarded this statement by observing that in many cases the people did not ge information of an intended search, and so were unable to disperse before the searc party arrived.

In many places, and particularly in the large towns of Bombay, Surat, Poona and Baroda, the search parties met with little success in detecting cases.

House searches in Bombay City.

Gen. Gatacre's Report, pp. 177-181.

The system of house-to-house searches was first tried in Bombay while Genera Gatacre was Chairman of the Plague Committee, and was continued there until March 1898. For some weeks in the early part of 1898, about a hundred men of th Durham Light Infantry and a few native soldiers were used in one ward of Bombay for the actual work of searching. But, with this exception, troops were not used i Bombay City for the actual searches: they were used only for supporting them b patrolling and cordoning the areas to be searched. The persons who conducted th actual searches were generally a qualified medical man, either European or Native and a lady doctor or nurse. They were accompanied by a sub-inspector and two or more police or military sepoys. The District Officer and the officers i charge of sections and sub-sections went round with one or other of the search parties, and, if possible—though this was often difficult—a local Justice of the Peace o Volunteer was attached to each.

Results of house searches in Bombay City.

8259.

10,949-51.

18,464.

489.

25 794.

16,987.

8257.

The general conclusion of those who have had most experience during the later stages of the epidemic in Bombay City, is that house-to-house visitation, although it resulted in the detection of a certain number of cases, was not a successful means for combating plague. "The people dislike it," said Captain Bingley, "and the more energetic the District Officer becomes, the more will the people attempt to elude the results of his work by removing their sick, and by going from one district to another to escape him. We could not get any information at all; at any rate it was very rare." Major Ross, with his large ward of 140,000 inhabitants, and only 150 men to use for searching, found it impossible to do more than make raids at odd times on the very worst localities where the lower class people lived. He thought that he did not discover even 30 per cent. of the cases that occurred. Mr. Roughton, indeed, succeeded by rigorous house-to-house searches in freeing his own district from plague, but he did so by driving it out of his district into other wards. This result also happened elsewhere. Lieutenant-Colonel Wilkins, I.M.S., states that when a search party came into one district, plague cases were carried into another one. Mr. Firth told us that his ward became gradually infected by plague coming from other wards. When the plague staff got energetic in other wards, he said, "they came into my section." Mr. Stewart found that "in Bombay, with its immense chawls full of lodgers, who have little kit and can shift at any moment, house-to-house visitation is of comparatively little use." In March 1898, the system of house-to-house visitation and searches was abandoned in Bombay, and replaced by the present system under which volunteer committees are used for the purpose of discovering cases of plague.

House searches in Surat.

24,417.

16,004.

Experience in Surat was very unfavourable to the system of house-to-house searches. Troops were not employed there. Each search party consisted of one or two European gentlemen and a party of police. "I am bound to say," says Mr. Moore the Collector, "that as far as the search went, it did more harm than good, because it was almost impossible to conceal from the people where we were going to. The consequence was that they removed patients before we came, and therefore infected fresh houses. Plague went on, and the mortality continued to rise until November." "House-to-house search," says Mr. Judge, who was engaged for some time with the search parties in Surat, "was of no practical utility, and inflicted the maximum of inconvenience on the people with minimum results as to the discovery of plague." It was abandoned at Surat in November, 1897.

House searches in Poona.

Mr. Rand's Report, paras. 6 and 7, and Chap. III.

Search parties were employed in the first outbreak at Poona. At first British soldiers alone were employed. With each party there went a native gentleman to act as interpreter, and to point out to the soldiers which the parts of the house were that custom forbade to them. Subsequently the constitution of the search parties was altered from three British to two British and one native soldier. The attitude of the inhabitants of the city towards the search parties is described by the late Mr. Rand, in his report upon the outbreak at Poona, as having been "on the whole friendly, though people who had sick in their houses usually tried to conceal them." And he sums up the results as follows:—"From March 13th to May 19th, 218,124 houses were searched, and 38 plague cases and 64 corpses were found. The houses in the city were searched, on an average, 11 times each. The sufferers from the plague were removed to hospital, and arrangements were made for the prompt disposal

of the corpses. The full value of the work done by the search parties cannot be gauged by the number of cases they discovered. The institution of the search by the military had the effect of causing the public to give information to the Municipal authorities of a large proportion of the cases that occurred, and in many instances even to take patients to hospital without official pressure having been exercised."

During the second outbreak at Poona search parties of British and native soldiers were again used, and were assisted in their work by native gentlemen. A comparison between the mortality in excess of the normal mortality and the reported plague mortality in the two outbreaks at Poona shows that, in spite of the search parties, people succeeded in concealing plague to a great extent. 21,494, 21,526, 21,576.

In Baroda City the State authorities organised search parties without soldiers. The result does not seem to have been satisfactory. The official in charge of the town did not consider that the committees had worked very satisfactorily, and it was found that the sick were carried about from house to house to escape detection. House searches in Baroda City. 14,600-3.

House-searching cannot be made a popular measure, and its efficiency, like that of most other unpopular plague measures, depends on the proportion which the establishment available for doing the work bears to the amount of work to be done. It is a struggle between the authorities on the one hand trying to discover the cases, and the people, on the other hand, trying to hide them, and whichever side is the stronger wins. It is, indeed, possible that, with very strong establishments and with a very limited amount of infection to deal with, a system of house-to-house searches may result in the detection of nearly every case of plague. Where such conditions exist, the advantages of house searches may possibly more than counterbalance the objections to them. But this will rarely be the case, and in other conditions house-to-house searches will be unproductive of good, as well as irritating to the people. Conclusions regarding house searches.

II.(1.)(*g.*)— *Conclusions regarding Measures for obtaining Information of Plague Cases before the Death of the Patients.*

582. As will be gathered from what we have said above, very great difficulty has been experienced in devising any effectual means for the discovery of cases of plague. With the exception of house searches, the objections to which we have just stated, and surveillance, which is obviously inapplicable to the permanent residents of a town of considerable size, every available method for discovering such cases depends on some one coming forward to give information. The essential difficulty is that no one will come forward to give information unless he sees reason to hope for some benefit as the result of his doing so. The members of the sick man's household have no motive for reporting his sickness, except when the patient becomes delirious and unmanageable: on the other hand they have every motive for concealing it lest they may be subjected to plague measures themselves. The medical attendant has no motive for reporting cases, because, if he does so, he will lose his practice. Even where the failure to report a case of plague renders the relatives or medical attendant of the sick man liable to punishment, the prospect of being punished is too uncertain to weigh against the certain inconveniences that must result from coming forward to make a report. The neighbours have, indeed, a motive for reporting a case of plague, viz., that of getting rid of a source of infection which may be dangerous to themselves. But this is neutralised and even outweighed by the fear that, as the result of making a report, they may be themselves troubled by plague measures. The only persons who have a direct motive for making a report are those who can feel assured that they will not, by doing so, expose themselves to any inconvenience, and can also obtain a pecuniary reward by reporting a case. News of an attack of plague, at all events if it is an indigenous case, is, however, seldom likely to penetrate to persons residing so far from the sick man's house that they can feel certain that their giving information of his sickness will not eventually involve them in some disagreeable consequences. General remarks regarding measures for obtaining early information of plague cases during life.

Ever since the epidemic of plague began, continuous efforts have been made to devise some means of overcoming the difficulties in respect of obtaining information of cases of plague during life. But the problem of inducing people to give information, which probably will, so long as there are any plague measures at all, involve them in inconvenient consequences, is one of which no satisfactory solution has yet been found.

II.(2.)—*Measures Adopted with a view to obtaining Information regarding the Occurrence of Plague Deaths.*

II.(2.)(*a.*)—*Registration of Deaths.*

583. Deaths have been registered in both urban and rural areas in India for a number of years, but improvement in registration has been of slow growth, and the system is not yet an efficient one, especially as regards the record of the cause of death. Death registration in India.

The laws in force in the Municipalities in different provinces in India all contain provisions regarding the registration of deaths within Municipal areas.

Provisions of the City of Bombay Municipal Act. Bombay Act III. of 1888, Chapter XVI.

In Bombay City there are District Registrars, and a Municipal Officer has also to be appointed at every place for the disposal of the dead for the purpose of registering particulars regarding the death of every person whose corpse is disposed of there. The obligation of reporting particulars concerning a death to the registering officer is imposed on the nearest relatives present at the death or in attendance on the deceased during his last illness, and, in default of such relatives, on each person present at the death, and on the occupier of the premises. In their default, each inmate of the premises, and the undertaker or other person causing the corpse of the deceased person to be disposed of, is required to make the report which has in all cases to be made at or about the time that the corpse is disposed of. In the case of a person who has been attended in his last illness by a medical practitioner, the practitioner is required to furnish a certificate of the cause of death to the Municipal authorities, and deaths in the hospitals have to be certified by the Medical Officer in charge. A breach of these obligations is punishable with fine.

Provisions of the Municipal Law in Calcutta. 6509–15.

In Calcutta the provisions regarding the registration of deaths are substantially the same as in Bombay. In speaking of the system there, Dr. Nield Cook, the Health Officer at Calcutta, observed, "We have men at the burning and burial grounds who register all the deaths of the bodies brought there for cremation or burial; so that, as a matter of fact, we get the numbers tolerably accurately. But, as most of these people have never been seen by men with any knowledge of rational medicine, and the registrars themselves have no knowledge of it, we have no exact information" as to the cause of death, and reliance has to be placed on the word of friends. How inaccurate the reports can be is illustrated by the evidence of Rai Bahadur 7525. Kailas Chandra Bose, within whose personal knowledge it came that the relatives of two persons who died of plague registered the deaths as due, in one instance, to chronic rheumatism, and in another to chronic dysentery. Speaking in general terms of the 7522. registration of deaths of Calcutta, this witness remarked, "the mortuary returns of our town are not very satisfactory; they only give an idea of the general number of deaths, but not an idea of the causes of death."

Provisions of the Municipal Law in Madras City.

In the City of Madras the corresponding regulations regarding the registration of deaths impose the obligation of reporting on the relatives or attendants of the deceased, or, in their default, on the occupier of the premises in which the death occurred, but no obligation is placed on medical officers to report, except in the case of patients dying in hospital. The Health Officer at Madras, Lieutenant Cornwall, in speaking of the returns of death in Madras City, described them as "fairly accurate." 4353. This remark applied only to the number of deaths, for, he added, "the people report a large number of cases themselves. There are peons at all the burial grounds, whose duty it is to report all the cases that come to be buried there. They take down names and addresses of the people, and the disease from which the person was supposed to have suffered. That, of course, merely resolves itself into either fever or cholera 4354. or diarrhœa." He stated that the cause of death was "generally inaccurate."

Provisions of the law in District Municipalities.

The provisions of the law in district Municipalities in Madras is the same as in Madras itself. The Commissioners of any Municipality within Bengal can be required, at the instance of the Local Government, to make provision for the registration of deaths. And in the Presidency of Bombay, in the North-Western Provinces and Oudh, and in the Punjab, the provisions of the law take the form of empowering a Municipal Committee to make byelaws regarding the registration of deaths with the approval or subject to the confirmation of the Local Government. We understand that as a matter of practice all Municipalities within British India have taken steps to provide rules for the registration of deaths. From evidence which has come before us, however, 23,308–11. 11,291. it would appear that the rules have, in the case of some Municipalities, not been strictly enforced, and in some cases the rules allow of an undue postponement of the death report.

Registration of deaths in rural areas.

In rural areas the duty of reporting is imposed on various village officers in different parts of the country and on the police establishments. The return of deaths made by the village officers eventually reaches the District Civil Authorities, the District Medical Officer, and the Sanitary Commissioner of the Province, but, when they have reached these authorities and an examination of them has disclosed the existence of any exceptional mortality, much time has been lost.

System of death registration in force before outbreak of plague inadequate.

584. When plague broke out in different localities in India, it was found that the system of death registration then in force was inadequate for the purpose of supplying the information which was required for combating the plague epidemic. This was, no doubt, due partly to the difficulty of recognising plague, and partly also to the fact that

plague deaths were concealed, while misleading or incomplete information was given with regard to the cause or place of death.

through difficulty of diagnosis and wilfully false reports of cause of death, and difficulty of ascertaining exact place of death.

The difficulty of recognising plague is not infrequently a real one. Easy as it may be for a medical man of superior qualifications to recognise an ordinary case of bubonic plague, it is a matter of some difficulty, even for him, to diagnose pneumonic or septicæmic cases. In the case of a native subordinate of the Government Medical Department these difficulties are greater, and greater still in the case of the unqualified native practitioner. The people themselves may have the greatest difficulty in recognising the disease. The Health Officer of Bombay said, "the real difficulty is the recognition of the disease." "In the great majority of cases," said the Sanitary Commissioner of the North-West Provinces and Oudh, "they would not know it was plague; they would omit reporting it from sheer ignorance." On the other hand, when plague is prevalent, there is the opposite risk that deaths due to other causes will be attributed to plague. Major Anderson, I.M.S., said, "When village officers once report plague, they are very apt to put down every death which occurs as plague. If death occurs within a week of a man falling sick it is put down as a plague case."

1050.
8867.
23,289.

Even when a death was known to be due to plague, it was often wilfully reported as having been due to some other cause.

There was in the largest cities the further difficulty of finding out the exact house of death. In his report on the plague in Bombay City, Sir James Campbell remarked, "During the past epidemic, in a large proportion of daily deaths, perhaps in one fourth of the whole, the place of death has not been recorded in the cemetery returns with sufficient clearness to enable the case to be traced." And, in another passage, he stated that "the efforts to conceal plague cases and death, with the object of escaping house disinfection, segregation of contacts, and other unavoidable annoyances, have been very successful, and have been, to a great extent, due to the imperfect registration of the house of death."

Sir James Campbell's Report, p. 60.

As we have already observed, the number of deaths not reported at the time to be plague in Bombay City has probably, since the place was first attacked, exceeded 30,000.

Improvements in system of death registration in places attacked or threatened with plague.

585. Owing to the ordinary system of death registration being ineffective in giving even an approximate return of the deaths from plague, attempts were made to improve it in places, chiefly towns, attacked or threatened with plague. The first object being that the plague authority should learn of a death as quickly as possible, the people were in many towns required to report deaths, not to the Municipal authorities, as in ordinary times, but directly to the Plague Officer. They were also required to report deaths immediately, instead of after the somewhat long interval sometimes allowed to them by the rules. In the largest towns, Calcutta and Bombay, no change was made in the ordinary system under which information of the occurrence of a death is given to registering officers stationed at the places where bodies are disposed of. But in these two towns, as in others, a system was instituted of making, as soon as possible after a death was reported, careful local inquiries for the purpose of fixing the exact house of death, and of learning all such circumstances attending the death as would show whether the cause of death was plague or not. These subsequent local inquiries have been very valuable. Even in the large city of Bombay they materially reduce, as mentioned by Mr. Du Boulay, the difference between the plague mortality reported each day and the excess of the actual over the normal mortality for that day, though there is no doubt a large number of cases in which the fact that an individual died of plague is not discovered as early as is necessary, and probably a smaller balance of cases in which the inquiries are inconclusive.

6791-3.
25,617.

II.(2.)(*b.*)—*Corpse Inspection.*

Corpse inspection carried out in certain places.

586. Another method employed in certain places for the purpose of determining whether death resulted from plague or not was the actual examination of the dead bodies. This measure has been used in two sets of circumstances.

It has been used in a place in which the presence of infection was suspected in order to discover whether infection existed or not. It has also been used in a place known to be infected, in order to secure, as far as possible, that no house in which a death from plague had taken place should remain undiscovered. From time to time, with either one or other of these objects, it has been put in force in the towns of Karachi, Hyderabad, and Kotri in Sind, in Poona City and Cantonment, at Sholapur and Satara, in the Porbandar and Palanpur States, in Bangalore City, Bangalore Civil and Military Station, Mysore City, and a number of small towns in the Bombay Presidency.

12,777-8, 18,132-4, 12,554, 21,356-77 21,261, 21,299. 23,881, 22,481. 13,730, 15 166. 25,101-5, 14,281, 22 185. 22,629, 14,665. 9191-9204. 10,252-7. 5928-31.

It was also adopted in some of the rural areas which were infected in the Bombay Presidency and in the Baroda State, in Hardwar and the neighbouring places attacked by plague, and in the infected villages in the Punjab, and in the Hyderabad State.

1129, 6513–5. It was not one of the measures put in force either in Bombay or Calcutta.

At a number of the places at which it has been tried, corpse inspection has not been carried out under the strict conditions requisite to gain the full advantages from it. These are, firstly, that every body in respect of which there is not a certificate from a trustworthy medical attendant that death was not due to plague, must be examined by a member of the medical staff employed on plague measures, and, secondly, that the body must be examined not at the burning ghat or cemetery but at the place of death, so as to ensure that the house in which the death took place is known. In some instances, female bodies were exempted from examination. At other places, the examination was conducted at the cemetery or burning ground.

Such relaxations undoubtedly make the system of examining dead bodies less irksome, and the fact that they were allowed at certain places shows that the authorities sometimes felt that it was not advisable to insist on its being strictly carried out. But, though such relaxation may lessen the people's objection to corpse inspection, they diminish the advantages which would result from rigidly carrying it out.

Attitude of the people towards corpse inspection.

587. As regards the attitude of the people towards corpse inspection, it is an undoubted fact that it has been undertaken at a number of places without any serious objection to it having been made by the people. No objections of any importance were made against it at Poona. That the people at Poona had not any strong feelings in the matter appears to be clear from the evidence of Mr. Patwardhan, a member of the Municipal Committee. He observed: "Examination of corpses was strictly carried out. It ensured correct registration of deaths from plague, and enabled the authorities to trace out every affected locality where a plague death had occurred. In some cases the examination was delayed for several hours. With us, the dead body is to be removed from the house to the cremation ground as early as possible. But if a death took place at 10 p.m. in the night, the dead body had to be kept in the house till 10 a.m. next morning, to allow of its inspection by the Plague Medical Officer. This delay in the examination of corpses caused great annoyance. Corpse inspection is a necessary measure in times of a plague epidemic, but arrangements must be made for examination at all hours of the day and night. It is a necessary measure, but it should be carried out with as little inconvenience to the people as possible."

21,260. 21,825. 21,373. 21,375. 21,549.

22,481–3 23,879–8 In the neighbouring town of Satara, corpse inspection was carried out without objection, and the same was the case at Sholapur, and in smaller towns in the Bombay Presidency in which it was tried.

11,284. In Sind also, corpse inspection seems to have been carried out without creating opposition on the part of the people. Mr. Giles, the Commissioner, observed: "Of course, it is very disagreeable to the people, although the people of Sind are much easier to deal with than people in most parts of India. We did not meet with violent opposition, and the people behaved extremely well. But there are many classes to whom it is no doubt repugnant to their feelings." Mr. Cadell said that no objection was found at Karachi. He did not think that the people liked it anywhere in Sind. It was not objected to at Kotri, Hyderabad, or Rohri. Mr. Gidumal Lekraj, who was employed as a volunteer on plague duty at Karachi, gave it as his opinion that the people objected to corpse inspection, even although it is well conducted, "because it is against the Muhammadan and Hindu religions to touch a corpse after death."

3802. 3806. 18,132–3. 12,521.

9192–3. Speaking of corpse inspection at Hardwar, Mr. Winter said: "They do not like it . . . There was no rioting over any question of corpse inspection. They disliked it, and that may have been one of the causes which led to a small disturbance that occurred, but it was not the immediate cause." Lieutenant-Colonel Thomson, I.M.S., said that the people disliked it, but modifications in the system whereby inspection was dispensed with in cases where the cause of death was certified by a regular practitioner, "induced them to adopt it without any serious opposition." Speaking of Cutch Mandvi, Major Hyde Cates mentioned incidentally that the relatives very much object to their dead being examined.

8809–10. 13,435. 13,552.

In the town of Mysore corpse inspection was carried out without objection. This was also the experience in the Punjab, both in the villages and in the smaller towns in which it was tried, and in the villages in the Satara district.

25,101–5. 10,253–7. 22,747–9.

Examination of female corpses.

588. Difficulties arose at some of the places at which corpse inspection has been tried, with reference to the examination of female bodies.

3803. Just before the second outbreak at Karachi the Muhammadans began to object, and asked for a lady doctor to examine female bodies. At Bangalore the Muhammadans often made objection to their houses being entered, and the Muhammadan females were examined only by nurses. At Porbandar the Muham-

2711.

madans objected to the examination of female bodies. When the plague was raging a nurse examined the Muhammadan females. After the disappearance of plague the authorities ceased to insist on the examination of females. In the villages of the Hyderabad State some of the Muhammadans objected to the examination of females, and, in consequence, female bodies were not examined. 13,730. 5930–1.

589. We have also some evidence as to how it might be regarded in Bombay City and some other places where it has not been attempted. The question of attempting to enforce corpse inspection in Bombay City was considered by the Plague Committee in March 1898. The idea was abandoned on the ground that any attempt to enforce corpse inspection was likely to give rise to active ill-feeling, and that the possible advantages from the practice were outweighed by the certainty of discontent. Dr. W. Venis and Major Ross (who were employed on plague duty in both Bombay and Poona), while they were in favour of corpse inspection as carried out in Poona, were of opinion that it could not be adopted in Bombay. Mr. Du Boulay thought that more complete information might be obtained from corpse inspection, but that it would cause much opposition. Dr. Blaney, whose experience as a medical man in Bombay has extended over a very long period, thought that it might be possible to have corpse inspection at the cemeteries in Bombay. Mr. Lund, a gentleman who had given much assistance as a volunteer on plague duty, considered that it would be objectionable to all the people, particularly Muhammadans, and that it would not be of material assistance. Mr. Macdonald, also a volunteer worker during the epidemics in Bombay, told us that the people allowed him to inspect corpses very freely, and, indeed, invited him to do so. His opinion was that the people were quite willing to allow a European to examine bodies, but would resent examination by a native of a different faith, and might even resent the examination of female bodies by any young doctor.

Feeling in Bombay City regarding corpse inspection. Sir James Campbell's Report, p. 23. 19,924–5. 10,983–5. 25,618. 17,756. 17,886–7. 17,967–70.

590. Dr. Nield Cook, the Health Officer of Calcutta, considered that corpse inspection would be of much help in determining whether plague was in Calcutta. When asked whether there would be much objection to corpse inspection, he replied, "I think it depends very much upon how it is done. With Brahman doctors, I think any objection on the part of a Hindu would easily be got over as far as the males are concerned. But as far as the females are concerned, I think it would be much better left alone. It would cause so much trouble. After all, if we inspected most of the males, it would give us a very good idea." He thought that the people would not like female bodies examined even by women doctors. Dr. Kailas Chandra Bose, a medical practitioner in Calcutta with considerable opportunities for arriving at an opinion on the subject, thought that the people there would object to the examination even of male bodies, and that they would resent being required either to get a death certificate from a medical man or to submit to corpse inspection.

Feeling in Calcutta regarding corpse inspection. 6605–7. 7547–51.

591. In answer to a general question as to whether, judged by his experience at Hardwar, corpse inspection would be possible during an outbreak of plague in one of the large cities of Northern India, Mr. Winter, the Collector of Saharunpur, said: "It would want great tact. It could be done to a certain extent. If plague breaks out in a very large town it would be most important to have corpse inspection at the beginning of the outbreak, in order to ascertain if plague is spreading or not, and to detect each of the earlier cases, to enable measures being in all cases taken to stamp out the disease if possible. But if plague spreads all over the town, there would be no necessity for insisting on corpse inspection. Corpse inspection at the beginning of an outbreak could be arranged if you can get the native leaders on your side." He thought that the people would object more later on than they would at first. "I am talking," he said, "of the first few isolated cases at the beginning. There would be many interested in stamping out the disease, and they would overcome the objections of the few immediately concerned. I do not think it would be possible to carry out the corpse inspection in a large town in which plague had broken out in epidemic form, nor would it be necessary."

Possibility of corpse inspection in large cities of Northern India. 9247. 9248.

592. There can, we think, be no question that corpse inspection is generally offensive to the feelings of the people. They dislike having to retain the dead body in the house till time has been given for the examination to be made. They object to a stranger touching any corpse, and particularly to his touching and stripping the corpse of one of their women. The fact that in places they have submitted to the examination of dead bodies without open objection does not, in our opinion, show that it has not been repugnant to them. The system has not been tried in the places where the people are likely to feel most strongly about it.

Conclusion regarding attitude of people towards corpse inspection.

593. We think that, to warrant the adoption of a measure, which the people regard as they regard corpse inspection, it must be clearly shown that a very high degree

The efficiency of corpse inspection as a plague measure.

of advantage will result from it. We propose, therefore, to inquire whether corpse inspection is so pre-eminently useful as to justify its application in face of the serious dislike with which it is undoubtedly regarded by the people.

It is evident that the inspection of a body after death cannot, as a measure for dealing with plague, be as useful as an efficient system for procuring the prompt report of an attack during life. Infection may be spread by a person who recovers, or before his death by a patient whose death does not occur for several days after his attack. In the first case corpse inspection is obviously inapplicable; in the latter case, the information which corpse inspection gives may come too late to prevent the spread of infection. At best, then, it must, as a plague measure, be regarded only as an inferior substitute for a system which would secure that attacks of plague are promptly reported during life.

Difficulty of recognising certain cases of plague by corpse inspection.

594. It may be argued, however, that the chances of discovering plague during life are small; that reliance must be placed chiefly on the discovery of it after death; that for this purpose the inspection of the body is essential; and that the great bulk of plague cases are bubonic, and therefore easily recognisable after death.

It may at once be granted that corpse inspection would by itself result in the discovery of the great bulk of deaths that have been caused by plague. There are, however, a certain proportion of cases in which the inspection of the body will not establish the fact that the deceased has died of plague. We have already referred to the difficulty of diagnosing certain cases of plague during life. The difficulty of recognising the disease is enhanced after death. Mr. Giles, the Commissioner in Sind, under whose orders a number of medical men were engaged on corpse inspection, observed: "After death our experience is that plague cannot always be diagnosed." This opinion is confirmed by that of medical officers who have been engaged on the work of corpse inspection. Captain Morgan, R.A.M.C., who had been employed on plague duty at Poona for two years, gave it as his opinion that it was impossible in many cases to diagnose a case of plague by mere inspection of the corpse. Lieutenant-Colonel Thomson, I.M.S., after expressing his opinion that corpse inspection is a measure which must be adopted, added that, in pneumonic cases, it is only possible to go by the history. He admitted that the value of inspecting a body in such a case without a *post-mortem* examination would not be great. Captain Chaytor-White, I.M.S., was not, on his experience at Hardwar, prepared to say that he "could, in every case, swear to a pneumonic case by corpse inspection," and thought that the matter could only be made certain by a *post-mortem* examination. Captain Elphick, I.M.S., had observed œdema on the front of the thorax, extending to the upper arms, sides of the chest, and abdomen, in all forms of plague which he had seen in Hardwar and the neighbourhood, and thought that a large number of cases of plague might be discovered by corpse inspection. But this form of œdema, which was so noticeable at Hardwar, has not been elsewhere observed, and Dr. Elphick himself could not be sure that one of the few cases, which he examined *post-mortem*, was a case of plague, even after that examination. Major Reade, R.A.M.C., expressed it as his opinion that pneumonic cases can generally be detected by medical examination after death. He said, "One arrives at it by a process of exclusion mostly. There is a history of cough, and there is sometimes a very frothy condition round the mouth of the corpse—a congestion round the mouth and nostrils." Dr. H. Venis, who was employed under Major Reade in carrying out corpse inspection in Poona, stated that he could discover a case of plague pneumonia from the history and appearance of the body. When asked what appearance of the body or what history would enable him to diagnose a case of plague pneumonia from a case of ordinary pneumonia, he replied that "it would have the appearance of plague," that if there was no bubo the clinical history would have to decide the point, and that a case of acute pneumonia would not necessarily differ from a case of plague. Dr. W. Venis, who was also employed at Poona, said: "In some cases we had a suspicion of pneumonic plague, but of course, we could not be absolutely certain." Lieutenant-Colonel Henderson, I.M.S., employed at Hyderabad (Sind) expressed the opinion that the presence of pneumonic plague could not be ascertained by means of corpse inspection. Major Collie, I.M.S., was emphatically of opinion that plague cannot be diagnosed with certainty in a dead body found in the street, unless a bacteriological or microscopical examination is made, and that there was nothing special about plague pneumonia after death.

11,366.

21,958.

8811-14.

9434-5

9544-9.

10,344-5; 10,379-83; 10,424-9; 12,403; 12,925; 15,926; 18,158-61; 18,557-8; 27,177-80.

9552-3.

21,732-3.

21,165-8.

18,922.

18,135.

20,017-8.

Corpse inspection in practice a defective method for discovering all cases of plague.

595. It is, we think, beyond doubt that a certain proportion of deaths from plague could not be attributed with certainty to that cause, even after the bodies had been inspected by thoroughly qualified European medical officers. The proportion which would escape would vary with the extent to which septicæmic or pneumonic plague might prevail. Where the medical officer employed on the duty of corpse inspection

as was the case, for instance, at Cutch Mandvi, Bulsar, Umreth, Satara, Rohri, Kotri, and Ankleshwar) is a Hospital Assistant, the proportion of cases which will not be discovered might be still larger. In the case of females it will be especially difficult to look for a reliable result from corpse inspection. The examination of their bodies by a male doctor would, in many parts of the country, be impossible. The number of qualified lady doctors available to conduct such examinations is very small. The examination of a corpse by a nurse can be of no use in determining whether the death was due to plague or not. II. 474, 16,170, 14,082-7, 22,482, 12,026, 12,735, 14,265-7.

596. It is impossible to say what proportion of plague deaths would remain undetected if corpse inspection alone were relied on as the means for discovering cases of plague after death; for wherever corpse inspection has been utilised it has been supplemented by independent inquiries into the cause of death. Such inquiries embrace the clinical history of the case, the connexion of the death with other suspicious deaths, the fact of the arrival, at the house, of an infected person, or of the deceased having himself gone to an infected place, and many similar questions. Corpse inspection, assisted by inquiries of this nature, would no doubt leave fewer deaths from plague undetected than such inquiries taken alone. The latter by themselves, if they are made promptly after the occurrence of death, ought, however, to make it clear in the great majority of cases, except in the very largest towns, whether the death was due to plague or not. In towns such as Bombay and Calcutta the residuum of deaths from plague which would remain undetected after such inquiries would, indeed, be not inconsiderable. But in these towns corpse inspection does not appear to us to be a practicable measure, though the President is of opinion that, even in these towns, circumstances may arise which would render the introduction of the measure beneficial. In other towns the residuum of cases in which the cause of death would remain doubtful would be considerably less. Amongst this residuum in which the cause of death is left undiscovered by inquiries alone, there would no doubt be a certain percentage in which the cause of death would be discovered by a medical examination of the body. Any measures for dealing with plague can be as effectually secured by adopting the assumption that these deaths were due to plague as by subjecting the bodies to a medical examination. This being so, and in view of the general repugnance of the people towards an examination of the body, we consider that corpse inspection is not to be recommended as a general measure, and that it should not be carried out anywhere except where Government sanctions it. Corpse inspection not recommended as a general measure; inquiries into history and cause of death will as a rule suffice for discovery of plague deaths.

II.(2.)(c.)—*Conclusions regarding Measures for obtaining Information of Plague after the Death of the Patients.*

597. In connection with the subject of obtaining early information of cases of plague, the question whether the ordinary system of death registration in India might not be made more effective for discovering deaths due to epidemic disease has naturally suggested itself We fear that it is not possible to effect any material improvement except very gradually; for its chief defects are not so much in respect of the numbers of deaths as in respect of the causes of them. They result in the main from the fact that very few of the people of India ever see a medical practitioner, qualified according to European methods, from the moment of their birth to that of their death. This is true, even in the most advanced towns in India. It is estimated that from one-third to one-half of the people of Calcutta have no medical attendant. The Health Officer considers that in Bombay the number is still smaller. He said "Previous to the appearance of plague in Bombay, I do not think that 10 per cent. of all dying were attended by medical men." In the smaller towns the proportion of the population attended by qualified medical men is even less, and the villagers are often dependent solely on the occasional visits of an itinerant medical attendant, who practises according to Oriental methods, and is entirely unqualified according to the European standard. The Head of the Indian Medical Service doubted whether five per cent. of the total population would before death be attended by a European medical officer or by a native medical officer practising with a diploma obtained at a Government medical school. It is obviously impossible to expect in a country so circumstanced a standard of death registration in any way comparable with that in Great Britain, where 98 per cent. of the deaths are certified either by registered medical practitioners or by coroners, or by similar officials after careful inquiry. The Indian Government cannot provide medical advice throughout the country generally. Thus the main cause of the imperfections in death registration is one that is beyond the power of the Government to reach. Direct information regarding the cause of death will, for many years to come, be very seldom obtainable; reliance General remarks on death registration in India. 7783-7793, 8012, 8054. 7701, 6635, 15,834. 1132-3.

will largely have to be placed on that indirect information that is to be gathered from all the circumstances of the death, and specially from its relation to any other recent unexplained deaths.

Dr. Wright and Dr. Ruffer dissent from some of the above conclusions.

598. From some of the conclusions arrived at above, two of us, Dr. Wright and Dr. Ruffer, dissent, and they have expressed their views in a note which is published as an Appendix* to this Report.

III. Measures to check Plague in Infected Places.

III.(1.)—*Removal of Plague Patients to Hospital.*

Removal of plague patients to hospital.

599. The non-isolation of plague patients must always involve a certain amount of danger to the community of which they are members. The necessity for providing against this danger, and the possibility that patients would have a better chance of recovery in well-arranged hospitals than in their own houses, suggested that it would be a useful measure to remove plague patients to hospital.

Policy adopted in Bombay City.

Mr. Snow's Report, pp. 6–7.

600. The removal of plague patients to hospital was one of the first measures taken on the outbreak of plague in Bombay City. The exact conditions under which it was carried out have varied a good deal at different periods during the epidemic. Up to the end of October 1896, the policy was to remove all plague cases to hospital. Any person certified by a duly qualified medical authority to be suffering from plague was liable to removal to hospital even though he might be already provided with proper accommodation. The attempt to take all plague patients to hospital, however, created hostility and alarm. Reports were freely circulated that the authorities took people to hospital merely to make a speedy end of them. Crowds constantly collected round the ambulance vans and stoned them, and the officials of the Health Department were often in danger of their lives. Matters culminated on the 29th October in an attack on the Municipal Hospital by nearly a thousand mill hands. The shopkeepers began to close their shops, and as looting and severe rioting were anticipated, and it was feared that the sanitary staff—without whom Bombay would in a few days become uninhabitable—would make common cause with the rioters, proclamations were issued modifying the existing policy.

Mr. Snow's Report, p. 15.

The modifications then made were carried out for about four months, viz., from the issue of these proclamations to the appointment of the Plague Committee under General Gatacre. All attempts to remove patients to hospital were not indeed abandoned in this interval. But where proper isolation and treatment could be carried out in the patients' own houses, arrangements were invariably made to transfer them to the best lighted and ventilated rooms, and every endeavour was made to dissuade all but the few necessary attendants from frequenting that part of the dwelling. It was only where circumstances made it impossible to make any suitable arrangement or where the patients were paupers or friendless, that they were removed to the hospital.

Gen. Gatacre's Report, pp. 15–24.

With the appointment of the Plague Committee in the beginning of March 1897 there was a reversion to the first policy, and an order was promulgated that all cases of plague must be removed to the hospital at once. House to house searches were accordingly instituted to find the cases; and the number of Municipal, private, and caste hospitals was largely increased.

923–933.
8258.
25,559–61.

This policy—relaxed somewhat till plague began to rise again in the second outbreak—continued for about a year, when, in consequence of the feeling of irritation produced by the more rigorous of the plague measures, forcible house to house searchings were abandoned in favour of a system of inquiries by local volunteer committees. From this time also Plague Officers were permitted to exercise a discretion in the matter of removing patients to hospital, and to allow a patient living in a house, not overcrowded, and with fairly good sanitary conditions, to be treated in his own room. This is still the principle acted upon in Bombay City. District Officers have a discretion to leave patients at home if proper arrangements for isolation and treatment can be made. This privilege is, however, but very rarely granted. The rule is to remove all but moribund plague patients to hospital.

Policy adopted in Calcutta.

6526.
6587–88.
6445.
II., 532.

601. In Calcutta also, during the first outbreak, though two or three patients who had "good airy upstairs accommodation," were allowed to remain in their houses, the practice was to take patients to hospital. As in Bombay, great difficulty was encountered in removing the patients, and all sorts of devices were resorted to to prevent the discovery of cases. We are told that "there was great opposition, at one time." In consequence of the experience in the first outbreak, it was determined in the second outbreak to leave the sick, if they preferred it, in their own houses.

* *See* Appendix IV.

602. In Karachi, after the first epidemic had reached its height, and generally in infected towns throughout India, the compulsory removal of plague patients to hospitals has been insisted upon. Policy generally adopted. 11,108, 11,620–11,952.

603. This measure has been everywhere unpopular. The public have shown a vague fear and detestation of the Government Hospitals. They have misunderstood the object with which the Government has directed the removal of the sick to hospital. For instance, there was in Bombay City a universal idea that the patients in the Municipal Hospital were all operated on during life and dissected after death. "I know," says Mr. Macdonald, who had large experience as a volunteer Plague Officer in one quarter of the City, "that those who recovered consider that their recovery was due to my having spoken for them to the doctors and nurses, rather than to the great care both doctors and nurses took of them." At Bandra the people thought they were going to be poisoned, and they objected altogether to go to hospital. So strongly did some of them fear poison that in many instances hospital patients not only refused to take medicine, but refused to take even food and drink. The people of the Sirohi State objected, Captain Grant, I.M.S., told us, to being medically treated; "they objected to our touching them." At Bangalore it was rumoured that the Government were poisoning the people. Mr. Madhava Rao, the Plague Commissioner, speaking of the concealment of cases and the flight of contacts to evade detection and removal to hospitals and camps, says, that "these measures, in spite of every effort to popularize them, were and are a source of horror and dread to the people." Attitude of the people towards removal to hospital. Mr. Snow's Report, paras. 7, 19. 17,950. 18,574 9884–5. 2628. 3239.

604. The unreasoning terror of the people at the idea of going to hospital was increased owing to the numbers who died after they were taken there. In view of the great fatality of plague, and in view of the fact that many patients were brought in in a moribund condition, the mortality at many of the Plague Hospitals has unfortunately been very high. In Calcutta it was about 80 per cent.; in the Arthur Road Hospital at Bombay, 71·44 per cent.; at Baroda, about 71 per cent; at the Sassoon Hospital at Poona, 74·4 per cent.; at the hospital in Bangalore City, 66·7; at the three hospitals in the Civil and Military Station at Bangalore, 63·4, 60·2, and 71·4 respectively. A large number of the people removed to the hospital must have been *in extremis* or actually dead when they arrived there. At Calcutta many people were taken to hospital in a dying state. At the principal Plague Hospital in Bombay (the Arthur Road Hospital), about 38 per cent. of the deaths occurred within 24 hours of admission. In the Plague Hospitals at Baroda and Ankleshwar, the deaths which occurred within 24 hours of admission numbered 143 and 61 out of a total of 342 and 141 deaths respectively. We have already, in a preliminary report, expressed the view that moribund patients should not be removed to hospital, and the removal of moribund patients has been prohibited by the Government of India. It is, however, extremely difficult for a Medical Officer to feel certain that a patient has no chance of recovery. Even when a patient at his house looks fairly well he may be in a moribund condition when he arrives at the hospital. It is for these reasons difficult, under a system for the compulsory removal of sick persons to hospital, to guard against the removal of persons who would reach the hospital in a dead or dying condition.

As, in his opinion, the evidence now in the possession of the Commission indicates that patients suffering from the bubonic, or the most frequent, form of plague become specially liable to communicate the disease to others a few hours before death, and patients suffering from the pneumonic form probably at all periods of the illness, and, further, as there are almost insuperable difficulties in determining that a patient is actually in a dying condition, the President considers that moribund patients should be removed to hospital wherever effective isolation in their houses is impossible, unless grave reasons of policy outweigh the serious risks that are otherwise incurred. Removal of moribund cases. 6550. 21,050. 15,266, 12,896; 25,024; III 286. 6551. 25,734. 15,266, 14,443.

605. The wide-spread apprehension of the hospitals was one of the reasons which led to the habitual concealment of cases. It is impossible to say how far this result was due to the fear of the removal of the sick person to hospital, and how far to the fear of other plague measures, especially the removal and segregation of the sick man's friends and relatives. We think that it must be attributed to both causes. At any rate in large towns where plague measures were strict, the people concealed plague to such an extent as to practically defeat the measures taken against it. In Bombay and Calcutta, as well as in other places, they carried plague-stricken persons from one house to another, and thus increased not only the number of infected houses but also the uncertainty as to what houses were infected. Concealment of cases an effect of fear of removal to hospital. 489, 6599, 16,229–32.

606. The fear of removal to hospital was most acute in the case of hospitals maintained by Government or authorities identified with Government. Early in the history of the Steps taken to render removal to hospital as little

objectionable to the people as possible.

epidemic in India attempts were made to render compulsory removal to hospital as little objectionable to the people as possible. The sick person was generally permitted to have with him one or two of his friends to take care of him. And since one of the objections to going to hospital was found to be a fear of treatment according to European methods, he was allowed to have native treatment if he preferred it. The establishment of private hospitals and hospitals maintained for the treatment of persons belonging only to particular classes or castes was encouraged. In October 1896 two non-Government hospitals for Hindus were opened in different parts of Bombay. At that time the terror of hospital life extended even to hospitals of this nature. But the next attempt produced better results. In December 1896 a Jain Hospital, a little later a Parsee Hospital, at the end of January a Hindu Hospital—all started by the leaders of the various communities for their fellow castemen—were opened, and they and a private hospital started by the Bombay Port Trustees for their own employés were all at work and doing excellent service before the Bombay Plague Committee was created. Subsequently, there was a great extension of this system of private hospitals. There were 28 of them while General Gatacre, and 36 while Sir James Campbell, was Chairman of the Committee; and both these officers are of opinion that the results were distinctly successful. In dealing with Orientals many intricate questions relating to caste and race habits, food, religious scruples, and similar matters arise, and they require very careful treatment. The establishment by the various communities of caste hospitals managed entirely by their own Medical Officers and staff, though supervised by the Government Medical Officer, was found in Bombay to help to allay the apprehensions of the people on these points. This was also the experience at almost all the places where the experiment was tried. But the antipathy to hospitals was not altogether removed even by the establishment of private hospitals. In particular at Calcutta private hospitals were not much used.

Mr. Snow's Report, p. 6.

Gen. Gatacre's Report, pp. 12–14. Sir James Campbell's Report, pp. 108–9

II., 533.

Compulsory removal to hospital not insisted on in certain places.

607. Such being the feeling of the people, the experiment has been tried in some places notably at Calcutta and Mysore, of leaving it to the option of the people to go or not to go, to hospital. The result was that in Calcutta, from 18th February 1899 to the end of August, only 103 went to hospital out of 1,638 patients, and in Mysore, from the beginning of the outbreak there in November 1898 up to 8th September 1899, only 312 out of 2,795. These results are not encouraging. Experience at these two places throws great doubt on the possibility of ever securing, on any system under which the people are allowed to exercise an option, the isolation in hospital of any substantial proportion of the sick. We must, therefore, look carefully at the results which have followed in places where there was little or no isolation of the sick. To give examples of places where the sick were not removed to hospital, and the total attack rate was very high, is unnecessary. Such places undoubtedly exist just as places exist—Poona in the rains of 1899 appears to be one—where the rigorous removal to hospital of every patient that could be found did not save the people from an outbreak of extreme violence. The question really at issue is whether the remaining of the sick in the houses is, in every place, accompanied by a very high rate of attack in a town, or a very high rate of attack in individual houses. This question would appear to be answered by what happened at Mysore City and Calcutta in the second outbreak, in both of which places, as mentioned above, no measures of compulsory removal were adopted, and the great bulk of the sick have remained in their own homes.

Abandonment of compulsory removal to hospital in Mysore City not accompanied by high rate of attack.

608. We will deal first with the facts relating to Mysore City. Inasmuch, as from the time when the outbreak in Mysore City began at the end of November 1898, down to the 8th September 1899, the number of deaths which actually occurred exceeded the normal mortality by 3,035, we may assume that this number approximately represents the number of deaths from plague that occurred. The number of cases of plague that occurred must have been considerably larger. Assuming that the plague mortality was as high as 75 per cent., there must have been at least 4,000 plague cases. Of these, only 312, as we have seen above, were brought into hospital. It is probable that we shall not be seriously over-estimating the number of plague cases treated in their own homes if we put them down at about 3,700. With regard to the number of houses in Mysore City in which more than one case of plague occurred, we are told by Mr. Cadell that up to the end of February 1899, only 119 houses in Mysore City had had more than one attack of plague in them, while in 76 of these there had been two attacks, and in 26 three attacks. In the remaining 17 the number of attacks had varied from four to ten, more than seven attacks having occurred in one house only.

25,031.

609. The experience at Calcutta was of much the same kind. From the time when Calcutta was declared to be infected in February 1899, up to the end of August, there were 1,638 reported cases of plague and 1,517 deaths. The total mortality during that interval was 13,017, as compared with a normal mortality of 11,842. We obtained from the special Health Officer at Calcutta, a report with regard to the effect of treating patients at home on the spread of the disease among the other occupants of the infected house, and among persons resident in the neighbourhood. He gave the following opinion :—" It is difficult to separate these questions, as bustee huts communicate so with one another that the distinction between a room and a separate house is not marked. I am of opinion that the results of the present measures do not suggest that they have tended in any way to cause the spread of plague; in fact, the indications are the other way. Although we still hear of cases chiefly through the death reports, I am of opinion that, with more stringent measures, there would have been a far more systematic suppression of cases, by disposing of the dead anyhow, and by giving false information. As well, there would have been an active opposition to disinfection and to moving patients, and although a few more cases would have been able to be shown as 'died in hospital,' the disinfection of places where plague patients had died and their vacation would have been less, and the probability of disease spreading greater, not taking into account the scare and dislocation of trade that would have been caused and the race animosities aroused. I would here point out that disinfection is not confined to plague cases, but is performed after nearly every death from fever. After death has taken place, the fact that a death has occurred is willingly volunteered, and disinfection is even sought for by the neighbours." As to the effect of this policy on other residents of the houses in which the sick persons were left, he said that, as a rule, many cases had not occurred in the same house. In some instances, however, the number attacked in individual houses had been large. These facts show clearly that plague did not spread in Calcutta in the manner in which it has spread in other large towns, and that, speaking generally, there was no great concealment of plague.

Experience in Calcutta similar to that in Mysore City.

II. 537.

Judged merely by the results set forth above, without taking any other circumstances into consideration, the policy of permitting the sick to be treated in their own houses does not appear, in the case of Calcutta or Mysore City, to have led to a violent outbreak.

610. We may now proceed to sum up our conclusions on this difficult question of the expediency of the compulsory removal of all plague cases to hospital. From the experience of the past, it seems to us to be clear that where the Government has been able to provide a staff sufficiently strong to defeat concealment, the compulsory removal to hospital has been a measure which has justified itself. It has, however, in only a few outbreaks been possible to provide a sufficiently strong staff. In most places, and particularly in the large towns, compulsory removal to hospital has led to great concealment of cases, with the result that infected houses have escaped disinfection. It has further led to the removal of the sick from one house to another, and has thus directly contributed to the spread of plague. Further, owing to the prevalent fear of hospitals, compulsory removal to hospitals of sick persons has, in some cases, had a prejudicial effect on the person so removed, particularly when he has been hovering between life and death. Therefore, where the staff available has not been sufficiently strong to secure the removal of all, or, at any rate, of the large bulk of plague patients to hospital, the measure has not been justified by the results. Since then the measure is, in many cases, inexpedient, the possibility of the extent to which people will bring plague patients into hospital on a voluntary system becomes a question for serious consideration. Experience hitherto gained shows that the great majority of people will not go to hospital unless they are compelled to do so. The aversion to hospitals may be to some extent lessened by encouraging the establishment of private and caste hospitals, by locating hospitals near to infected quarters so as to lessen as far as possible the distance the plague patient has to be carried, by limiting the size of hospitals and paying special attention to the provision of medical attendants and nurses and of ample and comfortable accommodation for patients, by providing accommodation close to each hospital for the friends of patients, and by permitting at least two friends to be in attendance on each patient so that the patient may never be left alone. We think, however, that it cannot be anticipated that, for the present at all events, any steps of this nature will have any very material effect on the attitude of the people as regards removal to hospital. In the great majority of cases they will evade going to hospital as far as they possibly can.

Conclusions regarding removal to hospital.

In most cases the choice would thus seem to lie between attempts to remove the sick compulsorily to hospital and treating them in their houses. We have, therefore, to consider what results may be anticipated to follow if the sick are left in their houses. There can be no doubt that in most cases this will tend to the spread of plague. But the experience in Mysore City and Calcutta—an experience, however, which cannot be generalised from—shows that the leaving of the sick in their houses is not everywhere followed by a very serious spread of plague.

To sum up, it appears that the adoption of a policy of removal to hospital, if it is unsuccessful, will, as we have seen above, carry evils in its train. On the other hand, evils will result from abstaining from all attempt to remove plague patients to hospital. The question as to which policy most contributes to the spread of plague is one that must be answered with reference to the particular circumstances of each place. In dealing with imported cases, or with an outbreak in a small place upon which it is possible to concentrate a sufficiently strong establishment, there can, in our opinion, be no question that the first measure to adopt is the compulsory removal of the sick to hospital. In dealing with early indigenous cases the same policy may be advisable. But if this policy prove unsuccessful in a large place a point may soon be reached where the authorities will not succeed in getting the bulk of the cases. When this point is reached, the fact must be faced that the evils of leaving the sick in their houses are being incurred, and that the attempt to remove to hospital may be superadding to these the evils that result from carrying about sick persons from house to house. At this stage the less the sick are interfered with the better. It appears to us that all that can then be done is to endeavour to palliate the evils that result from leaving people in their houses by arranging for continual visitation of the sick at home, the frequent disinfection of the infected room, and the encouragement of inoculation among persons left in the houses with the sick.

III.(2.)—*Segregation of Contacts.*

Grounds upon which segregation has been adopted.

611. The segregation of persons who have been in immediate contact with plague patients is a measure which has been adopted on the general ground that it is necessary to keep, for a period at least as long as the period of incubation, under continuous medical observation those who, there is reason to believe, may have contracted plague. Among Orientals of the classes specially affected by plague, the only effective way of carrying this out is to place them in a segregation camp. An incidental benefit claimed for segregation in camps is that it ensures the vacation of plague-infected houses until there has been time to disinfect the buildings, and so operates in the direction of protecting the contacts themselves from the risk of infection from these houses.

Conditions essential to success of a system of segregation.

To make segregation effective in protecting the community, the great majority, at all events, of the contacts must be found out and detained for the maximum period of incubation; and those in contact with each successive case of plague that occurs in the segregation camp must be detained for a further period of the same length. All clothing must be effectually disinfected on entry into the camp, and, if plague cases have occurred within the camp since the date of entry, at the time of departure also. Success depends on the active co-operation of the people, combined with the maintenance of large establishments. The staff available to carry out the segregation of contacts must be very strong relatively to the number of cases that occur. Opposition or indifference on the part of the people, and venality on the part of the guards, are difficulties in the way of effective measures for segregating contacts which have always to be reckoned with.

Attitude of the people towards segregation: measure unpopular.

612. Although the segregation of the healthy inmates of infected houses may not generally have been so distasteful to the people as the isolation of the sick, there is no question that it has been invariably resented by the residents in towns, and that in particular places the popular feeling against it has been very deep-rooted. The people dislike the confinement of the segregation camps. They do not like being removed from their houses, and from their relations and friends. They are put to great trouble in removing their goods and chattels, and they are exposed to the danger of being deprived of property by unscrupulous underlings during the process of removal. They do not like being absent when their houses are disinfected. Sometimes they object that the huts provided for them are insecure and liable to catch fire. Sometimes they object to go to the segregation camp because it involves a second move after only ten days. The well-to-do and those who are members of the higher castes object to being sent to the same camp as the poor and low classes. Probably many other reasons, which have a very real effect on the oriental mind,

though they would not all appeal to a European, have been operative along with the reasons specified above in creating among the people a very wide-spread fear of the compulsory segregation of contacts. In many cases contacts have run away rather than submit to it; in others they have stayed in the town and systematically evaded it.

Fear of segregation the cause of panic and concealment of cases.

613. The fear of segregation led to a great exodus from Calcutta before the measure was put into force. The same thing happened at Bangalore City and the Civil and Military Station. Colonel Robertson, speaking of Bangalore Civil and Military Station, said, "the fear of segregation and our measures is such as to render the segregation measures practically useless." Mr. Madhava Rao said of Bangalore City, "From the commencement, the people manifested the utmost abhorrence to compulsory segregation, and great difficulty was experienced in effecting it." Lieut.-Col. Benson, I.M.S., also speaking of Bangalore City, said: "From the very commencement it was clear that compulsory segregation as a measure for arresting the disease was objectionable to the people, and large and numerous meetings protesting against it were held. Concealment of cases occurred almost from the commencement, but it was not until the epidemic assumed greater proportions that they began to dispose of their dead by throwing them out of their houses at night, &c.; 15 dead bodies were found in locked-up houses, and no less than 392 unclaimed bodies were found and disposed of." In Sholapur, "as soon as a case occurred in a house, all the inmates at once fled to escape infection and segregation." The dread of segregation made people run away from Nasik. Writing after an experience of two outbreaks in Karachi, Mr. Giles, the Commissioner in Sind, thus referred to the question of the unpopularity of segregation camps; "the Plague Committee, with the Acting Commissioner's approval, passed an order permitting people to leave the city with their sick, and without detention in the regular segregation camp, provided that they notified the sickness. The result has been that the one great and overwhelming dread which ever abided in the minds of the people, namely, the dread of the government segregation camp, was removed, and with it the occasion for scattering like rabbits, whenever a case occurred, into houses and other streets, carrying infection with them, ended also." Other instances could be cited of the agitation produced in the public mind by the fear of segregation, but it is unnecessary to quote them.

6540–6544.
2622.
3282.
3480.
20,237.
13,833.
Paras. 17 and 18 of Report of 31 January 1899 to the Government of Bombay.

Contacts flying from segregation carry infection to uninfected places.

614. The result of the feelings entertained by the people towards segregation was clearly shown not only in the dispersal of contacts from infected into uninfected places at a distance, but also from the infected parts of towns into uninfected parts. A large proportion of plague cases come to the notice of the authorities when the patient is either dead or moribund. The contacts have had an opportunity of escaping before the case is discovered, and the testimony of all witnesses who were able to speak from personal experience is that many of them succeeded in doing so. This fact is proved beyond a doubt by the statistics at the different segregation camps.

History of segregation in Bombay City.

615. Segregation of contacts was not attempted in the earliest stages of the outbreak in Bombay City when the operations against the plague were under the control of the Municipal authorities. The vacation of infected rooms, for the benefit of the contacts, was regarded by those authorities as more important than the detention of the contacts in camp, for the benefit of the community. One of the measures which General Gatacre's Committee was intended to take up was a gradual introduction of an organisation for the segregation of the people living in the same room with, or in close attendance on, a person found to have the plague. The Committee were accordingly authorised to place the inmates of an infected building in segregation for a period not exceeding ten days. There was, however, no organised system of camps for the segregation of contacts under General Gatacre's Committee. Directly a plague patient was removed to hospital his family was required to vacate his room, and disinfection commenced at once. The majority of the contacts were allowed to find fresh accommodation where they pleased. Only a certain number of the relatives of patients attended them to hospital and were segregated in its neighbourhood. The Committee under Sir James Campbell did not issue rules for the segregation of contacts till the beginning of December 1897. These rules provided that "in large bawls, unless several attacks took place, it would not usually be necessary to segregate the inmates of more than two or three rooms. In the case of large private houses contacts might be segregated either in another part of the same house, or in an outbuilding in the same enclosure. Provision was also made that, when plague officers admitted its fitness, contacts might be isolated in a private segregation camp; that cases occurring in an eating-house would not necessitate the other inmates being made contacts; that leniency should be shown

Mr. Snow's Report, p. 18.
Gen. Gatacre's Report, p. 5.
Sir James Campbell's Report, pp. 12, 13.

to goldsmiths, handloom weavers, and other craftsmen who work in their houses. The detention of contacts was to be limited to ten days and might be reduced to seven, and a modified system of surveillance was sanctioned." Rations were provided in the public segregation camps, and in the case of these camps the bread-winner, whether man or woman, was allowed to go to work, provided the night was spent in camp. All clothes were disinfected at the camp, and suspicious contacts were, on arrival at the camp, kept separate until they either developed plague and were sent to the Plague Hospital, or until the fall of their temperatures made it safe for them to mix with the other inmates of the camp.

10,953. The segregation of contacts does not appear to have been very strictly carried out during the second outbreak at Bombay. Major Ross said in his evidence, "To a modified extent they were taken to contact camps—one or two of them—we were not allowed in Bombay to take the whole of the contacts." He estimated the contacts taken to camp 4029–33. at about one-fifth of the actual number of contacts. Major Bannerman, I.M.S., on an examination of the figures for 15 days in March 1898, came to the conclusion that only one contact was taken to camp for every four cases of plague. And Captain Bingley, 8258. who succeeded Major Ross, informed us that "if a family was fairly respectable and the place was not overcrowded, and there was good accommodation in a house, the contacts were allowed to be segregated in that house, the district officer keeping them under observation and visiting them every day. The removal to contact camps was reserved for cases of extreme overcrowding, and of very poor patients."

1354. During the third outbreak the measure has been still further modified, so as to be as little irksome as possible to the people affected by it. "The contacts," said Mr. Du Boulay, "are the only people whom we treat at all strictly, and we have very much modified regulations in regard to them. We merely tell them that they have to come back to camp at night. We tell them that they are not to sleep out. We have not found that that has resulted in many of them running away." And again, "We have not 25,629. what you call a contact camp. I mean the restrictions are almost entirely abolished. All we can say is that 'you must bring your cooking pots to camp, and you must live there.' They can go to their work, but they are expected to sleep there; they do not go to their own houses."

It is thus apparent that the only effectual system for the segregation of contacts, viz., that under which a contact is continuously detained under observation in camp for at least the full period of incubation, was not at any period strictly carried out in the City of Bombay.

Segregation in places other than Bombay City.

616. In the infected towns in the interior of the Bombay Presidency, such, for instance, as Poona, the segregation of contacts was attempted at an earlier date than it was at Bombay City, and the manner in which it was carried out in them was generally stricter. At Poona, during the first outbreak, after an initial period during which the policy was to allow the contacts to pursue their ordinary avocations in the city during the daytime, segregation was strictly enforced. Strict regulations regarding segregation were also in force at other places, among them at the big towns of Surat (during the second outbreak), Karachi (during the latter part of the first and the earlier part of the second outbreak), Bangalore City, and the Civil and Military Station of Bangalore, as well as in the infected places in the Saharanpur District of the North-Western Provinces, and in the infected villages in the Punjab, Sind, the Baroda and Cutch States, and elsewhere.

Mr. Rand's Report, p. 23.

24,417–9 11,157–9, 3261, 2576, 9331–7, 10,057, 12,027, 13,377 13,440–1, 14,619.

Modifications adopted to relax the strictness of segregation.

617. As experience was gained it was often found necessary to modify the strictness of the regulations for segregation. Thus in the later stages of the second outbreak in Poona the system of the strict segregation of contacts in a segregation camp was 21,694. definitely abandoned, and an arrangement, under which people were allowed out during the day but had to report themselves at night and to answer a roll-call in the morning, was substituted for it. When plague again became indigenous in Poona, in February 21,817-20. 1899, contacts were kept under observation for ten days; it was incumbent on them to sleep in the camp, but during the day they were given their liberty, and as soon as the disinfection of the house, which was commenced immediately after the removal of the patient, had been completed, they were permitted to re-occupy it during the day-time. Later on a further concession was made by allowing the contacts after sleeping three nights in camp to complete the period of observation in their own houses.

11,674-75. 11,822. 11,894. In Karachi again, the rule prohibiting contacts from re-entering the town was relaxed during the second outbreak, so as to allow people who had to go to offices within the town to leave camp during the day for business purposes. The subsequent

adoption of a system of voluntary camps, towards the close of the second outbreak and during the third outbreak, led to a general relaxation of the system of segregation. 11,258-65.

A similar relaxation of the rules at Bangalore Civil and Military Station, so as to permit the people to go out to earn their living, led, it was found, to the disappearance of those who should have returned to the camp, and the concession had to be withdrawn. 2670.

Later on it was determined in some places not to enforce strict segregation in the case of persons who had been inoculated, inasmuch as it did not seem likely that they would develop plague. At Calcutta, for example, when the first outbreak began, arrangements were made to exempt households, the whole of whom had been inoculated, from segregation on the occurrence of plague among them. Similar concessions were made in Bangalore City and the Civil and Military Station of Bangalore. The inoculated contacts were allowed to remain with the patient if he was living in a house of the better class, otherwise they were removed from the house but not required to go to the segregation camp. In Hubli the concession of not being taken to the segregation camp was given only to twice inoculated contacts, and they were allowed to re-occupy the house after it had been disinfected. 6537. 2637-8. 2662-4. 3292. 19,826-26. 23,375.

A further development at certain places has been the practical abandonment of the attempt to segregate contacts. This was in some places due, in the first instance, to the impossibility of carrying out the measure owing to climatic or physical difficulties. Segregation had to be abandoned during the height of the rainy season at certain places in the Bombay Presidency such as Bhiwandi and Kalyan in the Thana district and the city of Belgaum. The segregation of contacts was nominally one of the measures adopted in the outbreak of 1898 at Calcutta, but it was not actually enforced after the first case or two. When a patient was taken to hospital the people in the infected room were required to leave it and it was thoroughly disinfected. They were permitted to go where they liked. Calcutta is so closely surrounded by swamps that the area necessary for segregation, had plague assumed large proportions, could not have been found during the rains, and the rains were close at hand when plague made its appearance. In the outbreak of 1899, the segregation of contacts was in Calcutta designedly abandoned. The same policy was followed in the city of Mysore. 22,116, 22,176. 19,660. 22,807-8. 6534-35. 6591-95. II. 532. 24,956-62.

618. The average number of contacts to each plague case was estimated by some witnesses at about four, but one witness estimated it at seven or eight. The figures ascertained at the Census of 1891 give the average number of persons per house at 5·4 over India. In Bombay, where certain classes habitually live in large lodging-houses, the average number of persons per house is calculated at 14·4. Probably four contacts to each plague case would be the average number in rural tracts. In the larger towns the number is certainly much higher. As a matter of fact, nothing like these numbers were actually taken to the segregation camps. Statistics regarding segregation camps. 493. Mr. Baines' Census Report for 1891 p. 57.

In Bombay, the maximum number of contacts in the segregation camp at any one time under Sir James Campbell's Committee was 1,061 (on March 7th, 1898). The average daily number of attacks was high, and sometimes, as in February, exceeded 60. Thus, as stated in Sir James Campbell's report, "even after adding the number sent to hospital as attendants, this falls far short of the total number of contacts which the average daily plague cases in January and February should have yielded." The number admitted to the chief Bombay contact camp at Wari Bandar between January 1st and April 30th, 1898, was 5,790. The recorded plague cases in this interval were 5,053, and the real number was much larger. Sir James Campbell's Report, p. 13. 20,442.

In Karachi, during the second outbreak, the total number of contacts segregated between 25th March and 11th December 1898, was 10,215. There were in this interval about 3,000 ascertained cases of plague. The maximum number of contacts segregated in any one week was 934, but the recorded cases of plague were in some weeks over 500. 11,660. 11,628.

In six months during the second outbreak of plague in Surat, 6,551 persons were segregated as contacts. The recorded plague cases during this period were 1,672, so that nearly four contacts were segregated to every ascertained case of plague. But there were in Surat two reasons for what is, at first sight, a more successful result than was attained in other large towns. In the first place, the neighbours of a person suffering from plague were under certain circumstances liable to be segregated in the contact camp, and in the second place the inmates of every house in which an uncertificated death took place were all segregated. There were thus in the contact camp a number of persons who were not known to have been actually in contact with a plague case. 15,918. 16,004. 15,715.

Mr Rand's Report and Appendices.

The segregation of contacts was systematicaly enforced in Poona, from March 13th 1897. Up to May 20th, 1897, 4,058 persons were segregated, and the reported plague cases during this interval were 1,447, so that even at Poona, where the measures taken for the detection of cases and the segregation of contacts were more vigorously enforced than anywhere else, and where troops were used for the detection of cases, the proportion of contacts, which the authorities were able to find, was not

21,635. high. In the second outbreak at Poona there were 4,644 reported cases of plague between August 1897 and the end of February 1898. The number of contacts

21,834. segregated was 13,213, which represents a very slight improvement on the figures for the previous outbreak. The results were better during the third outbreak, when, as already explained, the system of the compulsory detention of contacts was replaced by one which was practically a system of surveillance. In his evidence, given on March 20th last, Major Reade was able to tell us that the number of contacts over

25,827. whom supervision had been exercised during the third outbreak was 2,013, or nearly seven contacts to each of the 290 ascertained cases of plague.

Para , above, 25,028.

Para. , above, 25,028.

In Bangalore City, the mortality from plague between the middle of August 1897 and the end of March 1899 must have been about 6,300. The number of admissions to the contact camp during this period was 3,960. In the Civil and Military Station of Bangalore the number of deaths from plague between the beginning of the outbreak in the middle of September 1898 and its close in March 1899 was probably over 5,200. The number of admissions to the two segregation camps was only 2,414.

21,268.

9178, 9213, 9230–1, 9244.

The results in Poona Cantonment and at Hardwar and the infected places in its neighbourhood are better. But in both cases the conditions were most favourable to success. In the Poona Cantonment a very large force of soldiers was employed, and the population dealt with was small and accustomed to semi-military discipline. At Hardwar and the neighbouring places, again, the population to be dealt with was not large, some influential sections of the community were on the side of the authorities, the physical conditions of the country rendered evasion of the regulations less easy than it was elsewhere, and the establishments employed were, relatively to the population to be dealt with, extremely strong. The results in the Punjab

10,272, 10,389. were also fairly good, the plague there being confined to a limited area, and the establishment employed in dealing with it being strong. At Ankleshwar, where the

14,214-5 population of 10,000 was reduced to 4,000 during the outbreak of plague, 1,627 contacts were secured to 331 recorded cases of plague. The fact that it was at these places possible to make the segregation of contacts an effective measure, does not, we think, support the view that it could be efficiently carried on under less favourable circumstances among a more unwieldy population and by an establishment of less comparative strength. And the fact remains that, in the large towns in which the policy of segregation was adopted, the number of contacts who were secured did not at all correspond with the number exposed to infection. The best results were those obtained during the latest outbreak at Poona, when the conditions of segregation were materially modified, and the system, except for the first three days, was, in fact, converted into one of surveillance.

Plague in segregation camps.

25,028.

23,935.

25,028.

1902.

I. 223.

9178.

20,442.

11,671–72.

25,028.

I. 224.

Mr. Rand's Report.

21,834.

25,827.

15,948.

619. In no place has there been a serious outbreak of plague in a segregation camp. The proportion of segregated contacts who have developed plague in camp has varied considerably. In Bangalore City, 238 (6 per cent.) out of 3,960 contacts developed plague; in the main segregation camp at Sholapur, 101 (7 per cent.) out of 1,421; in Mysore City, where the occupants of the camp had all gone to it voluntarily, 27 (6 per cent.) out of 452; and at Dharwar, 50 (5·3 per cent.) out of 927 contacts. In the latter place, 27 persons out of 34 who accompanied plague patients from the contact camp to the hospital developed plague in the hospital. At Hinganghat, 15 persons (5 per cent.) out of 306 segregated developed plague. In other instances the attacks among contacts in segregation camps were even fewer. At Jawalapur the number attacked was 24 (3·3 per cent.) out of 717; at the Wari Bandar camp in Bombay between January 1st and April 30th, 1898, 148 (2·5 per cent.) out of 5,790; in Karachi, between March 25th and December 11th, 1898, 169 (1·6 per cent.) out of 10,215; and in Bangalore Civil and Military Station, 76 (3 per cent.) out of 2,414. At Wardha, 100 persons were segregated, and one developed plague. At Poona, during the first outbreak, 53 (1·3 per cent.) out of 4,063 persons segregated developed plague; during the second outbreak, 146 (1·1 per cent.) out of 13,213; and in the third outbreak, up to the middle of March, 15 (·7 per cent.) out of 2,013. In Surat, 6,551 contacts were segregated between November 5th, 1897, and May 7th, 1898, of whom 36 (·5) developed plague. The relatively small proportion of attacks of plague in the camps at Surat is to some extent accounted for by the fact that a number of the inmates would not have been treated as contacts elsewhere. At Cutch Mandvi no

record of cases among the contacts was kept during the first outbreak. In the second outbreak there were only three attacks in the general contact camp, in which it is estimated that 1,000 to 1,200 persons were accommodated. At other places from which details as to the number of contacts in the camps are not given the number of contacts attacked in camp is small, *e.g.*, Kotri (one case in the first, and eight in the second outbreak), Porbandar (five cases), Kankhal (four cases), Khari, near Baadra (no cases), and Hyderabad, in Sind (19 cases). 13,390–1. 13,437–9. 12,573. 12,643. 13,722. 8797. 18,936. 18,125.

The percentage of attacks among the contacts was not in any case very serious, and the figures do not justify the conclusion that in an outbreak the contacts from a house in which a case of plague has occurred must be regarded, from the point of view of the protection of the rest of the community, as so dangerous that they ought to be compulsorily removed to a segregation camp.

620. The remarks which we have made refer mainly to the segregation of contacts as it has been carried out in the towns. When the area of infection is restricted, as in the single village of Khandraoni in the Gwalior State, and the villages infected in the Saharunpur district, or when, as in the area infected in the Punjab, it is possible, as an emergency measure, to employ large establishments among a considerable number of affected villages in close proximity to one another, it will be possible to carry out the segregation of contacts in rural areas in an effective way. The difficulty which exists in the towns of finding and getting the contacts into camp does not exist in the villages. It is at a subsequent stage that difficulties arise in carrying out the segregation of contacts in rural areas, viz., when the people are in camp and those who are being treated as contacts desire to mix with the other residents of the village in camp. This can be prevented only by strict supervision, and such supervision is impossible when the number of villages affected is as large as it has been in many parts of Southern India, and particularly in the Sholapur, Poona, Surat, Dharwar, Satara, and Belgaum districts and in the Mysore, Hyderabad, and Kolhapur States. Although contacts were segregated in some parts of the rural areas infected in Western and Southern India, it was in others found to be a hopeless task to attempt to keep the sick and those who had been specially exposed to infection apart from the rest of the community. The majority of those who have been engaged on the work of supervising plague measures over a number of villages recognised that they could not effectively control the movements of contacts in the camps, and with this we cordially agree. Segregation in rural areas. 15,108. 18,396. 1660–3. 3333. 22,743. 23,541–42.

621. Our conclusions as to the value of the segregation of contacts, regarded as a measure for the protection of the community generally, are as follows :— Conclusions as to value of segregation from the point of view of protection of community generally.

(1.) That where the infection is confined to so small an area that contacts can be accurately known, and establishments can be provided sufficient to secure the detention of every contact, the measure, if it is combined with the disinfection of the infected house, is a most efficient one, in rural as well as urban areas. It may be of the greatest value in dealing with imported cases where there is no indigenous plague.

(2.) That in urban areas where the infection of plague is widespread, segregation can easily be evaded, even where large establishments are kept up. In these circumstances it has, as a matter of fact, in consequence of its extreme unpopularity, been so largely evaded as to be quite ineffective for the protection of the community. Where the infection of plague is widespread in a city, it would, therefore, be better not to make any attempt to segregate contacts.

(3.) That in rural areas it is useless to attempt to segregate contacts when the infection is present in more than a few villages, since it is impossible to provide the establishments necessary to prevent the contacts from mixing in camp with the rest of the population.

III.(3.)—*Evacuation.*

622. We have dealt with the removal to hospital of those suffering from plague, and with the segregation of those who, though healthy, have been specially exposed to infection. We will now pass on to the emptying of all or some of the houses in an infected town or village, with the idea that they should not be occupied as dwelling-houses until it is believed that they can again be occupied without risk of infection. The practice was to establish the people thus removed in temporary camps in the open country. This measure is generally known in India under the name of "evacuation." A distinction is drawn between "partial evacuation," or the emptying and shutting up "Evacuation."

of individual houses or particular groups of houses, and "total evacuation," or the emptying and shutting up of all the houses in an infected place.

Essentials of ideal evacuation.

9263–5, 9281–3, 10,297–9, 10,363, 10,616–9, 12,107–19, 13,452, 17,611, 18,451–2, 18,582–4, 18,764, 19,639–41, 20,234–8, 21,639, 21,667, 22,085, 22,288–9, 22,599–607, 22,737, 23,169–74, 23,536–43, 23,589, 24,193–200, 24,314.

There can be no question of the theoretical advantages of effectual evacuation. It must be a good thing to remove people from infected houses or infected quarters, into uninfected surroundings. The question that has to be considered is how the measure works in practice. For to make evacuation thoroughly effective, certain conditions, which are not always easy of attainment, are required. The people should all be got out promptly; their effects should all be disinfected; each new case should be immediately isolated; suitable accommodation should be available; the people should be effectually prevented from visiting, or sleeping in, the infected houses until either the houses shall have been disinfected or until sufficient time shall have elapsed for the infection to disappear; and, lastly, the people should be prevented from scattering and spreading the infection in the neighbourhood. If it were possible to carry out evacuation with all these safeguards, it would, by the early extinction of the infection, benefit not only the inhabitants of the place actually infected, but also indirectly those of neighbouring places. On the other hand, the less perfectly the precautions can be carried out, the greater will be the danger, not only that the outbreak will continue in the infected place, but also that refugees may convey infection to other places.

Extent to which evacuation has been carried out in practice.

6580, 6591–5.

11–13, 1345.

623. Evacuation, total or partial, has been one of the principal measures adopted at almost every plague-infected place.

In Calcutta, however, the evicted residents merely transferred themselves to other houses. And in Bombay City evacuation has not, during any stage of the epidemic, been attempted on a large scale. The circumstances of these two big cities, the number, character, and employment of their population, and the absence in their immediate neighbourhood of suitable and sufficient open spaces for the accommodation of people removed from their houses, combine to render the evacuation of any considerable number of houses impossible.

5, 11, 1645, 1710, 3232, 3753–6, 4123–7, 4964–73, 4992, 4999, 9036, 9090–9113, 9173–8, 9276–8, 9821–2, 10,230–5, 10,921, 11,219–23, 13,453, 13,751, 13,790–6, 14,041, 14,613, 15,486–91, 15,883, 16,929–44, 18,070–5, 18,782, 20,231–3, 22,297, 22,596–8, 22,737–42, 23,241–3, 24,234, 24,253–5, 24,430–1, 24,545.

The results of evacuation must therefore be looked for in the towns and villages of the interior. We would first observe that the practice in towns has, generally speaking, differed from that in the villages. In villages, inasmuch as the population is comparatively small and compact, it has been the general custom, when once evacuation has been decided on, to resort at once to total evacuation It has been otherwise in the towns. There it has often been the practice in the earliest stages of an outbreak to evacuate only the infected house or houses. The authorities frequently waited a little time to see whether the vacation of the infected house or houses alone—by the removal of the sick to hospital and the contacts into segregation camp—would be sufficient to avert plague from the rest of the inhabitants. This, however, was rarely sufficient. As a rule cases kept cropping up in fresh houses. The evacuation of the neighbouring houses was then resorted to. This partial evacuation, though it has no doubt in many cases delayed the progress of the disease, has rarely succeeded in altogether preventing the spread of plague in a town. In a few instances partial evacuation apparently did succeed. These were, so far as we know, either cases where plague was confined to some detached part of the town, or cases where very few people were affected. In the end it was found necessary in a number of the towns, to make the evacuation total. Such total evacuation was resorted to, not only in the case of the smaller towns, but also in the case of two of the larger towns in the Bombay Presidency. These were Sholapur with a population of 62,000, and Ahmednagar with a population of 40,000. We are told that the whole of the residents of Baroda also (population 112,000) were removed from their houses. They do indeed appear to have been removed; but, judging from the extent to which business was being carried on in the town, they do not appear to have been effectively kept out of their houses.

Attitude of the people towards evacuation.

9912, 18,996, 10,239.

3321, 4970, 5007, 22,637–9, 23,675–6.

3413, 4161, 5462–3.

5455–6, 22,092, 22,622–3, 22,986.

3303, 12,086, 15,494, 22,089.

624. There was often difficulty, especially in the early stages of the epidemic, in getting the people to move out quickly from their houses. They hesitated to face the extremes of temperature at the different seasons, and the downpours of rain, which affect them more in their huts than in their houses. They were apprehensive of the safety of property left in their houses, and did not like to be parted from their goods and chattels. The Muhammadans were found more difficult to deal with than the Hindus, because of the seclusion of their females. The townspeople were more difficult to move than the inhabitants of the villages; and in the villages the shopkeepers, weavers, and persons engaged in industrial occupations were more difficult to move than the ordinary agriculturist. The residents of villages, in some instances, moved out of their own accord. These instances became more numerous as the people became

better acquainted with plague. Sir Andrew Wingate, when asked whether there was much difficulty in inducing the villagers to go out, replied that "there is no difficulty now. I got a report only last week of a village in the Nasik district in which there had been a good many deaths of rats, and the people of their own accord immediately turned entirely out of the village. The Collector sent down two officers to enquire. The people said they had fever. The officers enquired very carefully into it, but could not find a trace of plague. But the people had all gone out on account of the rats and insisted on remaining out. In the villages in the Thana district, for instance, they go out immediately before any officer goes near them. In all our trained districts, as we call them, where they have been through the epidemic and have seen the wonderful effects of evacuation, there is no opposition at all. Down in the Dharwar and Belgaum country, where they have not had any evacuation experience (this is their first year), I daresay they do not turn out quite so quickly. These districts usually have very late rains, and some villages have been turned out once or twice, and villagers have had to go back again owing to heavy rain coming on. It is a black-soil country, and heavy rain necessitates a return to their houses. Each time they have gone back they have got plague again. I think very shortly down there we shall have no trouble about evacuation." This is what was foreseen by Surgeon-General Planck, who, speaking from his large experience of Kumaun and Garhwal, where plague is endemic, and where the people voluntarily evacuate their villages, said "I think you will find that when plague becomes well-known in the plains of India the people will flee out of their houses of their own accord." Where the camps were, as we shall see was sometimes the case, maintained by Government and supervised by officials, the people were less ready to go into them than where the camps were private camps, under less official supervision. In the worst infected tracts of country, establishments sufficient to exercise sustained control over the village camps could not be provided. The people were practically left to manage their own affairs in their camps with the assistance of the village officers.

9. 26,575. 15,914.

625. The treatment of the people after being removed from their houses varied at different places. In some cases they were located entirely in camps provided by the authorities; in others, some lived in camps provided by the authorities, and others were permitted to reside in camps provided by themselves. Speaking generally, it may be said that the Government or the Municipal authorities provided the huts for the population removed from the towns, and that the villagers provided their own huts. But a portion of the huts required for the population of a town was sometimes provided by private individuals, and in some of the villages huts were provided by Government. In a few cases, as, for instance, at Bombay, Broach, and Jalgaon, large employers of labour provided camps for the occupation of those in their employ. The life of the people while out in camp varied chiefly with the amount of control that was exercised over them. In the best managed camps a Superintendent was appointed to be in general charge. Arrangements to prevent the people from running away were made by guarding the camp with police, and having a daily roll call. In order to ensure the prompt detection of cases, and the isolation of the sick, Hospital Assistants inspected the people once or twice a day. All cases of plague, or suspected cases, were as they occurred removed to hospital or to a separate shed for observation, and the hut in which the sick person was found was burnt. Endeavours were made to keep the people out of the infected village by placing a guard over it, and by locking or sealing the houses. Where the place evacuated was a commercial town, the arrangements for keeping the people under control were sometimes relaxed, so as to permit the business men, weavers, mill-hands, &c., to go to their shops, offices, and employments during the day on condition that they returned to camp at night. The same standard of supervision was not maintained everywhere. There were different degrees of strictness at different places, and in many of the village camps where the control over the people was slight, the people were practically at liberty to scatter themselves over the country, and to carry plague about with them.

Treatment of the evicted people. 1611, 1652, 2550, 3758-69, 6121-3, 8273, 9169, 9178-9, 9334-41, 11,319-25, 11,433, 12,770-6, 13,083-8, 13,163-5, 13,377-83, 13,423, 13,801-11, 14,421, 14,632, 15,502-3, 15,913-4, 16,016, 18,074-6, 20,231, 24,419, 24,554-62. Mr. Snow's Report, p. 18; 15,484, 24,563-4.

626. The length of time that the people have been kept out in camp before being allowed to re-occupy their houses has, of course, depended upon circumstances. The general principle was, that they should not be allowed back until it was believed that, not only they themselves, but also their houses, were free from infection. But if the monsoon was close at hand, or if any great storm occurred, or if the accommodation for them was very limited and was required for other people, then—whether the danger of infection appeared to be over or not—they had to be permitted to return. In large

Period for which evicted people kept in camp. 10, 2141, 2580-2, 5451, 5926, 8301, 10,054, 10,266-70, 10,567-70, 10,929, 10,957, 12,033-4, 12,791, 13,134, 13,456, 14,687,

cities like Bombay and Poona, where measures of chemical disinfection were applied to the houses, the people were often allowed back in 10 days, or even sooner. At Bangalore the people were re-admitted to their houses as soon as these were disinfected. In the Punjab and the Palanpur State, on the other hand, they were kept out for at least three weeks after the last case, and sometimes for a month. In the Bombay Presidency and the Hyderabad State the people were not allowed to return to their villages until at least a month after the date of the last case; and a further condition, which was generally insisted on, was that, unless a house had been chemically disinfected, it should not be re-occupied till two or three months after its vacation.

Evacuation with necessary safeguards impossible in certain places and under certain conditions. 22,038–49, 22,076–7. 1943, 2168–93.

627. It seems to us necessary before approaching the consideration of the utility of evacuation, to recognise, in the first place, that there are towns, such as Calcutta and Bombay, in which the removal from their houses of any material proportion of the population is impracticable. Even in towns of less size, total evacuation may not be a practicable measure. It might indeed be possible to remove all the residents of a large town, but it would be impossible to control them after removal. Again there are seasons in which evacuation must be productive of severe suffering and extreme inconvenience, and can at best be carried out only very ineffectually. The evidence given to us of the results of evacuation in the Satara and Dharwar districts during heavy rain, shows that evacuation as carried out in these conditions had, at all events in many of the evacuated villages, no sensible effect in checking the outbreaks of plague. It was found impossible in the stress of weather to prevent the people from returning to their houses. Where the rainfall is heavy and prolonged, the black cotton soil is converted into deep tenacious mud. It is vain to hope that the people will remain out of their houses at the times when these conditions prevail. Even where climatic conditions have not interfered to prevent evacuation from being carried out, it has often been impossible to take all, or even the majority, of the precautions which are necessary to obtain the best results from evacuation. With the large number of people and places that had at times to be simultaneously dealt with, the careful supervision necessary was frequently unobtainable.

Necessity for satisfactory test of results of evacuation.

628. What we have now to endeavour to see is whether evacuation of the degree of effectiveness which was as a rule obtained, has, on the whole, produced results sufficiently beneficial to counterbalance the discomforts that it inevitably entailed on the people. In this connexion we have chiefly to consider the effect of evacuation on the residents of the place evacuated, but, as we have seen, a subsidiary question comes up for consideration in connexion with the possible spread of plague to the residents of other places by the scattering of the evicted population.

We have a great deal of information regarding a large number of individual cases of evacuation. But before it can be established, from the statistical records placed before us, that in particular places the evicted people have been benefited by evacuation, we ought to have some satisfactory method of judging what would have happened to them had the place from which they removed not been evacuated. There is great difficulty in finding a test that is really applicable.

(i.) Test by comparing amount of plague before and after evacuation cannot be applied.

629. The most obvious method of investigating this question would be to compare the amount of plague before evacuation with the amount of plague after evacuation in individual places that have been evacuated. Judging by this method, it would appear that sometimes good results and sometimes bad results have followed from evacuation. In some instances the amount of plague has decreased, if not immediately, at all events within a short time, after evacuation. In other instances plague has continued for a long time after evacuation.

So many fallacies, however, enter into a comparison of this nature, that it is quite useless for the purpose of drawing any certain conclusion as to the results of evacuation.

In the first place, the actual process of evacuation in many cases occupied a long time. We, therefore, cannot in such cases take any particular dates and divide the period of observation into a period when the village was not evacuated, and into another period when the village was evacuated.

Again, it is impossible to ascertain how many of the persons attacked during the first few days after evacuation contracted the infection whilst still living in the infected place. Cases of plague thus originating ought to be reckoned as proof of the danger of remaining in the houses; they ought not to be adduced as disproof of the advantages to be derived from evacuation.

Moreover, even when the attacks after evacuation are more numerous than those before, it does not follow that they might not have been much more numerous still had there been no evacuation. In illustration of this, instances might be adduced in which evacuation, in all probability, did good, although attacks were more numerous after the vacation of the village site than before. We have for instance the facts reported from eight villages in the Dharwar district. These villages, on the occurrence of mortality among rats, were evacuated by Captain Colomb before any case had occurred amongst human beings. Within 10 days of evacuation 1·1 per cent. of the population died of plague. The disease then stopped. Similarly, in the Palanpur State, six villages were completely evacuated because mortality among rats was observed. In one village there followed four cases, in another two, in a third seven, in a fourth nine, in a fifth 10, and in a sixth 13. In the Dharwar and Palanpur instances alike, no one had died of plague before the evacuation, and people did die of plague after it. And yet, in all probability, the evacuation prevented a more serious outbreak than actually occurred. 23,583–4. 13,146–52.

Lastly, we may note that where the cases after evacuation are less numerous than they were before it, it cannot always be assumed that the decline is necessarily the consequence of evacuation. In some cases, we cannot exclude the possibility that the disease had already begun to decrease of its own accord, and was, indeed, on the point of expiring, at the time when the people came out. For instance, at Gharibabad, a hamlet of Sukkur town, with a normal population estimated at about 1,000, there was an outbreak in which about 400 were reported to have died in 16 days. On the 17th day the 601 people remaining in the hamlet were moved into camp. One developed plague within 24 hours of removal. None of the others were attacked. At Poona 4,800 persons were sent from infected parts of the city to the health camp at Bhamburda. Only one case of plague occurred among them after removal. It is impossible to assert positively that evacuation produced this immediate cessation of cases. On the contrary, the fact that only one person in each of these instances was actually incubating plague at the time of removal may be used as an argument that the outbreak was already at an end in the quarter before the people were removed. 3725–37, 3813–6, 11,162–70. 18,405–8. 21,832.

In view of the fallacies involved in a comparison of the statistics regarding the amount of plague before and after evacuation respectively, it is not possible by the adoption of this test to arrive at a certain conclusion as to the effectiveness or otherwise of evacuation.

630. A second method of testing the effects of evacuation, would be to compare two separate outbreaks at the same place and under the same climatic conditions, in one of which the people were removed from their houses, and in the other of which they were not. Unfortunately, no instance has come to our notice in which a complete comparison of this kind could be instituted. The instance of Alibag is the one in which the conditions most nearly approached those which are essential for the institution of this comparison. We think that the conditions at Alibag so closely approach the conditions which are essential, that the facts regarding the outbreaks in that town may be worth quoting. The first outbreak in Alibag was in the hot weather of 1897, the second in the hot weather of 1898. In the first there were 268 attacks in all; 54 in the four weeks ending 9th April, 167 in the four weeks ending 7th May, and 47 in the four weeks ending 4th June. In the second outbreak there were 26 attacks, viz., 17 in the last three weeks of March and nine in April. In the case of the first outbreak, there was very little evacuation, and it was resorted to only in the later stages of the outbreak; in the case of the second outbreak, there was a great deal of evacuation, though the town was not totally evacuated. The relative mildness of the second outbreak may, with probability, be ascribed to the adoption of evacuation, but it would not be fair to conclude that the good results in the second outbreak were entirely due to evacuation, and the bad results in the first outbreak entirely due to the failure to remove people from their houses. This test fails us for the want of any materials to which we can apply it. (ii.) Test by comparing two separate outbreaks at same place, and under conditions similar except with regard to evacuation. 17,614

631. A third method of comparison would be to contrast a place which was evacuated with another place similarly situated which was not evacuated. But there are hardly any towns or villages in which an outbreak has run its course without at least a large proportion of the people having removed from the infected place. A certain number of villages, however, in the Dharwar district were left unevacuated (iii.) Test by comparing place evacuated with a place similarly situated and not evacuated.

owing to the impracticability of removing the people in the heavy rain. The mortality in these villages was extremely high, as the following details show :—

1601, 2226.

Name.	Population.	Percentage to Population	
		Of Attacks.	Of Deaths.
Shelwadi	4,222	32·5	27·6
Ibrampur	1,716	50·4	35
Byahatti	3,589	30·5	21·7
Ingalhalli	2,203	47·7	36·8
Datnal	1,280	45·3	36·5
Bhandiwad	1,306	Unknown.	37·1

23,164–7. Major Anderson, I.M.S., supplied us with figures as to the results of evacuation in 23 villages in the same district at a period of the year when the conditions of the weather were not so unfavourable. Excluding one village, for which the figures are incorrectly given, the aggregate population removed from these villages numbered about 34,100, and among them there were 2,073 cases of plague. The percentage of the population attacked was thus 6·07. Thus, in these villages even though, owing to the absence of a sufficient staff, it was impossible to exercise complete supervision over the evacuation, the amount of plague was far less than in the six villages mentioned above, which were not evacuated at all. It would, however, not be fair to assume that all villages unevacuated suffered as heavily as these six villages; in fact, we may assume that there must have been many villages where, even in the absence of evacuation, no outbreak serious enough to attract the attention of the authorities took place, although the infection must have been imported.

(iv.) Test by comparing the residents of an infected place who removed from it, with residents who did not leave the place, or returned to it prematurely.

632. A fourth test that suggests itself is to compare those residents of an infected place who removed from it and consistently remained out with those who either did not go out at all, or, having gone out, returned for some purpose or other. This test is, for obvious reasons, not one capable of general application. But observations in some places have been recorded of the relative liability to attack of these two sets of persons, and in such cases the test, so far as it can be applied, is a valuable one.

19,513. The people of Bobleshwar (in the Bijapur district) went out voluntarily into camp after there had been five cases of plague. After this no case occurred among them for a week. One family had been left in the village and they almost all caught plague.

24,545, 24,561. In Bholana, in the Khandesh district, the cases that occurred after the people had been living out of the village for a week were all traceable either to contact with patients in hospital or to residence in the village subsequent to the general evacuation. Two families, in which there were women who were about to be confined, were allowed to stay in the village. Their houses were at a distance from those previously infected. Within 10 days of evacuation a case of plague occurred in each family.

13,449–50. In four villages in the Cutch State, members of the clan to which the ruler of the State belongs were left behind in their houses when the rest of the villagers were turned out. It is said that as a class they do not associate with the other villagers, and it appears unlikely that they would have gone out of their villages in order to communicate with the people in camp. As a fact, no instance of their having done so was detected. In each of these four villages these people eventually became infected. In one of the villages the first person attacked was a woman who lived in strict seclusion, and the next two were her female attendants. Similar

9217. instances occurred at Khankhal in the North-Western Provinces. "The second area evacuated in Khankhal," says Mr. Winter, "contained the house of one of the leading men of the town. The house was well built and ventilated and kept scrupulously clean. For various reasons, we made an exception in favour of this man, and allowed him and his family to remain in their house. Two members of the family were attacked, although all the surrounding houses had been evacuated. A man was attacked with plague in a house from which a woman was about to be confined. The family of nine persons were segregated in their own house, the surrounding houses being evacuated; five members of the family were attacked. In another block that was evacuated there was a house from which a woman, owing to recent confinement, could not be moved. She, with two attendants, were left in the house. Her sister was attacked within a week of the evacuation. An old man and his sister were left in an evacuated area to look after a temple. The sister was attacked with plague two months after the surrounding houses had been evacuated, and after the whole town had been evacuated. After the outbreak had, to all appearances, ceased, four cases

ccurred in a family living in a collection of huts separated from the town, which or this reason had not been evacuated. Two chaukidars on duty in evacuated areas vere attacked." At Banga, in the Punjab, where batches of people were removed from their houses on nine or ten different occasions, it was noticed that there was very little plague in the camps, whereas the infection kept spreading among the people left in the own. 10,172, 10,235.

It has also been observed that people who prematurely revisit a place that has been infected are specially liable to attack. This has been specially noticed amongst disinfectors. Mr. Winter speaking of Hardwar and the plague infected places in its neighbourhood, bserved: "Large numbers of men employed in disinfecting evacuated houses were ttacked, these attacks occurring for some time after the town or village had been vacuated." In the case of Jawalapur, one of the places in question, out of a total of 16 cases, 29 occurred among disinfectors or members of their families resident with hem. In 12 villages in the Punjab, of 102 cases which occurred after the tenth day ubsequent to evacuation, 69 were among people proved to have visited the village hortly before being attacked, and 21 of the cases were among disinfectors. Captain ames' experience led him to say that disinfection was the most dangerous work. he facts which he gave us upon this point from the village of Mahlgahla are nteresting. On the outbreak of plague there the whole village was evacuated, and he plague ceased. When subsequently the disinfection of the whole village was ndertaken, plague began again among the people engaged on this work. 12,645, 12,776, 13,807, 13,814, 15,776, 22,601. 9217. 9178 10,625, 10,139-41. 10,143-6, 10,515 -28, 10,603, 10,685. 10,000. 9987, 10,139, 10,349.

Similar results were noticed in the Cutch State. "There is no doubt, says Major Hyde-Cates, "that the disease is kept going by people visiting the villages . . . here is no doubt in my mind that people being allowed to visit their homes or doing o surreptitiously keeps up the disease." A fact observed at Hinganghat bears upon his question. On the 21st November 1898, 1,000 of the inhabitants of the town vere removed. There were no cases for some time in the evacuation camps. When, owever, on the 29th November permission was granted to return to the town emporarily to get some grain, plague cases occurred on the 2nd December. The same xperience has been frequent in outbreaks of plague in the Kumaun and Garhwal Hills. he feeling of the people there is so strong upon the subject that they are afraid o return to an infected village. 13,456. 6007-9, 6021-5. 26,576.

The results that ensue when the evacuated people in a body re-occupy their houses prematurely are specially marked. Particularly notable are the results that occurred t Chak Kalal in the Punjab, and in the Sirohi State. Dead rats were found in the illage of Chak Kalal on the 4th or 5th of April, 1898. The people thereupon left he village. But on 15th April a heavy shower of rain caused most of them to return. On 17th April the first case of plague occurred, and 33 persons were attacked between hat date and 27th April. In the four villages infected in the Sirohi State there were 8 deaths before plague was discovered. The villages were then evacuated. Subsequent o evacuation nine cases occurred, all within 10 days. The outbreak then stopped. On the people reoccupying their houses, however, plague broke out again in three of he villages. These villages were again evacuated. In this second outbreak there vere 20 cases before the people went out and 36 afterwards. It seems clear that he removal of the people from their houses saved them for the time from plague, vhich they ultimately got by going back too soon. 13,431, 13,456, 13,678, 14,658, 15,234, 15,739-41. 10,603 (8). 9709, 9714, 9792-3, 9825-6, 9862-4, 9873-4.

The instances to which we have referred of persons who have become infected either vhile disinfecting houses in an infected place, or by a premature return from camp o the infected place, show conclusively that the risks of people contracting infection re greatly diminished by the fact of their removal from their infected houses.

633. Finally, we have a fifth method of ascertaining to what extent the results of removing people from an infected place are beneficial. This is to compare the numbers of plague attacks and deaths in places from which the people were promptly removed vith those in places from which they were removed after some delay. The method appears to be a trustworthy one, provided that the comparison is between a large number of villages evacuated early and a large number of villages evacuated late. But for its proper application it will not suffice to have statistics which merely set forth the number of cases and deaths before and after evacuation, such as some of hose which have been furnished to us. It is necessary to have detailed accounts showing the circumstances in which each place was evacuated, and the facts attending he evacuation. The records given to us in evidence do not always show whether the evacuation of a place was real or nominal. In some instances, again, the case shown n the records as the first case of plague was apparently unconnected with the outbreak on account of which the place was evacuated, and was, at any rate, separated from it by a long interval of time. In other instances, where the process of evacuation took

(v.) Test by comparing course of plague in places promptly evacuated and places not promptly evacuated.

considerable time, numbers of cases are shown as having occurred during the course o evacuation. An assignment of some of these cases to the period before, and of som to the period after, evacuation, would be arbitrary and misleading.

In the case of the infected villages in the Punjab, however, the statistics in the tabl furnished by Captain James, I.M.S., are supplemented, regarding each place evacuated by the detailed account given on pages 16 to 92 of his Report on the outbreak o plague in the Punjab in 1897–98 (not reprinted with these Proceedings). Thi enables us to exclude from account certain villages which are not in all respect suitable for comparison, and to compare only those places in regard to which the information is complete. In respect of these villages there are full materials fo coming to a conclusion as to whether plague was checked in promptly evacuated place more easily than in those in which evacuation was somewhat delayed. Captain James deals with 91 outbreaks of plague—75 in the Jullundur district, and 16 in the Hoshiarpur district—which occurred in 86 villages, of which five were infected twice A consideration of the details given in Captain James' report and in his evidence shows firstly, that in the case of 11 outbreaks, viz., the first outbreak in Jhander Khurd, the second outbreak in Aur, and the outbreaks in Khan Khanan, Malpur, Piragpur, Pharala, Sadhowal, Sanwali, Kulewal, Garhi, and Palewal, the date of the first case is not definitely known; secondly, that in the case of seven outbreaks, total evacuation was not effected, viz., in the cases of Naura and Garhshankar and of the first outbreaks in Aur and Karnana and the second outbreaks in Khatkar Khurd, Jhander Khurd, and Bahrwal; thirdly, that in the cases of Banga, Mukandpur, and Masani, the process of evacuation was protracted; fourthly, that in the case of Chak Kalal, the village was evacuated before any one of its residents was attacked, and that the people were attacked only when they were driven back to their houses by a storm; and, lastly, that in the case of one village, Parkhowal, no details are given regarding the dates on which cases occurring after evacuation happened. The evacuations connected with the outbreaks just enumerated in the 23 villages named, present features that prevent accurate comparison with the other cases of evacuation for which Captain James gives details. They must, therefore, be left out of account in the comparison which we are now about to institute. That comparison will relate to the cases of 68 outbreaks only in the same number of villages, in regard to which we have full details, showing, with fair accuracy, the date of the first case of plague, the interval between the first case and the completion of evacuation, the number of cases before evacuation, the number of cases after evacuation—divided into (1) the number of cases in the first 10 days after evacuation, and (2) the number of cases which occurred more than 10 days after evacuation—and, finally, the date of the last case of plague. In order to ascertain what the effect of evacuation was, we have divided these villages into (1) those which were evacuated within 15 days of the occurrence of the first case of plague, (2) those which were evacuated more than 15 days, but less than a month, after the first case of plague, and (3) those which were evacuated more than one month after the first case of plague. This division of periods is purely arbitrary; but it will serve as well as any other.

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The results of this comparison are shown in the following table:—

Villages.	Population.	Cases before Evacuation.	Percentage of 3 on 2.	Average Number of Days between the First Case of Plague and Evacuation.	Number of Cases within 10 Days after Evacuation.	Number of Cases more than 10 Days after Evacuation.	Percentage of Cases after Evacuation Population.	Total Cases.	Percentage of Total Cases on Population.	Average Number of Days between Evacuation and Cessation of Plague.	Average Number of Days between First and Last Case.
1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	12.
GROUP I.											
Thirty-five villages evacuated within 15 days of first case.	23,313	221	·95	6·3	465	159	2·7	845	3·6	22·3	28·6
GROUP II.											
Twenty-five villages evacuated over 15 days, but less than a month, after first case.	25,623	541	2·1	22	481	209	2·7	1,231	4·8	20·5	42·5
GROUP III.											
Eight villages evacuated more than a month after first case.	10,166	218	2·1	58	139	109	2·4	466	4·6	32·5	90·5

Over seven-eighths of the villages dealt with fall within the first two groups. The umber of villages in each of these two groups, and, perhaps, in the third also, is ufficiently large to eliminate the element of chance, and the population of the villages comparatively uniform within each of the different groups. The three groups of illages apparently differ from each other only in the facts that they were evacuated at ifferent intervals after the outbreak of plague, and that there were considerable differnces between the populations of the villages in the three groups, the average population eing about 670, 1,025, and 1,270 respectively, in the first, second, and third groups. a comparison is made between the severity and duration of outbreaks in villages vacuated within 15 days of the first case of plague and those evacuated after an nterval of more than 15 days, but less than a month, it will be seen that the results btained in the villages promptly evacuated were distinctly more satisfactory than hose obtained in villages in which evacuation was delayed. The interval between the rst and last case of plague was in the first group less by about one-third than that in the cond group. The incidence of attack taken over the whole period during which ague lasted was in the former 3·6 per cent. and in the latter 4·8 per cent. on the opulation. Thus, in the two essential points, viz., in the reduction of the number cases and of the period for which the outbreak lasted, a material advantage sulted from prompt evacuation. The third group of villages, viz., those not vacuated till more than one month after the first case, numbers only eight. The sults in these villages do not show quite as large a total mortality as those in roup II., but a larger mortality than in Group I. The period of outbreak was more an twice as long in this group as in Group II., and more than three times as long as Group I.

The facts here exhibited, if they were not complicated by the circumstance that they late to groups of villages of different average populations, would appear to afford very stinct proof of the beneficial results of prompt evacuation in villages when special eans are adopted for disinfection of the effects taken out into camp, the isolation the sick, and the proper control of the people in camp.

634. The above statistical tests, so far as they can be applied, show that evacuation certainly a beneficial measure where the special precautions to which we referred ove have been taken, and that, considered generally, it is probably a beneficial easure. Opinions of the officials and people regarding benefits of evacuation.

The statistical evidence, however, is not the only evidence laid before us with spect to the results of evacuation. There is a large body of opinion and it is actically unanimous in declaring evacuation to be a good measure. This opinion is ot merely that of Government officials, whether European or Native, but also that of e officials of Native States and of persons who are not officials. Many of those ost experienced in plague administration consider evacuation the most effective all measures. They have assured us that the removal of the people from an fected place is followed by a diminution and gradual decline in the number of ses. They assert that, at any rate in well-controlled camps, cases are more sily discovered, and that the infection spreads less than in the village site, and that eople who develop the disease after they have been out more than a few days have enerally contracted it not in the huts but by visits authorised or surreptitious to the uses. A body of opinion like this which is based upon the experience of practical en throughout the length and breadth of the infected districts is, we think, entitled great weight. With a view to showing the opinions which are held by officers who ave actually been engaged on plague duty, we may quote the following. "It is fairly ell established," says Captain James, I.M.S., speaking of the Punjab, "that in vacuation of villages we have a method of quickly and certainly stamping out plague. not a single village in the areas under consideration could it be said to have failed. was successful in every season of the year, though it took longer in the winter than the summer. It was equally successful in large villages of from three to four housand people as in little hamlets." Experience gained in the outbreaks at Hardwar nd its neighbourhood convinced Colonel Thomson, I.M.S., the Sanitary Commissioner the North-West Provinces, that evacuation is the best remedy. The Commissioner Sind says: "Evacuation is the one thing: every officer recognises that vacuation is successful." Sirdar Muhammad Yakub Khan, who had experience the plague in Sind, is strongly of opinion that evacuation stops an extending idemic. And Mr. Kaka, the Health Officer of Karachi, considers that the one easure that has proved successful in combating plague is the compulsory evacuation infected areas. The Civil Surgeon of Surat thinks that of the several measures dopted the most valuable in the treatment of an epidemic is evacuation. Mr. Monteath, e Assistant Collector of the Thana district, says "the most effective measure seems

9, 1699, 3396, 4292, 6034–6, 7854, 9225, 10,461, 10,757, 11,482, 11,960, 12,654, 12,755, 16,969, 17,666, 18,421–2, 18,585, 19,482, 19,952, 20,233, 20,441, 23,928–9, 24,549, 26,543–4.

10,361.

8791.

11,248.

25,186.

11,849–50.

15,764.

19,684.

to be to turn all the people out, or as many of them as possible, into the fields
24,449, 24,456. Evacuation and thorough ventilation of all the infected houses for a mont
kills plague, says Mr. Moore, who had experience both in Surat and Nasi
21,667. Evacuation and isolation are, Assistant-Surgeon Bharucha considers, very important
evacuation produces better effects, he says, when isolation is added. In spite
the fact that in the Satara district attempts at evacuation were often—chief
22,031. it seems because of heavy rain—productive of so little good, the Collector conside
that the most effective practical measure for stopping plague is "trying to evacua
and evacuating as much as possible." The general impression produced on his min
22,046. in regard to complete evacuation properly carried out is that "plague undoubted
does begin to decrease soon after the people have gone out; within, I should say,
22,737. fortnight." His assistant, Lieutenant Steen, while thinking that unless evacuatio
comes very promptly before many people have become infected it will not stop plagu
thinks apparently that it will at any rate diminish it. Lieutenant Anderson, wh
22,595. worked in the same district, thinks that the great remedy when a village is infected
23,168. "entire evacuation immediately." Major Anderson, I.M.S., considered that evacuatio
had been very successful in the part of the Dharwar district of which he had charg
23,507. And Mr. Bhardi, who had served in the same district, thinks that the best measu
20,238. for suppressing the epidemic is evacuation. The Collector of Sholapur told
that, in his opinion, "the immediate and complete evacuation of all villages, and
towns up to 40,000 or 50,000 inhabitants, when this is considered possible, is th
22,891. best way of treating an outbreak of plague." Similarly, the Collector of Belgaun
when asked what he considered the best measures to adopt, the most effective step
to take, when plague had occurred in a district or village, replied, "for the people
21,320-1. go out and evacuate a village." Major Ross writes—and Lieut.-Colonel Fawcet
R.A.M.C., entirely endorses the view—that at Poona "the effect of health campin
13,452. was almost magical." "Most of the villages," says Major Hyde-Cates, referrin
to places attacked by plague in the Cutch State, "that have been turned out earl
gave good results, and I have not the least hesitation in saying that earl
evacuation properly carried out is the best thing." In the Mysore Stat
3306. Mr. Madhava Rao, the Plague Commissioner, found that when the villagers campe
out there was "abatement of plague. There have been no more cases so far as th
15,221. camping out is concerned." "The best of all measures," says Mr. Dhurandhar, a
14,662. officer of the Baroda State, "is evacuation." "In cases where we could evacuate th
villages," says Mr. Samarth, employed in the same State, "the evacuation was ver
satisfactory; the disease stopped very soon after they went into the fields." An
15,115, 15,124. Mr. K. B. Jadhav, the District Magistrate of Naosari, in the Baroda State, says tha
evacuation often stops plague within a week, and "is the only measure, which th
5926. people are ready to adopt of themselves." Mr. Mazhar Hussain, who served in th
Hyderabad State, says, that "after the evacuation of a village the fall in the mortalit
22,532. was very striking." "Evacuation," says Mr. Ghanekar, a member of the Municipa
24,312-3. Committee of Satara, "is the most successful remedy." Mr. Cardmaster,
Ahmednagar, is "quite positive" that evacuation is the most effective measure.

9, 11,575, 14,635, 22,635. It is not only the officials of the British Government and of the Native States wh
believe in the efficacy of evacuation; the villagers themselves, as we have seen abov
are becoming convinced of the necessity of vacating their houses on the appearance
plague.

Effect of evacuation in causing spread of plague to places other than that evacuated.

635. We have now to consider the subsidiary question as to whether evacuatio
indirectly causes a spread of plague to places other than the one evacuated. Whethe
evacuation will have an effect in spreading the plague to uninfected place
depends, firstly, upon the extent to which plague has spread among the inhabitants
the evacuated place, and secondly to the extent to which these inhabitants after evacua
tion disperse themselves over the country. The interests of agriculturists are in the
5891, 13,084, 20,237, 21,148, 23,680, 23,988-91, 24,067-74, 24,359. fields, and when removed from their houses they have no inclination to go away to
distance, so that the turning of such people out of their houses probably does no
result in the dissemination of plague, even though there may be many of ther
infected. But where infection is widely distributed in a large place containing trade
and artisans there is the danger that evacuation may result in the dissemination
plague, because, as trade comes to a standstill, these classes run away to other place
and carry the infection with them. It was by people flying from Sholapur—befor
and after the evacuation—that the country round, both in British territory an
5425, 23,332-8, 23,912, 24,016. Hyderabad territory, was flooded with plague. The infection in Sholapur died ou
but the villages in the Hyderabad State, infected by refugees from the town, hav
remained continuously infected ever since.

The view might be taken that even though the evacuation of a place results in the infection of other places, still, less harm will be done by evacuating an infected place than by leaving it unevacuated. In support of this contention it may be urged that where there is a large place infected, other places are certain to get infected from it, whether it be evacuated or not; that the secondary foci of infection which would be started in great numbers by the evacuation of such a place could all be rapidly extinguished by a series of successive evacuations; and that the result will thus be that the plague will be extinguished sooner than if it had been allowed to smoulder on for an indefinite period in the original focus of infection, and to spread out slowly from that focus, with the ultimate result that the loss of life would, without evacuation, be greater. The point is no doubt a difficult one, the question being one of the balance of advantage. So far as experience has gone at present it points, in our opinion, to the advisability of agitating the people of a large place as little as possible, when infection is widely distributed among the inhabitants, so as not to drive them out of the town. 26,546-7.

636. Our conclusions are to some extent stated in our previous remarks. Before summing them up, we may note here that a policy of evacuation only comes into consideration in cases where no effective alternative policy can be adopted. Evacuation would be unnecessary if plague could be checked by disinfection or inoculation.

If it were possible to disinfect in a prompt and efficient manner every infected house, it would not be necessary to resort to evacuation. It would be sufficient to remove the people from their houses and to keep them out only for such time as might be necessary for the completion of the process of chemical disinfection. We shall, however, see that efficient disinfection requires the employment of properly trained and costly establishments, and these are not everywhere available. It will be impossible to attain by chemical disinfection the objects aimed at by evacuation, except in a comparatively few instances.

Again, if it were possible to immunise the people so as to prevent them from contracting the infection while they remained in their plague infected houses, evacuation would become unnecessary. Although we are satisfied of the benefits of inoculation, according to Mr. Haffkine's method, we have seen that inoculation as at present practised does not protect absolutely from an attack of plague, and moreover there is great practical difficulty in getting the whole of the residents of a place inoculated so quickly as to keep the plague in check by this means. We cannot anticipate that it will, in the near future, at all events, be possible to attain completely by means of inoculation the object aimed at by evacuation.

637. The importance of evacuation as a policy having thus become manifest, we may sum up our opinion regarding it as follows. We consider that evacuation is generally beneficial, that it is highly beneficial when the safeguards necessary to its giving its best results can be maintained, and that it is beneficial even when they cannot be completely maintained. There are, however, two sets of conditions in which evacuation cannot be carried out without doing harm which may more than compensate for its good results. Evacuation, in the first place, does more harm than good when amongst the people who are removed from their houses there is a considerable number who belong to the trading or artisan classes, for, inasmuch as traders and artisans may lose their occupation by being removed from the town, they are likely to run away and infect other places. Evacuation, in the second place, almost always does more harm than good during the rainy season, especially in certain tracts of country where the effects of the rains are most severely felt, and it is impossible to keep the people, after they have once been removed from their houses, from going back to them; and in such circumstances the evacuated people are exposed to great inconvenience and misery without deriving any countervailing advantages from evacuation. Conclusions regarding evacuation.

III.(4)—*Disinfection.*

638. Under the heading of disinfection we may consider all the various agencies which operate in the direction of destroying the germs of plague when these have been disseminated in nature. Disinfection.

III.(4.)(*a.*)—*Question as to what is the most Effective Chemical Disinfectant for Plague.*

639. We may conveniently begin by the consideration of the action of chemical disinfectants. Disinfection by chemical means.

The question as to what is the most effective disinfectant to employ in the case of a particular disease can be answered only by determining by experiment what quantities of each particular disinfectant are required for the destruction of the micro-

organisms of that disease. This information can be obtained from two distinct sorts of experiments:—

(*a.*) It may be deduced, on the one hand, from experiments in which additions of disinfectants are made to pathogenic micro-organisms suspended in a medium which is both inert with respect to the bacteria, and which does not exert an important neutralizing action on the disinfectant. The information derived from experiments of this kind will only in exceptional cases have a direct application to practice, inasmuch as the media which surround the pathogenic micro-organisms in nature are media which will decompose and combine with the disinfectant in such a manner as to diminish its action on the pathogenic bacteria.

(*b.*) On the other hand, the information as to which is the best disinfectant to apply may be derived from experiments in which the disinfectants are applied under the conditions which actually obtain in nature, or under conditions which closely approximate to these. The information derived from experiments of this kind will manifestly be information which is more likely to be directly applicable to practice.

The minimum strength of particular disinfectants required to devitalise the plague bacillus in an inert medium.

640. We may begin with the consideration of the more important experiments which have been instituted with a view of determining what is the minimum strength of each particular disinfectant which is required for the complete devitalisation of the plague bacillus when suspended in a practically inert medium. We may deal only with the more important disinfectants.

(1.) *Carbolic Acid.*

(1.) Carbolic acid. British Medical Journal, 7th July 1894.

Kitasato found that a minimum addition of 1 per cent. of carbolic acid was required for the destruction of the plague bacillus in bouillon cultures within one hour. Carbolic acid in a strength of 0·75 and 0·50 per cent. did not destroy the bacillus within one hour in bouillon cultures, but did so after two hours.

Centralblatt für Bacteriologie, 1897, p. 497.

Abel found that the plague bacillus on agar cultures was not destroyed by exposure to 1 per cent. of carbolic acid for a period of six hours, but was sometimes killed after 24 hours; it was always killed in his experiments in 48 hours. On cover glasses smeared with infected pus or with agar cultures, the bacillus was alive after one hour's exposure to 1 per cent. of carbolic acid, but not after two hours.

8552 (II., 5).

Mr. Hankin found that the plague bacillus was not invariably destroyed by an exposure of 20 minutes when introduced into 1 per cent. carbolic acid.

Report of German Plague Commission, p. 278.

The German Plague Commission found that plague bacilli were destroyed in one minute when introduced into 5 per cent. carbolic acid. They were killed in 1 per cent. carbolic acid in a period of 10 minutes. They survived exposure for an hour to $\frac{1}{2}$ per cent. carbolic acid.

In a series of experiments conducted by us, the addition of carbolic acid to a bouillon culture of plague till the mixture contained $\frac{1}{2}$ per cent. of carbolic acid did not kill the bacillus in 24 hours. An addition of 1 per cent. of carbolic acid effected the destruction of the bacillus in less than 10 minutes, and an addition of 5 per cent. killed the bacillus immediately.

(2.) *Phenyle.*

(2.) Phenyle.

Phenyle, which is the commercial name for a "distillate containing various phenols and cresols," is, as will be seen further on, a disinfectant which has been very extensively employed in India.

8552 (II., 5).

Mr. Hankin found that the plague bacilli were killed in 5 to 10 minutes by an emulsion of phenyle of strength of 1 in 1,000.

Capt. James' Report, p. 111.

Dr. Watkins Pitchford (Report from the Petit Laboratory, April 23rd, 1898, quoted by Capt. James, I.M.S., in his Report on the plague in the Punjab in 1887–8), found that plague bacilli were killed within five minutes in a solution of 1 in 400 of phenyle.

(3.) *Soft Soap Solution.*

(3.) Soft Soap. Report of German Plague Commission, p. 278.

The German Plague Commission found that plague bacilli remained virulent for mice, after exposure for 15 minutes to a 3 per cent. solution of soft soap, kept at a temperature of 30° C. The bacilli lost their virulence after exposure to the same solution for half-an-hour at a temperature of 30° C.

(4.) *Lysol.*

(4.) Lysol. 8552 (II., 5).

Mr. Hankin found that plague bacilli were killed by an exposure of five minutes to a $\frac{1}{4}$ per cent. solution of lysol. No experiments are reported with weaker solutions.

The German Plague Commission found that plague bacilli were killed in five minutes by a 1 per cent. solution of lysol, and in one minute by a 2½ per cent. solution. No experiments are reported with weaker solutions. Loc. cit. p. 278.

(5.) *Perchloride of Mercury (in Neutral Solution).*

(5.) Perchloride of mercury (in neutral solution).

Mr. Hankin found that perchloride of mercury in 1 in 5,000 solution killed off plague bacilli. We understand that the plague bacilli were, in this experiment, exposed to the action of the disinfectant only for one minute. 8552 (II., 6).

Abel found that 1 in 5,000 solution of perchloride of mercury killed plague bacilli in one hour, while 1 in 1,000 of perchloride of mercury killed plague bacilli within 10 minutes. 18,828. Loc. cit.

The German Plague Commission found that plague bacilli when brought in contact with a 1 in 1,000 solution of perchloride of mercury instantaneously lost their virulence for mice. Report of German Plague Commission, p. 279.

Dr. Watkins Pitchford found that perchloride of mercury in a solution of 1 in 5,000 killed the plague bacillus within five minutes. Capt. James' Report, p. 111.

In our own experiments it was found that the plague bacillus was, in all cases, killed in 15 minutes by 1 in 5,000 solution of perchloride of mercury; in one case it was killed in 15 minutes in a 1 in 10,000 solution; in another case it was killed by half-an-hour's exposure to a 1 in 20,000 solution of perchloride of mercury. 26,347 and 26,434A.

(6.) *Mineral Acids.*

(6.) Mineral acids.

Mr. Hankin found that the plague bacillus was killed by exposure (for a period of time which is not stated) to nitric acid in a strength of 1 in 333, to hydrochloric acid in a strength of 1 in 500, and to sulphuric acid in a strength of 1 in 1,500 nearly. 8552 (II., 7–8).

The German Plague Commission found that the plague bacillus was killed in five minutes by sulphuric acid in a strength of 1 in 2,000, and within half-an-hour by hydrochloric acid in a strength of 1 in 1,000. Loc. cit., p. 279.

Our own experiments gave similar results.

(7.) *Caustic Lime.*

(7.) Caustic Lime.

Mr. Hankin found that plague bacilli were not killed by exposure to the influence of a solution which contained caustic lime in excess. Mr. Hankin has omitted to specify the duration of the exposure in question. 8552 (II., 6).

The German Plague Commission found that a minimum exposure of 30 minutes was required to effect the destruction of plague bacilli. The solution which was employed also contained an excess of caustic lime. Loc. cit., p. 280.

(8.) *Permanganate of Potash.*

(8.) Permanganate of potash.

Mr. Hankin found that plague bacilli were killed by exposure for a period of five minutes, to 1 in 10,000 solution of permanganate of potash. 8552 (II., 9, 10).

(9.) *Chloride of Lime.*

(9.) Chloride of Lime.

Mr. Hankin found that plague bacilli were killed by an exposure of five minutes to a 1 in 10,000 solution of chloride of lime. 8552 (II., 9).

The German Plague Commission found that a minimum exposure of 15 minutes was required to effect the destruction of the plague bacillus by a 1 per cent. solution of chloride of lime. Loc. cit.

641. Before we proceed to formulate the conclusions which can be drawn from the results of the experiments which have been summarised above, it would be well for us to place clearly before us the fallacies which may vitiate such experiments as these. Fallacies which may vitiate experiments of which results given above.

The fallacies in question are the following :—

(*a.*) The experiments may be undertaken with cultures which have already become attenuated. There would result from this an over-estimation of the efficacy of the disinfectant.

(*b.*) The fluid which is employed for suspending the bacteria may itself exert a bactericidal effect. There would result from this an over-estimate of the efficacy of the disinfectant.

(*c.*) The fluid in which the bacteria are suspended, and which is introduced with them into the solution of the disinfectant, may neutralize some of the disinfectant, with the result that the strength of the disinfecting solution may be reduced below the standard to which it is supposed to correspond. There would result from this an under-estimation of the efficacy of the disinfectant.

(*d.*) When, for the purpose of testing the survival or non-survival of the bacteria which have been introduced into the disinfecting solution, these are transferred to a nutrient medium, some of the disinfectant may be carried across into the nutrient medium, with the result that the growth of the bacteria on this medium will, to a greater or less extent, be inhibited. Where this is the case, the inference that the bacteria have been devitalised might be erroneously made. There will thus be a risk of the devitalising efficacy of the disinfecting solution being over-estimated.

In the case of the experimental results which have been summarised above, information is not always available as to the details of the experiments upon which these are based. Where these particulars have been omitted we can have no certainty that all fallacies were avoided, and it is possible that certain discrepancies in the experimental results summarised above may be referable to such fallacies. Again, in other cases, for instance where the disinfectant employed is a very unstable or volatile disinfectant, such as chloride of lime, the discrepancies between the results of various observers may be due to differences in the strength of the disinfectant employed. On the whole, however, we may notice that there is, on fundamental points at any rate, a very satisfactory amount of agreement between the results obtained by the different observers.

Inferences that can be drawn from above experimental results.

642. We learn from the above summary of the experimental results first, that carbolic acid when employed in dilute solutions (1 per cent. or under) is not a very powerful disinfectant.

We learn, further, that caustic lime is not an effective disinfectant.

We learn, lastly, that perchloride of mercury, mineral acids, permanganate of potash, and solutions and preparations which contain free chlorine (fresh chloride of lime, and, possibly, freshly prepared chlorinated sea water, come under this description), are extremely effective disinfectants.

We learn, in other words that mineral acids on the one hand, and oxidising agents on the other hand, are, as Mr. Hankin has already pointed out, effective disinfecting agents, whereas caustic alkalies are relatively ineffective.

Question of practical application of above inferences.

643. With regard to the practical application of these experimental results, attention has already been drawn to the fact that inasmuch as the conditions of experiments such as these are altogether different from those under which disinfectants are applied in actual disinfections, the results of such experiments will only in exceptional cases have a direct application to practice. We have in view here such experiments as those which refer to caustic lime. It would appear to be legitimate to infer from the inefficacy of this disinfectant in test-tube experiments such as those considered above that the whitewashing of houses cannot, even under the most favourable circumstances, be a very effectual disinfecting measure. On the other hand, we cannot infer from the similar experiments with permanganate of potash that this disinfectant, which is so effective when brought into relation with plague bacilli suspended in an inert medium, will also prove an effective disinfecting agent when applied in actual practice. This question will be determined primarily by the degree to which permanganate of potash will be decomposed and neutralised by the particular media in which the plague bacillus is found in nature.

Considerations bearing on the value of permanganate of potash as a disinfectant for use on cow-dung floors.

8552.

II., 10.

644. The issue, so far as it relates to the particular case of permanganate of potash, in its application to the disinfection of earth and cow-dung floors which have everywhere to be dealt with in India, is brought clearly before us by the following facts. On the one hand, we have the fact that permanganate of potash, even in a solution of 1 in 10,000, exerts a devitalising action on the plague bacillus. On the other hand, we have the fact that permanganate of potash is notoriously subject to decomposition when it is brought in contact with any kind of organic material. Let us note as an indication of the degree to which permanganate of potash is decomposed when it comes in contact with cow-dung, the fact that cow-dung is, as Mr. Hankin has shown, capable of exerting a deoxidising effect upon its own weight of a 4 per cent. solution of permanganate of potash. It, will however, appear on consideration that the degree to which the disinfecting power of permanganate of potash will be diminished when it is applied to an earth and cow-dung floor cannot be directly deduced from the above observations, which tell us that cow-dung is capable of decomposing $\frac{1}{25}$th of its weight of the disinfectant. We cannot, in other words, draw from the experiments in question the conclusion

that permanganate of potash will fail to kill off the plague bacillus in cow-dung when it is employed in a solution weaker than 4 per cent. For to assume this would be to assume that the permanganate would completely expend its force upon the oxidation of the cow-dung before it began to attack the plague bacilli incorporated in that cow-dung. Now we have no warrant for asserting that this is what actually occurs. On the contrary, it is quite conceivable that the permanganate might exert its oxidising power first upon any plague bacilli which might be present, and only afterwards upon the cow-dung with which these were incorporated. The question as to whether the disinfectant would attack the bacilli before it attacked the cow-dung, or the cow-dung before it attacked the bacilli, is a question which is determinable only by direct experiment. We may, however, see an indication of the answer which would be arrived at in the fact that Mr. Hankin found that a solution of 1 in 200 of permanganate of potash was capable of killing off plague bacilli when added to an agar culture of these. It is manifest, therefore, that plague bacilli may be killed off by solutions of permanganate of potash even in the presence of an excess of oxidisable organic material. 3552 (II. 9).

645. The case is not essentially different with regard to perchloride of mercury. As stated above, the plague bacillus when suspended in an inert medium was, in one of our experiments, killed off in 15, and in another of our experiments in 30, minutes, in solutions of 1 in 10,000 and 1 in 20,000 of perchloride of mercury respectively. These dilutions are not, as will be seen, substantially different from the minimum dilutions in which permanganate of potash is effective. Now, just as in the case of permanganate considered above, so here, we must keep in view that these figures refer to conditions where the disinfecting solution comes into operation on bacteria suspended in an inert medium. We must not therefrom infer that it will suffice, in a case where we desire to effect the devitalisation of any plague bacilli which may be contained in an earth and cow-dung floor, to impregnate the floor with a solution of perchloride of mercury of a strength of from 1 in 10,000 to 1 in 20,000. For it is manifest on general chemical principles and, indeed, it has also been shown by direct experiment, that an earth and cow-dung floor contains considerable quantities of ammonia, and large quantities of other substances, which will decompose and neutralise the perchloride of mercury. Further, it is manifest from general considerations that a considerable quantity of albuminous material will be contained in every earth and cow-dung floor. This albuminous material also will enter into chemical combination with the perchloride of mercury. How much perchloride of mercury would be decomposed in these ways by an ordinary earth and cow-dung floor does not appear ever to have been directly determined by chemical methods. But we have to note that, as in the parallel case of permanganate of potash, the efficacy of perchloride of mercury as a disinfectant could not be directly inferred from a chemical determination of the amount of perchloride of mercury which an earth and cow-dung floor is capable of decomposing. The problem which it really concerns us to solve is the problem as to how much of the disinfectant would be decomposed by the floor before a devitalising action is exerted on the plague bacilli incorporated in that floor. The question is plainly one which can only be determined by direct bacteriological experiments.

Considerations bearing on the value of perchloride of mercury as a disinfectant for use on cow-dung floors. 26,434 A. 26,347.

We may, therefore, now pass on to consider the experiments which have been instituted with a view to resolving this problem.

III.(4.)(*b.*)—*Experiments to test the Practical Value of Perchloride of Mercury as a Disinfectant for Cow-dung or Earthen Floors Infected with Plague.*

646. It would, on first approaching this question, appear to be a simple matter to determine by bacteriological methods the exact amount of disinfectant which would have to be applied to bring about the destruction of plague bacteria in a plague-infected floor. The difficulties of determining this by direct methods are however very considerable. How great these difficulties are will be clearly seen from the fact which we have pointed out that, in spite of the many efforts which have been directed to this end, no observer appears ever to have been successful in recovering plague bacilli from an infected floor. Such being the case, it is manifest that it is impossible to base any inference upon the fact that the plague bacteria are not recovered from the floor which has undergone a process of disinfection with a particular disinfectant, or with a particular strength of disinfectant. Pending the discovery of a method which shall allow of the isolation of the plague bacilli incorporated into unsterilised cow-dung or similar material, it will not be possible to obtain evidence of the efficacy or otherwise of a disinfectant by searching for the plague bacillus in the infected house after the completion of a disinfection process.

Difficulty of experiments to determine the strength of disinfectant required to devitalise the plague bacillus when incorporated in a cow-dung or earthen floor. Para. 222 of this Report.

Methods of conducting such experiments.

647. We are thus, when we desire to determine the effectiveness or otherwise of disinfecting process, constrained to have recourse to less direct methods of experimen tation. If we insist upon judging of the efficacy or otherwise of a disinfection proces by ascertaining the destruction or survival of the plague bacillus in the floor materials we may adopt (*a*) Dr. Marsh's method of artificially introducing the plague bacillu into the sterilized or unsterilized floor material in such a manner as to make sure that i shall be possible subsequently to recover it, should it survive the disinfection process Failing the adoption of this method, we may leave altogether out of view the plagu bacillus and find our criterion of the efficacy of a disinfecting process either in (*b*) large reduction of the total number of the bacteria in the floor, or again we may find it in (*c*) a destruction of all but the more resistant forms. Of the two methods o testing the efficacy of a disinfecting process which have been last mentioned, the forme has been adopted by Mr. Hankin, and also in some instances by Dr. Marsh, while th latter has been employed in a series of experiments made by ourselves.

(*a*) Experiments to test the destruction or survival of plague bacilli introduced into floor materials treated with perchloride of mercury. Dr. Marsh's experiments. 18,829–46. Para. 641 of this Report.

648. We may begin by the consideration of the very careful series of experiment which were instituted by Dr. Marsh, with a view of determining in what strength perchloride of mercury solution ought to be applied to the disinfection of a cow dung floor. The details of the method of experimentation which Dr. Marsh adopte are as follows:—The cow-dung, or mixture of cow-dung and earth, was first very finel pulverised. Weighed quantities of these materials were then filled into a series of tes tubes, the quantities in the various test tubes being so arranged as to constitute graduated series. A plague-infected glass bristle was in the case of each tube thrus down into the depth of the pulverised material in such a manner as to stand upright With a view to avoiding the third source of fallacy, which was referred to above whe we were considering the fallacies which are incident to experiments on the efficacy o disinfectants, this bristle was first washed so as to remove any excess of the culture medium that might be adhering to it. Ten cubic centimetres of a perchloride o mercury solution were then introduced into each tube. After 18 hours the glass bristle were withdrawn from the pulverised floor material into which they had been thrust With a view to removing any adhering perchloride of mercury, the bristles were the agitated in a weak solution of carbonate of soda. This done, the bristles were separately inoculated into nutrient broth, or into liquefied nutrient agar which wa afterwards poured out into a Petri's dish, so as to obtain isolated colonies.

The results of the two most important series of experiments which were instituted b Dr. Marsh were as follows:—10 c.c. of a solution containing 1 in 725 of perchloride o mercury, 1 in 1,015 of chloride of ammonium, and 1 in 152 of hydrochloric acid, is th minimum volume of this solution which suffices to devitalise plague bacilli, incorporate by the method described above into (*a*) 2 grammes of previously unsterilised cow-dung or (*b*) into 0·7 gramme of a mixture of cow-dung and earth from an ordinary floor.

Criticism of Dr. Marsh's results with regard to the form in which they are expressed.

649. Two points in particular seem to invite criticism in these experiments.

The first of these points relates rather to the form in which the results of th experiments are expressed than to the essentials of the experiments. We shall find i convenient to dispose of this first. We may introduce what we have to say in thi connexion by pointing out that in the case of test-tube experiments, when solutions o a disinfectant are employed which exceed a certain minimum concentration, it wil probably be a matter of indifference, so far as the results are concerned, whether th disinfectant is brought into relation with the material which is to be disinfected i the form of a more or of a less concentrated solution. It would, for instance, in the cas of such test-tube experiments as we are here considering, probably be a matter o indifference whether the quantum of perchloride of mercury which was employed to effec the devitalisation of the plague bacillus is introduced into the test-tube in the form o 10 volumes of a 1 in 10,000 solution, or in the form of one volume of a 1 in 1,000 solution provided always that the quantity of disinfecting solution employed suffices for th saturation of the cow-dung. For, inasmuch as the fluid cannot here run off, every portio of the disinfectant would manifestly sooner or later come into contact with the materia which is to be disinfected. The conditions in test tubes are in this respect quite othe than those which exist when the disinfectant comes to be applied in the practical disin fection of a room. It will not suffice for practical guidance in the disinfection of room to have arrived at the conclusion that 2 grammes of cow-dung can be disinfecte by 10 c.c. of a particular disinfecting solution, for, even if there were a ready metho of estimating the weight of a floor which is to be disinfected, and the amount of dis infecting solution required for it, it would hardly be possible effectively to apply th enormous volume of disinfecting solution that would be required. The principle whic

must in practice govern the application of a disinfectant to an infected floor is to use a disinfecting solution of such strength that the volume of fluid which will be absorbed by the floor shall contain a sufficiency of the disinfecting element to produce the effect desired.

If now, taking the results of Dr. Marsh's experiments as given by him, we proceed to calculate out the minimum strength of disinfecting solution which would be required for disinfecting a cow-dung floor, we must obviously take as a basis for our calculations the amount of fluid which can be absorbed by such a floor. On the question as to the amount of fluid which would be absorbed by an ordinary cow-dung floor, we have very few data available. Such data as we have been able to find consist (*a*) in a statement of Mr. Hankin, from which we gather that 1 gramme of cow-dung will absorb at most 1 c.c. of fluid, and (*b*) in a statement of Dr. Marsh, that 2 c.c. of water suffice to moisten thoroughly 1 gramme of cow-dung. If in default of completer information we take the mean between these two estimates as the basis of our calculations, we may assume that every gramme of cow-dung floor will absorb 1·5 c.c. of fluid. Now, neglecting for the reasons given above, the quantity of fluid in which the disinfectant was applied in the test-tube experiments, we find that Dr. Marsh ascertained ·014 grms. of perchloride of mercury to be necessary to devitalise the plague bacilli incorporated in 2 grms. of unsterilised cow-dung. Thus, we have two facts to consider together, viz., that every gramme of a cow-dung floor will absorb 1·5 c.c. of fluid, and that it will require ·007 grms. of perchloride of mercury for its effective disinfection. It is from these facts clear that to obtain an effective disinfecting solution, every 1·5 c.c. of the solution in which the disinfectant is applied must contain ·007 grms. of perchloride of mercury. The solution must, therefore, be one of a strength not below 1 in 215 if employed for the devitalisation of plague bacilli incorporated in cow-dung. If employed for the disinfection of an ordinary earth and cow-dung floor, the solution employed must, on a similar calculation, be of a strength of 1 in 75. These results are sufficiently startling in view of the fact that a strength of 1 in 1,000 or thereabouts has been the strength of this particular antiseptic, which has ordinarily been thought to be sufficient for the sterilisation of a cow-dung floor.

Re-statement of the results of Dr. Marsh's experiments.

8552 (II., 10).

18,842.

650. The significance of Dr. Marsh's results having been made clear, we now proceed to the consideration of his experimental procedure. We have seen above that the method which was employed by Dr. Marsh was to thrust the plague-infected extremity of a glass bristle down to the depths of the powdered material, and then to pour the disinfecting solution on the surface. In accordance with these dispositions it is obvious that the disinfectant had to penetrate the whole of the material before it reached the plague bacilli at the extreme end of the glass bristle. In other words, the arrangements in the experiments were such that the disinfectant could not take effect upon the bacilli at the end of the glass bristle until it had passed through and had inter-acted, so far as it was capable of doing so, with every portion of the enveloping material. We can no more make an inference from this as to what quantity of perchloride of mercury would, under ordinary circumstances, be required for the devitalisation of the plague bacillus in a medium of cow-dung, than we can, from the data given us by Mr. Hankin with respect to the reducing power which cow-dung is capable of exerting on permanganate of potash, make an estimate of what amount of permanganate of potash will be required under similar circumstances. Again, the disposition of the plague bacilli on an earth and cow-dung floor must ordinarily be entirely different from the disposition of the plague bacilli in Dr. Marsh's experiments. This will be manifest when we consider that under natural conditions the large majority of the plague bacilli will, unless the floor material has been renewed, be found on the surface of the floor, or in cracks which directly communicate with the surface. The majority of the plague bacilli being thus disposed will of necessity come in contact with the disinfecting solution before any of the less exposed portions of the floor. Hence, even if they have, as compared with cow-dung, less chemical affinity for the disinfectant, they will, owing to their position, which secures that they shall be more fully exposed to the disinfectant, be more likely to be acted upon by it.

Criticism of Dr. Marsh's experimental procedure.

18,829.

18,838.

II. 10.

For the reasons that have just been given, we may put on one side Dr. Marsh's very careful experiments, as experiments which do not resolve the practical question which is here at issue, as to what is the minimum concentration of perchloride of mercury which will suffice to bring about the desired disinfection.

651. We have now to pass on to consider the next of the methods of experimentation which have been referred to above. In the case of the experiments which we have now in view (and such experiments have been undertaken both by Mr. Hankin

(*b*) Test of effectiveness of a disinfection process by comparing number

of bacteria obtainable from the floor material before and after disinfection.

and also by Dr. Marsh), an endeavour was made to judge of the efficacy of various disinfection processes by making comparative enumerations of the number of bacteria which are present in a floor before and after disinfection.

Mr. Hankin's experiments. 8552 (II., 10–13).

With regard to the particular procedure which was adopted by Mr. Hankin in his experiments, we may observe the following points. The quantity of floor material which was employed for his bacterial enumerations was not measured in any accurate manner. That quantity was, Mr. Hankin says, "as much as could be taken up on the end of a flattened needle." Further, it is to be noted that the number of colonies which appeared in the plate cultures was, as is pointed out by Mr. Hankin, influenced to a very considerable extent by the moister or dryer condition of the floor material. When the material of the floor was dry it did not break up readily in the nutrient medium. Hence the dryer floor material yielded relatively fewer isolated colonies than the wetter floor material which broke up more readily. It is obvious that experiments, into which such serious sources of error as these have been admitted must be accepted with a great deal of reserve. Indeed, Mr. Hankin himself does not claim for his experiments any absolute value. He claims only that results of such experiments would be of "some relative value if repeated sufficiently often." In conformity with these considerations we shall, in appreciating the experimental data with which Mr. Hankin has furnished us, do well to leave out of view the discordant and paradoxical results of certain individual experiments, and to take into consideration only the results which repeat themselves over and over again in the course of his experiments.

Proliferation of bacteria surviving the process of disinfection renders the above test untrustworthy.

652. It would appear that a somewhat complex effect is exerted upon an ordinary earth and cow-dung floor by the application of a disinfecting solution. The first effect apparently consists in a diminution in the number of bacterial colonies which can be obtained from the floor: this effect is no doubt directly referable to the bactericidal action which is exerted by the disinfectant. Supervening upon this, there would appear to be a secondary effect in the form of an increase in the number of bacterial colonies: this last effect would appear to be attributable to a proliferation in the moistened earth and cow-dung floor of the bacteria which have survived the process of disinfection.

It will be manifest that the occurrence of such a secondary increase in the number of bacterial colonies is a phenomenon which will, if it occurs early, and if it occurs to any appreciable extent, make it impossible to place any confidence in any results which have been obtained by the method of enumeration. Now, it would appear, if we may accept as accurate the experimental data given by Mr. Hankin, that, though the maximum diminution in the number of the colonies is, in some cases, found only after the expiration of two hours, the secondary increase in the number of colonies may already have become noticeable after the expiration of only half-an-hour. Again, it would appear from the experimental data furnished by Mr. Hankin that the number of colonies found after disinfection may, in some cases, greatly exceed the original number of colonies found before disinfection. As an example of this, we may refer to experiments made by Mr. Hankin to ascertain the disinfecting effect exerted on a mud and cow-dung floor by a 1 per 1,000 solution of perchloride of mercury, a 1 per cent. solution of chloride of lime, a 1 per 500 solution of sulphuric acid, a 1 per cent. solution of phenyle, and a 1 per cent. solution of lysol, respectively. The excess of the bacterial colonies found after the use of the disinfectants enumerated above was, as compared with the number of bacterial colonies originally obtained, represented by 30 per cent., 215 per cent., 200 per cent., 38 per cent., and 120 per cent. respectively.

8552 (II., 11).

It will thus be obvious, that even if we could leave out of consideration the serious causes of error which, as we saw above, are attached to the experimental procedure adopted by Mr. Hankin, the fallacies which result from secondary proliferation of the surviving bacteria are of themselves quite sufficient to render the method of enumeration an untrustworthy method of gauging the efficacy of a disinfection process, or the relative efficacy of two different disinfectants.

But conclusions as to the effectiveness of certain disinfectants can legitimately be arrived at by this test.

653. While we desire to emphasise this point, we are of course ready to admit that where a very striking diminution of the numbers of bacterial colonies has been obtained after the employment of a particular disinfectant, it will be possible to assert with confidence that the disinfection process which has taken place has been an effectual one. To take an instance, we may legitimately draw a conclusion as to the effectiveness of the disinfectant employed from such results

8552 (II., 11).

is were obtained by Mr. Hankin from two portions of a floor which had been disinfected (in the one case with a solution containing 1 in 1,000 of perchloride of mercury and 1 in 500 hydrochloric acid, and in the other case with a solution containing 1 per cent. of sulphuric acid and 1 per cent. of permanganate of potash). Here, 24 hours after disinfection, the number of colonies found in the disinfected floor was represented respectively by 1 per cent. and 7 per cent. of the original numbers. A similar conclusion as to the efficacy of the disinfectant employed may be drawn from results obtained by Dr. Marsh. In an experiment recorded by this observer, the number of colonies obtained from the disinfected floor was represented by only 6 per cent. of the number present before disinfection. On the other hand, —and this clearly exhibits the difficulty of drawing a definite conclusion from the method of enumeration—we are left in doubt whether a reduction of 65 per cent. in the number of colonies, such as was obtained by Dr. Marsh in another experiment, can be considered as conclusive evidence of the effectiveness (*quo-ad* the destruction of plague bacilli) of the disinfection process which was there employed. 18,862. 18,816.

654. In view of the above, some better method of determining whether a disinfection has or has not been effectual, appeared to be urgently required. It was manifest that such a method could be obtained only by substituting a qualitative for a quantitative test to the bacteria which are obtained from the floor after the completion of disinfection. We have made some preliminary experiments in the direction of elaborating such a method. In the experiments in question, we have taken it as an axiom that, if all the species of micro-organisms, recovered from a floor which has undergone a disinfection process, are micro-organisms which present a considerably greater resistance to the particular disinfectant employed than the plague bacilli whose devitalisation is aimed at by the disinfection, then this disinfection must be regarded as having effected the devitalisation of any plague bacilli which might have been present in that floor. 26,434A. (c.) Method of judging of the effectiveness of disinfection processes by consideration of the nature of the surviving bacteria.

The details of the method which we employed are as follows. Samples of the floor material which has undergone a disinfection process were collected. The samples, inasmuch as they were to serve only for the determination of the varieties of micro-organisms which might survive the disinfection process, were not measured in any way. What was actually done was to collect a little material by rubbing in each case a sterile cotton-wool plug, extracted with antiseptic precautions from a sterile test-tube, here and there over the floor or wall which was to be tested. This done, the plug was replaced, and the samples thus collected were conveyed to the laboratory. When the examination was taken in hand, a small quantity of each soiled cotton-wool plug was placed in a tube of nutrient broth. Care was taken to employ a relatively large quantity of this nutrient medium so as effectually to dilute any disinfectant which might be carried over. This precaution having been taken, the tubes of broth were incubated for 24 hours. In cases where the tubes, after this interval, showed any signs of growth, isolated colonies of the various bacteria were obtained. This was for the most part done by making zigzag cultures on agar. Pure cultures of any bacteria which survived the disinfection process having thus been obtained, graduated dilutions of the disinfectant which was employed in the disinfection process were prepared in a series of test-tubes. As many series of such tubes were employed as there were species of bacteria to be tested. Into each dilution of the disinfectant was introduced a sufficiency of these pure bacterial cultures. After an interval of 5 minutes, a loopful was taken from each solution of the disinfectant which had received a portion of the pure culture of the bacteria recovered from the disinfected floor. This loopful was transferred to a tube of nutrient broth which was incubated at the temperature of the room. By this means information was obtained as to the capacity of the bacteria which were recovered from the disinfected floor for resisting the action of the disinfectant. This resisting power was then compared with the resisting power of the plague bacillus when subjected in similar manner to the action of the same disinfectant, and in cases where the resisting power of the last was found to be very distinctly less than the resisting power of each of the various species of micro-organisms which were recovered from the disinfected material, the disinfection was assumed to have been adequate. Details of method employed in our experiments.

In the experiments which were instituted by Lieutenant Liston, I.M.S., under our direction, with a view to determining by this method the adequacy of a series of disinfections which were done in the Mandvi quarter of Bombay with a 1 in 500 solution of perchloride of mercury, it was found in one case out of seven that the material obtained from the paved or cemented floors which were tested was completely Results of our experiments.

sterile. In the case of two other floors the only growth obtained was a growth o moulds. In the case of the four other floors the cultures gave bacterial growths When the resistance of these last to perchloride of mercury was tested, it wa ascertained that the bacteria, which had survived the disinfection, were all (excep in a single instance where one of the micro-organisms which was recovered possessed a slightly less power of resistance) able to resist an exposure of 15 minutes to a solution of perchloride of mercury of a strength of 1 in 1,000. Plague bacilli tested in a similar manner in no case survived an exposure of 15 minutes to a solution of 1 in 5,000 of perchloride of mercury. It would thus appear that the particular disinfections whose efficacy was tested by this method were perfectly adequate.

Probable utility of above method.

The information which is obtained by this method with regard to the efficacy of a disinfectant may appear to be gained by a somewhat laborious method, but we foresee that a bacteriologist who was occupied in such work would very soon possess a knowledge of the bacterial flora of the locality in which he was working such as would enable him to judge at a glance of the efficacy of the disinfectant by noting the morphological characters of the colonies of the bacteria obtained from the disinfected material. It seems to us that by some such system of bacteriological examination as that which has been outlined above, it would be possible not only to determine in any particular case the efficacy or otherwise of a particular disinfection, but also that it would be possible to determine for general guidance not only the question as to which is the most effective disinfectant, but also the question as to the minimum strength of that disinfectant which would be effective.

III.(4.)(*c.*) *Efficacy of Burning, Heat, and Steam, as Disinfectants for Plague.*

Disinfection by the agency of heat.

655. We may now pass from the consideration of the principles of chemical disinfection to consider the general question of disinfection by heat.

In connexion with the application of heat to the sterilisation of plague-infected objects, we may consider here the application—

Firstly, of the extreme temperatures which are developed in a flame;

Secondly, of dry heat, meaning thereby temperatures short of the charring point;

Thirdly, of temperatures ranging from 100° C. upwards to 130° C., or thereabouts, such as those which are developed when water is vaporised in a closed vessel under pressure of a few atmospheres; and

Fourthly, of temperatures of 60° C. to 100° C., such as those which are obtained when water is heated in open vessels.

Conditions under which different methods of disinfection by heat are appropriate.

656. Stated generally the destruction of objects by burning is a method to be resorted to only where there is difficulty in insuring that the whole of the infected material shall be adequately disinfected by other means.

Of quite exceptional application is, also, the agency of dry heat, in the form of temperatures short of the charring point. It will suffice, since the influence of dry heat below 100° C. will come into consideration in connexion with the discussion of the influence of desiccation, here to consider the influence of the application of temperatures between 100° C. and 200° C., when charring may be said to begin. With regard to the effect exerted by such temperatures on the vitality of the plague bacillus, information is afforded by the experiments of Gladin. This observer found that the vitality of the plague bacillus was destroyed in 20 minutes by an exposure to a temperature of from 100° to 110° C., in five minutes by exposure to a temperature of from 130° to 140° C., and in one minute by an exposure to 160° C. It will thus be seen that when the agency of dry heat is employed, the destruction of the plague bacillus is rapidly achieved only at temperatures approaching the charring point. It is, therefore, manifest that the agency of dry heat is not a convenient disinfecting agency for employment in ordinary life.

Centralblatt für Bacteriologie, 1898.

Where the agency of moist heat is to be brought into application as a disinfectant, our choice will lie between the employment of temperatures ranging between 60° C. to 100° C. and the employment of temperatures ranging between 100° C. and 130° C. Before considering the various methods which may be adopted for bringing these temperatures into application, we shall do well to place before ourselves the following facts:—

(*a.*) The plague bacillus is killed instantly at a temperature of 100° C. and, of course, at all higher temperatures.

(*b.*) The plague bacillus is killed after an exposure varying from a quarter of an hour to a few seconds when it is exposed to temperatures between 60° C. and 100° C.

It will be manifest from this that so far as the efficacy of the disinfection is concerned the temperature which is developed when water is boiled in an open vessel is amply

ufficient for the instant sterilisation of plague-infected objects. Briefly stated, he conditions under which an advantage will accrue from the use of the higher ange of temperatures which can be obtained by means of steam generated under ressure are limited to cases where objects would be destroyed by immersion in oiling water, and to cases such as that of the disinfection of the personal effects of ravellers, where the subsequent drying of the disinfected articles would entail delay nd discomfort.

In view of the fact that these inconveniences can be avoided when disinfection s done by steam under pressure, we may briefly consider the different modes in which team under pressure is brought into application for disinfecting purposes.

Methods of applying steam under pressure for disinfection purposes.

657. It must be distinctly understood that the temperatures which are associated with he use of steam under pressure are not in all cases brought into effective operation in team sterilisers. When tight bundles of clothes are introduced into such sterilisers, nd when steam is admitted into the disinfecting chamber merely to be shut off after he chamber has been filled (this is an arrangement which was adopted in many xtemporised steam disinfectors in India), the pressure, and in correspondence with this he temperature, of the steam may be reduced, by condensation on the outside of the undle, until it entirely ceases to be effectual. Again, even where the pressure of he steam is continuously kept up in the disinfecting chamber, the heat, though well ept up in the disinfecting chamber as a whole, may none the less, owing to the fact hat the air has not been displaced, fail to penetrate into the interior of the bundles of lothes.

Precautions against failure, due to this last cause, are therefore taken in all the more odern patterns of steam sterilisers. In some of these the steam as it is given off om the boiler is sent into the disinfecting chamber, and is allowed to issue thence a continuous stream through a safety valve. It is intended that the steam which thus passed through, shall evacuate all traces of air from the interior of the clothing a such a manner as to permit the heat diffusing itself throughout the whole mass. t would, however, appear that this expectation is not always fully realised. The eam will, in many cases, since it will follow the most unhindered path, find its ay to the escape only through the larger vacant channels left between the bundles of othes. In consequence of this, the air will only be very slowly evacuated from the terior of the clothing, and in correspondence with this the heat will take a long me to penetrate.

Another, and a more effective, method of achieving the desired evacuation of the air, to fit the steam steriliser with an arrangement which allows of the disinfecting amber being evacuated and refilled with steam several times in succession. By this ocedure the air is drawn out from between the interstices of the clothing, with the sult that the hot steam is enabled to penetrate rapidly.

In view of the above, it will be readily understood that effectual disinfection by eam under pressure can be ensured only when clothes are thoroughly opened up or hen steam sterilisers are employed which allow of the air being effectively evacuated om the interior of bundles of clothing. It will be wise always to control the working the steriliser by placing a maximum thermometer (preferably a signal thermometer) the interior of the bundles of clothing. To secure that attention shall be given to ese essential points, it will be necessary in all cases to commit the working of the eriliser to a skilled and trustworthy operator. Where such an operator cannot be cured, we think that steam sterilisers are likely, in practice, to be found quite effective.

III.(4.)(*d.*) *Efficacy of Direct Sunlight as a Disinfectant for Plague.*

Disinfection by exposure to the direct action of the sun.

658. We may next turn our attention to the disinfecting agency of the sun. A fficiency of experimental data is available to enable us to form some judgment of e influence which is exerted by the direct rays of the sun upon the vitality of the ague bacillus. Experiments on this question have been instituted, among others, by tasato, by Wilm, by Abel, by the German Plague Commission, and by ourselves.

It is clearly brought out by the experiments in question that direct sunlight, th in India and in temperate climates, exerts a very considerable devitalising effect the plague bacillus, more particularly where the culture which is exposed to the ys of the sun is spread out a very thin layer. In such circumstances it was found at exposure for one hour to the sun's beams sufficed to devitalise the plague bacillus. hen the bacillus was spread out in thicker layers the sun's rays took much longer effect their object. Naturally the time required for the devitalisation is longer when plague bacillus is covered up in any way. The German Plague Commission found

that the bacillus, when incorporated in wool and exposed to sunlight, maintained its ful virulence after eight hours' insolation. The experiments which were made by ourselve 26,342–6. are detailed in the evidence of Lieut. H. J. Walton, I.M.S. We, like the German Plagu Commission, found that agar or broth cultures of plague preserved their vitality afte exposure to direct sunlight for three hours. It was, however, noticed, that when subse quently to insolation the contents of the test tubes were inoculated on new culture media the growth of plague was very much retarded, the first signs of growth making thei appearance only after the fifth day instead of on the first day. Plague cultures o agar and broth were not finally killed off until after an exposure of four hours to th direct light of the sun. When spread on loosely woven calico the plague bacillu was killed off by a three hours' exposure to sunlight, even when it was covered u by as much as six layers of the same material. On the contrary, the sun's rays di not exert any disinfecting action even in $3\frac{1}{2}$ hours through a single fold of flanne Again, in another experiment done with the hemmed margin of an ordinary pocke handkerchief, plague could be recovered by cultivation after exposure to strong su light for three hours. In this experiment the growth was, however, very considerabl retarded. Lastly, in an experiment in which thin twisted wisps of cotton-wool, whic had been infected with plague, were exposed to the sun's rays, it was found that three hours' exposure did not suffice to devitalise the plague bacilli.

We were on no occasion able to observe that any growth-retarding or devitalisin action was exerted on the plague bacillus by the action of diffused daylight.

III.(4.)(*e.*) *Efficacy of Desiccation as a Disinfectant for Plague.*

Disinfection by desiccation.

659. A great number of observations have been made by Kitasato, Wilm, Abel, an the German Plague Commission, to determine the effect which is exerted upon th plague bacillus by desiccation. The more important results of these experiments ma be summarised as follows:—Desiccation exerts a devitalising effect upon the plagu bacillus which is proportionate to the degree to which the desiccation is carried This last is determined on the one hand by the degree of humidity of the air, and o the other hand by the degree to which the bacilli are exposed to, or sheltered from th desiccating influence of the air. In ordinary circumstances the humidity of th air will be influenced more than by any other factor by the factor of temperature and the degree to which the bacilli are exposed to the desiccating influence of the ai will depend in great measure upon the nature of the material with which they ar incorporated.

Period of survival under desiccation under different conditions.

The influence of the temperature is clearly brought out by Abel's experiments plague bacilli desiccated at a temperature of 35° C. being devitalised in two to thre days, whereas plague bacilli desiccated at 16° to 20° C. survived six to nine days.

The influence of the material with which the plague bacillus is incorporated is show in the experiments of Abel and others by the fact that the plague bacillus whe included in woollen, silk, or other fibre, or in pieces of infected organs, has a muc longer period of survival than when it is disposed on the surface of non-porous objects This result is probably in the main referable to the mechanical protection fro desiccation which is afforded by the materials in question to the plague bacilli in thei interstices. It is, however, quite possible that the water-retaining properties of th materials themselves may also come into account.

With a view to appreciating the application of these principles, we may consider wha the actual period of survival is when the plague bacillus is placed under certain definit sets of conditions. We may take first the case of a plague culture which has bee spread out in the form of a thin film on the surface of a coverglass, which is placed i a desiccator over sulphuric acid or calcium chloride. In these circumstances, bot in Wilm's experiments and in our own, the plague bacillus survived only a very fe hours. We may pass from this to the case of plague bacilli incorporated in silk, woo or other fibre, and subjected only to such desiccation as will occur at a temperatur of 18° to 25° C. under ordinary atmospheric conditions, and for the most part in th dark. Information regarding the period for which the plague bacillus can surviv Report of German Plague Commission, page 277. under such circumstances is available from several sources. We have an experime of the German Plague Commission showing that the plague bacillus may survive und these conditions for 28 days. We have, further, experiments by Forster, Löffler, an Centralblatt für Bacterioiogie, December 20th, 1899, page 722. Gladin, which show that the plague bacillus may survive when dried on silk, wool, an cotton cloth, for as long as 45, 56, and 76 days respectively. Lastly, we have a experiment carried out by ourselves under these conditions, in which the plague bacill was found to survive on calico for a period of 70 days. Long periods of survival ha also been observed to occur in the case of plague bacilli incorporated either

albuminous discharges, or in the organs of animals dead of plague. A reference may here be made to the experiments of Katzarof, and to those of Gladin, who showed that the plague bacillus retained its vitality for 16 to 36 days in the organs of plague-infected animals. Finally, we may consider what the period of survival has been in the case of experiments performed under the atmospheric conditions which obtain in the fair weather in Bombay. The data upon which we can draw for information with regard to the behaviour of plague in these conditions are the data of the experiments made by the German Plague Commission at Bombay, and the data of a few similar experiments instituted there by ourselves. It was found in the experiments of the German Commission that the devitalisation of the plague bacillus even when it was incorporated in woollen or other textile fabrics or in albuminous discharges was in every case effected within eight days. The few experiments which we made gave us very similar results. The longest interval after which we recovered the plague bacillus was seven days. This was in the case of a gunny bag which had been artificially infected with plague. In the case of grain artificially infected the bacillus was not recovered by us after seven days, though it was recovered on the third day.

Loc. cit.

26,344-6.

26,386.

The results of the experiments done in Bombay would appear to be very important results because of their application to India. But it must be borne in mind that they relate only to plague bacilli disposed on the surface of non-porous objects, and to plague bacilli disposed on threads, or on comparatively small pieces of various fabrics fully exposed to the air. Now these manifestly are conditions which only rarely obtain in actual life. And where objects are piled up one on the top of the other, and generally where large masses have to be dealt with, effectual desiccation may quite well be a much slower process than it was in these experiments. Moreover, these experiments will not afford any basis for an estimate as regards the period for which the plague bacillus can survive in colder, and at the same time moist, climates; and in our consideration of Mahamari, we have seen that there are facts which suggest that the plague bacillus may survive in Kumaun and Garhwal for months, if not for years, outside the animal organism.

Remarks on period of survival of plague under desiccation in Bombay.

Para. 375 of this Report.

III.(4.)(*f.*) *Efficacy of Ventilation, Cleansing, and Washing, as Disinfectants for Plague.*

660. There is but little to be said about disinfection by ventilation and by ordinary cleansing and washing. The following remarks are all that seem called for. The mere admission of air into an infected house or room cannot be expected to bring about the devitalisation of the plague bacilli which exist there. Ventilation, however, operates as a disinfecting agent in so far as the currents of air which are set up conduce to the desiccation and to the mechanical removal of the infected material. In conformity with this a sharp distinction must be drawn between mere admission of air and ventilation. Manifestly, cleansing and washing are effectual only in so far as they mechanically remove infected material.

Disinfection by ventilation, and by ordinary cleansing and washing.

III.(4.)(*g.*) *The Crowding Out and Destruction of the Plague Bacillus, without Special Measures of Disinfection, by the overgrowth of Saprophytic Bacteria.*

661. We may next consider the question of the crowding out of pathogenic germs in the struggle for existence by the overgrowth of ordinary saprophytic bacteria. This disinfecting agency, which is continuously and everywhere at work in nature, may be regarded as one of the most important of all the disinfecting agencies which operate in the direction of destroying pathogenic bacteria. How greatly the growth of the plague bacilli is, in external nature, hampered and checked by competition with the ordinary saprophytic organisms which lie about everywhere, will be best appreciated when we place before ourselves the extent to which these organisms effect the growth of plague in cultures. When a plague culture has been contaminated with any considerable number of the common saprophytic bacteria, it becomes, owing to the more luxuriant growth of these last, a matter of the very greatest difficulty to recover the plague bacillus by cultivation from the impure culture. It would even appear that certain saprophytic bacteria, and notably, as pointed out by Bitter, the bacillus coli communis, which is invariably found in fresh cow dung as well as in excreta of other animals, and, according to Gladin, also the micrococcus prodigiosus, the bacillus subtilis, and certain pathogenic micro-organisms, exert a direct restraining influence on the growth of the plague bacillus. We were able to obtain some evidence of the restraining influence which is exerted by the toxins of the bacillus coli communis on the growth of plague. The experiments in

Disinfection by the crowding out of pathogenic germs in the struggle for existence by the overgrowth of ordinary saprophytic bacteria.

26,341, 26,391.

Bitter, Report on Plague in Egypt, p. 52. Centralblatt fur Bacteriologie 1898.

26,150.

question will be found detailed in the evidence of Capt. Spencer, R.A.M.C. Facts of this sort are of capital importance, inasmuch as they seem to indicate, and the indications in question are borne out also by practical experience, that uncleanly houses do not necessarily, when infected, become hotbeds of plague. Again, in this connexion it may be well to keep in view the fact that the sweepers of Bombay who deal with all the excreta of the plague-infected city, were for a long period almost free from plague. Up to the date of our departure from India they seem from the point of view of their liability to contract plague to have compared favourably with the rest of the community. These facts may perhaps be to some extent accounted for by the processes of self-disinfection which take place in infected filth.

Para. 306 of this Report.

Having thus placed before us the general principles of disinfection, we may proceed briefly to review what has been done in the way of disinfection in India. We may first consider the disinfection of houses.

III.(4.)(*h.*) *Measures of Disinfection which have been applied to Houses in India.*

Measures of disinfection which have been applied to houses, and extent to which these measures have been employed.

662. The disinfection of houses was one of the earliest plague measures adopted in Bombay City. It has throughout been vigorously resorted to. In the earlier period phenyle and permanganate of potash solutions were the disinfectants which were most largely employed. In addition sulphur fumigations were at this period carried out in the streets and houses; tons of carbolic acid were poured into the sewers; and in certain instances the drain pipes were taken down from the houses and were steeped in hot water. Chloride of lime, and creosote and carbolic powders, were strewn about without system; whitewashing was superadded to the chemical disinfection, and the houses were, after disinfection, vacated for a considerable period. At one time a system was in vogue by which houses situated on the line which plague was expected to follow were whitewashed in advance, this measure being in some cases confined to the outside of the houses. Infected houses were also, in some instances, burned down. In many cases they were completely or partially unroofed. In combination with these last measures, a system was largely adopted of breaking holes in the partition walls between the different rooms in the overcrowded chawls. Obstructions were removed from the verandahs. In some cases ventilation was further improved by making holes in the floors and ceilings, so as to allow a current of air to pass up through the buildings. Many of these measures have now been abandoned. Chemical disinfection of the houses is now carried out with a solution of perchloride of mercury in hydrochloric acid. Whitewashing after disinfection has been largely given up, and the people are now allowed to return to their houses as soon as these have been disinfected. With regard to non-chemical methods of disinfection, it will suffice to note that, with a view to admitting "light and air" to the houses, a process of partial unroofing is largely resorted to, not only in the case of infected houses, but in the case of insanitary houses generally. Further, the pipe water supply has been cut off from the interior of the houses. This last has been done with a view to diminishing the dampness which arises from the disposition of the natives to leave water taps running.

The measures of house disinfection adopted in Bombay City were, speaking generally, adopted also in other towns. We may note, however, as a point of difference that in Hubli the very drastic measure of burning down a whole street was resorted to. Further, we may note that in some places the process of kiln burning the floors, which is described below, was resorted to in conjunction with or in lieu of the process of chemical disinfection.

1645, 1954.

Para. 665 of this Report.

In the case of villages several different lines of policy were adopted. In some cases chemical disinfection, whitewashing, opening up, and keeping the houses unoccupied, were all carried out. In other cases chemical disinfection was omitted and the houses were merely opened up and vacated. Again, in widely infected tracts like the Mysore State and the Deccan districts of Bombay, where there were scores of infected villages, systematic measures of disinfection were impossible; and little could be done beyond encouraging the people to clean out their houses, to keep them vacant for some time, and to make ventilation openings in the roofs. In the Hyderabad State, in the second year of the epidemic, the kiln-burning of the floors was resorted to, to the exclusion of all other measures.

3324, 2145-6, 24,037.

5443-9, 5846.

Just as there were differences in different places with regard to the measures of disinfection which were applied, so there were differences with regard to the extent to which the infected houses were brought within the scope of the disinfecting operations. It will suffice to refer to a few points in illustration of the different

practices which have been pursued in different places. In Bombay City the practice, in the case of chawls, has been to disinfect the two rooms on each side of that occupied by the plague patient. In Calcutta, disinfection is restricted to the room which is inhabited by the plague patient. In Baroda, disinfection was applied to every house to which a death of any kind could be traced. Disinfection, *en bloc*, was resorted to in the case of the combined towns of Hardwar, Kankhal, and Jawalapur. Here the entire towns appear to have been disinfected house by house, and room by room. Similarly, in villages where chemical disinfection was undertaken, the village was disinfected from end to end. 25,640. 6591. 14,631. 9045, 9115, 9175.

663. The agency which was employed for the disinfection of houses may with advantage be considered next. In a few places, as, for instance, in Bangalore and in the Bellary district, native soldiers were employed as disinfectors. In other places, as in Karachi, the disinfection of the houses was committed to the house-owner, the Municipality finding the disinfectant; but this was a more or less exceptional practice. Generally, however, throughout India the disinfection was done by gangs of coolies specially engaged for the purpose, with a native overseer at their head. Wherever possible it was arranged that the work of the disinfecting gangs should be supervised either by Hospital Assistants, or, where such were available, by British soldiers. The general supervision and initiation of the disinfecting arrangements was in most cases committed to the British Staff Corps officers who were employed on plague duty. Only in a very small proportion of cases were the disinfecting arrangements directly supervised by anyone, medical or lay, who had any special scientific training or any previous experience of disinfecting operations. Agency employed for disinfection of houses. 7971–6.

In connexion with the agency employed on disinfection operations mention deserves to be made of the system, initiated by the Surgeon-General with the Government of Bombay, of providing, with a view to their being sent to newly attacked places, trained gangs of disinfectors, with a regular equipment of wooden pumps and other implements and a supply of disinfectants. The constitution of these "flying columns," as they were called, was in intention as follows. The column was to be placed in charge of a Commissioned Medical Officer or other English doctor. Two native Medical Assistants, and, where possible, also a female Hospital Assistant, were to be provided. The working staff was to consist of a couple of native overseers and 20 or 30 coolies recruited from among the disinfecting gangs employed in Bombay City. In actual practice, as soon as notice was received from a Collector of the occurrence of a new outbreak of plague, a column was despatched ready to begin work at once, and to serve as the nucleus of a larger establishment. It was naturally impracticable, with plague spreading everywhere, to supply anything like the intended staff. In many cases it was impracticable to send even a medical subordinate of any kind. "Flying columns" 16,992, 17,158–9, 21,618–29.

664. The actual process of house disinfection, to take the particular case of the Punjab, where disinfection appears to have been very scrupulously carried out, is thus described by Captain James, I.M.S., who was in charge of the operations :— Procedure adopted in chemical disinfection of houses.

"The disinfecting gang was collected in the street near the house, and all the materials required were placed in readiness in some convenient open space, if possible, near the centre of the area to be dealt with. The owner of the house was brought in by the Hospital Assistant or the constable from the camp where he was living, and allowed to open the lock of his door, and then made to sit down in the street, somewhere where he would not be in the way, and where he could watch all that was being done to his house and property. But he was not allowed to take any active part in the disinfecting or to enter any of the rooms till they had been disinfected. With pumps and syringes a stream of antiseptic solution was made to play on the closed doorway till the woodwork was thoroughly wet. It may seem unnecessary to begin using the antiseptic so soon, but the object was to impress the workers that they had to deal with infected places, and it was necessary to teach them thoroughness from the very commencement and to insure the good work, without which the object for which they strove would not be obtained. The door was then thrown wide open, and before anyone was allowed to enter a stream of antiseptic was made to play over as much of the interior of the room as was visible from outside. Thus the dust inside was laid, and it was fairly safe for persons to enter. The pump-men then went inside and from the centre of the room continued the pumping till every part of the ceiling, walls, and floor were wet and running with antiseptic solution. Other coolies now entered and carried out into the open every piece of furniture or movable property, while they were still wet with the lotion, and placed them either in the roof in one corner or in some convenient place in the 10,015.

yard, or even in the street, where they could get the full benefit of the direct rays of the sun. All rubbish, rags, or articles of no value to the owner were taken out into the open and burned. Small articles were dipped bodily in vessels containing antiseptic fluids which were kept in readiness near the doorway. The owners were encouraged to take as much as possible of their property away into their camps after it had thus been thoroughly disinfected. We found this necessary, because when people went out hurriedly into camp, afterwards they found they wanted things. The Hospital Assistant and the disinfecting gangs and officers were all present, and we could disinfect their articles and let them take them out with them. Corn, seeds, flour, turmeric, and such articles which would be spoiled by placing in water were spread out in the sun for a day; the vessels which had contained them were disinfected *secundum artem*, and, when dry, the various materials were returned. Books and manuscripts were also exposed to the sun's rays. . . . . The dry places, left by removing articles from the room, were at once tackled by the pump-men, who directed a jet of antiseptic towards every space thus left undone. In the same way, articles removed were subjected to a thorough drenching with antiseptic, when they got outside."

The process of chemical disinfection which is here described, with the difference that in most places perchloride of mercury took the place of the phenyle solution, was also the procedure which was adopted throughout India. In particular, the practice of pumping, or, where pumps were not available, of throwing the disinfectant over the walls, appears to have been everywhere adopted.

Method of kiln-burning floors.

665. As a substitute for the process of chemical disinfection described above, the process of "kiln-burning" the floors of infected houses was, as we said above, in some instances resorted to. This was notably the case in the Hyderabad State. The following description taken from a memorandum prepared by H. H. the Nizam's Deputy Plague Commissioner will give an idea of how this kiln method was carried out.

I. 455. "The surface of all floors, and the lower portion of walls two feet from the floor surface is first to be painted over with rather thick limewash and allowed to dry. This serves two useful purposes. (1) It lays the dust, and thus lessens the risk of inoculation to the coolies working in the house; and (2) it colours white every part of all surfaces that have to be removed and burnt. The whitened surface is easily seen, and there is little or no chance of any portion of the infected earth remaining behind and being left unburnt. While the lime-wash is drying on the surface, the foundation or floor of the kiln in which the surface earth is to be burnt, should be laid. The kiln should be made in any sufficiently large sized courtyard, garden, or open space in or near the house, or the road or pathway immediately facing the front entrance of the house can be used. A circular kiln will be found the most convenient in shape. The floor of the kiln is made of two layers of "ooplies" (cowdung cakes), four inches deep, laid flat, and packed closely together. The mud floor of the infected house should be carefully dug out to the depth of $1\frac{1}{2}$ inches from every room, verandah, and yard which has been whitewashed, and filled into iron baskets, or empty kerosine oil tins, if possible, without spilling or raising any dust. The dug out earth should now be carried to the kiln and evenly spread over the "ooplies," which form the floor of the kiln to a depth of $1\frac{1}{2}$ inches. This, the first layer of infected earth is then completely covered with a layer of "ooplies," 2 inches deep, upon which a second layer of earth is spread and covered with "ooplies," then a third layer of infected earth, and so on, with alternate layers of infected earth and "ooplies" until the kiln is complete. One and a half to two feet is the best height for a kiln. Lastly, the kiln is covered in from top to bottom with two layers of "ooplies." It should then be fired at the base, and allowed to slowly burn itself out, a process that usually takes about 48 hours. It is, however, advisable not to disturb the ashes of the burnt kiln for a week, as it continues to smoulder for about that time. When the kiln has cooled down, it should be opened in one or two places and examined, and, if found completely burnt out, the ashes should be removed to cultivated lands and used for manure. The entire limewashed surface of the infected floors having been dug out and burnt in the kiln, all that remains to be done to the infected house is to fill in the empty floor space with fresh field earth, moisten it with water, and consolidate it by ramming; subsequently, mud-plastering, or "leeping" it. Finally the new floor surface and walls should be well drenched with perchloride of mercury solution of the strength 1 to 1,000 parts of water. The house is now perfectly disinfected, and as soon as the floors have dried it is fit for occupation."

666. The methods which were adopted in opening up the houses differed in different places. In some cases, as has been said above, the houses were completely unroofed, the walls alone being left standing. This radical measure was, however, comparatively seldom resorted to. In Bombay City, as we have already seen, and in other towns, the system that was most commonly adopted was that of taking off alternate rows of tiles, and of stacking these up upon the top of the rows which were left in position. Methods of opening up the houses to sun and air.

In the case of the villages, as a substitute for complete unroofing, the general custom was to make an opening through the thick mud roof, so as to admit "light and air" into the windowless inner room of the village house. The result achieved of course varied with the size of the opening which was effected. In many cases openings were so made as to admit a considerable amount of sunlight; in some cases a mere pencil of light was admitted into an otherwise perfectly dark room. Occasionally the hole which was broken in the roof was supplemented by a window broken in the wall. In most cases the principal room of the village house was not interfered with, light and air being assumed to come in in sufficiency through the doorway.

667. The general attitude of the people towards the measures of disinfection which have been applied to their houses has been an attitude of resignation. In the recently infected parts of India, no instance has been brought to our knowledge similar to those which have been reported from plague-stricken villages in the district of Kumaun and Garhwal, where the people after their long and bitter experiences of plague have begged the authorities to proceed to the extreme measure of burning down the infected houses. The nearest approach to a recognition of the advantages of applying measures of disinfection to their houses is, perhaps, to be found in the fact that when Lieut. Thomas placed a premium on disinfection by making a charge for the labour and a further charge for the disinfectant, the inhabitants of Hubli were willing not only to pay these charges, but also to vie with their neighbours in incurring extra expense for additional disinfectant. This statement is, however, to be taken in connexion with the fact that we are told of these same inhabitants of Hubli, that they had been bribing the coolies to write up disinfected on their houses and to go away and leave them alone. However this may be, we are told of the closely adjoining town of Dharwar that the people probably did not believe in disinfection, but applied for it "as a means of re-admission to their houses." In addition to being actuated by a natural reluctance to have their houses interfered with, the natives in India generally appear to dislike the idea of their houses being treated with "chemicals." The people had also a strong aversion to the greater interference and damage to their houses which is associated with the system of untiling and of breaking holes in the roofs and windows in the walls. The attitude of the people to the whole of such measures is clearly brought out in the fact that owing either to their desire for privacy, or owing to a feeling of insecurity which they associate with the existence of openings in the walls, or owing, perhaps, to their dislike to draughts, these openings were at the first opportunity very generally closed up again. Attitude of the people towards the measures of disinfection which have been applied to their houses. II. 362 (262). 23,454. 23,366, 23,399. 23,776. 7520, 9740, 9893, 16,137. 867, 3419, 10,271, 12,077, 19,515, 23,404, 24,203-9, 24,361-2.

III.(4.)(*i.*) *Efficacy of the Measures applied in India for the Disinfection of Houses.*

668. In arriving at an opinion with regard to the efficacy of the processes of house disinfection which were carried out, it is manifest that, in addition to the experimental data already considered which bear on the efficacy of the various disinfecting agencies employed, we must take into consideration, on the one hand, the evidence which has been placed before us regarding the manner in which the different disinfecting processes were carried out in practice, and, on the other hand, the opinions formed by actual observers as to the results which were achieved by the disinfecting operations. Efficacy of house disinfection by fire.

In certain cases, as, for instance, where the method of burning down infected houses was resorted to, a doubt as to the complete destruction of all the infective material cannot possibly arise. A doubt as to the completeness of the disinfecting process does, however, arise in cases where less radical methods of disinfection by fire were brought into use. With regard to the method of kiln-burning we may note first, that this method deals only with the germs which are incorporated with the floor, or which are to be found in the lower portions of the wall surface. We may note further that even where steps are taken to whitewash the floors and walls, there must be great difficulty in insuring the removal of all infected material. There will thus, unless the kiln-burning is afterwards followed up by chemical disinfection, be a risk of a source of infection

5449, 5851. remaining behind in the houses. It is stated, it is true, that in the Hyderabad territo no case has come to knowledge where plague has recurred in a house which has be treated by kiln-burning. But this would not be conclusive of the efficacy of t system, inasmuch as it does not appear that the people were re-admitted to th houses immediately after the kiln-burning operation had been completed.

Efficacy of the particular disinfectants in the particular strengths employed in chemical disinfection of houses.

669. Coming now to the question of the efficacy of the measures of chemical disinfecti which have been so widely employed throughout India, consideration shows that t question resolves itself in every case (*a*) into a question as to whether the measu were measures which would, if properly carried out, have insured the disinfection all the infected material, and (*b*) into a question as to whether those measures we efficiently carried out.

With regard to the efficacy of particular disinfectants and the particular strengths disinfectants employed, there is very little in the form of fact definitely approved experience which can be added to the experimental data considered above. That th should be so will not be surprising when we take into consideration the circumstanc in which the disinfecting operations were carried out. First, we have the fact th the amounts of the disinfectants employed were only very rarely measured wi accuracy. Secondly, we have the fact that the work which has to be done in a plag epidemic is so laborious that it leaves no leisure for the collection of precise data wi regard to the efficacy of disinfection. And thirdly, we have the fact that in t large majority of cases, chemical disinfection was supplemented, first, by a process whitewashing; secondly, by a process of opening up the houses; and lastly, keeping houses empty for a considerable interval after these disinfection processes h been applied. It will be manifest that in such circumstances it is practical impossible to say how much of such success as was attained was achieved by th chemical disinfection, and how much by the subsequent processes.

Manner in which the disinfectants were brought into application in chemical disinfection of houses.

670. Passing now from the question of the efficacy of the disinfectants adopted for u to the consideration of the manner in which these disinfectants were actually broug into application, we may consider the following facts brought before us in eviden relating to perchloride of mercury which was, as we saw, the disinfectant most wide employed in India.

21,310. The perchloride of mercury employed was often very impure. For instance, in th case of the perchloride of mercury tested by Colonel Fawcett, R.A.M.C., at Poona, on 50 per cent. of its weight was soluble in water. In addition to being impure, it wa 21,307. in the particular case of Poona, where the matter was inquired into by Colonel Fawcet found to be deficient also in point of quantity, packets of the disinfectant being issu whose actual weight was much smaller than their nominal weight. In other cases and these were not at all infrequent—the powders of perchloride of mercury whi were issued for the purpose of making up the disinfectant on the spot remain undissolved at the bottom of the tubs. This was afterwards to some extent remedi by the addition of common salt. In other cases, again, metal buckets and met syringes were used for the preparation and application of the perchloride of mercu solutions. This must have resulted, at any rate in some cases, in an appreciab reduction of the strength of the disinfectant. With the view of preventing the was of the perchloride of mercury thus entailed, the use of wooden buckets and Chine pumps was introduced. We found that only in very few cases was sufficient provisi made to secure that the disinfectant used should be accurately made up to the intend standard of strength. At Igatpuri the perchloride of mercury was, we were told, ma 23,317. up in a strength of 1 in 10,000, in lieu of in the intended strength of 1 in 1,000. 5100–8. Guntakal, and we have reason to believe at other places also, perchloride of mercu was mixed into the limewash, with a view to bringing both these disinfecting agen into operation simultaneously. The perchloride of mercury must have be rendered perfectly inert. With regard to the material which was employed for whit washing we found that, owing to the fact that the disinfecting staffs did not apprecia the essential difference between caustic lime and other whitewashing materials, t whitewash often consisted either of chalk, pure and simple, or of lime which had large part forfeited its caustic properties by prolonged exposure to the atmosphe These errors are serious; and equally fundamental errors were committed in the ca of the other disinfectants employed, particularly in the matter of making up t solutions to the required standard of strength. Such errors must have, in themselv rendered the disinfection operations in many cases nugatory.

A source of failure, which is not of less importance, is to be found in the fact that it was in many cases practically impossible to exert anything like an effective control over the disinfectors, and to see that the procedure prescribed was actually carried out. We have already referred to the fact of the disinfectors at Hubli having been bribed to omit disinfection altogether and to mark the houses as having been disinfected. The way in which things were done at Surat—and what was done here may be taken as typical of what was done in every case where effective supervision could not be exercised—may be gathered from the evidence of Mr. Moore, the Collector. He said:—"When I was in Surat, it was my daily work to ride round the city and examine the houses which had been disinfected. I frequently found that a room had been left undisinfected, or, perhaps, the walls had been done and the floor left untouched, or the ceiling had not been disinfected, or such like omissions; parts of the house had been soaked, while the rest was comparatively dry." 7862, 21,096. 21,154, 21,174, II. 13. 24,450.

671. Two distinct issues arise in connexion with the question of the efficacy of chemical disinfection as applied to houses. The first is the question as to whether the application of the chemical disinfectants employed destroyed the infective material present in the house when the disinfection was undertaken. The second is the question as to whether the risk of the re-introduction of infection into a disinfected house, either, previously to the re-entry of the people by the agency of rats, or, subsequent to the re-entry of the people, by the agency of rats or men, is so great as to detract from the value of chemical disinfection as a policy. This last is a very important question which will be referred to hereafter, and here we will confine ourselves to the narrower issue of the efficiency of the processes of chemical disinfection actually carried out. Opinion as to the efficacy of chemical disinfection of houses as actually carried out.

In view of the circumstance that disinfecting operations were, owing to the want of efficient supervision, often carried out in the negligent manner which we have described above, and that serious technical errors such as those adverted to above were committed in the preparation of disinfecting solutions, we have no doubt that much of the chemical disinfection done in India was inefficient. Where, however, skilled and efficient control was exercised, as in the operations carried out under the personal supervision of Capt. James, I.M.S., and other European officers in the Punjab, of Col. Fawcett, R.A.M.C., and Major Ross conjointly in Poona, and of Mr. Winter and his staff in the North-Western Provinces, we have little doubt that chemical disinfection exerted a preponderating influence in destroying the infection. It is true that in these cases measures of chemical disinfection were combined with the opening up or the prolonged vacation of the houses—both measures calculated to bring the action of desiccation and sunlight into operation. But that the freeing of the houses from infection might have been achieved independently of these measures will, we think, be clear from the following facts relating to the Punjab and to Poona. At Banga, in the Punjab, as we learn from a report by Capt. James, "In some of the larger double-storied infected houses . . . we had to trust entirely to phenyle . . . The result was quite satisfactory." In Poona it was the regular practice to allow the people to return to their houses 10 days after they had been vacated. In view of the uncertainty of the time required for the destruction of plague infection by the agency of desiccation, this interval would, in our opinion, be probably insufficient to secure the freeing of the houses from infection, particularly in the case of houses of two or more storeys, which could not be thoroughly opened up; in fact, if we understand Major Ross correctly, the opening and limewashing of houses was found to be ineffectual when disinfection by the application of perchloride of mercury was not resorted to. Capt. James' Report. 15, 10,927-37. 10,972.

For reasons stated separately,* the President does not agree with the opinion expressed in this paragraph—that "where skilled and efficient control was exercised" in the operations of disinfection, as in the instances mentioned above, "chemical disinfection exerted a preponderating influence in destroying the infection." The President does not agree with the opinion expressed in this paragraph.

672. With regard to the question of the opening up of infected houses, it will be manifest that the disinfecting agencies which this opening up process is calculated to bring into operation are (*a*) the disinfecting action of sunlight, and (*b*) the disinfecting action of desiccation. We shall do well to consider to what extent these disinfecting agencies are brought into play by the opening up operations which have been described. Efficacy of measures taken of open up infected houses to "light and air."

It will be plain that, where a house is completely unroofed, and the interior is everywhere exposed to the action of the sun and the wind, the agencies which have just been spoken of above will, in the fair weather, be brought actively into play. It is impossible to suppose that, under these combined influences, the germs of the disease could

* *See* Appendix V.

Para. 659 of this Report.

survive for any considerable period in a hot climate. In conformity with the experiments on desiccation and insolation which have been described above, we have reason to suppose that the germs of infection would probably all be destroyed within a comparatively few days. While this is true generally, it will be seen, by a reference to the section of our Report in which we have considered the question of the infectivity of plague houses, that there are instances on record where the infection of plague apparently persisted for three or four weeks in houses which had not only been otherwise disinfected, but had also been completely unroofed.

Section VI.(6)(*a*.) of Chap. III. of this Report.

Where, on the contrary, merely a small hole has been made in the roof, or where only some of the tiles have been removed, direct sunlight will come into operation upon only a small portion of the interior, and the remainder will be illuminated only by diffused light. In such a case no dependence could be placed upon the disinfecting action of direct sunlight, and, as we have said above, there is no evidence to show that diffused light has any influence upon the vitality of the plague bacillus. Thus the question of the utility of knocking holes in the roofs and walls resolves itself into a question as to the utility of these openings for bringing the agency of desiccation effectually into operation. We are of opinion that they are not necessary for this purpose. It is true that, to the extent to which the circulation of air is increased by the making of these openings, desiccation will be accelerated. But inasmuch as there are in practically every house openings which allow of a certain circulation of air, and inasmuch as the floor of practically every native house is raised above the level of the surrounding ground, we think (and this opinion would appear to be borne out by what we have seen of the condition of evacuated houses previous to their being opened up) that the disinfecting agency of desiccation would be effectually brought into operation if, without breaking any holes in the structure, steps were taken to remove from the houses, in addition to the human inhabitants and the cattle, any moist accumulations such, for instance, as accumulations of manure.

The President does not concur with the opinion expressed in this paragraph.

The President does not concur with the above opinion regarding the value of measures taken to open up houses, and he has expressed his opinion on the matter elsewhere.*

Period required for the disinfection of houses by the agency of desiccation.

673. With regard to the question of what period of time ought to be allowed for the completion of the spontaneous process of disinfection which goes on when material infected by the plague bacillus are exposed to the influence of desiccation, we may for information on this point have recourse either (*a*) to the experimental data which have been considered above, or (*b*) to such data of actual experience, as bear on the question of the length of time which is required for the dying out of the infection in houses where plague has occurred.

Para. 659 of this Report.

The experimental data are, as we have seen, to the effect that at temperatures of 20° C. or over (these are the temperatures at which all the Indian experiments have been made) the destruction of the plague bacillus by desiccation under the conditions obtaining in the experiments has always been effected within an interval of eight days. The experiments to which we refer, inasmuch as they were instituted in the damp climate of Bombay, and in laboratories rendered more or less steamy by the evaporation of water from the sterilisers and other sources, must be assumed to have been done under conditions which are more favourable to the survival of the plague bacillus than the conditions which obtain in evacuated houses in the drier climate of other parts of India. Thus, judging by the light afforded by the few experiments which have been done, we might, always assuming that the houses are thoroughly cleared out, expect to find all infection extinguished in dry hot weather, by desiccation, in less than eight days. But such an estimate as this is not one upon which much confidence can be placed, for while the period indicated would, no doubt, suffice for the destruction by desiccation of bacilli disposed on the surface of the walls or the woodwork, a much longer period would probably be required for the destruction of plague bacilli incorporated in a moisture-retaining material, such as the cow-dung of the floors. In view of the great importance of the matter, we think that it would be well to institute experiments in order to determine how long the plague bacillus survives when it is incorporated in moist sterilised cow-dung and earth and cow-dung in a cold or damp climate.

When we turn to consider the data of actual experience with a view to determining after what interval of time houses, which have merely been opened up and vacated may be re-occupied with safety, we find that the experience of the Bombay Presidency, as summed up by Sir Andrew Wingate is to the effect that it seems to be safe to allow infected houses to be re-occupied after they have been vacated for two or three months

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* *See* Appendix V.

The experience of Lieutenant Bruce was that re-occupation after the houses had been vacated for three months was practically safe. Mr. Giles, from his experience of plague operations in Sind, was of opinion that it was sufficient to keep the people out for two months. Mr. Moore stated that he did not permit evicted people to return to their houses "for a month after the last death or recovery from plague"; with respect to the time required for disinfection by the free access of air and light, he went on "the principle of at least one month; the more the better, but at least one month." 24,040. 11,380. 24,450.

III.(4.)(*j.*) *The Disinfection of Household and Personal Effects and of Grain.*

674. The disinfection of household effects, and the disinfection of personal effects, except in so far as this relates to the effects of travellers, is intimately bound up with the question of the disinfection of houses. We have already seen, in connexion with the description of house disinfection which was carried out in the Punjab, how the effects were there dealt with. The methods described do not differ in any important respects from the methods employed in many other places in India, except in the fact that perchloride of mercury was employed more frequently than phenyle. We may, however, note that in certain places it was the practice, in lieu of disinfection by chemical means, to boil clothing; in other places it was the practice to burn all clothing and to break up and destroy in the fire smaller articles of furniture and earthenware vessels and cooking utensils. Where this was done compensation was paid to the owners. In the case of personal effects and clothes carried out into health camps, it was the practice in many places, in lieu of chemical disinfection, to spread out the clothes in the direct rays of the sun. In other cases, again, the disinfection of clothing and personal effects in camp was committed to the agency of the ordinary "light and air." Measures of disinfection applied to household and personal effects and to grain.

It is to be noted that, in connexion with the general scheme which was adopted with a view to preventing the spread of plague by railways and over-sea traffic, extensive arrangements were made for the disinfection in steam sterilisers of the personal effects of travellers. Para. 706 of this Report.

675. Consideration will show that it is much easier effectively to disinfect clothes by chemical means than it is effectively to disinfect a cow-dung floor by chemical means. For in the case of clothes, even if they be dirty, we have to deal with a relatively much smaller quantity of substances which are capable of decomposing the disinfectant. Efficacy of above measures of disinfection.

The question of the efficacy of the various chemical disinfectants which were applied to clothes and other effects is not one which requires to be separately discussed. We have, therefore, here in connexion with chemical disinfection to consider only whether the chemical disinfectants were properly applied. There can be no doubt that the infective material was duly destroyed when the clothes containing it were steeped in the disinfecting solutions, but where, as in some cases, the process of steeping was not resorted to, and where, as we ourselves have seen, the clothes and other objects were merely sprinkled with disinfectant, plague bacilli may easily have escaped destruction. Chemical disinfection of clothes.

We have, further, in this connexion to consider the efficacy of the measures of disinfection which have been applied as substitutes for chemical disinfection.

We may begin by considering the question of the efficacy of the steam-sterilisation of the clothes of passengers and others as it has actually been carried out. We had opportunities for inspecting these disinfecting arrangements at various railway stations, and we are of opinion that owing to the absence of specially constructed steam-sterilisers, and owing to the insufficient training of the staff employed on this work, the disinfecting processes must in many cases be pronounced to have been very ineffective. This criticism, however, applies only to the disinfection of the personal effects of railway travellers. We were able to satisfy ourselves that the arrangements for disinfecting the personal effects of the crews and native passengers shipped at Bombay were quite satisfactory. Disinfection of clothes in steam sterilisers.

The next point which merits consideration relates to the question of the efficacy of exposing clothes to the action of direct sunlight. If we judged by the experimental data which we have considered above, it would appear that the plague bacillus when incorporated in woollen goods, or in thick or closely woven fabrics, is capable of surviving a very prolonged exposure to sunlight. The general impression of experienced officers would, however, appear to be that this measure of disinfection is, in practice, a very effective one. It is quite possible that this was the case, for in addition to the agency of direct sunlight the agency of desiccation was here brought into operation. Disinfection of clothes by direct sunlight.

Disinfection of clothes by sun and wind in camp.

The next point to consider is the question as to how far the destruction of the infection in clothing is brought about, apart from the direct laying out of these in the sunlight, by their incidental exposure in camp to the influence of sunlight and air. On general principles we might reasonably expect the destruction of the infective material to be brought about by the conditions of camp life which would bring the agency of desiccation into operation. These inferences would appear to be in harmony with the teaching of actual experience, for there is little doubt that where, as for instance in the case of Kankhal and Jawalapur, large quantities of infected personal effects were carried out into camp, these ceased after a time to be infective, although they had not intentionally been subjected to any process of disinfection.

9294–7.

Disinfection of grain by direct sunlight.

Lastly, with regard to the closely related question of the efficacy of the exposure to direct sunlight which was resorted to as a measure of disinfection in the case of the stores of grain contained in the village houses and in the larger grain stores. With regard to this we can base our conclusions only on laboratory experiments. These last would appear to indicate that inasmuch as the sun's rays do not penetrate much below the surface, the exposure of the grain in the open probably operated only in so far as it brought about the completer desiccation of the corn.

Attitude of the people to the measures of disinfection which were applied to their personal effects.

676. Little need be added here to what has been said on the subject of the attitude of the people to the measures of disinfection which were applied to houses. The only further point which it would seem important to note is that clothes and personal effects were often, with a view to avoiding the disinfection, secreted or carried away surreptitiously. As an illustration of the ruses which were resorted to with this object, reference may be made to the fact that in the course of disinfection *en bloc* of the town of Kankhal, as many as thirty-nine walled-up rooms were discovered in which clothes and goods had been secreted with a view to preventing their disinfection. In other places, as in the case referred to by Captain J. B. Smith, I.M.S., the disinfecting coolies were bribed to abstain from steeping the clothes in the disinfecting solution.

9256, 24,393.

8780, 9224.

21,174–5.

III.(4.)(*k.*) *The Disinfection of the Person.*

Disinfection of the person.

677. Disinfection of the person by washing in antiseptic solutions has been carried out in connexion with the plague operations in many places in India. In certain instances it was insisted upon in the case of all travellers from infected areas. At Poona railway station in particular very elaborate arrangements, which were inspected by ourselves, and which have been described in evidence given before us by Major Bannerman, were made for the disinfection of the person of all travellers. Further, disinfection of the person has frequently been carried out in the case of people who were removed into segregation or health camps, or who were returning to their houses from such camps. In some of these cases arrangements were made for completely douching each person with an antiseptic solution on his arrival in camp. Finally, disinfection of the person of convalescents leaving plague hospitals has in many cases been carried out.

4027.

The disinfectants which were employed for this purpose were most frequently either solutions of phenyle or solutions of perchloride of mercury.

Efficacy of disinfection of the person.

678. That the disinfection of the person is effective is indubitable. In proof of this it is only necessary to point to the disinfection of the hands by means of chemical disinfectants which is universal in bacteriological laboratories and in surgical practice. The question as to whether the measure is an expedient measure, as applied to travellers, is discussed below. In the case of persons taken out from plague-infected houses into health camps, and in the case of plague convalescents leaving hospital, we think the measure may have been a useful one.

Attitude of the people towards disinfection of the person.

679. There is no doubt that the disinfection of the person is one of the most unpopular of plague measures, and naturally so, for the measure not only involves a great deal of personal discomfort, but it may also, when the disinfecting solution is a fairly concentrated one, entail a certain amount of severe smarting or subsequent irritation.

4298, 21,510.

III.(4.)(*l.*) *Recommendations with regard to Disinfecting Operations.*

Recommendations with regard to disinfecting operations.

680. In turning now from our survey of the disinfecting operations which have been instituted in India to make certain recommendations as to the future conduct of such operations, we may consider first the question of the agency to be employed in the direction of the disinfecting operations; secondly, the question as to what measures of disinfection ought to be applied; and, thirdly, the question as to what objects ought to be brought within the scope of the disinfecting operations.

(I.) Recommendation regarding agency for

(I.) Firstly, then, as to the agency. We would recommend that the general scientific direction of the disinfecting operations, and the arrangement of technical details,

such as the making up of the disinfecting solutions and similar matters, should be committed to a member of the Sanitary Service, the establishment of which is recommended in a subsequent Chapter of this Report. We think, further, that the officer who is actually placed in charge of the disinfection operations should possess at any rate a certain elementary knowledge of the properties of the chemical disinfectants employed, and of the disinfecting agency of heat, desiccation, and sunlight in their application to plague. We think this is of cardinal importance, inasmuch as in our opinion many of the technical errors which have been noticed above as having been committed in the course of disinfecting operations are directly imputable to the fact that these operations were, in most cases, not placed under any scientific direction, but were placed under officers who, being quite new to the work, had to begin almost at the beginning and to find their way by a system of trial and error. The necessity for employing a strong and capable supervising staff wherever chemical disinfection is undertaken will be manifest, in view of the facts which have been placed before us in evidence with regard to the ignorance and negligence of the subordinate disinfection staffs.

carrying out disinfecting operations.

Chap. VII. of this Report.

(II.) In connexion with the disinfection operations, as distinguished from the agency, we may consider first the question of the disinfection of houses.

(II.) Recommendation of chemical disinfection of houses wherever possible.

The actual burning down of houses seems to us a practice which should be resorted to only in the case of temporary structures which are of little or no value, or in the case of houses which are unfit for human habitation.

Owing to the risk of infection from the blowing about of the floor dust, and owing to the fact that it is practically impossible to secure that the whole material shall be carried away, we think that the digging up of the floors with a view to kiln-burning them ought not to be resorted to.

Kiln-burning and unroofing not recommended.

Similarly the complete unroofing of houses, with a view to exposing the interior to the disinfecting agency of sunlight, appears to us to be a needlessly drastic measure for effecting an object which could be brought about by chemical disinfection more expeditiously, without any destruction of property, and with at least as much success. That neither chemical disinfection, nor any of the more drastic measures which involve the unroofing and the prolonged vacation of houses, can be absolutely relied on in every case entirely to extinguish all the infection contained in houses will be clear on a consideration of the instances cited in the section of our Report which deals with the infectivity of houses.

Section VI.(6.)(*a.*) of Chap. III. of this Report.

Chemical disinfection ought, we think, to be resorted to wherever possible. Of all measures of disinfection it is the only one which, when effectively carried out, immediately destroys the infective material present in a room, and renders the room at once practically safe for re-occupation. It must, however, carefully be borne in mind that a disinfected room, though it has been rendered fit for immediate re-occupation, is liable to be re-infected by the agency of rats or men.

The President is unable to agree with the opinion implied in this paragraph, that the opening up of houses is unnecessary because its objects could be brought about better by disinfection by chemical substances, and his views are expressed elsewhere.*

The President does not agree with this recommendation.

(III.) We would recommend that the disinfection operations should in towns be restricted to the actual houses in which either plague cases have occurred or dead rats have been found. Where dead rats have not been found, we are of opinion that, in the case of houses containing a number of unconnected families, only the room or rooms which have been occupied by plague patients should be disinfected; while, in the case of houses containing only one family, disinfection should be applied to the whole of the house.

(III.) Recommendations regarding parts of houses which should be chemically disinfected.

We would recommend that the disinfectant should be applied in ordinary cases only to the floors and to so much of the walls as is in reach of the disinfectors, and to all recesses and places which might have served for the reception of infected clothing. We do not think it is necessary to take any measures with regard to the upper parts of the walls, or to the ceilings, or to the roofs of houses. It is true that in exceptional instances infective material may have been deposited by rats in the roofs—especially in the mud roofs of village houses—but we would urge that the devitalisation of such infective material, deposited as it is in places which are not readily accessible to man, might quite safely be committed to the agency of desiccation, which will, as we have seen, bring about this devitalisation without any assistance on the part of the disinfector. The impossibility of compassing by artificial means the devitalisation of every plague bacillus, and the expediency of committing that devitalisation in all cases where it is safe to do so to the disinfecting

* *See* Appendix V.

agencies of nature, are considerations which ought, we think, to be constantly present in the mind of the disinfector.

(IV.) Use of perchloride of mercury provisionally recommended.

(IV.) The chemical disinfectant which seems to us, under present conditions of knowledge, to be most likely to render efficient service is perchloride of mercury, which may for the purpose of securing complete solution be made up either with hydrochloric acid or with common salt or any other soluble chloride. In view, on the one hand, of the fact that solutions of 1 in 5,000 and 1 in 10,000 are capable of devitalising the plague bacillus, and, on the other hand, of the fact that the plague bacilli, at any rate in a house where the floor has not been freshly re-covered with cowdung, may be expected to lie on the surface of the floor, we think that it will not be necessary to exact that the solution employed shall be sufficiently concentrated to exert a disinfecting action down into the substance of the cow-dung floor as far as the solution penetrates. We think, that in lieu of impregnating the whole floor with a solution of perchloride of mercury in a strength of 1 in 75—the minimum concentration which Dr. Marsh's experiments would seem to indicate as necessary for this purpose—a sufficient margin of safety would probably be provided for if a solution of not less than 1 in 1,000 were employed. The evidence of actual experience in Poona and elsewhere, where an acid solution of this strength was employed, would seem to confirm the inference that a solution of this strength is sufficient.

While provisionally recommending that no change should for the present be made either in the disinfectant, or in the strength of that disinfectant, at present widely adopted for use, we would again insist that the question as to which is the cheapest effective disinfectant and as to what is the minimum strength of that disinfectant which may effectively be employed still awaits resolution. As said before, we think this question could be resolved by experiments conducted on the lines of those instituted by ourselves.

Para. 654 of this Report.

(V.) Method of carrying out the process of chemical disinfection of houses.

(V.) In connexion with the procedure adopted for chemical disinfection, we think the following points ought to be carefully insisted upon :—First, where perchloride of mercury is employed as the disinfecting agent, we think that this disinfectant should be made up by some responsible authority in the form of a concentrated solution in acid. Clear instructions ought to be issued with the disinfectant as to the amount of water to be added. With a view to securing that the dilutions prescribed should be the dilutions in which the disinfectant shall actually be employed, we would insist on the necessity of providing that the capacity of the vessels used for measuring out the perchloride of mercury solution shall be accurately adjusted to that of the wooden buckets in which that solution is diluted for use. We think that the buckets employed should be of such capacity as to hold just the quantity of water sufficient to dilute the measure of concentrated solution to the desired degree.

With regard to the actual means of applying disinfectants, we would suggest that the methods of pumping and throwing them over the walls by means of dippers or other vessels, and of sprinkling them over the household effects with the hands, which methods are at present very widely resorted to, should be abandoned in favour of a system of pouring the disinfectants over the floors and vigorously brushing down the walls with the disinfecting solutions. We make this recommendation in view of the fact that—as shown by certain experiments which we instituted—the disinfectants rapidly run off from smooth, and especially from greasy, walls, without effecting their disinfection.

Before beginning the disinfection of a house or a room, we think, and this is in accordance with the practice which actually obtains, that all the household effects and such furniture as there is should be cleared out and should be placed in the open. When the room itself has been dealt with, the effects ought to be taken in hand. Suggestions as to the procedure to be adopted with regard to these are made below. As soon as their disinfection has been completed, the household effects ought to be replaced, and as soon as the rooms are dry, the inhabitants ought to be allowed to return. It seems to us that, in cases where the disinfection has been carried out with due care, there is nothing to be gained either from a subsequent process of white-washing or from keeping the houses vacated. The appropriate remedy to apply in cases where the disinfection process has been negligently carried out is not to resort to a less effective disinfectant, such as caustic lime, or to a slower disinfective agency such as the agency of desiccation, but to do the chemical disinfection over again.

(VI.) Where chemical disinfection of houses impossible, desiccation should be relied on, houses

(VI.) In cases where it is not possible to carry out chemical disinfection—and cases where it may not be possible to carry out the measure are sure to occur at any rate in villages—the disinfecting agency of desiccation should be relied upon. For this purpose the house ought to be thoroughly cleaned out. The removal from the house of cow-dung and other moist accumulations should be particularly insisted upon, since

these will, by giving up moisture to the air, tend to prevent the destruction of the plague bacillus by desiccation.

being vacated ordinarily without structural interference.

We are of opinion that the breaking of holes in the roof or walls and partial untiling will not accelerate the process of desiccation to such a degree as to counterbalance the damage done to the house. While this holds true generally, we think there may be occasions, as for instance when the rains are imminent, when every means of accelerating desiccation may be reasonably adopted.

The President does not concur with above opinion.

The President, for the reasons expressed elsewhere,* is unable to concur with the opinion that "the breaking of holes in the roof or walls and partial untiling will not accelerate the process of desiccation to such a degree as to counterbalance the damage done to the house."

In view of the general tenor of practical experience in the past, and in view of recent observations, which show that under favouring circumstances the plague bacillus, when incorporated in moisture-retaining and close-textured materials, may preserve its vitality for long periods, we think that it would not be an unwise measure to allow, as at present, an interval of at least two months of dry weather to elapse before authorising the re-occupation of the house; in damp and cold climates—and, needless to say, during the rains in India—we think that disinfection by desiccation cannot be relied on.

(VII.) Recommendation that personal and household effects should be broken up and damaged as little as possible in disinfection.

(VII.) We have above, in considering the attitude of the people towards the disinfection of their household effects, referred to their aversion to have their goods and clothes disinfected, and to the endeavours which they make to secrete or convey away clothing and other property. There is reason to believe that infection was in this manner often spread to those who became the depositaries of clothing removed from infected houses. This difficulty makes it a matter of particular importance to consult, so far as possible, the feelings of the people in the carrying out of the disinfection of their personal effects. The difficulty cannot, we think, be met by any system of giving compensation for goods destroyed. It can, however, we think, to a large extent, be met by proscribing all destructive methods of disinfection. In particular, we would recommend that nothing which could by any possibility have any value for its owner should be broken up or destroyed by fire.

(VIII.) Personal and household effects best disinfected by boiling water, or by steeping in a solution of chemical disinfectant.

(VIII.) We have already pointed out that mere momentary immersion in boiling water will, so far as the plague bacillus is concerned, effectually sterilise any infected object. It would, therefore, suffice to boil a cauldron of water, and, while keeping up a brisk fire underneath, to immerse in it the various infected objects. As an alternative to this momentary immersion in boiling water, the infected clothing might, as we saw was the practice in the Punjab and in Poona, be steeped in a solution of a disinfectant. The acid solution of perchloride of mercury, which has been recommended for the disinfection of houses, would, since it would injure the clothing, not be a suitable disinfectant to apply. It would therefore be necessary to make up as was, for instance, done in Poona, in addition to the solution of perchloride of mercury required for the disinfection of the houses, a neutral solution of the same disinfectant for the disinfection of the clothes. All complications such as this are, however, liable to lead to error and confusion unless, perhaps, when all the disinfecting operations are carried out under the immediate supervision of trustworthy Europeans.

(IX.) Recommendations regarding employment of steam sterilisers.

(IX.) We do not think that the procedure of conveying clothes to a distance, to a central steam-steriliser, to undergo sterilisation, is a procedure which is to be recommended. In general we think that steam sterilisers, in view of the fact that they can be relied upon to do their work only when they are placed in the hands of intelligent and perfectly trustworthy persons, are not suitable for general application in India. In most cases the required disinfection can be more simply and equally effectively accomplished by the means recommended above. On the other hand, we recommend that the steam-sterilisation of the effects of native crews and native ship-passengers which is now carried out should be continued.

(X.) Recommendations regarding disinfection by exposure to sunlight and air.

(X.) Where dealing with fabrics which might be injured by immersion in boiling water—we have in view here in particular such articles as valuable silks—these might perhaps be disinfected by exposing them to the direct rays of the sun. In accordance with what we have seen above, a very prolonged exposure—an exposure possibly of several days—would be required to effectually secure the devitalisation of the plague bacillus. During a prolonged exposure, even if the rays of the sun failed to penetrate the fabric, the infective material would inevitably be devitalised by desiccation.

* *See* Appendix V.

(XI.) Recommendations regarding disinfection of heaps of corn.

(XI.) Lastly, in connexion with the disinfection of village houses and shops, we have to consider the question as to what steps can be taken with respect to stocks of corn which may possibly have become infected by the agency of plague-stricken rats. We think that inasmuch as chemical disinfection is inapplicable, and inasmuch as disinfection by firing is out of the question, the destruction of the plague bacilli which may eventually have been conveyed into these stocks must of necessity be committed to the agency of desiccation. We think that the present method of spreading the corn out in a thin layer in the sun is a very appropriate measure, not so much because it brings into operation the direct devitalising effect of the sunlight, but because it conduces to a more complete desiccation.

(XII.) Recommendations regarding disinfection of the persons of travellers.

(XII.) We think very little indeed is to be gained by the disinfection of the persons of travellers; we therefore recommend that this should be abandoned. We base this recommendation on the consideration that it is unreasonable, at the price of the infliction of considerable personal hardship, to attempt the destruction of the infective material which may conceivably be carried about on the persons of travellers when it is quite impossible to provide against the much greater danger of plague being carried about by persons who are in the incubation period of plague, and when it is impracticable, in the case of railway passengers, as will be shown later on, to take any effectual measures to compass the destruction of the infective material which may be carried about in their personal baggage.

(XIII.) Recommendations regarding disinfection of drains, cess-pits, and latrines. Para. 661 of this Report.

(XIII.) We may next consider the question of the practical utility of applying measures of disinfection to drains, cesspits, and latrines. The question, in so far as it relates to drains and cess-pits is, for all practical purposes, resolved by the facts which have been referred to above in connexion with the disinfection of filth by the ordinary processes of nature. The case of latrines is somewhat different, inasmuch as the infective material deposited in these places might possibly spread from person to person before the disinfecting agency which is brought into play by the growth of the saprophytic organisms has had time to effect the devitalisation of the plague bacillus. It will therefore be advisable to continue the disinfection of latrines.

(XIV.) Recommendations regarding disinfection of merchandise. Para. 228 of this Report.

(XIV.) Lastly, we may briefly advert to the question of the disinfection of merchandise.

In this connexion reference may be made to the fact that there was, as we have elsewhere explained, no scientific justification for the statement, circulated in the Press in India, to the effect that the plague bacillus had been discovered, in the bacteriological laboratory at Hyderabad, in cotton which was packed for transport. Further, in connexion with the question of the necessity of applying measures of artificial disinfection to the gunny bags, which form an important article of exportation from India, we found in an experiment already referred to, that the plague bacillus, at the temperatures and under the atmospheric conditions which prevail in Bombay, would appear to be destroyed on gunny bags by desiccation before the expiration of eight days. Para. 659 of this Report.

We have already said what is essential in connexion with the matter of disinfection of grain in the course of our discussion of the possibility of taking effective measures for the sterilisation of the stores of grain which may be contained in infected houses. What applies to grain applies equally to all other articles of merchandise. It would not, in the case of any variety of merchandise, be practicable to employ any method of disinfection which would bring about the immediate devitalisation of any plague bacilli which might chance to have been introduced into the materials in question. In the case of merchandise the agency of desiccation is the only agency which can be brought into operation. The only possible precaution which could be taken against accidental conveyance of infection by merchandise, would be to insist that merchandise should be received for transport only when it is thoroughly dry. But it will be manifest that security for this is already in large measure afforded by commercial and economic reasons, inasmuch as merchandise would, if shipped damp, inevitably undergo deterioration in transit.

In view of the above considerations, we are of opinion that it is neither advisable nor necessary to take any steps towards the disinfection of ordinary merchandise. Rags, as we have seen, cannot be classed with ordinary merchandise, inasmuch as they are very likely to carry infection. Their disinfection in bulk is almost impossible, Paras. 227 and 695 of this Report.

inasmuch as they are for the most part exported in the form of compressed bales. We have elsewhere expressed our opinion with regard to the exportation of rags.

IV.—Measures to prevent the Spread of Plague.

IV.(1.) *Measures to prevent the Spread of Plague by Sea to Foreign Countries or other Places in India.*

Communication between ports in India and foreign countries.

681. The independent States on the North-Western frontier of India take their own measures to prevent plague from extending from India into the countries that lie north of it, and it appears very unlikely that, even if plague became virulent in Northern India, people would attempt to fly from it in this direction. The only manner in which plague appears likely to be taken from India to any European or distant country is by the sea traffic from Indian ports. These, both in British India and the Native States, are numerous. From the great majority of them vessels run only to other places in India. The ports on the Continent of India, from which passengers and cargo are mainly taken to European ports or to Burma, Ceylon, the Straits Settlements, China, Japan, the Persian Gulf, Africa and Arabia, are Bombay, Calcutta, Madras, and Karachi. There are, however, other places which communicate with Europe, China, and intermediate ports, and there are a number of ports from which small sailing vessels proceed to Burma, the Arabian Coast, the Persian Gulf or Madagascar. Some of them are situated in British India and some in the Native States.

Medical inspection of outward-bound vessels.

682. The medical inspection of outward-bound vessels at the port of Bombay was systematically undertaken in the beginning of February 1897. Rules were then brought into effect to prevent any vessel from leaving the harbour until every one on board had been medically examined, and any person suffering from plague had been removed. A week later similar rules were brought into operation at Karachi; and, in order that there might be no doubt as to the freedom from infection of vessels sailing from Calcutta and Madras, the Governments of Bengal and Madras, with the approval of the Government of India, brought rules of the same purport into force at those ports. Similar regulations were subsequently introduced at many of the smaller ports which communicate with places out of India.

Principles of the Venice Convention.

683. At the Conference held at Venice in March 1897 general principles for regulating the departure of vessels from plague-infected ports were formulated. The following regulations were prescribed by the Convention: firstly, that the medical examination of every person proceeding by the ship should be made on shore immediately before embarkation; secondly, that every contaminated or suspected article should be disinfected on shore in a particular manner; and thirdly, that no person showing symptoms of plague should be allowed to embark.

Examination of passengers on shore at Bombay and Calcutta.

684. The rules in force at Indian ports at the time when the Convention was passed provided that any person showing symptoms of plague should be prevented from departing, but it had been the practice, except in the case of vessels taking Muhammadan pilgrims to the Hedjaz, to conduct the medical examination of crews and passengers on board the ship just before its departure, and considerable difficulty was found in arranging that it should take place in all cases on shore. Consequently, the examination of passengers both at Bombay and Calcutta continued, for a considerable time after the Venice Convention was passed, to be conducted on board ship after embarkation. This procedure was abandoned at Bombay on September 1st, 1898, since which date all crews and passengers on ships leaving Bombay for foreign ports, and many of those on ships leaving for other ports in India, have been examined on shore. At Calcutta the rule requiring crews and passengers to be examined on shore was brought into operation in June 1898 in the case of all vessels leaving for ports out of India, or for the port of Rangoon, or any ports in the Madras Presidency. When Calcutta was declared free from plague in October 1898 the medical examination of crews and passengers was continued, but the regulations then brought into force permitted it to be either on board or on shore as might be most convenient, and, in the case of passengers, certificates from officers of the Indian Medical Service given not more than twelve hours before embarkation were accepted in lieu of the examination by the Port Health Officer. Since Calcutta has again been declared infected, the practice of requiring crews and passengers to be examined on shore has been resumed.

Examination of outward-bound passengers at Karachi

685. During the earlier period of the outbreak at Karachi, crews and passengers were medically examined, and suspicious persons detained. At a later period, all passengers desirous of leaving by sea were required to obtain a pass, which was only granted when the possibility of a passenger being infected was remote, or when there were urgent business reasons to justify it. In the absence of special circumstances passengers were detained under observation for ten days. They were then sent to the steamers under police escort with passes given to them in the camp. This system, which appears to us to have been unnecessarily rigorous, was subsequently abandoned in favour of medical examination of the same nature as that conducted at Bombay. The rules now in force are that the crews and passengers must be medically examined on shore in the case of vessels sailing for ports out of India. The examination may, in the case of vessels from one Indian port to another, be conducted in the docks or stream, provided that they are not merely touching at Karachi on a voyage to a place out of India.

Karachi is a port from which emigrants are in the habit of leaving for Africa and other places. Emigrants leaving under a contract are subject to the provisions of the Indian Emigration Act, and at present they are prohibited from leaving Karachi. An exception has been made in the case of emigrants going to Uganda, but only on condition that they must have been under observation for ten days previously in a camp where their clothes are disinfected, and be taken straight on board ship from the camp by a police escort. About 7,000 emigrants have been sent under these precautions while plague has prevailed in Karachi.

11,991.

Examination of outward-bound passengers at Madras.

686. Madras, the fourth port from which there is considerable intercourse with Europe, has not yet been infected with plague. But rules for medical inspection at this port and Tuticorin were made in February 1897, and in September 1897 the Madras Government prescribed an examination on shore at these two ports and at seventeen others in the case of vessels sailing to places out of India. An exception to the rule requiring that the examination must be on shore was made in the case of vessels anchored in the Madras roadsteads, and at out-ports the inspection of the officers and crew of a vessel may be conducted on board. Owing to the propinquity of Ceylon, a special regulation was made to prevent any person from embarking on board a vessel bound for Ceylon, who had within the preceding ten days resided in a part of India believed by the Health Officer to be infected with plague. In the case of deck passengers or persons belonging to the labouring classes, a further requirement was made that they should not leave Tuticorin for Ceylon without satisfying the Medical Officer of the Ceylon Government, stationed at Tuticorin, that they had not within the previous ten days resided in an infected part. They were also required to obtain from him a certificate that they had not plague. Vessels belonging to the Ceylon Government leaving with emigrants for Ceylon were exempted from these regulations.

Disinfection of clothing.

740–4.
809–10.
25,486–7.

687. We found on our arrival at Bombay, at the end of November 1898, that the Port Health Officer was not in the habit of treating articles of clothing from an infected area as contaminated or suspicious, unless there was any special reason for so regarding them. It was his practice to decide in each individual case whether an article was to be regarded as contaminated or suspicious or not. It appeared to us that, in view of the possible conveyance of the infection of plague by means of contaminated clothing, it would be better to make it the rule, instead of the exception, to disinfect the clothing of the crews and of the lower classes of native passengers. We accordingly made a recommendation that the effects of crews and native passengers should be disinfected, and the rules now require the disinfection on shore of the clothing and bedding of native crews, third class or deck passengers, and native servants in the case of vessels leaving Bombay, Karachi, or Calcutta for ports out of India.

Work done by the Port Health Officer at Bombay.

25,479–81, App. X. and LXXV.

688. Bombay is the only one of these ports that has been continuously infected since the plague reached India. The evidence of Major Crimmin, V.C., I.M.S., shows the enormous amount of work that has been done by the establishments maintained first under Major MacCartie, I.M.S., C.I.E., and then under himself, as Port Health Officer, in the examination of the crews and passengers of outward-bound vessels. We think that the statistics regarding this work are of sufficient interest to be quoted. Between January 1st, 1897, and March 15th, 1899, 110,851 vessels leaving Bombay on both long and short voyages were inspected, and their crews numbering 980,448, and passengers numbering 962,573, were medically examined. The number of persons examined, whose destination was Aden, the Red Sea, or Europe, was 133,147. Twenty-five thousand and eleven persons were prevented from sailing from Bombay, of

whom 840 desired to go to Aden, the Red Sea, or European ports, 892 to other ports out of India, and 23,279 to ports in India. Two hundred and forty-three of the persons detained are known to have developed plague; but since, at one time, the persons prevented from sailing were not placed under observation, the number that actually developed plague may have been larger.

689. Major Crimmin has described in his evidence the manner in which the medical examination is conducted. No vessel can leave the harbour without a port clearance, or obtain a port clearance without a bill of health, which is not given until the crew and passengers have been medically examined. The passengers for European and other distant ports are examined at the Ballard pier. The passengers for ports in India are collected for examination in a shed divided into two parts, the larger for the men and the smaller for the women. All women and children are examined by lady doctors. Method of examination at Bombay 717–741.

The method of examination is best described in Major Crimmin's own words. He said:—

"I will divide vessels into two classes, first, those bound for Indian ports, and, second, those bound for ports out of India. The crews and passengers bound for Indian ports may be examined on board according to the present regulations, but the majority of them are examined on shore. For instance, in the case of the Shepherd Company, who run 35 coasting passenger ships in the week, all the crew and passengers are examined in sheds on shore before they go on board. If a ship is bound for a port out of India, the crew are brought on shore from such vessels for examination in accordance with the Venice Convention. The passengers for such vessels are also examined on shore before they go on board. When there are large numbers of native passengers they are fallen into a line in batches of 100 or 200, according to the size of the examination shed. They open their body clothing, and the Medical Officer feels each man's chest with both hands, which enables him to detect any increase of temperature. The superficial glands in the neck, arm-pits, and groins are then examined. The tongue and eyes are looked at. If he is found to be suffering from a rise of temperature or marked enlargement or tenderness of any of the glands, he is sent to a place set apart for such cases, to undergo a more rigorous examination. His temperature is taken with a Hicks $1\frac{1}{2}$ minute thermometer in the arm-pit, and if he is found suffering from a marked rise of temperature, or enlarged tender glands, he is rejected. The crew are examined in precisely the same way as passengers. * * * If ships are bound for ports out of India the crew are invariably examined on shore to meet the letter of the Venice Convention. In the case of vessels, which are in dock, and which do not want to go to the inconvenience of bringing their crew on shore after they go into the stream, such crews are examined on shore the day before the vessel sails. We again examine such crews on the day of sailing and as near as possible to the time of departure, after which no loading of cargo or communication with the shore is allowed. We carry out this second examination for two reasons. First, for our own reputation, and to do the best we possibly can to keep plague off ships; and, secondly, to meet the wording of the Official Bill of Health, which states that the vessel, with so many crew, and so many passengers, is at the time of sailing free from plague and certain other infectious diseases. * * * At the examinations for plague, if we find any person suffering from a marked rise of temperature they are detained until such time as the temperature goes down to normal, or until we are certain that the fever from which they are suffering is not that due to plague. We go so far as to take out natives with chronic enlargement of the glands of the groin, more especially on ships bound for foreign ports. We do this in the interests of the ship, and very often at the request of the captain. Because such a person with enlarged glands was unfortunate enough to be suffering from fever on his arrival at a foreign port from Bombay, it is possible that the Health Officer would class him as a plague case, or return him as a suspicious case." 721. 723. 725. 726.

In the case of vessels such as those of the Messageries Maritimes Company, which make Bombay a port of call, and have no communication with the shore during their stay in the harbour, the examination is confined to the passengers who embark at Bombay.

When the medical examination of passengers has been completed the passengers are, if the ship is in dock, passed down a gangway on to the ship. The vessel is then moved off the dock wall until such time as the Dockmaster can take her out of dock. If the ship is in the stream the passengers are conveyed direct to her, and she goes to sea at once. In either case, till the vessel has actually left, it remains in charge of a 732.

Customs Officer and one of the European Sanitary Police, and the passengers and crew are prevented from having any communication whatever with the shore.

738. In addition to the medical examination of the crews and passengers steps are taken for the cleansing of the ship. "The vessel itself," said Major Crimmin, "is overhauled and a high sanitary condition insisted on. This includes the painting or lime-washing of crew's quarters; crew's boxes are treated in the same manner. Bilges of native craft are thoroughly washed and cleaned, and the holds of all vessels in ballast are swept clean, as well as disinfected when required, as in the case of coal-ships whose tween decks are often used as latrines by the coolies discharging the cargo. Ships' commanders are in addition advised as to the advantage of the periodical inspection of their crew, and of the frequent exposure of the crew's kit and clothes to the sun and wind. Printed instructions are sent to the shipping agents for issue to commanders of vessels without medical officers, describing the symptoms and the treatment to be adopted should plague break out on their ships."

742. Should a person removed from a ship as suspicious develop plague after the ship has departed, intimation of the fact is sent by telegram to the first port at which it is to touch.

Method of disinfection of baggage.

690. As we have already mentioned, it was not the practice to disinfect the personal baggage of crews and passengers at the time when we first examined Major Crimmin at the beginning of December 1898. The kits of all crews were exposed to the sun, and the baggage of any passenger proceeding to Europe who was known to have had plague was disinfected. There had been only a few instances in which the disinfection of a passenger's luggage had had to be carried out under this rule. When we examined Major Crimmin, the second time, in March 1899, he stated that the practice of disinfecting the clothes, bedding, and boxes of all native crews proceeding to ports out of India, either directly or by way of other Indian ports, had been adopted.

25,487. Major Crimmin thus described his procedure in the matter of disinfection: "The new native crews are collected in the disinfection shed by some official from the shipping office, who brings a copy of the Articles. Native crews from ships in the harbour are brought by one of the officers of the ship. Roll-call from the Articles takes place so as to make certain that we have the total number of native crew, as well as their property. The shed is divided into two portions. In the larger portion the crews' kit is sorted. The crew dress themselves in the cleanest clothing they have, and the remainder of their clothing and bedding is put through an Equifex stove and subjected to saturated steam at a pressure of 10 lbs. to the square inch. This pressure gives a temperature of 239° F., which is said to give absolute disinfection in the case of the most resistant organisms yet isolated. Clothes and bedding are kept in the stove for 15 minutes, and during that time they are subjected to three changes of steam. They are then dried for about five minutes with hot air. Boots and shoes and rain coats are disinfected with a spray of perchloride of mercury of the strength of 1 in 1000, or, if they are greasy, they are sprayed with live steam through a hose from the boiler. The crews' boxes are brushed inside and out, or washed if they are very dirty. They are then rolled about in a large tub containing a solution of perchloride of mercury, 1 in 1000. The kit and boxes are then passed into a second part of the shed, where they are packed, and are then conveyed to the ship under a Police escort. One of the European Sanitary Police goes on board to see that no bedding is stowed away in the forecastle, forepeak, or other parts of the ship. Sometimes they say they have no bedding at all, but we make them find it somehow or other. The floor of the shed is then swept, and the sweepings destroyed in the furnace of the stove."

Since Major Crimmin was last examined the rules as to disinfection of the kits and bedding of native crews have been extended to deck passengers and native servants proceeding to ports out of India. There are very few passengers of this class who embark for Europe, and the linen of all passengers on board a ship which touches at a Continental port is required to be disinfected during the voyage.

Opinion as to manner in which examination and disinfection are carried out.

691. We have had the opportunity of seeing the medical examination of crews and passengers and the disinfection of the kits and bedding of the crews performed by Major Crimmin and his staff. The work is of a very exacting and disagreeable nature, and we found that it was carried out in a most thorough and conscientious manner and so as to completely satisfy the letter and spirit of the Venice Convention. We were not able to make as close an inspection of the arrangements at Calcutta (which had not been declared an infected port at the time of our visit there) or Karachi, but we have no reason to believe that the medical inspection is not carried out at these ports with the same care and exactness as at Bombay, and in accordance with the rules which now strictly conform to the Venice Convention.

692. It is necessary, in connexion with the measures taken in India to protect other countries, to refer to the regulations enforced at Aden, the port of call between India on the one side, and the Red Sea and Europe on the other. In October 1896 quarantine was imposed at Aden against Bombay. The regulations prescribed 15 days' quarantine in the case of vessels on which plague occurred, and eight days' quarantine in the case of vessels on which no case had occurred, but which did not carry a qualified Medical Officer. They allowed immediate free communication to a vessel on which no case of plague had occurred and which carried a duly qualified Medical Officer. The Port Health Officer was required to make a medical inspection of a vessel before granting free pratique. In December 1896 the quarantine regulations at Aden were applied to Karachi as well as Bombay. Regulations enforced at Aden.

Under the terms of the Venice Convention of 1897, a healthy ship is defined to be any ship, even though coming from an infected port, which has had no death or case of plague on board, either before departure, during the voyage, or on arrival. A healthy vessel is entitled to free pratique, but the authorities at the port of arrival can impose restrictions if they think fit, the maximum of such restrictions being those laid down by the Venice Convention. Consequently, the rules now in force at Aden admit immediate free pratique to all healthy vessels, even though they may have less than a ten days' passage from an infected port. They provide that the crew and passengers on healthy vessels, which have left an infected port less than ten days before arriving at Aden, shall be medically inspected; that the soiled linen and personal effects of the crew and passengers which appear contaminated shall be disinfected; that a watch shall be kept over the health of the crew and passengers for ten days from the date of the arrival of the ship; and that the crew shall not be permitted to land except on duty. Vessels using Aden merely as a port of call have the option of remaining in quarantine instead of seeking free pratique, and this course is found to be the more convenient to them since the medical examination, which is the condition of their receiving free pratique, results in delay. Any cases of plague discovered on board ship are removed in the ship's own boats to Alia Island. The number of cases landed at Aden since plague began in Bombay has been very few, and in no case has any person at Aden become infected owing to the landing of a plague-stricken person.

693. The arrangements made for the control of vessels leaving ports of India or ports in Europe seem to us to have been, on the whole, most successful. The evidence given to us has shown that since the outbreak began in India in 1896, very few vessels have carried away the infection of plague, and that the cases of plague on board ship discovered at Suez, or west of it, have been very few. The only cases which have come to our notice officially are those of the two Goanese stewards who contracted plague on arrival in London, apparently after using clothes which had been closed up in a box since the time of departure from Bombay, the case on the troopship *Dilwara*, in March 1897 (which occurred before the troops, women, and children going by troopships were subject to examination by the Port Health Officer), that of the fireman on the Peninsular and Oriental Company's s.s. *Carthage* in July 1898, and that on board the British India Steam Navigation Company's *Golconda*. In the latter case (which occurred in December 1898), the symptoms shown by a passenger after leaving Marseilles raised a suspicion that he was suffering from an attack of pestis minor. It was not known that he had before leaving Calcutta been in communication with any one suffering from plague. Beyond the fact that the man had enlarged glands, there was nothing to show that he had plague. It has come unofficially to our notice that two cases of plague were discovered at Suez on the steamer *Dictator*, which, having left Bombay for Liverpool *viâ* Karachi and Perim on June 14th, arrived at Suez on July 9th, 1899. Smallness of number of plague cases that have passed through testifies to efficiency of control over vessels leaving India. Local Government Board Report for 1896-7, pp. 129-133. 755-6. 799-808. 27,415.

Apart from the cases discovered at Suez, or west of that port, a certain number of cases have been detected on ships while in Eastern waters on their way from India to Europe or the Red Sea. There were two cases on the *Pekin* a pilgrim vessel, 752-4. in 1896, one on the *Carthage* before reaching Aden on the same voyage on which 757. the case already referred to occurred, one on the Peninsular and Oriental Company's s.s. *Shannon* in 1898 while taking the mails from Bombay to and from Aden, 759-60. one on the Peninsular and Oriental Company's s.s. *Clyde* in April 1898 while 761. employed on the same duty, a case on the Peninsular and Oriental Company's s.s. *Bengal* 763. in August 1898,—which, though detected on board, was contracted four days after the ship returned to Bombay, and, therefore, may be left out of account,—one case each on 765 the Peninsular and Oriental Company's s.s. *Ballarat* and *Bengal* in August and October 1898 respectively between Bombay and Colombo, and five cases on the *Patna* (British Indian Steam Navigation Company) in March 1898 while on a 763; 12,243-4. voyage from Bombay to the Persian Gulf. During the recent recrudescence of plague in Bombay the only cases reported up to the time of our leaving India were the two

cases alleged to have been detected at Suez on the Peninsular and Oriental Company's s.s. *Caledonia* in November 1898, a case on board the British Indian Steam Navigation Company's *Buldana* between Bombay and Rangoon, and the seven cases on the British Indian Steam Navigation Company's *Bhundara* during her voyage from Karachi taking emigrants to Uganda in December 1898. We think it is clear from the reports of the Medical Officer at Suez that one of the cases on the *Caledonia* was not plague, and it seems to us very doubtful whether the second one was. The occurrence of the cases on board the *Buldana* does not seem to us to have been due to any defect in the medical examination of the crew at Bombay, and no member of the crew was attacked. But as the clothing of the crew was not disinfected at Bombay, and as some of the emigrants may possibly have become infected by escaping for a time from the Kiamari detention camp at Karachi, the infection of the ship may have been due to either of these causes. A case of which we have indirect information is that of the *Annie Maude*. In July 1897 this ship was placed in quarantine by the United States Quarantine authorities, on the ground that one of the crew died shortly after the vessel left Calcutta, and two more of the crew suffered with buboes from which they recovered.

25,489-90. 25,491. 12,250 25,495-7. 26,934.

In view of the enormous amount of outward-bound traffic from India, the discovery of this small number of cases of plague on vessels bound for European and other ports testifies, we consider, to the thorough and efficient manner in which the medical examination of crews and passengers at the more important Indian ports has been conducted.

As it had been alleged in the public Press that the plague reached Tamatave, in Madagascar, from Bombay, we examined Major Crimmin as to the possibility of the infection having been taken in this manner. No vessel had left Bombay for Madagascar since August 1897, and it is clear that the origin of the infection at Tamatave in the autumn of 1898 must be sought for elsewhere. It is possible, though there is no direct evidence on this point, that it may have been taken by one of the sailing vessels that ply between Cutch Mandvi and Madagascar.

25,494. 13,347 ; 13,513-5.

Little danger from rats on ships, and disinfection of holds impracticable.

694. On the *Shannon* and *Patna* dead rats were found when cases of plague occurred on those ships. It is quite possible that the rats on these ships were infected by the same cause as the human beings. There is, therefore, no proof that on board ship men have become infected from rats. On the other hand, no mortality of rats was observed on 13 ships on which plague occurred among human beings. As far as we know great mortality among rats has not been observed on ships that have left Bombay, and it has certainly not been observed on ships in Bombay harbour. Although, theoretically, it is possible that plague-infected rats might carry the disease from one country to another, there is absolutely no evidence that infection has ever been carried in this manner. We think that the suggested fumigation of the holds of ships so as to destroy the rats is both unnecessary and impracticable. The fumigation would have to be done before the cargo is put in the hold, since it would be impossible to fumigate the hold afterwards.

17,677.

Practical extent of danger of spread of plague by sea, and measures necessary to guard against it. Paras. 227-9, 262, and 659 of this Report. Centralblatt für Bacteriologie, 20th December 1899.

695. The question of the infectivity of articles of merchandise has been discussed elsewhere, and it will suffice here to note that articles of merchandise are not, in our opinion, often likely to be the means by which plague is conveyed from one port to another. The reports in the public press as to the manner in which Oporto has become infected with plague, are, we are aware, to the effect that the infection was brought in the cargo of a ship from Bombay. We find, however, that Dr. Kossel, one of the German delegates who proceeded to Oporto to study the outbreak there, reported that there was no direct communication between Oporto and any plague-infected places, and that the *City of Cork*, the ship which was at first suspected of having introduced plague, traded regularly between Oporto and London only. The manner in which plague was introduced into Oporto has remained undetermined. In comparison with the danger of the importation of plague infection from India into Europe by means of germs of the disease carried either by human beings, or by clothing, baggage, and rags, the danger of the importation by means of germs carried by articles of ordinary merchandise seems to us to be insignificant. We have already expressed our opinion that the period of incubation is, as a rule, much shorter than ten days. Although it is a defect of any system of medical inspection that it can result in the detention of a member of a crew or of a passenger only if he has either got plague upon him or has been attacked by the febrile symptoms which herald its approach, it is extremely improbable that a person who is infected when he embarks on a ship would not develop plague very soon after the commencement of a voyage. The occurrence of any of the symptoms of plague on board a ship should lead to the immediate discovery of the case, and if every suspicious case is at once isolated and treated as a case of plague, there is little or no

Para. 197 of this Report.

danger of its spreading. Experience in hospitals confirms that gained on board ship, viz., that the spread of plague can be immediately checked if ordinary precautions for proper ventilation, disinfection, and sanitation are taken at once. The danger then that plague will be taken into Europe by persons who have developed plague during their voyage from India is, we think, extremely small—so small, indeed, that it may safely be ignored. There is, however, it appears to us, a possibility of infection being carried to Europe by means of contaminated clothing; and for this reason we attach the greatest importance to the system for disinfecting the clothing and bedding of the crews and lower classes of passengers that has lately been introduced. So long as plague remains widely diffused over India, we think that it is desirable to enforce at all ports in India from which ships sail to ports out of India—and in the category of ports out of India we would include the ports of Burma—the same rules as to medical examination and disinfection that are at present in force in Calcutta, Bombay, and Karachi. No measure short of this can give security that a passenger may not embark with infected clothing at an uninfected port. It is also, we think, most essential that vessels of any kind leaving ports such as Cutch Mandvi for Madagascar, Zanzibar, Arabia, and the Persian Gulf, and other places out of India, should do so only after full precautions have been taken to prevent them from carrying the infection.

With regard to merchandise, we have elsewhere seen that rags constitute the only kind of merchandise which is likely to spread plague. We have also seen that it is impossible effectually to disinfect rags when compressed in bales for exportation. We, therefore, think it is a wise precaution to prohibit the exportation of rags from an infected district.

Prohibition of export of rags a wise precaution.

696. In addition to the ordinary traffic of passengers and merchandise from India, there is the traffic of Muhammadan pilgrims to the sacred places in the Hedjaz. From inquiries made in Bombay and Karachi, we ascertained that pilgrims for the Hedjaz from India go entirely by sea, and that no large caravans start, as has been sometimes stated, from the Persian Gulf. The traffic by sea has for a long time been regulated by special rules designed to prevent the introduction of epidemic disease from the East into Europe. Those now in force are based on the Convention of Paris of 1894, the conditions of which are embodied in Chapter I. of the Venice Convention, and in the Indian Pilgrims Ships Act (No. XIV. of 1895), and the rules issued by the authorities in India under the powers conferred by that Act. The Act requires that the pilgrims shall invariably, whatever may be the state of health at the port from which the vessel sails, be medically inspected on shore before they are allowed to embark, and that all articles suspected of being contaminated by persons suffering from cholera or any dangerously infectious or contagious disease shall be disinfected. Under ordinary circumstances nearly all the pilgrims from India, including those who have come from Central Asia, take their departure from the port of Bombay, and in accordance with these rules, two vessels left Bombay for Jedda in December 1896 and January 1897 respectively. Sixty pilgrims also sailed from Calcutta, which was then a healthy port, in the same month. It has already been mentioned that two cases of plague occurred between Bombay and Aden among the passengers by one of these steamers (the *Pekin*), but the vessel was not further infected. Steps were taken to prohibit, with effect, from February 1st, 1897, the departure of pilgrim ships from either Bombay or Karachi, and every effort made to dissuade Muhammadan pilgrims from proceeding to the Hedjaz. Subsequently, on February 16th, 1897, a regulation was issued under the Epidemic Diseases Act, prohibiting all persons residing in the Bombay Presidency or Sind (the only two parts of India which were then infected with plague), or who had entered the Bombay Presidency or Sind with the object of going on the pilgrimage, from embarking at any port for the Hedjaz, and special arrangements were devised for subjecting to the strictest observation any residents of uninfected areas who might persist in their wish to go on the pilgrimage. But before these arrangements could be brought into operation, the apprehensions felt by all the European Governments, including Turkey, that the plague might be communicated to Europe led Her Majesty's Government to decide that the pilgrimage must be completely suspended for the time being. The Indian Government accordingly directed, on February 20th, that the pilgrimage should be suspended, and no more pilgrims were permitted to leave India for the pilgrimage of 1897. The port of Bombay has been continuously infected since that date, and no pilgrims have been allowed to leave it for the Hedjaz. But the Government of India, with the sanction of the Secretary of State for India, determined, when the time for the pilgrimage of 1898 was approaching, that it was desirable to withdraw the absolute prohibition against the departure of pilgrims from India, and to permit those resident in uninfected areas to depart under the most stringent precautions from ports which remained free from plague. The ports from which it was determined to permit pilgrims to depart were Karachi and Chittagong. By a notification under the

Arrangements connected with pilgrim traffic to Hedjaz. 12,265–8.

752–4.

Epidemic Diseases Act issued on 20th November 1897, the Government of India prohibited the embarkation of any pilgrim to Mecca from the Bombay Presidency and Southern India. All other persons resident in other parts of India were required to go into a camp of observation for ten days before they were permitted to embark, under the usual precautions as to medical inspection and disinfection, at either Karachi or Chittagong. While withdrawing the prohibition against the departure of healthy pilgrims from uninfected ports, the Indian Government made every endeavour, through the influence of their officers and of the religious heads of the Muhammadan community, to dissuade intending pilgrims from going to the Hedjaz. As the result of these measures, the only vessels which took pilgrims from India to the Hedjaz in 1898, were one which sailed from Karachi with 200 passengers, and another from Chittagong with 708 passengers. The port of Karachi was re-infected in the spring of 1898. It has remained so ever since, and no more pilgrims have sailed from it. In November 1898, the Government of India determined to continue, with regard to the pilgrimage in 1899, the prohibition against the departure of pilgrims resident in infected areas, and to permit residents of uninfected areas to sail only from the port of Chittagong, and only after stringent precautions as to observation, medical inspection, and the disinfection on shore of every article taken on board by the pilgrims. Two vessels have sailed from Chittagong in accordance with these regulations, one in January 1899 with about 740 pilgrims, and one in February 1899 with about 530 pilgrims.

In addition to the stringent precautions taken before the pilgrims for the Hedjaz sail from India, the Turkish authorities impose quarantine for 15 days on the pilgrim vessels after their arrival from India at the quarantine station at Camaran.

Under existing arrangements little fear of importation of plague to Europe from India *viâ* Hedjaz.

697. The Venice Convention does not prohibit the departure of pilgrims even from a plague-infected port, so that the precautions which have been taken in India to prevent pilgrims who are resident in infected areas from going on the pilgrimage, and to permit pilgrim vessels to sail only from uninfected ports and under the strictest precautions, go beyond what is required by the Convention. They appear to us, however, to be well devised. So long as they are maintained it need not, we think, be apprehended that the plague will be imported into Europe by means of the pilgrimage of Muhammadans from India to the Hedjaz. No case of plague has been taken from India to Mecca since the outbreak began in India. Mecca is, in our opinion, far more likely to be infected by plague from the indigenous foci in its neighbourhood at Assyr, than by the infection being brought from India; and indeed it is probably from Assyr that the plague, which has appeared in Mecca in the last three years, has been derived.

IV.(2.) *Measures taken at the Place of Arrival of ships from Infected Ports.*

Quarantine.

698. The British and Indian Governments are opposed to the imposition of quarantine, but, owing to the anticipation that foreign countries might impose quarantine against non-infected Indian ports unless quarantine against vessels arriving from Bombay was imposed at them, quarantine against Bombay was, in October 1896, imposed at Calcutta, Madras, Karachi, and Rangoon. The regulations enforced at Karachi were similar to those already referred to as having been imposed at Aden. The rules at the other three ports were less strict, in that if satisfied that there had not been a case of plague on board the vessel, the Port Health Officer was authorised to give free pratique, whether the vessel carried a qualified Medical Officer or not.

The quarantine regulations at the ports of Calcutta, Rangoon, and Madras were extended to Karachi in December 1896. The regulations against Bombay, at the latter port, were at the same time relaxed so as to allow free pratique to be granted at once to a vessel on board of which no case of plague had occurred, provided that it carried a Medical Officer. Eight days' quarantine was imposed in the case of a vessel with no Medical Officer, even though no case of plague had taken place on it. When a case had occurred on any vessel the sick and their attendants were landed and segregated, while the other passengers were allowed to land under precautions such as the Port Health Officer considered necessary, and the vessel was disinfected.

Subsequently, as the epidemic of plague increased in the Bombay Presidency, the quarantine regulations were extended to a number of the minor ports on the South, West, and East Coasts of India, with a view to their own protection.

Measures other than quarantine.

699. The changes made by the Venice Convention of 1897 in the treatment of ships arriving from ports infected with plague rendered it unnecessary to maintain quarantine, and regulations, based upon the principles laid down in the Convention, have since been prescribed for observance in the case of vessels arriving at other ports in India from infected places on the seaboard.

Although Bombay has remained continuously infected since plague was discovered there in 1896, there have been times when the infection at other ports was more serious than the infection at Bombay. It was feared that people returning to Bombay from infected places at the time when plague was declining in Bombay might cause a re-infection of the city. The inspection of incoming vessels from infected ports was, 768.
therefore, undertaken at Bombay in April 1897. Up to 30th April 1898, all people coming from ports between Karachi, on the north, and Bhatkal, on the south, of Bombay, were examined. Since that date the inspection has been limited to those coming from infected ports.

The regulations of April 1897 required all native vessels arriving at Bombay to bring up at one of three anchorages. Of passengers from ports other than Cutch Mandvi, and those situated in the district of Kolaba (which two places were then very seriously infected), only those whose temperature was above normal were detained, and they were discharged from 24 to 48 hours afterwards, if the temperature fell to normal in the meanwhile. Passengers from the Kolaba district were detained five days, and those from Cutch Mandvi eight days. At a later stage the system of surveillance by medical inspection of new-comers in their houses was tried, but had to be abandoned for two reasons. Great difficulty was experienced in finding the house to which the newcomer had gone, and the inspecting officer found it impossible to satisfy himself that the people he examined in the house were the people who had passed out of the camp under surveillance. It was, therefore, found necessary to 772.
bring people under surveillance to the observation camp for examination, and an attempt was made to prevent the danger of personation by photographing groups of persons admitted to surveillance.

Regulations at present in force at Bombay.

700. The rules in force in Bombay, though they apply to a port not itself free from plague, may be taken to illustrate the practice in force at ports which are not infected. A ship from a non-infected port, or a port which is supposed to be non-infected, is on arrival at Bombay boarded by the pilot, who inquires whether there are 781-93.
any cases of certain specified diseases on board, or whether there have been such cases during the voyage. It is only when there have been cases of such diseases, or suspicious deaths during the voyage, that the doctor's flag is hoisted, and then the ship has to remain in quarantine until she has been visited by the Health Officer and has received free pratique. Every vessel, including native craft, which arrives from an infected port, or has been in communication otherwise than orally or by signal with a vessel from an infected port, or has plague on board, or has had a case of plague on board within 12 days before arrival, is required before entering the harbour by day to hoist a yellow flag, or to show the quarantine signal by night, until permitted by the Health Officer to remove it. Vessels are required to anchor at specified places, and are allowed to take the pilot, police, or customs officer on board, but are not permitted to hold any communication, except orally, with the shore or with any other vessel or boat until pratique has been granted. The Health Officer then inspects the ship, and classes it as "infected," "suspected," "ordinary healthy," or "healthy," as the case may be. The definitions of "*infected*" and "*suspected*" vessels are those contained in the Venice Convention. An "*ordinary healthy*" vessel is a vessel, not being an "infected" or "suspected" vessel, which has within the period of 10 days preceding her arrival left an infected port or communicated otherwise than by signal or orally with a vessel which has left an infected port within that period. A "*healthy*" vessel is a vessel which is not "infected" or "suspected," and has left an infected port more than 10 days before arrival, has not had a death on board from unknown or suspicious causes, and has not within the period of 10 days communicated, except orally or by signal, with a vessel from an infected port. In the case of "healthy" vessels it is in the discretion of the Health Officer to medically examine the passengers and crew, and to disinfect any of the soiled linen and personal effects of the passengers and crew which he may consider infected. In the case of "ordinary healthy" or "suspected" vessels, these two measures must be undertaken, and the passengers and members of the crew who are, in the opinion of the Health Officer, likely to spread infection are sent to the appointed place and kept under observation. The remainder of the crew stays on board under the observation of the Health Officer, and the rest of the passengers have to comply with all directions as to medical inspection or otherwise that may be given to them in written orders. In the case of "suspected" ships, all parts of the vessel which have been occupied or frequented by plague patients must be disinfected, and all other parts of it may be disinfected at the discretion of the Health Officer. In the case of an "infected" vessel, in addition to these precautions, the sick are disembarked

and sent to the camp or hospital. The persons under observation are detained for such period, not exceeding 10 days, as may be determined on, having regard to the sanitary condition of the vessel and the manner in which it has been classed. In the case of an "ordinary healthy" vessel the detention terminates on the eleventh day from the departure of the vessel from an infected port, or the date of her communicating with a vessel from an infected port, whichever is more recent. In the event of a case of plague occurring among the persons under observation, the patient is sent to hospital and a fresh period of 10 days' detention of the other persons in the camp commences from the time when the patient suffering from plague has been removed. The Medical Officer in charge of the camp may permit a person under observation to proceed to his home on condition of his complying with directions as to medical inspection and other matters given to him in the form of a written order. There are difficulties in India which do not occur in Europe in the matter of allowing persons to proceed, under surveillance, to their homes. In the large ports many people are unable to give an accurate, or even approximately accurate, address, and there is always a danger of personation, of the duty of reporting being ignored, and of a person changing his residence. Many of the difficulties in the matter of surveillance can be, and have been, removed by the use of photography, and by getting the leaders of the different communities to become responsible that the members of their body allowed to go to their homes shall appear at the prescribed time for medical inspection; but there will always, it is feared, be a considerable residuum whose addresses cannot be properly determined, and who cannot be relied on to fulfil their obligations as to coming to be medically inspected, and it will be necessary to keep these under close observation for the prescribed period.

Statistics of examination of ships arriving at Bombay and Karachi.

25,479.

771–2.

12,145–6.

12,160.

12,151.

12,162.

701. The examination of passengers on inward-bound vessels from infected ports has imposed very onerous duties on the Health Officers at Bombay and Karachi. From January 1st, 1897, to March 15th, 1899, 51,213 vessels, with crews numbering 404,934, and passengers numbering 520,699, were examined at Bombay. Of those examined up to the beginning of December 1898, 873,197 in number, 43,477 were segregated for periods varying from two to ten days. Forty-eight of those segregated developed plague. At Karachi between August 1st, 1897, and December 1st, 1898, crews and passengers from infected ports, numbering 69,366, were examined at Karachi. Only one person was found to be suffering from plague at the time of inspection. The number of passengers detained in the camp was 35,360. The large number of passengers detained is accounted for by the fact that between the termination of the epidemic of 1897 and the beginning of that of 1898, all passengers were detained for 10 days. Among the passengers detained, five cases of plague occurred, and one man belonging to a crew who was sent ashore as a suspicious case, developed plague.

Conclusions regarding examination of vessels arriving from infected ports.

702. The number of cases of plague which developed at Bombay and Karachi among people arriving from infected ports was very small. The results do not, we think, compensate for the labour and expense involved in examining passengers who arrive at an infected port from another infected port. If, however, it be considered necessary to maintain this form of examination, we think that in the case of a "healthy" or "suspected" ship, coming from one infected country into another, free pratique should, provided no case of sickness has occurred on board, be given at once without any further formalities. It also appears to us undesirable to make a distinction between an "*ordinary healthy*" and a "*healthy*" ship, which is not contemplated by the Venice Convention. Lastly, in the case of an "infected" ship coming into any port, all that appears necessary is to provide that, if possible, the sick shall be landed and isolated, and the personal luggage of crews and passengers and the ship disinfected. The other passengers should be allowed to land, and should be kept under observation at their own homes for a period not exceeding 10 days after leaving the ship. Only those who cannot be kept under observation in this manner need be placed under detention in a camp.

IV.(3.) *Measures to prevent the Spread of Plague from one part of India to another by Railway.*

Summary of measures adopted on railways in India.

703. In connexion with the possibility that plague might be spread by travellers on the railways, it had to be considered whether any restriction should be placed on railway travelling from an infected area. The idea of prohibiting such travelling

altogether could not be entertained: but it was decided that an attempt might, and ought to, be made—

First, to prevent the departure from the plague-stricken locality of anyone showing symptoms of plague:

Secondly, to detect and remove from the train anyone who, though he might not have shown signs of plague at the time of departure, had developed them in the course of his journey:

Thirdly, to keep under observation, for some days after their arrival at their destinations, passengers from infected areas who might be in the incubation stage of plague, though they had not shown any symptoms of the disease up to the time of their arrival:

Fourthly, to prevent passengers from carrying the infection in their dirty clothing or bedding.

With this object arrangements were made, early in October 1896, by the civil and railway authorities working in co-operation under sections 47 and 71 of the Indian Railways Act (IX. of 1890), for the medical examination of outward-bound passengers at each of the railway stations in Bombay City. The following precautions were also adopted at about the same time throughout India generally:—

(1.) A system of medical examinations was instituted at intervals along the railway routes which led outwards from the infected area:

(2.) A system of surveillance was adopted by which arrivals from infected areas were kept under observation for the period of incubation either at their homes, or in special camps, according as they could or could not be depended upon to report the occurrence of plague, or to give a correct name and address at the place of arrival:

(3.) Arrangements were made to disinfect so much of the clothing and effects of travellers from plague-stricken places as seemed likely to be contaminated with plague.

These are the precautions which have from an early stage in the outbreak been regularly adopted throughout India generally. There are, however, two others which were subsequently to some extent resorted to, but only at certain places, and for a limited time. One was—

(4.) A system of detention camps established with a view of detaining, for the period of incubation, travellers from infected areas, whether suspicious or not.

The other was—

(5.) The stoppage of all booking of passengers from an infected place generally, or the stoppage of booking from an infected place to a particular place which it was feared would become infected.

We may deal with each of these five measures separately.

(1.) The medical examination of travellers at intervals along the railway routes which lead outwards from the foci of infection.

704. The medical examination at the different stations in Bombay City was calculated to prevent such persons as were actually suffering from plague from leaving the infected area. But it was necessary also to endeavour to prevent persons who might before the beginning of their journey have contracted plague, but who had not up to that time shown symptoms of it, from carrying it about the country after it had been developed. With this object arrangements were made at the beginning of October 1896, for the medical examination of passengers from Bombay at a number of selected railway stations in different Provinces. And as plague increased in Bombay these arrangements were made more stringent. The law, however, as it then stood, did not give sufficient power to the authorities; and it was not till the Epidemic Diseases Act was passed, in the beginning of February 1897, that the arrangements were extended and thoroughly systematized, and the whole scheme of medical examination of passengers by railway was brought into proper order.

"The Plague in India, 1896–97."

At that time the chief centres of infection were the cities of Bombay and Karachi. Nine lines of railway lead from the direction of the former, and two from the direction of the latter place, in most cases joining with the lines from the other Provinces of India. The arrangements made on the passing of the Epidemic Diseases Act are very clearly and fully explained in Chapter X. of Mr. Nathan's book on the plague in India, and it is not necessary to reproduce the details of them here. They involved the medical examination of passengers at certain principal railway stations selected as suitable for the supervision of through traffic, and also that of passengers alighting at important centres of population. All persons found at the inspection to be suspicious were detained. In addition to the arrangements at

the main places for medical inspection, the local governments instituted examinations by native medical assistants at subsidiary stations on the main and branch lines. The inspection stations became very numerous, and the staff employed on them very large. Special arrangements were made that the examination of female passengers should be conducted, as far as possible, by female medical practitioners and nurses.

An idea of the extent to which endeavours were made to medically examine through travellers may be gathered by referring to the arrangements made for the examination of passengers on the journey from Bombay to Calcutta. The first principal examination was at Kalyan Junction, 34 miles from Bombay; the second at Manmar Junction, near the borders of the Hyderabad State, 128 miles from Kalyan; the third at Bhusawal near the borders of the Bombay Presidency the Central Provinces and the Hyderabad Assigned Districts, 114 miles from Manmar; the fourth at Itarsi Junction in the Central Provinces, 188 miles from Bhusawal; the fifth at Manikpur Junction in the North-Western Provinces, 316 miles from Itarsi; and the last at Chausa, 199 miles from Manikpur, and just within the borders of the Province of Bengal. A passenger from Bombay to Calcutta would thus have to be examined six times during his journey, and like precautions were taken in the case of passengers by all other routes.

(2.) The system placing arrivals from infected areas under surveillance in their homes for the period of incubation. Para. 707 of this Report. Section II.(1.)(*b.*) of this Chapter.

705. The importance of keeping a watch for the maximum period of incubation on the health of persons arriving from the infected area was recognised in all Provinces at an early stage of the outbreak in Bombay; and by March 1897 rules with this object had been very generally issued. Subsequently, after the plan of detention in quarantine camps (to be presently alluded to) had been tried and abandoned, the system of surveillance was still more extensively employed. We refer to it elsewhere, and need not therefore say more on the subject here.

(3.) The disinfection of suspicious clothing and baggage of travellers. Para. 674 of this Report.

706. The measures which have been described above were, in many cases, re-inforced by measures which were taken to disinfect the luggage and persons of travellers. Arrangements were made for the disinfection of the clothing and effects of each person removed from the train and detained, and, at stations on the frontiers of the Bombay Presidency, the disinfection of the clothing and effects of all third-class passengers from infected areas was arranged for. It was at one time the practice to disinfect the persons of all those detained in the camps, but this has been generally abandoned. These measures have been dealt with under the heading of disinfection; they need not, therefore, be specially described here.

We pass now to the exceptional and temporary measures—detention and stoppage of booking—that were resorted to in particular parts of the country and at particular times.

(4.) Detention camps on or near railway routes for detention of passengers from infected areas. 1285. 14,106–57. 14,165–93.

707. When the second epidemic of plague was raging in Bombay City, and when the plague was widespread in the other parts of the Presidency, the Bombay Government considered it impossible to arrange for medical examination so thorough as to check the spread of plague. In order more effectually to check the travelling of persons with plague in them, they instituted a system of detention camps, at which passengers were detained for a fixed period, and all third-class passengers were disinfected. These detention camps were stopped just before the commencement of the rainy season of 1898, and in October 1898 it was determined not to re-open them for the fair season. A description of the detention camp at Anand, on the Bombay, Baroda, and Central India Railway, is given by the officers who were employed on it. It was opened on the 24th November 1897, and closed as a detention camp in May 1898. The period of detention varied from time to time, the maximum being ten days. Every one from an infected locality was detained with the following exceptions:—

(*a.*) Europeans;
(*b.*) Government servants, or servants of a Native State travelling on duty;
(*c.*) Railway servants travelling on duty;
(*d.*) Persons holding certificates showing that they came direct from an authorised camp of detention where they had been under observation for at least a period of 10 days;
(*e.*) Persons holding such passes as were from time to time notified by the Plague Commissioner as conferring exemption from detention;
(*f.*) Persons giving satisfactory proof that they had come from uninfected localities of which they were residents.

Practically the whole train was emptied out, and all the passengers detained, and examined medically twice a day during the period of detention, the persons who came within the exceptions mentioned being very few. This system was a form of

quarantine modified only to exempt certain classes as such, and not entirely because they were less likely to carry plague than some of the passengers who were detained.

Besides the Government detention camps at certain points on the railway for all down passengers found in the trains there, whether through passengers or alighting passengers, the Municipalities at most of the chief towns in the Bombay Presidency lying on or near the railway maintained detention camps of their own, in which persons from infected areas on alighting from trains were detained for ten days before being allowed to go to their houses.

When in October 1898 the Bombay Government determined not to re-open their own detention camps, they decided that those also maintained by the Municipalities should be abandoned. In place of detention camps they determined to substitute throughout the Presidency an arrangement for the medical examination of each traveller, and the disinfection of suspicious articles. 1285.

Detention camps similar to those in the Bombay Presidency were adopted also at a few places outside its limits. In Baluchistan a camp of this nature was started at Sibi, in March 1897, and still existed when we left India. The only routes by which Baluchistan can be entered are the railway and the Karachi-Somniani Kafila road which passes through miles of desert. The system adopted on the railway in the first instance had been medical inspection at frequent intervals commencing at Sibi, and the removal of all passengers exhibiting suspicious symptoms. No booking of passengers was allowed between Jacobabad and Sibi. From March 1897, all, or nearly all, native and other passengers have been treated as plague suspects, and detained for ten days for observation and disinfection. 8085-92.

At Khana, also, in Bengal, on the East Indian Railway, it was at one time the practice to treat all arrivals from infected areas as suspicious, and to detain them for the period necessary to complete ten days from the time of their departure from the infected area. This arrangement was subsequently changed to one under which only persons found to have suspicious symptoms were removed from the train and detained. 6711-2.

At the medical examination stations in Rajputana all third-class passengers from infected areas were detained, at first for ten days, and afterwards for five, before being permitted to go to their homes. Europeans and those who could be watched were not stopped. 9729-34

At Tungabhadra, the station where passengers from the Bombay Presidency and the Hyderabad State to the Madras Presidency were examined, pilgrims from unknown areas, who could not be depended upon to state where they had come from, or where they were going to, and were very dirty, were detained for ten days. 4490-515.

(5.) Stoppage of the booking of passengers.

708. The stoppage of the booking of passengers was not often adopted, but it was adopted at several places. In some instances it was intended to defeat the attempts of travellers to evade detention or medical examination by alighting at railway stations just short of the places where these measures were carried out. In others it was used to prevent people escaping surveillance at their destination by alighting at a railway station just short of the end of their journey, and walking the remainder of it. Lastly the measure was resorted to in order to prevent the rush of infected persons into places where large concourses of people usually assemble in connexion with religious fairs or similar gatherings. In many cases, under circumstances such as these, the booking of travellers to the place of pilgrimage has been altogether prohibited as a temporary expedient. Lastly, in some cases the booking of passengers from an infected place has been prohibited, not absolutely, but in the case of persons not provided with passes from the local plague authorities.

Results of the system of medical examination. 1291.

709. At Kalyan, on the Great Indian Peninsula Railway, 3,320 persons with suspicious symptoms were removed from the trains in eighteen months; 234 had plague, of whom 50 were palpably suffering from it when found in the train. At Palghar, on the Bombay, Baroda, and Central India Railway, near Bombay, 192 persons were taken out of the train in four months, of whom 45 turned out to have plague. At Anand, 1,811 persons were removed from the trains in a period of a year. Fifteen of them had plague unmistakably developed on them when found in the train. At Bhusawal, on the Great Indian Peninsular Railway, 28 passengers were removed from the train in two years. Two of these were evidently suffering from plague at the time of their removal. At Hotgi, 579 persons were taken out on suspicion in two and half months, of whom two turned out to have plague. At Belgaum, 103 persons were removed in six months, of whom two had plague. At Hubli, out of 30 persons removed in ten months, 15 had plague, and 12 of them were unmistakably suffering from it when found in the train. At Guntakal,

5130–1, 5140. where up to the beginning of September 69,050 persons were examined, and thereafter from 10,000 to 15,000 a week, the number of people detained as suspicious was between February and December, 1898, 111, of whom 4 had plague.

4508–16. At Tungabhadra, on the borders of the Madras Presidency and the Hyderabad State, 31,272 persons were examined between April and December 1898; no case of plague was detected. The number examined in the North Western Provinces is not

8753. known, but only 2 undoubted cases of plague were detected amongst them. Lieutenant

4256–65. Cornwall examined many thousands of passengers at Malir and Karachi, but did not discover anybody actually suffering from plague while travelling. This, however, was while plague was in Kotri, not when it was in Karachi. At this time, moreover, there were only a few passengers travelling. When Karachi was attacked a second

12,425–45. time, 13 passengers suffering from plague were discovered at the medical examination

8092. at Malir. The examination in Baluchistan led to the discovery of 2 cases of plague. Between the commencement of the outbreak in Bombay and the end of

5952–6. December 1898, several hundred suspicious cases were removed from the train in the Central Provinces and segregated. About 20 of them were found to have plague.

13,886 Seventy-seven cases of plague were detected at the railway station at Ahmedabad between October 5th, 1896, and February 16th, 1897. At Poona in the course of

12,850–3. a year from October 1896, 61 persons were found to be suspicious on arrival at the railway station, and 40 of them turned out to have plague. It is stated that in

12,863. the earlier part of the period the examination was not sufficiently strict. At Surat

15,544–7. many thousands were examined before the town was attacked; only 18 arrivals by railway were detained as suspicious, of whom ten turned out to have plague. During

15,871. the second epidemic only one case was detected. In March 1897, when it became necessary to protect Bombay against being reinfected by the return of people to their

Chap. IV. of General Gatacre's Report. homes from places in infected districts, the arrangements made for the examination of the people returning to Bombay by railway resulted in the detection of 315 cases of plague in a little over three months. Considerably over 1,000 persons were detained as suspicious during this period.

Results of the system of detention camps. 710. At the twelve detention camps maintained in the Bombay Presidency proper, 131,074 passengers were detained in a period of about five months. Two hundred and one persons developed plague. In the Anand camp 78, out of 58,000

26,537; Bombay Government Resolution 4,989 P. dated November 8, 1899. persons detained, had plague; at Kalyan, 63 out of 17,633; at Poona, 23 out of 13,670; at Manmar, 10 out of 12,968; at Bandra 14 out of 5,790; at Igatpuri 6 out of 2,824; at Dharwar, 4 out of 3,185; at Bhusawal, 1 out of 3,494; and at Kudchi, 1 out of 3,445; at Londa, Gadag, and Nasik, where 4,534, 2,509, and 1,276, respectively, were detained, no case of plague occurred.

4510, 4516. At Tungabadhra. 1,472 pilgrims were detained for ten days; no cases of plague occurred amongst them.

4264. At the Malir camp during the first outbreak in Karachi, out of many thousands

12,429–34. detained about a dozen developed plague. In the second outbreak during the months of April, May, and June, 1898, 7,925 persons were detained. This number includes passengers by rail from Karachi, and also others who tried to pass beyond the camp by road. Among the former, 95, and among the latter, 9, developed plague. Of

12,065, 12,072. 12,097, 12,100. 266 persons detained at Rohri, only one developed plague, and possibly he contracted it from a contact camp adjoining the detention camp.

8092. In the detention camp at Sibi, in Baluchistan, one person developed plague.

6712–19. Out of 1,826,656 persons examined at the camps in Lower Bengal up to December 1898, 41,854 were detained. There were 6 cases of suspected plague, but for want of bacteriological evidence it was not possible to decide whether any of them were true cases of plague. How many of the 6 cases were detected on medical examination, and how many developed the illness only after detention, is not clear.

9729. Amongst the thousands of people examined, and the large number detained, at the different railway stations in Rajputana, 38 cases of plague were discovered; but whether in the examination, or only in the detention camp, is uncertain.

It is not quite clear how the passengers from infected areas were treated at the medical examination stations within the Hyderabad State. It was stated by

5151. Lieutenant-Colonel Lawrie, I.M.S., "that no persons who are suspected of being likely to bring the germ of plague into Hyderabad, either in their bodies or their

25,912. clothing, have been permitted to enter the city, either by railway or road." About 12,000 persons were examined every month at the three observation camps on the railway at Wadi, Gulbarga, and Raichur, and during 18 months the number detained

5789. was 47,628. It is evident from the fact that so many were detained, while only

163 of the number had fever in the camp, that the camp was more like a detention camp than a place for medical examination only. Seventeen cases of plague occurred amongst the people detained.

Comparison of the results of detention camps and of simple medical examination.

711. Sir Andrew Wingate in his evidence before us contrasted the results when detention camps were in use, with those obtained since passengers have only been subjected to medical examination. He said:—"Last year at 12 stations, during a period of 5·14 months, 131,074 people were detained for periods varying from five to ten days. Out of those 131,000 people, 201 cases of plague were detected, and nearly 131,000 people were disinfected. . . . At 14 stations this year during a period of 3·8 months—a shorter period—116,000 people have been disinfected, against 131,000 in the previous year; and 160,000 people have been subjected to the clinical thermometer test. Out of these, nearly 12,000 people have been detained on account of a rise of temperature, and 266 cases of plague have been detected. It is too soon to say what the results in the districts may be of the re-infection from Bombay. You cannot tell that until the next two months have expired. Up to date, at all events, the proportion of the exodus of the two years is not very dissimilar, and 266 cases of plague have been caught, against 201 in the previous year; and only some 12,000 people have been detained, instead of 131,000. From what I know, I think that the medical examination has been still more thorough than the detention, and has probably led to fewer cases; I say fewer cases, because if you get a case into a camp, there have been a good many instances of cases after 9 days, 11 days, and 12 days, which seems to me to point to infection within the camp from one to another. On the other hand, by the use of the clinical thermometer, and the detention of all people who show the least temperature, 266 cases of plague have been detected, and, apparently, now the cases all develop in the first few days. These statistics seem to me indicate that there is a very small amount of risk from the exodus of the population of an infected town under precautions for carefully medically examining the people." Statistics are now available, to carry the comparison between the results of the system of detention and of that of medical examination, still further. It appears that in 5 to 7 months 261,495 travellers by railway in the Bombay Presidency were tested by the clinical thermometers, that 19,302 were detained, and that 413 of them developed plague. The number disinfected during this period was 204,312.

26,537.

Bombay Government's Resolution No. 4989 P. of 8th November 1899.

Whether the system has been the detention until the period of incubation has expired of all travellers from infected areas, or the less rigorous one of removing from the train those persons who appear, after medical examination, to be suspicious, the actual number of cases of plague which have been discovered among railway travellers while actually travelling, or under detention before they have reached their houses, has not been very numerous. So far as the results show, the less rigorous system has been quite as effective as the more severe one, and it must have caused far less inconvenience.

Indirect results of medical inspection in preventing the travelling of people likely to spread plague.

712. It is, however, claimed that the indirect results also of the medical examination of travellers and the disinfection of their baggage are very beneficial. Captain Jennings, I.M.S., who was in charge of these operations in the Bombay Presidency, urged that the measures connected with the medical examination of passengers by railway "are most useful owing to their deterrent effect against actual plague cases or people who are probably infected travelling." The figures, he said, "go to prove that very few actual plague cases are found travelling, and the probability is that all those actually suffering from plague are detected. It is only those in whose system the plague is incubating that can pass."

1297.

In referring to the fact that the medical examination of passengers by railway in the North-Western Provinces and Oudh resulted in the detection of only two undoubted cases of plague (one at Agra and one at Bareilly), Lieutenant-Colonel Thomson, I.M.S., observed, "This may seem a small result, but the preventive action of trains inspection is very great."

8753.

Sir Andrew Wingate, speaking generally of the value of railway examination, observed, "It is not only valuable because of the cases which it detects, but it is probably still more valuable in preventing sick people from leaving the infected areas. If you look at the weekly list for the Thana district, where the restrictions this year were specially relaxed compared with the year before, you will see that there were 756 imported cases in the first year and 163 imported cases last year, when there were detention camps; whereas this year the number has increased to 212 up to date. These figures are not confined to cases imported from Bombay, but indicate

25,538.

generally the variations in the number of cases imported from Bombay. In the Thana district last year there were less than 1,200 deaths, whereas this year there have already been over 3,000. The medical examination was purposely taken off between Bombay and one part of the Thana district in order to give a certain amount of play to the Bombay population, and not to tie them up too tight. The medical examination acts in those two ways,—that you catch the case that does go out, and you prevent from going out a great number of cases which would otherwise go out." Again, he observed, "If the examination were taken off, I think that the country would be flooded with plague cases immediately."

26,541.

To what extent this view is correct it is impossible to say, since, as Lieutenant-Colonel Thomson, I.M.S., has well said, "it can never be known how many infected persons or persons actually sickening with plague would have swarmed all over India, but for the knowledge that they would be subjected to inspection on arrival in fresh localities."

8753.

Question whether expense, inconvenience, and trouble involved in medical examination are compensated for by results attained.

713. On the other hand, even admitting that the results of medical examination at railway stations may have been beneficial in this respect, it appears at least to be open to doubt whether they sufficiently compensate for the expense, inconvenience, and trouble involved in maintaining the system. The results of any such system can at best amount to only a partial attainment of what is aimed at by it. The number of actual plague cases detected was very small as compared with the enormous number of passengers—many millions—who have gone through these examination stations. The only cases likely to travel to any extent are the non-infectious, mild, bubonic cases. Medical examination, even the most severe and painstaking, cannot detect a case of plague in the incubation period, and, as the ordinary period of incubation may extend to four days or more, the chances are that a person in the incubation period will escape detection if the period of his railway journey does not extend beyond one day. The number of imported cases which are known to have been brought into big towns by railway was large compared with the number detected before the journey's end was reached, and numbers of which no record exists must have been conveyed in the same way into rural areas. Any system of detention on railways must also tend to dislocate trade, to interfere with people in pursuit of their occupations, and affect the profits of the railway companies. The medical examination of passengers at particular stations produces in all probability similar results, though in a smaller degree. We have no estimate of the expenditure that has been incurred on railway examinations, but it has certainly been very large. And, in addition to the actual expenditure of money, these arrangements have involved the continuous occupation on them of many Medical Officers of all grades. The supply of qualified medical men familiar with the languages and people of India is limited, and the employment of these officers on railway examinations has naturally affected the supply of them available for employment in the suppression of plague at infected places.

Medical examination on railways can fully attain its object only if accompanied by detention for maximum incubation period and efficient disinfection; the fulfilment of these conditions cannot be secured.

714. It would be necessary, if the object is to prevent any case of plague being imported into a place by railway, to combine medical examination with the detention of every traveller from an infected area for the maximum period of incubation, and with an accurate disinfection of all personal effects. This would, in our opinion, be quite impossible. Even assuming that it would be feasible to make arrangements for the actual detention of all persons travelling, it would, we are convinced, be absolutely impossible to efficiently disinfect their personal effects. Disinfection, if imperfect, is useless; to carry it out properly is a most laborious task, requiring constant attention to details, and the thorough overhauling of each individual's baggage. We are not, we feel assured, making too high an estimate of the establishment that would be required for disinfection, when we calculate that to disinfect the luggage of 1,000 persons in a day of twelve hours, two doctors, six trained supervisors of disinfection, and a gang of about sixty men, would be required, with two large disinfecting stoves and the other necessary paraphernalia. When, as in some stations in India, many thousands of travellers have to be dealt with daily, the amount of work entailed in such a proceeding seems infinitely greater than any government could possibly undertake. It must also be borne in mind that any system of detention opens a gate to every form of fraud, corruption, and evasion. To anyone familiar with Oriental ways it would be surprising if a system for the detention of passengers by railway could be worked so as to be free from the selling of certificates, the buying of tickets at intermediate stations, the transfer of tickets from one person to another, and the bribery of subordinate officials. And, as a matter of fact, the arrangements at different places in India have been, in consequence of such practices, frequently evaded.

715. Our conclusions in connexion with the question of placing restrictions on the movements of travellers by railway in order to prevent the spread of plague, are :— Conclusions regarding measures for restricting movements of travellers by railway.

(1) That, while the spread of plague has taken place in a very large number of cases along the railway line, the number of cases of plague detected among travellers, under the different systems adopted, has not been large ;

(2) That any system of land quarantine imposed with the object of detaining all passengers from infected areas, either at a point where the infected and uninfected areas join, or at different places within the uninfected area, results in great hardship to the travelling public, and has not been found in practice to prevent the spread of plague ;

(3) That in the special instance of Sibi, the objections to such a system are, owing to the fact that the traffic inwards is concentrated and easily controlled, not so great as elsewhere, while it is very important to take every precaution to prevent plague passing over the North-West Frontier of India ;

(4) That any system of medical examination on railways which falls short of keeping travellers under detention for the ordinary period of incubation must be defective as a measure for stopping all infected persons ;

(5) That, considering the small results attained by medical examination on railways, it is not necessary or expedient to maintain so many places for examination as hitherto : two of us, Dr. Wright and Dr. Ruffer, are of opinion that it would be well altogether to abandon medical examination on railways, except on the confines of important unaffected areas which communicate with an infected area by only one line of railway ;

(6) That it appears to be unnecessary to medically examine people travelling between places within an infected area ;

(7) That it may be a wise precaution, in cases where large concourses of people from infected areas are expected to go to a place for a religious festival or similar gathering, to prohibit their travelling ;

(8) That the disinfection of the personal effects of travellers by railway should not be attempted because—

(*a*) disinfection, if carried out at all, should be thorough, and would then cause an amount of delay and inconvenience which will not be justified by the results ; and

(*b*) people when themselves travelling can evade the disinfection of their personal effects by despatching them by goods train. The disinfection of articles sent by goods train would be an impossible task.

IV.(4.) *Measures to prevent the Spread of Plague by means of Communication in India other than by Sea or by Railway.*

716. Though not universally or even generally adopted, cordons were resorted to at certain places in India for the purpose of checking the spread of plague. Cordons.

In considering to what extent cordons have been effective in checking the spread of plague, several points have to be borne in mind. One is, that in dealing for any length of time with any large number of people, there are certain individuals—such as Government servants, and, specially in the case of a trading community, persons called away on urgent business—who must be permitted to cross the line. Thus, however effectually the guards may stop everyone whom they are directed to stop, plague may be carried through by persons who are entitled to pass. Again, it must be remembered that every single person who escapes from a plague infected place may carry with him infection, which may set up a fresh epidemic. Thus a cordon, though it may not be ideally efficient and stop all the people that it ought to stop, may, if it stops some of them, be doing a certain amount of good. Lastly, the fact that plague has not spread outwards beyond a cordon is not in all cases a safe criterion by which to gauge the success of the cordon, for the existence of that cordon may have had nothing to do with the result. The people whom it was desired to shut in may possibly have been people who would not have gone out had there been no cordon. Again, the cordon may have been broken through, but it may have been broken through only by people who were free from infection or by people who, though infected, did not originate an outbreak in the place to which they went. So that the mere fact that in a few instances the disease did not extend through a cordon is not of itself conclusive evidence in favour of the general efficacy of a system of cordon, as a measure for preventing the diffusion of plague.

717. Having premised this, we pass on to notice the chief instances in which cordons have been employed. Instances of cordons established in various places.

Cordon at Umra. 2051-2.

When plague appeared in the village of Umra two miles from Surat town, Umra was surrounded by a cordon in order to prevent Surat from becoming infected. The number of people to be dealt with was only about 500. No one broke the cordon, and the infection did not reach Surat. We may note that the number of people here cordoned was very small; further we understand that a European officer was in the village to see that the guard did its duty.

Cordon at Sirohi. 9737. 9901.

There were cordons of police and sepoys to keep the people of the five infected villages in the Sirohi State from leaving their camps and going to other villages. Plague did not spread outwards from the cordoned villages to any others.

Cordon at Rohri. 12,046, 12,078, 12,083, 12,088.

A cordon was employed at Rohri, in Sind, to prevent the spread of plague into the Punjab, and, as a fact, plague did not spread from Rohri into the Punjab.

So far as these three cases go, the evidence appears to us to support the view that the cordons did prevent plague from passing through them.

Cordon at Karachi (Malir cordon). 11,130-5. 11,293. 11,300-3.

During the early part, at any rate, of the first outbreak at Karachi, the policy had been to encourage the people to leave the town and go to other places. The outbreaks in the other towns of Sind, and probably that in the North-West Provinces, were directly or indirectly due to the immigration of these people. Accordingly, when Karachi was attacked a second time, the policy was adopted of keeping the people in Karachi and its neighbourhood as much as possible. For this purpose a detention camp was instituted at Malir, a point on the railway a short distance out of Karachi. The down passengers by rail were removed from the train there and taken to the camp, and kept under observation for 10 days before being allowed to proceed. To intercept persons who might be attempting to escape by road a cordon of police was stretched across the narrow neck of land through which the railway and the road alike run. "Karachi," Mr. Giles explained to us, "is singularly well situated for these measures. . . There is the sea on one side, and there is the desert on the other. The creeks come round to a great extent, and practically leave a small channel . . it was comparatively easy to shut Karachi in . . ." Nine plague-stricken persons were intercepted by the cordon. We have no information as to the number of other persons who were prevented from passing the cordon, nor is it known whether the cordon did or did not intercept all the persons that it ought to have intercepted. It is, however, the case that infected persons did pass beyond Malir. But, while from the evidence of one witness they would seem to have been persons who had special passes exempting them from detention, or persons who developed plague after an abnormally long incubation period, another witness speaks of some, at any rate, of them as though they had succeeded in avoiding the camp and the cordon altogether. However that may be, the authorities of the places to which they went detected them before they had communicated the infection to any one there. Thus, during the time the Malir camp and cordon existed, no outbreak was originated by a refugee by rail or road from Karachi.

11,240. 11,293-7. 12,433-4. 3778-9. 3822-7. 4261-2. 18,140.

Cordon along Mahi river. 14,014-17. 14,193-9.

During the outbreak in the Baroda State in 1897-98 a cordon was placed along the Mahi river to protect the adjoining Bombay districts against infection. Men were stationed at every ford and place where the river was likely to be crossed, and inspectors patrolled the river and supervised the watchmen at their posts. The posts were never nearer one another than about a mile; mounted police patrolled backwards and forwards between them; and all boats were seized and detained by the authorities. The cordon was removed in June 1898, when the river became unfordable and plague was dying out in the Baroda State. It is alleged that plague did not cross the river while the cordon existed. It is not, however, stated that any plague-infected person was actually intercepted. There is thus no positive evidence of the success of the cordon during this period. On the other hand, when it was put on again in the middle of the following September it did, as a fact, fail to prevent infection from being brought over. We think that it would be impossible with such an establishment as was available, stationed along a length of 80 or 90 miles, to bar the passage of such a stream as the Mahi against persons determined to cross it.

14,200. 14,054-5.

Cordon against importation of infection by land from Daman. 15,959. 15,997.

We may next consider the efficiency of the arrangements made to prevent, by means of a cordon, the introduction by land of infection from Daman territory into British territory. These arrangements were not made immediately on the appearance of plague in Daman, where the outbreak, which began in February 1897, continued severe till the end of May. During the earlier period of the outbreak immigration into British territory was not interfered with, and in February and March 1897 a large exodus took place from Daman by land into, or, at any rate, through the neighbouring British districts. It was not till the 30th March 1897 that the arrangements now under consideration were made. By these arrangements it was intended to establish

a plague cordon along the land frontier of Daman by strengthening and utilising, for the special duty of preventing refugees from Daman escaping into British villages, two lines of guards which were ordinarily maintained along the border. The first of these lines was that of the customs guard, maintained for the prevention of smuggling into British territory; the other line was that of the police posts, permanently stationed along the frontier to prevent dacoits from coming over into British territory. The result of the special measures adopted was that during the day there was a man at watch at every 552 yards, while at night there was a man patrolling every 440 yards, along the 38 miles to be guarded, the vigilance of the sentries and patrols being checked by surprise visits on the part of inspectors. From the 30th March to the 16th April 1897, intercommunication between Daman and British territory by land was entirely prohibited. During this period 139 persons were arrested for attempting to pass the cordon. From the 16th April 1897 to October 1898, intercommunication by land was permitted under certain rules. In accordance with these rules, 2,768 persons were detained on the frontier for 10 days; four only of these persons developed plague, and the rest were permitted to cross the cordon. It is to be noted that in the period anterior to the restriction of intercommunication, which was enforced from the 30th March 1897, no neighbouring British districts were infected by refugees from Daman, and that after the 30th March these British districts continued free from infection. A reason put forward to account for the absence of infection during the period of unrestricted communication is that the majority of the refugees belonged to Bombay and other distant places, and that even before the cordon was put on the inhabitants of the neighbouring villages had been ordered not to admit strangers into their villages. We think, however, that villagers could hardly be relied upon to exclude outsiders at a time when plague was so little known as it was in the spring of 1897; and indeed, no instance could be cited in which village officers arrested or refused to receive into their village refugees from Daman. Moreover, with reference to the period during which communication was restricted, it seems clear that many people may have passed the cordon without carrying the infection and spreading it in the neighbouring districts, for, as we have seen above, only four cases of plague occurred among 2,768 persons detained by the cordon. It is true that no British village is proved to have been infected by any person who passed through the cordon, but as the British territory in the neighbourhood of the cordon remained uninfected both before and after the cordon arrangements were made, we cannot consider that this immunity is a proof of the efficiency of the cordon. There is indeed evidence that the cordon was, to a certain extent, useful in that the officials employed on it intercepted a number of persons who attempted to cross it, but we find it difficult to believe that, with only one sentry to every quarter of a mile at night, it would be possible to stop persons determined to cross the line.

11,969–77. 15,979. 15,965. 15,989. 16,608–9. 15,993–4. 15,964. 16,000. 15,968.

Khandraoni, a village of only 435 inhabitants, is the only place which became infected in His Highness the Maharajah Scindia's territory. Plague had existed in this village for two months before the outbreak come to the knowledge of the authorities. After the authorities arrived, the village was evacuated, and a cordon was put round the camps in which the people were lodged. The officer in charge had at his disposal a disciplined force of 456 sepoys for the purpose of maintaining the cordon and watching the people when they were gathering in their crops. The plague did not spread to any other village. We cannot impute this to the cordon, however, inasmuch as only four more cases occurred after the cordon was established. This was a very small number compared with the number of cases that had occurred before the establishment of the cordon, yet even then no other village became infected. This was probably due to the fact that the residents of the other villages, when they saw the heavy mortality in Khandraoni, refused to have any communication with the people of that village. We can thus, from the case of Khandraoni, learn little as to the general efficiency of cordons. We may, however, note that even with a cordon of a strength such as obviously could be provided only in the most exceptional circumstances, one person is known to have succeeded in getting through.

Cordon at Khandraoni. 9642. 9697. 9648, 9669. 9693–4. 9654. 9672. 9669.

The geographical circumstances of Kotri in Sind are favourable to the success of the cordon which was established there; the town has the Indus on one side and a sandy desert on the other side. Booking at the railway stations was forbidden for those without passes. The number of people cordoned was not large—some 2,600 in all. The area enclosed was 213 acres. To guard this there were, independently of 22 policemen at the different camps, a force of native troops of such a strength that 30 sepoys (with five officers) were always on duty at any one time, while at night half-a-dozen English soldiers were employed to patrol the cordon. By day the sentries were 300 to 350 yards apart on the river side,

Cordon at Kotri. 12,603 to 12,637.

and 100 to 200 yards apart on the land side. At night the cordon was drawn closer. "It was not, of course, possible," says Mr. Cadell, "to cut off all communication with outside. The railway works constituted a great difficulty. Not only did a large number of persons live in the town who had to go daily to their work in the railway quarters, but the large semi-European population in those quarters needed supplies from the town, and the cordon could not be extended round the railway. For many other of the necessaries of daily life also, such as milk and vegetables, the town was dependent on other places." It is a question to what extent people allowed such liberty as this can fairly be said to have been cordoned at all. Plague did not spread from Kotri. Only two people are known to have escaped, and as the presence of the cordoned persons was checked by a roll-call it is probable that no more than two went away. That a larger number did not go away is apparently due rather to the hopelessness of their getting by rail to any other place than to the impossibility of their getting through the cordon.

Report on Kotri by Mr. Cadell (not published with Proceedings of Commission).

12,627–33.

12,680.

When plague first appeared at Hubli in the railway chawls, a cordon was put round the chawls; but the cordon could not be made absolutely efficient. The authorities had everything in their favour, there being but a comparatively small area to enclose. Nevertheless the infection was carried across the cordon, and it was through the default of the police who were working the cordon that the failure occurred.

Cordon round chawls at Hubli. 1581–4, 2407, 20,148–53.

To prevent evicted people from returning secretly to their houses, cordons were in certain places—most frequently in the Punjab—placed round the evacuated village. Judging from the fact that a considerable number of persons appear to have passed through them these do not appear to have been strictly efficient.

Cordon to prevent return to infected houses. 10,057, 10,529.

718. Whatever may be the amount of success that can fairly be claimed for the above cordons, the inefficacy of the great majority of the cordons that have been employed with a view to checking the spread of plague, seems clearly established. The cordon that was employed round the Sadr Bazar at Sholapur was a complete failure, and yet the authorities had what seemed a sufficient number of police, and the place was well suited for a cordon, for it is compact, with open ground on all but one side. The attempt to stop the people getting away from the town of Sholapur, was a still greater failure. The inhabitants circumvented the guards and patrols posted on the roads; they waited till night, and "crept through the fields and hedges, anywhere, and in any way, to get away. Nothing less than a fence round the whole town would have prevented them." When the town of Sukkur became infected a cordon was put on to protect Rohri, which is just on the opposite bank of the Indus. But Rohri ultimately became infected in spite of the cordon. The situation of Hardwar and Kankhal is, it is stated, very favourable to the maintenance of a cordon. It is, however, admitted that the cordons established there, though they were efficient to a certain extent, could not be made absolutely efficient. Speaking of them, Lieutenant-Colonel Thomson, I.M.S., said that it was "quite impossible in practice to maintain anything like an efficient cordon." In the Punjab, with a view to preventing the people of infected villages visiting other villages, it was the practice to place a cordon round the village areas in such a manner as to allow the villagers to go out into their fields by day, while it prevented their passing beyond the areas cordoned; at night these cordons were drawn in round the camps. The moral effect is said to have been considerable; it kept large numbers of people within bounds. But there were only about eight guards to the mile; and it is stated that people got through on all sorts of pretext. "Time after time," writes one of the plague officers in speaking of a certain point in the cordon round the village of Kariam, "when I arrived on duty, time after time, I found policemen off their posts or asleep." No one, we were told, who really wanted to get through and was prepared to pay would have had difficulty in getting through the Punjab cordons. It is also clear that many plague infected persons did, as a fact, pass through them. In no less than 45 out of the 67 villages known to have been infected by human agency, was the infection brought by persons who must have broken through a cordon. The Chief Medical Officer of the Cutch State put cordons round several of the infected villages that he was dealing with. He claims thereby to have prevented plague from spreading to neighbouring places. He described the cordons he used as "strong" cordons, but it appears that there were never at any one time more than about 10 or 12 men on duty to guard eight or nine hundred people. The existence of these cordons was apparently not known to the Political Agent, who was only aware of two cordons and those in villages other than those to which the Chief Medical Officer refers. The Political Agent's experience of these cordons was that they were not of any use. When careful inquiries were made into the working of the cordon of cavalry put round Poona City, Cantonments, and suburban limits, to deflect the traffic into the main roads where the medical examination took place, it was found that "anyone who chose to get through it, especially at

Great majority of cordons inefficacious.

20,229–30.

20,237.

12,023–5.

9213.

9366.

8782.

10,056.

10,060–7.

Letter No 402/P. of 16/4/99, from Mr. French, Assistant Commissioner, Nawashahr (not reprinted).

10,242.

10,059.

13,584–9, 13,649–53.

13,501–3.

21,299–300.

21,328.

night, could do so with the greatest facility. One could take a horse, or a carriage and four, through it if one wished. There was not the least difficulty."

Attempts to check the movements of the people by preventing travelling without passes.

719. In some places the cordon system was modified so that the main roads and the railways alone were guarded, people from the infected place being allowed to proceed along these only if provided with passes. Great precautions were taken to prevent people running away from the town of Surat. Passes were insisted on, and the precautions were, it is stated, successful in preventing plague from spreading out of the Surat district. This contention, however, is invalidated by the fact that the pass system did not prevent people from going out of Surat town into the neighbouring villages and spreading plague among these. At Bangalore the authorities were unable to prevent people who had not passes from going away, as the police force was very inadequate to guard all the roads, and a number of people went out at night across the fields. It would, in Colonel Robertson's opinion, have been impossible, without an enormous number of men to prevent people going away without passes. He did not think he could have made a cordon effective, even if there had been at his disposal a large number of men: "I think people who could pay would be able to get out." As a fact, over 50 villages round Bangalore became infected. At Hubli—and possibly at Dharwar also—people were not supposed to go away either by road or rail, except on passes. How the system worked at Dharwar there is no evidence to show. As regards Hubli, it was, in respect of departure by rail, presumably efficient; but in respect of departure by road it was practically useless. Many infected people from both places went to the villages round about and spread the disease. At Jalgaon there were policemen on the roads, and a pass system; but it was not successful. "The people got away as much as they liked, and carried infection into the next taluka. The place was too big to deal with properly; one could not keep an eye on 15,000 people in the fields camping all over the place."

24,426–7, 24,460–3. 2641–3. 19,812–4. 19,852. 19,825, 23,770. 25,554–6.

Road posts intended to protect uninfected places by intercepting refugees from infected places.

720. Again, when there was plague in Hubli, but, as yet, none in Dharwar, there were, Mr. Cappel says, "road posts to prevent intercourse between Hubli and Dharwar except under pass: these were doing work the whole time." How many people they stopped, and whether they stopped all that they ought to have stopped, we do not know. Before plague appeared in the Dharwar district there were road posts to guard its frontier against the adjoining infected district of Belgaum. Of their effectiveness or ineffectiveness, we are unable to judge. But the Collector says that they "undoubtedly did good, because they frightened the plague fugitives from coming in." When there was plague in Kaladgi in the Bijapur district, all the roads leading into the neighbouring town of Bagalkot were patrolled, to prevent people from Kaladgi entering Bagalkot secretly. To what extent the patrol was efficient and of practical use there is no evidence to show. Bagalkot did not become infected.

1585. 23,635.

Conclusions regarding measures to check the spread of plague by means of communication other than by sea or by railway.

721. The conclusion follows, we think, from the results of the different efforts made in this direction, that unless circumstances are very specially favourable the attempt to keep a large mass of people in or out of a particular place by means of a cordon or a similar device is doomed to failure. It must be admitted that a cordon has a moral effect, and that it may by this means deter people from passing in or out of a cordoned place unless in the case of persons who have strong motives for doing so. But, under ordinary circumstances, the physical characteristics of the plains of India, covered as they are with countless paths and tracks, present great obstacles to the success of a cordon. In certain cases, however, as at Malir, where there is virtually a *cul de sac*, or where there is a river fordable only at certain definite and well-known places, the physical difficulties are less. In such cases an attempt to restrict the movements of the people by means of a cordon may meet with some measure of success. Where, again, the infected area is very limited, and very strong establishments can be concentrated upon it, the restriction of the movement of the people by means of a cordon, if it could not be made absolute, could be made nearly so, provided that reliance could be placed on the watchfulness and the integrity of those employed on the cordon. But we fear that this is rarely the case in India; the men cannot be depended upon to remain at their posts, awake and alert; and they cannot be trusted to refuse bribes. The consequence, therefore, will almost always be that, even in the rare cases where the physical conditions are favourable to the success of a cordon, the carelessness and venality of the guards on it will bring about failure. And in the great majority of cases the area to be guarded will be so great, and the possibilities of evading a cordon at night so many, that, even with establishments which could be implicitly relied on, it will be found impossible to make a cordon an effective means for preventing the spread of plague.

V.—General Remarks on Plague Measures.

Owing to fundamental changes of conditions in India between present time and time of last outbreak in India, measures adopted in 1896 were necessarily experimental.

722. It is impossible to form a just appreciation of the operations undertaken against the present outbreak of plague in India, unless it be remembered that, previous to the commencement of this outbreak, plague had for 60 years been unknown in the plains of India. Moreover, the last outbreak, that of 1836, was purely local, and confined to Rajputana. There has, in the interval, been a fundamental change in the conditions of life within India, and in the commercial relations between that country and the rest of the world. Sixty years ago communication between England and India was slow and not very frequent; between India and other countries it was even rarer. Now, steamers arrive daily at the chief ports of India, not only from England, but also from the main trading centres of the Eastern and the Western world. Within the continent of India the opening of railways and the improvement of communication by road have entirely revolutionised the habits of the native community both in the towns and in the villages. Plague, when it reappeared in India in 1896, found both the people and the servants of the Crown in India without any practical acquaintance with its clinical or other characteristics or any recorded experience to show how an outbreak could best be dealt with in an Oriental country circumstanced as India is to-day. How different the conditions of the present time are from those of former times may be gathered from the fact that, when in 1815 a medical board was appointed in Calcutta to consider the danger of the introduction of the plague into that city, the direction from which danger was anticipated was from Arabia, and not from any part of the continent of India, though at that time plague was severe in a part of the Bombay Presidency. The various measures taken on the occurrence of the outbreak in 1896 were thus to a large extent experimental.

Every possible measure applied and tested, and every possible effort made to check the epidemic.

723. It was recognised, alike by the Government and by the more enlightened classes in India, and by public opinion elsewhere, that no reasonable measure should be left untried to prevent, if possible, the spread of plague. The feeling of one and all was that its progress must, if possible, be stopped at all hazards. But at the time no one was able to foresee with certainty how far each particular measure which appeared to be feasible and to be theoretically good could be effectively carried out in the existing conditions of native society. In these circumstances it is not surprising that not all the measures applied have been found to be beneficial, for the anticipated good effects of some of them have been more than counterbalanced by unanticipated evil effects. The evidence given before us shows that every possible experiment has been tried, and every possible effort made to restrict and eradicate the disease. If the results have not always been commensurate with the labour and expense incurred by the authorities who devised the measures, or with the trouble and inconvenience felt by those to whom the measures were applied, it must, we think, be recognised that, considering the nature of the problem with which the authorities in India were called upon to deal, this was unavoidable. If in our judgment it has appeared that all the thought, time, labour, and money devoted to the campaign against the plague have, at some places, not resulted in any great alleviation of its severity, or in the interruption of its progress, this conclusion involves no condemnation of the action taken. It is comparatively easy, on an examination of the results of a particular measure, to form an opinion of its success or failure. It was, we think, impossible to predict, when the outbreak commenced, which measure would succeed and which would not succeed. No one, however, can fail to admire the fertility of resource and the unceasing vigilance of the different Local Governments and Administrations which had to deal with outbreaks of plague, and in particular the Government of Bombay, upon which the great brunt of the work has fallen; the readiness and energy with which the rulers of Native States followed the lead set to them in British India; the zeal and devotion, not only of the officers of Government, Civil, Medical, Military, and the subordinate officials, but also of many willing unofficial helpers who have sacrificed their time, and some of them their lives, in the execution of plague duty; and the patience and fortitude of the great mass of the people in the calamity with which they were attacked.

The measures likely to be most successful with plague are those now adopted in all countries for infectious diseases in general.

724. In our survey of the measures taken in Indian towns and villages, we have noticed that a certain amount of success has been claimed for almost every measure, but that no single measure has been uniformly successful. We have already summed up briefly our opinion as to the degree of success or ill-success which has attended the various plague measures adopted in different places throughout the country, and it remains for us to consider the question as to what combination of measures will produce the maximum of good.

For indications as to what methods are likely to prove most successful in dealing with plague in India, we must look to the methods which have been most useful in dealing with other infectious disease in all countries. The old methods of treating an outbreak of epidemic disease—methods such as quarantine, cordons, and segregation of the contacts,—which imposed unnecessary restrictions on the healthy, are everywhere being gradually but surely given up, and in their place, notification of disease, isolation of the sick, and disinfection are being substituted, and inoculation is being resorted to as a prophylactic measure. Theoretically there is nothing to be said against the older measures and, indeed, nothing could be more well advised than to prevent people in the incubation period of disease from entering a country, or to isolate those who have been in contact with the sick, or to prevent such people from leaving an infected district, and yet these measures are in practice gradually being given up, simply because they are impracticable, this impracticability being due to the facility with which they can be evaded by healthy people. In comparison with the foregoing the measures of isolation of the sick, disinfection, and inoculation are more easily applied, provided a sufficient staff is available, and their application entails but little hardship on the friends of the patient while the inducement to hide cases is reduced proportionately.

Even with the best measures rigidly applied, danger from plague will still subsist.

725. If every case of plague could be isolated at once, and if the personal effects, clothes, and house of a patient were disinfected, and, further, if all plague-infected rats were removed and burnt, and the houses in which these had been found were disinfected, and, finally, if the healthy persons who are exposed to infection could be inoculated, the ravages of plague would be minimised. But remembering how imperfectly any measure is likely to be applied in practice, it is not to be expected that these measures, whether separately or in combination, will suffice to extirpate the disease. Isolation of the sick, for instance, can only in rare instances be applied so early that all contact of healthy people with the patient can be avoided, disinfection can not protect those already infected, and inoculation is unsuccessful in a number of cases. The fact must be faced, therefore, that there are no means of stamping out the present epidemic of plague in India, that, even with the best measures most rigidly applied, a certain amount of danger subsists, and that all that can be done is to lessen the danger as much as possible.

Staff and resource necessary for strict application of measures must soon be exhausted in a widespread epidemic.

726. A point all important in connexion with this question is that of the staff and funds available for the execution of any measures. Even supposing the funds to be practically unlimited, the time will soon come when, in any country, and especially in India, the staff available will be insufficient, and we cannot too strongly insist on the fact that, for effectively carrying out any plague policy in an extended epidemic, a reliable and enormous staff is a necessity. In illustration of this we may point to the following instances. In the North-West Provinces, the outbreak extended to one town of 16,000, two towns of about 5,000 inhabitants out each, and eight villages in their immediate neighbourhood. Excluding the Europeans, the establishments employed on plague duty in this small infected area on April 1st, 1898, comprised 92 native medical subordinates, male and female, 45 other native subordinates, 106 village watchmen, and a police force of 342 officers and men. In the Punjab, the outbreak of 1897–8 was confined to a couple of small towns and 84 villages situated in the adjoining tracts of two districts. The establishments employed in carrying out the measures against plague, exclusive of those employed in searching for plague in villages not known to be infected, were at one time, in May 1898, 16 European Medical and Civil officers, six subordinate native civil officials, 94 native medical subordinates, male and female, and a force of police of 2,872 officers and men. With establishments of this strength, and operations concentrated over a compact area in which there were no great physical and climatic difficulties, measures of compulsion were successful in preventing the spread of the infection to any considerable distance. It is impossible, however, to estimate what establishments would be required to carry out measures successfully in Calcutta and Bombay, or in places as widely infected as the Dharwar, Belgaum, Satara, Sholapur, and other districts in the Bombay Presidency were at one time or another. It appears certain that the resources of Government would be insufficient to provide establishments for the area already infected in India even if the whole of the Army, Civil, Medical, and Police establishments were employed solely on plague duties. Even for such measures as the isolation of the sick, disinfection, and inoculation, it is clear that in a badly infected district the available staff and resources will soon be exhausted, and hence it will be necessary, before adopting a particular policy in each locality, to consider the staff that is available to carry out that policy.

Measures recommended for places of over 2,000 or 3,000 inhabitants.

727. It appears to us that in places containing more than 2,000 to 3,000 people—the President and Mr. Hewett are of opinion that the limit of population should be 10,000—the only measures which can be applied are disinfection, inoculation, and, possibly (so long as infection has not spread throughout the town) isolation of the sick, and the evacuation of specially infected quarters: the President considers that isolation of the sick should be carried out in all places. The value of disinfection is especially great when it is applied to imported cases or the first indigenous cases. Segregation of contacts and a general eviction of the inhabitants of large towns, have, in our opinion, done a great deal of harm. In particular, the wholesale evacuation of large towns containing a considerable artisan or trading population can be carried out only at great risk, inasmuch as the immediate result of its application will almost always be the flight of many infected people into another area, and the consequent spread of the disease. The isolation of the sick can, we are convinced, be carried out satisfactorily only when they are removed to hospital, but we see no reason why, in circumstances in which removal to hospital is impracticable, isolation should not be attempted in the patient's house. Disinfection is a measure which appears likely to influence the course of the epidemic, but it must be remembered that efficient disinfection requires not only a large staff, but a trained staff, and that throwing a certain amount of perchloride of mercury on greasy walls is not disinfection. Further, it must be remembered that, as already pointed out, houses are after disinfection liable to be re-infected by the agency either of rats or of men. It is impossible to provide against this effectively except by postponing disinfection until such time as the plague has died out both among the rats, if these have been affected, and in the human community; but this policy, though it may be carried out in places where evacuation is resorted to, is plainly impracticable in the case of large towns. In these, even though there is always a risk of the re-infection of disinfected houses, yet the advantage of destroying a considerable mass of infective material is so great as to render chemical disinfection clearly advisable. It is probable that the cost of chemical disinfection might be considerably lowered by limiting the operations to such things as really do require to be disinfected, or by adopting non-destructive methods of disinfection. It would be an advantage also, provided that the inoculations can be performed under the safeguards more particularly referred to elsewhere in this Report, to inoculate disinfecting gangs, those engaged in plague operations, and as many of those who run any risk of infection, as are willing to submit to inoculation.

Among measures of disinfection which should be carried out, the President would include the opening up of houses, wherever necessary for the introduction of a sufficiency of light and air.

In large towns in which plague cases are continuously occurring, and in which the amount of plague is subject to great seasonal variations, we think it a matter of great importance that plague measures should be carried out with especial vigour at the season when plague is at its minimum. At such a season, in view of the amount of personal supervision that could be given to each case with the relatively large subordinate staff that would be available, the chances of extirpating plague in such a a town are very much greater than at the season when the disease is most prevalent.

Measures recommended for villages.

728. In villages also chemical disinfection and inoculation should be applied; but very soon, in a badly infected district, the available staff and plant must become insufficient to cope with the work. We are of opinion that, as soon as this point is reached, chemical disinfection should be concentrated on one or two villages with a view to making it effective there, and that it should be totally abandoned in the other villages, where it would only worry people and where it could not be carried out properly. In such villages the measure which must be chiefly relied on is evacuation. In the opinion of the President the evacuation should be conjoined with an opening up of the houses sufficient for the admission of sunlight and air, wherever this is required. But evacuation carried out with an insufficient staff is, in many ways, an imperfect measure. In the circumstances it will not be possible to secure the isolation of the sick in village camps. In respect to this, nothing more can be done than to advise the people to isolate their sick. The necessity for this, with reference to certain more familiar infectious diseases, is understood by the people, and they, to some extent, endeavour to carry it out themselves. Again, evacuation with an insufficient staff is an imperfect measure, inasmuch as the evicted people carry out with them into camp plague-infected clothes and other articles, which it will be impossible to disinfect. Further, in the circumstances which we are considering, it will not be possible to prevent the people from revisiting their infected houses. Lastly, in many cases there will be a risk of some of the evicted people dispersing to spread the plague to previously uninfected

places; this risk, however, is probably small where the evicted people are agriculturists whose interests will be concentrated in their fields. Moreover, evacuation is only a cumbersome method of committing to the slow operations of nature the work of disinfection which might be efficiently carried out much more quickly by artificial means. But, in spite of its imperfection, evacuation would appear to be the only measure applicable to villages in cases where chemical disinfection cannot be applied. It is, however, to be borne in mind that climatic conditions are a most important factor in determining whether evacuation can be carried out or not. Except in a few districts in India, it appears hopeless to achieve effectual evacuation during the rains, especially in villages situated on a black cotton soil. No amount of persuasion will induce people to stay out in camp under such conditions, and they will, almost inevitably, at any rate attempt to return to their houses to sleep. The only result of an attempt to keep people out of their houses will be an immense amount of dissatisfaction unaccompanied by corresponding advantages. Whether, during the rains, evacuation could be effectively carried out on a light sandy soil depends upon various local conditions which cannot be enumerated here, but we are strongly of opinion that, before submitting people to the discomforts of evacuation, there should be no doubt that the hardships inflicted on the people will in all probability yield a balance of advantage.

Where it is found that disinfection and evacuation are both impossible, then in particular, we think that inoculation should be resorted to. Much can be done in this direction with even a small staff, for a properly trained Assistant Surgeon could in a short time inoculate the population of a village. Of course, neither in such cases nor in any other case, could inoculation by itself extirpate the plague, because a number of people will get plague in spite of inoculation.

Advisability of carrying out some one measure thoroughly, if staff not available to carry out all measures recommended.

729. We believe that by a careful application of a policy such as that outlined above, much might be done to modify the course of an epidemic, and many valuable lives would be saved. In the application of these methods, it must, however, be clearly understood that it will always be better to apply any one of them efficiently and thoroughly than to try and apply all of them with an inadequate and insufficiently trained staff. Unfortunately we cannot anticipate that India can now be freed from plague by the measures we have suggested any more than Europe has been entirely freed by sanitary measures from other zymotic diseases.

A precise statement of measures recommended by the President stated separately.

730. The President believes that a precise statement of the measures applicable at the beginning and at a later stage of an outbreak of plague in places of large and small size, respectively, would with advantage be added to the above. He has expressed his views in an Appendix* to this Report.

Attitude of people towards plague measures.

731. We may now proceed to review again the attitude of the people in its relation to the application of the plague measures dealt with above. One point which we particularly desire to emphasise is that measures which press with severity on the inhabitants of an infected place, as a whole, or on the friends and relatives of the patient as distinguished from the patient himself, can never be enforced in the case of a widespread epidemic by the staff available in India. The coercion of the people into compliance with regulations against plague is not a practical policy. But we believe that a great deal can be done by persuasion and by practical demonstrations of the utility of certain measures. A formidable obstacle to the recognition by the people of the necessity of enforcing measures against the plague is, indeed, to be found in the fatalism which the native of India shares with other Orientals. It appears to us that, though an Oriental may theoretically assert that fighting against fate is useless, this belief will not prevent him from defending his life, from calling in the native hakim when ill, or from flying from an epidemic. What he does bitterly resent is to be obliged to submit to measures, the use of which he does not understand, and of which he is naturally suspicious. Signs, however, are not wanting that, among the population that has been affected by plague, the fatalistic attitude of the people towards the plague has been modified. From the evidence placed before us, we are convinced that in parts of the country the people have begun to recognise the advantages of taking certain measures against the disease, and to appreciate the motives of the Government in taking steps for its extirpation.

Good effects of evacuations appreciated.

732. With regard to evacuation, there can be no doubt that, in many places, the agricultural population, at all events, have learned to believe in it, and they unquestionably will appreciate its good effects still more when further demonstrations are at

* *See* Appendix V.

hand. We have seen that in some parts of India the natives evacuate their villages spontaneously on the appearance of plague or the discovery of dead rats.

Better methods of disinfection likely to lessen unpopularity of this measure.

733. Chemical disinfection, on the other hand, is a measure strongly objected to, perhaps because in India disinfection has often been inefficient, and perhaps also because it has been accompanied by other measures often extremely distasteful to the people. The methods of disinfection which have been in use have, as we have seen, sometimes entailed no little damage to the goods of the people. Pots and pans have been broken; rags have been burnt, which, although of no marketable value, may have been of value to the owner. True, the Government has paid compensation, but Orientals attach great importance to the manner in which things are done, and are frightened by anything like a display of force or violence such as is entailed in the destruction of crockery, the burning of clothes, the digging up of floors, and the breaking up of furniture. The moral effect as well as the efficacy of every disinfecting process should be considered, and, in our opinion, nothing should be needlessly destroyed. If this principle is kept in view, we may hope that the people will be conciliated to the necessary disinfecting operations.

No reason why inoculation, encouraged by persuasion, should not become popular.

734. The principle of inoculation is one with which, thanks to small-pox vaccination, the natives are to some extent familiar. They will learn to appreciate the advantages of anti-plague inoculation more and more as better results are obtained. However, in encouraging inoculation, due warning should be given that the protection which is conferred by the process is by no means absolute. In some instances where the people have not been duly warned, they have declined to have anything to do with inoculation, because some of the inoculated died from plague. In such cases people are apt to forget the good which has been achieved, and the intensity of their disappointment leads them to condemn utterly a system from which they have allowed themselves to expect too much. The people, therefore, should be distinctly told that inoculation is not by any means a certain prophylactic, but that it considerably reduces the chances of the inoculated getting plague, and that an inoculated person when attacked has increased chances of recovery, and that the process, if properly conducted, is harmless. It seems to us that attempts to make the people resort to inoculation should be limited to persuasion, and that no direct or indirect form of compulsion should be adopted. We see no reason why anti-plague inoculation, if furthered in this manner, should not become a popular measure.

Increased communications now render spontaneous disappearance of plague not so likely as in past times.

735. In view of the attitude of the people towards plague measures, and the imperfections of the measures which can be applied, we may, before leaving this subject, consider the possibility of the disease disappearing spontaneously, as it has in times past disappeared from Europe and from India. We may well hope that this may occur again in India. But we must point out that the conditions are essentially different now from what they were in former centuries, or even at the beginning of this century. Inasmuch as plague has been shown to travel along the lines of human communication, the chances of a place being infected and re-infected from any plague-stricken localities in India have increased correspondingly with the greater facilities for intercommunication. Hence the extinction of plague may possibly be a much slower process than formerly, for in every place the chances of re-infection have been increased.

With further spread of epidemic the natives of India must in the end depend for protection against plague largely on their own application of plague measures.

736. If, accordingly, plague should spread, the staff available for combating it will become proportionately smaller. Soon a time will come when the amount of supervision which can be given to each village must be very small, and nothing can be more certain than that in the end the native of India will have to depend for his protection against plague, to a very large extent, upon his carrying out the appropriate plague measures for himself. He can be prepared for this, not by the adoption of impracticable measures, such as cordons and quarantine, but by giving him practical illustrations of the good achieved by measures which experience has shown to be the most effective.

Sanitary improvements.

737. The effect of sanitary surroundings on plague has been considered elsewhere in the Report (paras. 337–365), and we have already set forth our conclusions regarding the character and extent of the sanitary improvements of which the adoption can be recommended as a measure for the suppression and prevention of plague.

The President has set forth his personal views on the subject in Appendix III. to this Report.

CHAPTER VII.

RECOMMENDATIONS REGARDING THE ORGANISATION OF THE SANITARY DEPARTMENT IN INDIA.

I.—Existing Arrangements of the Sanitary Department in India.

Necessity for improving the organisation of the Sanitary Department in India.

738. In the course of our inquiry into the different questions referred to us in connexion with the spread of plague, we have formed some conclusions as to the necessity for the improvement in certain directions of the organisation of the Sanitary Department in India with the object of dealing more effectively with outbreaks of plague and other epidemics and with the general sanitation of India. Our conclusions we shall endeavour to set down as briefly as possible.

Existing arrangements of the Sanitary Department in India.

739. The existing arrangements of the Sanitary Department in India are as follows. The office of Sanitary Commissioner with the Government of India is combined with that of Director-General of the Indian Medical Service. The officer who holds these combined offices, as Sanitary Commissioner advises the Government of India on sanitary matters. He makes tours of inspection, in the course of which he inquires into sanitary questions. His reports are made to the Government of India, by which all directions regarding sanitary matters are given to the Local Governments. Except through this channel, he has no authority over officials in the Sanitary Department serving under any of the Local Governments. These latter exercise entire control over the Sanitary Commissioners and Deputy Sanitary Commissioners serving under them. Under the Local Governments in Madras, Bombay, Bengal, the North-Western Provinces, and the Punjab there is a Sanitary Commissioner, who is a member of the Indian Medical Service. These officers are charged with the duties of advising the Local Governments on all sanitary matters; of inquiring into all outbreaks of epidemic disease; of controlling all operations connected with vaccination in their Provinces; and of supervising the registration of vital statistics within them. In each of the other Provinces the duties of Sanitary Commissioner are undertaken, in combination with those of the supervision of Civil Hospitals, by the Inspector-General of Civil Hospitals. Under the Sanitary Commissioners in the larger Provinces there are a varying number of Deputy Sanitary Commissioners. The largest number, viz., five, are employed in the Bombay Presidency. The districts supervised by the Deputy Sanitary Commissioners are in all cases so extensive that it is not possible for a Deputy Sanitary Commissioner to visit in any one year more than a small number of the villages which call for inspection. In all Provinces, excepting Bombay, the Civil Surgeon, who is the Medical Officer of the district or unit of civil administration within a Province under the charge of a Collector, has also to undertake the duties of Health Officer of the district, and to advise the local civil authorities on all matters connected with sanitation. The Civil Surgeon of a district in the Bombay Presidency is not required to undertake these duties. As a rule, no subordinate establishments are separately entertained within a district for sanitary duties, but the establishments employed in the conduct and inspection of vaccination are utilised also in sanitary work.

Sanitary Boards.

740. Outside the Sanitary Department there are, under the different Local Governments, Sanitary Boards composed of a varying number of members belonging to the medical, sanitary, civil engineering, and civil branches of the Government's Services, with some non-official members. All the larger measures of sanitary improvement, such as projects for improvements in water supply, have to be considered by these Boards before they are undertaken.

Existing arrangements regarding Health Officers.

741. In the Presidency towns of Calcutta, Bombay, and Madras, European Health Officers are employed by the Municipal Corporations. These officers are sometimes selected from the Indian Medical Service and sometimes not. They are directly subject to the authority of the Municipal Corporation by which they are employed.

A few separate Health Officers, who may or may not be members of the Indian Medical Service, are employed also by other large Municipalities, to which they are directly responsible. But as a general rule a Municipality does not employ a separate Health Officer, but is entitled by regulation to the services and advice of the Civil Surgeon in his capacity as such. This rule, however, is not followed in the Bombay Presidency, the Civil Surgeon there having no responsibilities in connexion with Municipalities.

In the chief ports, Port Health Officers are employed under the orders of the Local Governments to supervise the arrangements connected with the granting of bills of health and similar matters.

Sanitary Commissioners, under existing system, not necessarily sanitarians, and liable to recall to military duty.

742. It appears to us that the existing organisation is defective in some very important particulars. In the first place, the system of selecting the Sanitary Commissioners, who are the chief inspecting officers of the Department, seems to be faulty. They are appointed for a term of four to seven years from among members of the Indian Medical Service. It is not necessary that they shall have served an apprenticeship in the Sanitary Department. In selecting officers for these appointments regard is, no doubt, given to the knowledge which they possess of sanitary questions, but there is no guarantee that an officer selected to be a Sanitary Commissioner should have made a special study of sanitary science.

It also appears to us to be a defect in the present system that a Sanitary Commissioner should be liable to be recalled to military duty in case of war. If it be urged that, although this liability exists, it is never enforced, it must be admitted that the Sanitary Commissioners are not an effective part of the military reserve, and that no harm would result from taking them out of it.

In Calcutta and Bombay, Health Department undermanned and position of Health Officers not sufficiently independent.

743. There are other points also in which the present organisation appears to us to be defective. The Health Department in towns such as Calcutta and Bombay is not sufficiently strong in Medical Officers for the duties which it has to perform even in ordinary times. It is a matter of Imperial interest that the sanitary arrangements in these cities should be effectively supervised and controlled, and that the medical establishments for this purpose should be adequate. It appears to us to be quite impossible for one European Medical Officer to properly perform the duties of Health Officer in the cities mentioned and the other larger towns of India. Moreover, the Health Department in these places appears to us to be at a disadvantage, not only by reason of its being undermanned, but also by reason of the position of the Health Officer not being sufficiently independent. The appointment of Health Officers at Bombay and Calcutta is made by the Municipal authorities, subject to the approval or confirmation of the Local Government. The Health Officer has no means of carrying into effect sanitary measures of essential importance if the Municipal authorities are not favourable to them.

Civil Surgeons unable, in addition to performing their other duties, to control sanitary administration of Municipalities and rural areas in their districts.

744. We have observed that the Civil Surgeon has ordinarily, except within the Bombay Presidency, to advise the Municipal Committees within his district upon sanitary matters. His ordinary duties are of a multifarious character, and often do not leave him with the leisure necessary to devote proper attention to sanitary questions even within the Municipality at the headquarters of his district. Moreover, his functions are simply those of an adviser; he has no power to carry out any recommendation that he makes. Thus, even in large Municipalities outside the Presidency towns in India, insuperable difficulties may be thrown in the way of a Civil Surgeon who is endeavouring to carry out sanitary improvements.

In his capacity as Health Officer of his district, the Civil Surgeon is hampered by the numerous duties at headquarters which prevent him from travelling about his district, and by the fact that he has no subordinates with Medical and Public Health Qualifications whom he can utilise for making visits of inspection into the interior.

No agency at present for bacteriological work.

745. Lastly, the existing organisation seems to us to be defective in that it comprises no agency for inquiries into bacteriological and other questions connected with the Public Health, with the single exception that the Chemical Examiner in the North-Western Provinces is also bacteriologist to the Local Government.

II.—Recommendations for the Reorganisation of the Sanitary Department in India.

Questions to be considered with reference to suggested improvements.

746. We would recommend that the Sanitary Service should be reorganised, and that an Imperial Sanitary Department should be established in India, somewhat on the lines which we have sketched below.

We have to consider in connexion with the proposed Sanitary Department:—

(1.) The question of the present and future recruitment of the staff of the Sanitary Department.

(2.) The question of the augmentation and strengthening of the present Sanitary Staff.

(3.) The disposal and functions of the various members of the Department.

(1.) Present and future recruitment of the staff of the Sanitary Department.

747. We propose that the following might immediately be brought on to the permanent staff of the Department:—

(*a.*) The European Medical Officers of Health who are at present in the service of the various Municipalities.

(*b.*) The Port Health Officers of Bombay, Calcutta, Karachi, Madras, and Rangoon.

(*c.*) The Medical Officers who now hold appointments as Sanitary Commissioners and Deputy Sanitary Commissioners of the different Provinces.
(*d.*) The bacteriologists who are now in the employ of the Indian Government.
(*e.*) Civil Surgeons and their Assistants, in so far as they perform sanitary duties, and the staff of the Vaccination Department.

With regard to the future recruitment of medical men for the permanent staff of the Department, we would suggest that, as the services of men of special training and ability will be required, the emoluments of the appointments in the Department should be fixed on a scale sufficiently liberal to attract men of the class required, and that these appointments should be opened, on the one hand, to officers of the Indian Medical Service who have gone through a special course of training, and, on the other hand, to other medical men who have special Qualifications in Public Health. In the case of appointments from the Indian Medical Service, we would suggest that the appointments might, with advantage, be limited to officers of less than seven years' service, provided that before assuming duty they shall have studied Public Health in courses equivalent to the requirements for Degrees or Diplomas in Public Health.

748. We consider that the administrative Department as thus constituted would urgently require to be strengthened in several directions. We think that it ought, in the first place, to be strengthened by the appointment of a number of additional Medical Officers of Health. We would recommend that a special European Medical Officer of Health should be attached to every city in India which contains a population of more than 100,000 inhabitants. And, inasmuch as the limit of population which a single Medical Officer of Health is capable of supervising effectively may be fixed at 100,000 inhabitants, we propose that in the case of cities one European Assistant Health Officer should be provided for every 100,000 inhabitants in excess of the limit specified above. We think that, while the Municipalities to which Medical Officers of Health may be appointed in accordance with our recommendations should provide for the salaries attached to the appointments of such Officers and their Assistants, they should not have the power of nominating to these appointments or that of removing officers who have been appointed.

(2.) The augmentation and strengthening of the present Sanitary Staff.
(*a.*) Medical Officers of Health.

Further, we would recommend that the staff of the Sanitary Department should be strengthened by the recruitment of a number of specially trained Medical Officers to fill the various scientific posts which ought, in our opinion, to be created.

(*b.*) Special scientific officers.

Again, it might be useful to include the Chemical Examiners in the staff of the Department, in order to have available within it officers capable of conducting chemical analyses; but the duties which the Chemical Examiners have to perform in connexion with medico-legal work may possibly be an obstacle to this being done.

(*c.*) Chemical Examiners.

We think that the Civil Surgeon in the Bombay Presidency should be given similar duties in respect of sanitary matters to those which are entrusted to the Civil Surgeon in other Provinces.

(*d.*) Civil Surgeons.

In order to assist the Civil Surgeon in his duties of Health Officer of the district, it is desirable that an Assistant Surgeon, qualified by his knowledge of hygiene, should be appointed in each district. This Assistant Surgeon could be employed at times in temporary charge of the Hospital at the district headquarters so as to admit of the Civil Surgeon making tours of inspection in the interior of the district; and at other times the Assistant Surgeon could himself be engaged on the duty of making such tours. For these purposes men of special attainments and qualifications would be required, and we think that, as a preliminary to their employment in the Sanitary Department, a certain number of them might be employed in the higher grades of what is now the Vaccination Department, where their qualifications and abilities could be tested.

(*e.*) Assistant Surgeons.

Inasmuch as the Inspectors and Superintendents of Vaccination have, under present arrangements, no medical qualifications and no special knowledge but that which they acquire in the course of their employment in the Department, we think that it would be of great advantage from a sanitary point of view to appoint Assistant Surgeons to the position of Inspectors or Superintendents of Vaccination. Under such an arrangement the work of vaccination would not be less efficiently performed than heretofore, while that of the Sanitary Department would be materially improved.

(*f.*) Inspectors and Superintendents of Vaccination.

749. With regard to the disposal of the staff, it follows from what has been said above that the various members of the Department should fill the posts (*a*) of Medical Officers of Health to the larger cities, (*b*) of Medical Officers of Health to the various sub-divisions of each Province or Presidency, (*c*) of Port Health Officers to the various large ports, and (*d*) of administrative or supervising officers.

(3.) The disposal of the various members of the Sanitary Department.

In addition, we propose that a certain number of members of the Department should be employed on scientific investigations in connexion with important questions of Public Health; and the President proposes that a certain number, also, should be employed both in scientific investigations and in the teaching of Public Health, as explained in paragraph 754 below.

Functions of Medical Officers of Health.

750. We do not think that it is necessary here to attempt anything in the way of an enumeration of the various duties which should be undertaken by the Medical Officers of Health. It will suffice if we advert to certain duties which appear to us to be particularly important.

(*a.*) Sanitary control over lodging-houses and the erection of houses.

We have already, in connexion with the consideration of the influence of insanitary conditions on the severity of plague epidemics in India, adverted to the fact that the condition of many of the dwelling-houses in the large towns in respect of overcrowding and in respect of structural defects is such as to make a healthy existence in them practically impossible. In connexion with the duties of Medical Officers of Health of large cities, such as Bombay or Calcutta, we would particularly emphasise the importance of exerting a sanitary control over lodging-houses for the prevention of overcrowding. We are of opinion that structural defects might slowly, but effectually, be remedied, at any rate in the larger towns, if the Medical Officers of Health were empowered to veto the erection or the rebuilding of grossly insanitary buildings within the limits of the Municipalities to which they were attached. The President, however, is of opinion that the prevention of "overcrowding," in so far as this implies a limitation in the number of the occupants of rooms based merely upon their cubic capacity, can, at present, have little beneficial result; for in the great majority of the dwellings, whether lodging-houses or not, a reasonably pure air would not thereby be secured, on account of structural defects and insanitary surroundings. He considers also that the remedying of the prevailing structural defects of existing buildings urgently requires attention, and that the measures which are being carried out with this object in Calcutta, Bombay, and other cities and towns, should be continued and extended.

(*b.*) Collection and scrutiny of accurate death reports.

751. Another of the most important duties of all Medical Officers of Health, whether in the large cities or in the districts, will in our opinion be the duty of collecting accurate death reports. It ought to facilitate the introduction of sanitation into the villages and towns if provision were made that weekly reports of death should in all cases be sent in to the Civil Surgeon as Medical Officer of Health for his district, with a view to their being carefully scrutinised in order that the existence of plague or any other epidemic disease may be brought to light. In the event of any serious mortality being detected by this method in the villages or towns, we think the authorities at the Head Quarters of the Sanitary Department should be immediately informed, and that inquiries into the cause of the excess of mortality should be instituted without delay. For this purpose we think that the Assistant Surgeons whom we have proposed to attach to the vaccinating staffs might very appropriately be utilised.

(*c.*) Measures against plague and other infectious diseases should be undertaken with advice and under control of members of the Sanitary Department.

752. Again, one of the most important of the functions which we would propose to entrust to the Medical Officers of Health (including Civil Surgeons acting in this matter under the directions of the Sanitary Department) is the duty of advising with regard to all measures taken with a view to combating plague and other infectious disease. We consider that the functions of the Medical Officer of Health ought not to be limited to advising. We propose that the direct conduct of the operations which are undertaken should, so far as this does not involve an abdication of authority on the part of the Civil Officer, be entrusted either to the Municipal Health Officer or one of the Health Officers who are now styled Deputy Sanitary Commissioners, or to the Civil Surgeon in his capacity of District Health Officer. Where the local Medical Officer is, for any reason, unavailable for the duty, we suggest that another member of the Sanitary Department should be deputed to take charge of the operations.

In advocating that plague operations should be under the direct supervision of Medical Officers who are members of the Sanitary Department, or who are acting under instructions from the Sanitary Department, we are advocating a definite departure from the practice of committing the conduct of plague operations to Civil as distinguished from Medical Officers. We would, however, in connexion with this, point out that what we advocate is merely that the practice which has long been adopted in connexion with the vaccination operations directed against small-pox in India should be extended to the operations which are taking place against plague and other epidemic diseases. We are convinced that in the case of plague, just as in the case of small-pox, efficiency

can be secured only by placing the staffs and the general direction of the operations under the control of medical men who possess the necessary technical knowledge. While we are convinced of this, we, as we have indicated above, do not fail to recognise that, just as a veto is reserved for the civil administrator in cases where the undertaking of vaccination operations may involve political or financial considerations, so it will, on both these grounds, similarly be necessary in the case of plague operations, and we recognise that the veto will be more necessary in the case of plague than in the case of small-pox, where circumstances seldom call for its exercise. Subject to this necessary veto, and, of course, to the provision that the local Civil Authority must, with a view to exercising that veto, always be fully informed beforehand of the exact nature and extent of the operations which it is proposed to undertake, we think, as stated before, that the conduct of the operations ought in all cases to rest with the Sanitary Officer.

Among the things which impressed us most in India was, first, the utter absence of a uniform system of dealing with plague, and, secondly, the fact that very little of the experience which was gained in one place was rendered available for use in another. This absence of uniformity was seen not only in the case of general plague policy, but it was seen also, and even more glaringly, in connexion with matters of a purely technical character. We have referred in our survey of the disinfection operations to the futility of many of the methods which were adopted and to the technical errors which were repeated over and over again in scores of different places. And this was necessarily so, for the officers in charge of the plague operations in each place had to work out for themselves a system of disinfection quite regardless of the experience which had been gained in other places. While we have nothing but admiration for the zeal and energy which was everywhere displayed, we are of opinion that there was much misdirection of effort. Now this could in large part be remedied by securing that the direction of plague operations, and in particular the direction of technical matters in connexion with those plague operations, should be placed in the hands of an Imperial Sanitary Department.

Mr. Hewett and Mr. Cumine dissent from the above paragraph.

Mr. Hewett and Mr. Cumine do not concur with the above remarks. They do not agree with the recommendation made in this paragraph that the Medical Officer of Health of a Municipality or some other member of the Sanitary Department should, so far as this does not involve an abdication of authority on the part of the Civil Officer, invariably have the direct control of all measures undertaken to combat plague and other infectious diseases. They think it sufficient to point out that there may be—and in their opinion there often are—reasons which make it advisable to place in charge of the actual operations against plague an officer other than a Medical Officer.

(*d.*) Necessity for establishment of bacteriological laboratories.

753. So far we have dealt only with the functions of the administrative as distinguished from the scientific branch of the Sanitary Service. The functions of this last branch are in our opinion not less important in their way than the functions of the administrative branch. To this branch would naturally be committed the bacteriological diagnosis of doubtful cases of plague or of other epidemic disease, the conducting of experiments to determine the effect of various disinfectants and to determine the mode of life and history of the various pathogenic agents, the mode in which these effect an entrance into the organism, and many other questions of similar nature. Further, there would naturally be committed to this Department, the manufacture of anti-plague and anti-small-pox vaccine, and of all other vaccines which may prove themselves to be effectual in the prophylaxis of disease. The efforts of the Department might, we think, usefully be directed also to the endeavour to produce thoroughly effective sera for the therapeutic treatment of plague and other infective fevers and of snake poisoning.

We think that it might be well to concentrate the manufacture of vaccines and therapeutic sera and the special study of questions relating to these in a Central Institute of Public Health, which for climatic considerations ought, we think, to be situated somewhere in the Hills, in a place readily accessible by rail. Without this the utility of a central laboratory would be curtailed in many ways. Such a central laboratory in the Hills would, however, not be available either for clinical research or for the study of the conditions under which pathogenic micro-organisms exist in the different parts of India. Neither would it, on account of the distances to which material would have to be sent, be available for the bacterial diagnosis of doubtful cases of disease in different parts of the country. We are, therefore, of opinion that it is essential that Public Health laboratories should be established in the chief town of each of the Presidencies and Provinces, and these laboratories ought, we think, to be equipped,

not only with a view to their being utilised for directly practical purposes, but also with a view to their being utilised for purposes of research. These laboratories might with advantage be established in connexion with the Medical Schools of the selected towns, where they could be utilised also for teaching purposes.

Remarks by the President on the advisability of improving and extending teaching in Public Health.

754. In the opinion of the President this utilisation of the Public Health Laboratories would greatly increase their scientific usefulness, while the efficacy of the teaching carried on in them would be enhanced by the association with research work. The President has elsewhere* pointed out that insanitary conditions exist in the rural as well as urban districts which urgently call for improvement, and in his opinion these conditions are in great measure caused by want of information on the part of the people regarding the most simple and elementary requirements for healthy existence, with the result that, irrespective of deaths produced by infectious diseases, the general mortality from other preventible diseases is extremely high. As Native practitioners are brought into closer relationship with the people than European practitioners, the President, considering that they are thus more frequently in a position to give simple and elementary, but still valuable, instruction and advice to Natives in matters pertaining to healthy existence, and being of opinion that the recommendation above made to employ Assistant Surgeons in sanitary work would largely depend for its success upon the thoroughness of their education in Sanitation, deems that it is advisable to improve and extend the teaching of Public Health in the Medical Schools where the Native Medical Practitioners and Assistant Surgeons are educated. This might, the President thinks, best be done, and thereby much benefit conferred upon the people of India, by—

(*a*) restricting the Public Health teaching appointments in the Medical Schools to professors or teachers specially qualified in the subject;

(*b*) providing that the Public Health laboratories recommended to be established in each Presidency and Province should be attached to the chief Medical School of the Presidency and Province, and should be properly equipped not only for purposes of clinical and scientific research, but also for practical instruction in Public Health; and

(*c*) requiring that all candidates for medical degrees or licences should go through a course of effective practical instruction in one of these laboratories, in addition to the qualifying course of systematic lectures in Public Health elsewhere attended.

Organisation of the permanent staff of the Sanitary Department.

755. Our recommendations with regard to the organisation of the Sanitary Department are as follows:—

(1.) We recommend that all the members of the Sanitary Department should be borne on one list.

The provision that the members of the Sanitary Department should all be borne on one list would have great advantages in facilitating the training of Medical Officers of Health. Another advantage would be that it would be possible to transfer a member of the Sanitary Service, who had gained experience in connexion with a particular epidemic disease or a particular sanitary measure, to any locality where that experience might be required. A further advantage would be that stagnation would be avoided and that active and tactful Medical Officers of Health would find fresh stimulus to exertion in the knowledge that their work would come under expert scrutiny, and that promotion to a higher post would be open to them.

(2.) We recommend that all appointments in the Sanitary Department should be made by the Head of the Department, subject to the approval of the Local Government, and that all the members of the Sanitary Department, whether engaged on executive duties as Medical Officers of Health or on exclusively scientific or scientific and teaching duties relating to Public Health, should be placed directly under the orders of the Head of the Sanitary Department; and one or more inspecting Medical Officers of Health might, we think, be appointed to assist the Head of the Sanitary Department in his work of supervision.

In support of this recommendation, we point out that the organisation of all Medical Officers of Health under the orders of a single Head would ensure, first, that the experience gained in one place could be utilised in another; and, secondly, that all measures which were adopted throughout India for combating infectious diseases should be carried out with the same degree of efficiency, and should be informed by one and the same plan and purpose.

* *See* Appendix No. III.

Mr. Hewett and Mr. Cumine are unable to concur with the recommendation that the members of the Sanitary Department should all be directly under the orders of the Head of the Department. They see no objection to the officers employed exclusively on purely scientific duties being placed directly under the supervision and control of the Sanitary Commissioner with the Government of India on the understanding that they would be required to undertake enquiries at the direction of the Local Governments. The executive officers, however, of the Sanitary Service, whether employed as Sanitary Commissioners and Deputy Sanitary Commissioners—to use the designations hitherto used for them, which seem to be suitable—or as Health Officers within Municipalities should, in the opinion of Mr. Hewett and Mr. Cumine, be subject to the orders and control of the Local Governments, and not to those of any outside authority. The complete subordination of members of the Sanitary Service to the Local Government under which they may happen to be serving is not in any way inconsistent with their being placed on one list and being liable to serve in any part of India. The Government of India would place an officer at the disposal of a Local Government, and, until they removed him from its control, he would remain entirely under its authority. As Mr. Hewett and Mr. Cumine understand the proposals of the majority of the Commission, they would have the effect of taking away from the Local Governments the control over sanitary matters within the territories administered by them. Any curtailment of the powers of the Local Governments in these matters seems to Mr. Hewett and Mr. Cumine open to the greatest possible objection, as it would result in the centralisation of the control of measures adopted against epidemics in the hands of the Government of India, who have neither the leisure nor the local knowledge to carry out such control as efficiently as Local Governments can.

(3.) We recommend that the Head of the Sanitary Service should occupy the position of Sanitary Commissioner to the Government of India. In view of the new administrative duties, which we would propose to commit to this Officer, the majority of us think that the office of Sanitary Commissioner to the Government of India should be erected into a separate office, and that it should not, as at present, be held in conjunction with the Director-Generalship of the Indian Medical Service. We consider that, besides controlling the Sanitary Service and advising the Supreme Government on all matters relating to Public Health, the Sanitary Commissioner should inform himself, as far as necessary by personal visitation, regarding the insanitary conditions, sanitary action, and prevailing diseases in all parts of India; that he should review the work of the scientific sanitary establishments; and that he should prepare an annual report dealing with these subjects. In order that his duties may be efficiently performed, we also consider that he should be provided with an adequate Medical and Clerical Staff, and be supplied with reports bearing on the above-mentioned subjects, especially including reports on Sanitation and on Vital Statistics, from each of the Presidencies and Provinces.

Mr. Hewett and Mr. Cumine agree that if the Sanitary Commissioner with the Government of India is given all the duties suggested for him in this Chapter, it will be necessary to separate his office from that of the Director-General of the Indian Medical Service. They are, however, not satisfied that, if the position of the Sanitary Commissioner remains as it is now, the separation of his office from that of the Director-General of the Indian Medical Service will be necessary. Some advantage, they think, might indeed result from such separation, but the union of the two offices is economical, and this aspect of the question must not be overlooked.

(4.) In view of the importance of the functions of the Sanitary Department and of the Imperial interests which are involved, and in view of securing that absolutely independent advice on sanitary matters should be placed directly before Government in a country, where, owing to the absence of independent medical public opinion, too much weight is likely to be given to the possibility of administrative difficulties arising in the carrying out of sanitary measures, and finally, with a view to avoiding the delays incidental to the submission of proposals to Government through an intermediary, we recommend that the Head of the Sanitary Department should be made a Secretary to Government.

The President considers that, if questions of administration should raise important difficulties against the adoption of the above recommendation No. (4), however desirable it may be in the interests of sanitation, the following should be proposed as an alternative, viz. :—that the Head of the Sanitary Department should have the right, on all occasions in which he deems it expedient, personally to submit his proposals and

explain his views to the Member of His Excellency the Viceroy's Council dealing with matters relating to Public Health.

Mr. Hewett and Mr. Cumine see no reason to alter the position of the Sanitary Commissioner with the Government of India, which is that of adviser to the Supreme Government in sanitary matters. He has already free opportunities for stating his views both in writing and verbally to the Member of Council in charge of the Department dealing with Public Health, but for the adoption or rejection of those views the Supreme Government alone can be responsible.

THOMAS R. FRASER,
President.

J. P. HEWETT.

A. E. WRIGHT.

A. CUMINE.

M. A. RUFFER.

C. J. HALLIFAX, *Secretary.*

26th July, 1901.

APPENDIX No. I.

Statistics relating to certain Clinical Characteristics of Plague.

I.—General Case Mortality.

A.—Trustworthy Figures relating to Places in which all Cases and their Results probably came on to the Record.

Authority for Figures.	Place of Observation.	Number of Cases.	Number of Deaths.	Percentage Case Mortality.	Remarks.
Evidence, 10,330 -	Punjab - -	2,706	1,697	62·7	
„ 13,159 -	Palanpur - -	815	503	61·7	
„ 9539–40 -	Jawalapur - -	116	93	80·1	
„ App. LXXXVIII., Memorandum A.	Byculla Jail -	25	12	48	Uninoculated only.
Evidence, App. LIX.	Umarkhadi Jail -	16	9	56·2	Uninoculated only.
	Total - -	3,678	2,314	62·9	

B.—Fairly Trustworthy Figures, probably affected to some extent by the Omission of Mild Cases, while Severe and Fatal Cases have generally been brought on the Record.

Authority for Figures.	Place of Observation.	Number of Cases.	Number of Deaths.	Percentage Case Mortality.	Remarks.
Evidence, 14,214 -	Ankleshwar Town -	331	245	74	
„ 14,214 -	Ankleshwar Villages	96	60	62	
„ 14,637 -	Baroda City - -	1,582	1,304	82	Uninoculated only.
„ II. 517, App. R.	Baroda Villages -	1,118	853	76	The principal villages only.
„ 15,424–6 -	Baroda Cantonment -	119	94	79	Uninoculated only.
„ 14,482 -	Bhiwandi - -	1,788	1,255	70	
„ 13,692 -	Porbandar - -	465	357	77	
„ III., 90 -	Igatpuri - -	512	411	80	
„ III., 90 -	Ilkal - -	284	238	84	
„ II., 233 -	Umreth - -	110	91	83	
„ 12,649 -	Kotri - -	100	68	68	
	Total - -	6,605	4,976	75·3	

C.—Hospital Figures, which do not take account of all Severe Cases (such as those resulting in death before Discovery by the Authorities) or of all Mild Cases.

Authority for Figures.	Hospital.	Number of Cases.	Number of Deaths.	Percentage Case Mortality.	Remarks.
Evidence, III., 457 -	Bombay, Arthur Road	3,919	2,800	71	
Gatacre's Report, p. 111; Campbell's Report, p. 134.	Bombay, Grant Road	1,575	1,177	74	
Gatacre's Report, p. 48; Campbell's Report, p. 115.	Bombay, Parel -	1,296	684	52	
Hospital Report for period from 1/6/97 to 31/3/98.	Poona General Hospital	4,179	2,836	67	
Evidence, 25,024, 25,027.	Bangalore and Mysore	3,834	2,505	65	Uninoculated only.
	Total - -	14,803	10,002	67·5	

Dr. N. H. Choksy, of the Arthur Road Hospital, Bombay, after collecting the records of about 11,600 cases in the various Plague Hospitals of Bombay, arrives at the conclusion (Bombay Medical and Physical Society, September 1900) that in 1896–97 the average hospital case mortality ranged from 61·53 to 68·28 per cent. in different hospitals; that in 1897–98 it ranged from 78·55 to 79·26 per cent.; that in 1898–99 it ranged from 78·97 to 81·40 per cent; and that in 1899–1900 it ranged from 79·54 to 80·95 per cent. From these figures, by a process which is not plain to us, Dr. Choksy concludes that "the normal mortality rate of plague in our public hospitals may safely be put down at 80 per cent."

II.—General Case Mortality as Influenced by Sex.

Authority for Figures.	Place of Observation.	Male.			Female.		
		Cases.	Deaths.	Case Mortality per cent.	Cases.	Deaths.	Case Mortality per cent.
Gatacre's Report, pp. 48, 52, 111, 124 - - - Campbell's Report, pp. 139, 141, 151 - - - Evidence, III., 459 - -	Bombay Hospitals	5,047	3,664	74·5	1,455	1,202	82·6
Hospital Report for period from 1/6/97 to 31/3/98 - -	Poona General Hospital.	1,705	1,244	72·7	1,697	1,152	60·6
Evidence, II., 424 - -	Karachi - -	2,174	1,707	78	1,064	828	77·5
„ 13,159 - -	Palanpur -	290	188	64·7	382	238	62·3
„ II., 484 - -	Porbandar -	91	63	69·2	113	62	55
„ 15,645 - -	Surat - -	78	57	73	36	26	72·2
„ 12,649 - -	Kotri - -	65	43	66·2	35	25	71·5
„ 14,047 - -	Umreth - -	64	52	81·1	46	39	85
„ I., 474 - -	Calcutta - -	152	126	83	38	29	76·5
	Total - -	9,666	7,144	73·9	4,866	3,601	74·0

Dr. N. H. Choksy, of the Arthur Road Hospital, Bombay (Bombay Medical and Physical Society, September 1900), gives the following tabular statement of observations made in the Bombay Plague Hospitals, in 6,000 cases (more than 5,000 being cases of Hindus), regarding the influence of sex and age on case mortality:—

Case mortality per cent. in Bombay hospital cases.

Class.	Males.	Females.	Children.	Total.
Hindus - - - -	79·71	77·47	69·57	75·33
Muhammadans - - - -	64·76	78·04	58·82	65·73
Native Christians - - -	63·77	68·36	58·97	64·34

III.—General Case Mortality as Influenced by Age.

Authority for Figures.	Place of Observation.	Age Periods.	0–5.		5–10.		10–20.		20–30.		30–40.		40–50.		50–60.		Over 60.	
		Total Cases observed.	Cases.	Case Mortality per cent.	Cases.	Case Mortality per cent.	Cases.	Case Mortality per cent.	Cases.	Case Mortality per cent.	Cases.	Case Mortality per cent.	Cases.	Case Mortality per cent.	Cases.	Case Mortality per cent.	Cases.	Case Mortality per cent.
Evidence, III., 459 -	Bombay, Arthur Road Hospital.	4,000	57	57·8	188	66·4	714	66·6	1,333	70·5	1,028	69·2	414	83	168	86·9	98	85·7
Gatacre's Report, p. 111; Campbell's Report, p. 140.	Bombay, Grant Road Hospital.	1,230	34	70·5	99	80	249	74·2	457	73·6	231	80·9	104	80	40	75	16	43·7
Evidence, 10,330 - -	Punjab - - - -	2,706	199	68·3	310	59·6	634	57·8	599	57·5	420	64·7	268	69·7	190	74·2	86	62·7
„ 12,343 - -	Karachi Hospital -	515	21	33·3	57	65	100	60	173	54·9	93	60·2	42	64·3	20	80	9	55·6
	Total of above -	*8,451*	*311*	*64*	*654*	*68*	*1,697*	*64*	*2,562*	*67*	*1,772*	*69*	*828*	*77*	*418*	*79*	*209*	*76*

															50 and over.			
Campbell's Report, p. 156	Bombay, Mahratta Hospital	650	11	63·6	67	74·6	127	78	276	85·8	108	90·7	32	84·3	29		86·2	
Evidence, 12,649 - -	Kotri - - -	100	4	0	14	71	35	57	19	78	13	84	8	75	7		43	

		Age Periods.	0–10.			10–20.		20–30.		30–40.		40–50.		50–60.		Over 60.	
Evidence, II., 200 -	Palanpur - - -	815	96		52	210	58·2	186	59·2	148	62·2	92	75	61	72	22	72·7
„ II., 233 - -	Umreth - - -	110	19		73·7	22	95·6	22	82	19	89·5	20	75	8	75	—	—

		Age Periods.	0–5.		5–10.		10–20.		20–40.			40–60.			Over 60.	
„ I., 474- - -	Calcutta - - - -	183	6	50	8	87·5	39	66·7	81		86·5	36		89	13	77

		Age Periods.	0–5.		5–15.		15–25.		23–35.		35–45.		45–55.		Over 55.	
„ II., 484 -	Porbandar - - -	465	14	57	128	69	113	73·5	90	86·7	51	78·6	41	85·3	28	89·2

IV.—Relative Frequency of Bubonic and Non-Bubonic Plague.

Authority for Figures.	Place of Observation.	Total Cases Observed.	Bubonic Cases.		Non-bubonic Cases.					
					Pneumonic.		Septicæmic.		Total	
			No. Observed.	Per cent. to Total Cases observed.	No. observed.	Per Cent. to Total Cases observed.	No. observed.	Per Cent. to Total Cases observed.	No. observed.	Per Cent. to Total Cases observed.
Evidence, III., 458	Arthur Road Hospital, Bombay.	2,723	2,463	90·5	110	4	150	5·5	260	9·5
Campbell's Report, p. 138.	Grant Road Hospital, Bombay.	798	671	84	88	11	39	5	127	16
Campbell's Report, p. 141.	Mody Khana Hospital, Bombay.	724	535	74	143	20	46	6	189	26
Campbell's Report, p. 150.	Jain Hospital, Pinjrapole.	510	435	85	32	6·5	43	8·5	75	15
Evidence, 9539–40–3.	Jawalapur -	116	81	70	19	16	16	14	35	30
Total - - -		4,871	4,1$_{8}$5	86	392	8	294	6	686	14
Hospital Report for period from 1/6/97 to 31/3/98.	Poona General Hospital.	4,146	3,152	76	—	—	—	—	994	24
Evidence, 10,198 -	Punjab - -	3,128	2,608	83·5	—	—	—	—	520	16·8
Grand Total - -		12,145	9,945	81·9	—	—	—	—	2,200	18·1

Dr. N. H. Choksy, of the Arthur Road Hospital, Bombay (Bombay Medical and Physical Society, September 1900), classifies 11,600 cases of plague observed in the Plague Hospitals of Bombay as follows:—

Type of cases.	Per cent. to total cases observed.
Simple bubonic cases (including cases of pestis minor) - - - - -	78·15
Septicæmic cases - - - - - - - - -	14·25
Pneumonic cases - - - - - - - - - -	4·10
Atypical and mixed cases (and cases with necrosis of the skin after the development of a specific plague rash) - - - - - - -	3·50

V.—Relative Frequency of Bubonic and Non-Bubonic Plague as Influenced by Sex.

References same as in Table IV.

Place of Observation.	Males.				Females.			
	Total Cases.	Percentage.			Total Cases.	Percentage.		
		Bubonic.	Pneumonic.	Septicæmic.		Bubonic.	Pneumonic.	Septicæmic.
Grant Road Hospital, Bombay	529	83	11	6	269	85·5	11·5	3
Mody Khana Hospital, Bombay.	491	75	19	6	233	72	20·5	7·5
Jain Hospital, Pinjrapole, Bombay.	417	84	9	7	93	89	4·5	6·8
Poona General Hospital	1,724	73	27		1,605	81	19	

VI.—Case Mortality as Influenced by the Bubonic or Non-Bubonic Form of Plague

References.	Place.	Total Number of Cases observed.	Case Mortality, Per Cent. in							
			Bubonic Cases.		Non-bubonic Cases.					
					Pneumonic.		Septicæmic.		Total.	
			No. of Deaths.	Case Mortality per cent.	No. of Deaths.	Case Mortality per cent.	No. of Deaths.	Case Mortality per cent.	No. of Deaths.	Case Mortality per cent.
Evidence, III., 458	Arthur Road Hospital, Bombay.	2,723	1,730	70·2	105	95·4	102	68·0	207	79·5
Campbell's Report, p. 138.	Grant Road Hospital, Bombay.	798	569	85	87	98·8	39	100	126	9
Campbell's Report, p. 141.	Mody Khana Hospital, Bombay.	724	399	75	123	86	32	69·5	155	82
Campbell's Report, p 150.	Jain Hospital, Pinjrapole.	510	213	72	25	78	43	100	68	91
Evidence, 9539–40–3.	Jawalapur - -	116	59	72·7	19	100	15	94	34	92
Total - - -		4,871	3,070	73·3	359	91·5	231	78·5	590	86·0
Hospital Report for period from 1/6/97 to 31/3/98.	Poona General Hospital.	4,146	2,102	66·6	—	—	—	—	734	74
Evidence, 10,198 -	Punjab - -	3,128	1,509	57·4	—	—	—	—	366	70
Grand Total - -		13,145	6,681	67·2	—	—	—	—	1,690	77

Dr. N. H. Choksy, of the Arthur Road Hospital, Bombay (Bombay Medical and Physical Society, September 1900), records the following summary of observations made in the Bombay Plague Hospitals from 1896–97 to 1899–1900:—

Type of Plague.	Number of Cases Observed.	Case Mortality per cent.
Bubonic - - -	9,500	77·25
Pneumonic - -	514	96·69
Septicæmic - - -	1,562	89·62

VII.—Relative Frequency of Buboes, in Cases of Bubonic Plague, in various Situations, with the Case Mortality associated with the Situation of the Buboes.

(1.)—*Arthur Road Hospital, Bombay.*

(*See* Evidence, Vol. III., p. 458.)

Situation of Bubo.	Number of		Percentage of	
	Cases.	Fatal Cases.	Cases to Total Bubonic Cases.	Case Mortality.
Head and neck - - - -	240	178	9·7	74
Axilla and upper extremities - -	439	322	17·8	73
Groin and lower extremities - - -	1,672	1,156	67·9	69
More than one bubo in different parts of the body.	114	75	4·6	66
Total - - -	2,465	1,731	100·0	70·2

(2.)—*Punjab.*

(*See* Evidence, 10,198.)

Situation of Bubo.	Number of		Percentage of	
	Cases.	Fatal Cases.	Cases to Total Bubonic Cases.	Case Mortality.
Head and neck	340	212	13·0	62·3
Axilla	343	218	13·1	63·2
Groin	1,735	957	66·6	55·4
More than one situation	139	86	5·3	61·8
Situations other than the neck, axilla, or groin	52	36	2·0	69·2
Total	2,609	1,215	100·0	46·5

(3.)—*Karachi.*

(*See* Evidence, 12,394.)

Situation of Bubo.	Number of		Percentage of	
	Cases.	Fatal Cases.	Cases to Total Bubonic Cases.	Case Mortality.
Neck	45	23	12·2	51
Axilla	60	34	16·3	56·6
Groin	245	131	66·6	53·5
Multiple	18	12	4·9	66·7
Total	368	200	100·0	54·3

Dr. N. H. Choksy, of the Arthur Road Hospital, Bombay (Bombay Medical and Physical Society, September 1900), analyses 9,500 cases of bubonic plague observed in the Plague Hospitals of Bombay City as follows:—

Situation of Bubo.	Percentage of Cases to Total Bubonic Cases Observed.	Case Mortality per cent.
Cervical	6·72	78·87
Parotid	1·68	70·34
Axillary	21·85	81·29
Femoral	30·87	72·56
Inguinal	23·25	77·62
Multiple	12·95	75·87
Other situations	1·68	71·42

VIII.—Relative Frequency and Fatality of Bubonic Plague as Influenced by the Situation of the Buboes and by Sex and Age.

Karachi, 12,394.

Situation of Bubo.	Men.				Women.				Children.			
	Number of Cases				Number of Cases				Number of Cases			
	Observed.	Fatal.	Case Mortality per cent.	Per Cent. of Cases with Buboes in each situation to Total Cases observed in Men.	Observed.	Fatal.	Case Mortality per cent.	Per Cent. of Cases with Buboes in each situation to Total Cases observed in Women.	Observed.	Fatal.	Case Mortality per cent.	Per Cent. of Cases with Buboes in each situation to Total Cases observed in Children.
Neck	13	9	69	6·2	11	6	54·5	12·8	21	9	42·8	28·4
Axilla	29	17	59	14·0	15	10	66·6	17·4	16	12	75·0	21·6
Groin	156	81	52	75·0	57	26	45·7	66·3	32	19	59·4	43·3
Multiple	10	7	70	4·8	3	1	33·3	3·5	5	3	60	6·7
Total	208	114	55	—	86	43	50	—	74	43	58·1	—

IX.—Frequency and Fatality of Plague with Bubonic Enlargements in the Head and Neck.

(1.) *When the Buboes are single.*

Buboes.	Situation.			Percentage of Cases to Total Number (2,465) of Bubonic Cases observed.	Number of Fatal Cases.	Case Mortality.		
	Right.	Left.	Total.			Right. Per cent.	Left. Per cent.	Total.
Cervical	67	29	96	3·89	69	68·6	79·3	71·8
Parotid	12	20	32	1·30	20	58·3	65	62·5
Sub-maxillary	19	9	28	1·13	21	68·4	88·8	75
Occipital	—	—	2	·08	1	—	—	50
Total	—	—	158	6·41	111	—	—	70·2

(2.) *When there are more than one Bubo.*

Buboes.	Situation.			Percentage of Cases to Total Number (2,465) of Bubonic Cases observed.	Number of Fatal Cases.	Case Mortality.		
	Right.	Left.	Total.			Right. Per cent.	Left. Per cent.	Total.
Cervical + parotid	10	18	28	1·13	22	80	77·7	78·5
Double cervical + parotid	—	—	5	·20	5	—	—	100
Cervical + parotid + sub-maxillary.	—	—	6	·24	4	—	—	66·6
Cervical + sub-maxillary	—	—	12	·48	10	—	—	83·8
Double cervical	—	—	20	·81	19	—	—	95
Double parotid	—	—	11	·44	7	—	—	63·6
Total	—	—	82	3·33	67	—	—	81·7

X.—Frequency and Fatality of Plague with Bubonic Enlargements in the Axilla and Upper Extremities.

(1.) *When the Buboes are single.*

Buboes.	Situation.			Percentage of Cases to Total Number (2,465) of Bubonic Cases observed.	Number of Fatal Cases.	Case Mortality.		
	Right.	Left.	Total.			Right. Per cent.	Left. Per cent.	Total.
Axillary	240	162	402	16·31	298	72·5	76·5	74·1
Pectoral	5	4	9	·36	6	60	75	66·6
Supra Trochlear	4	3	7	·28	4	50	66·6	57·1
Total	—	—	418	16·96	308	—	—	73·7

(2.) *When there are more than one Bubo.*

Buboes.	Situation.			Percentage of Cases to Total Number (2,465) of Bubonic Cases observed.	Number of Fatal Cases.	Case Mortality.		
	Right.	Left.	Total.			Right. Per cent.	Left. Per cent.	Total.
Double Axillary - -	—	—	5	·20	4	—	—	80
Axillary + Supra Trochlear + Brachial.	8	8	16	·65	10	50	75	62·5
Total - -	—	—	21	·85	14	—	—	66·6

XI.—Frequency and Fatality of Plague with Bubonic Enlargements in the Groin and Lower Extremities.

(1.) *When the Buboes are single.*

Buboes.	Situation.			Percentage of Cases to Total Number (2,465) of Bubonic Cases observed.	Number of Fatal Cases.	Case Mortality.		
	Right.	Left.	Total.			Right. Per cent.	Left. Per cent.	Total.
Femoral - - -	365	310	675	27·40	429	64·3	62·5	63·5
Inguinal - -	180	180	360	14·60	246	69·4	67·2	68·3
Iliac - - -	4	7	11	·44	9	75	85·7	81·8
Popliteal - -	4	4	8	·32	4	50	50	50
Total - -	—	—	1,054	42·76	688	—	—	65·3

(2.) *When there are more than one Bubo.*

Buboes.	Situation.			Percentage of Cases to Total Number (2,465) of Bubonic Cases observed.	Number of Fatal Cases.	Case Mortality.		
	Right.	Left.	Total.			Right. Per cent.	Left. Per cent.	Total.
Femoral + inguinal - -	137	191	328	13·31	250	74·4	77·4	76·2
Femoral + inguinal + iliac	77	44	121	4·92	91	72·8	79·5	75·2
Femoral + iliac - -	33	29	62	2·52	47	75·7	75·8	75·8
Inguiral + iliac - -	40	33	73	2·96	57	79·5	78·7	78·0
Double femoral + inguinal -	—	—	12	·48	7	—	—	58·3
Double femoral - -	—	—	9	·36	7	—	—	77·7
Double inguinal - -	—	—	7	·28	7	—	—	100
Double femoral + double iliac.	—	—	4	·16	1	—	—	25
Double iliac - - -	—	—	2	·08	1	—	—	50
Total - -	—	—	618	25·07	468	—	—	75·7

XII.—Frequency and Fatality of Plague with several Buboes in different Situations.

Buboes.	Number.	Per Cent. of Cases to Total Number (2,465) of Bubonic Cases observed.	Number of Fatal Cases.	Case Mortality per cent.
Femoral + axillary + inguinal - - -	24	·98	13	54·1
Femoral + cervical + parotid - - -	15	·60	12	80
Femoral + iliac + cervical - - -	13	·53	8	61·5
Femoral + axillary - - - - -	11	·44	6	54·5
Axillary + parotid - - - -	8	·32	7	87·5
Femoral + parotid - - - - -	7	·28	5	71·4
Femoral (right) + inguinal (left) - -	6	·24	5	82·6
Femoral + iliac + parotid - - - -	6	·24	4	66·6
Cervical + sub-inguinal - - - -	4	·16	4	100
Axillary + parotid + double supra trochlear -	4	·16	3	75
Femoral + inguinal + cervical - - -	4	·16	2	50
Axillary + inguinal - - - -	4	·16		50
Femoral + inguinal + cervical + axillary + brachial.	3	·13	2	66·6
Femoral + inguinal + axillary + supra trochlear	2	.08	1	50
Double parotid + supra trochlear - - -	2	·08	1	50
Inguinal + cervical - - - -	1	·04	0	0
Total - - -	114	4·62	75	65·8

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APPENDIX No. II.

DESCRIPTION OF THE CLINICAL FEATURES OF PLAGUE BY THE PRESIDENT.

I.—Introductory.

1. The present epidemic of plague in India has been characterised not only by a high mortality, but also by a great diversity in the symptoms of individual cases; and in both respects it has reproduced the most conspicuous features of the numerous epidemics of this disease which from a remote period have devastated extensive districts of Asia, Africa and Europe, under the names of the Black Disease, Black Death, Pest, Septic Pest, Plague, and Oriental, Levantine or Egyptian Plague. In these and in all other important respects it has also exhibited a close resemblance to the previous epidemics of plague in India, regarding whose characteristics details are in existence, such as the epidemics of Cutch, Gujarat, Sind, Kathiawar, Marwar, Kumaun and Garhwal.

Thesis on the Nature and History of Plague as observed in the North Western Provinces of India, by Frederick Forbes M.D., 1840; Transactions of the Medical and Physical Society of Bombay, Vols. and 2, 1838, 183 Papers by Whyte McAdam, Gilder and Forbes.

II.—Clinical Varieties or Types of Plague.

2. The diversity in the symptoms has led to numerous types or groups being recognised, mainly on the bases of (a) the severity of the symptoms, and (b) the prominence of one or several of them. Either basis might well be adopted for purposes of classification, but having regard to the present state of knowledge of the essential or real cause of the disease, which indicates its oneness under whatever symptomatic modifications it appears, the simplest subdivision would probably be obtained by adopting the basis of the severity of the illness. From this, however, an exception may conveniently be made in the case of prominent and early existing lung inflammation, as this represents a type having special interest in regard to the infectiveness of plague as well as to the application of measures for controlling its extension. At the same time, it is to be recognised that as plague produces pneumonia by the same virus or toxin as causes all its other manifestations, inflammation of the lungs may and does often occur, though to less extent, in other types of plague, and the above or any other subdivision of the disease is to be justified only on the grounds of convenience.

Adopting, therefore, as a fundamental basis of classification that of the degree of severity of the disease, and giving effect in this classification to the most formidable and conspicuous of the variations in its manifestations, the following types may for convenience be recognised, viz.:—

1. *Pestis minor*, the mild or phantom plague, including the abortive, atypical and so-called ambulatory forms.
2. *Pestis major*.—The disease presented in its most usual full development, the standard type, having many general and local symptoms, and especially glandular enlargements, elevation of temperature and great prostration; always a virulent affection, and occasionally so much so that death is caused before all of the usual manifestations of the type have had an opportunity of being produced, as in the so-called Septicæmic plague or Pesticæmia.
3. *Pestis pneumonica*, resembling in many respects the more severe form of the second group, but especially characterised by the early appearance of inflammation of the lungs.

In India, *Pestis minor* and *major* are often grouped together as Bubonic plague, nd Septicæmic and Pneumonic plague considered as a second and third group, espectively.

3. *Pestis minor*.—In this the mildest type of plague, the patient suffers from headache and sleeplessness, from slight pyrexia, lasting only a few hours, from tenderness and pain over one or more superficial glands, usually those in the groin, which are frequently enlarged, and occasionally also from nausea and vomiting. The duration may be as brief as only two or three days, but not infrequently the enlargement of the glands is more persistent, and the duration on this account, or because suppuration has taken place in the glands, is protracted to 10, 20, or even 30 days. All cases terminate in recovery. The patient is rarely confined to bed, and if so, only for a day or two, and the general disturbance of health is often so unimportant that not only can the patient go about, but even ordinary avocations are not interrupted. As

20,556.

10,365-8; 13,0 22,349.

7638–44; 24,117–9; 27,143–5; 26,989; 10,098–103; Inglis' Report, p. 13.

Captain T. E. Dyson, I.M.S., Couchman's Account of Plague Administration in the Bombay Presidency, p. 244; Captain C. H. James, I.M.S., Report on the Outbreak of Plague in the Jullundur and Hoshiarpur Districts of the Punjab, 1897–8, pp. 3, 4, 10; 10,334; 14,667; 15,226; 24,117; Choksy's Report on Plague, p. 16. 24,117. Gatacre's Report, p. 137; James' Report, p. 10; 10,335; Report on Bubonic Plague, by N. H. Choksy, p. 16. 18,064–5. 21,951–7; 21,967–74.

patients suffering from this type of plague rarely place themselves under medical treatment, there are few facts bearing upon its infectivity, but the endemic or epidemic character of *Pestis minor* and the progressive increase of cases, as well as the fact that the bacillus of plague has been found in the buboes and in pus and other discharges from them, leaves little room to doubt that it is capable of propagation from infected persons or things. Cases occur not only during the prevalence of virulent forms of plague, but also before these forms have become general, and during the decline or at the end of outbreaks. While, therefore, cases of *Pestis minor* are of relatively little importance to the affected persons, they are of great significance in the application of plague measures. Their occurrence in the absence of an epidemic of virulent plague may be the prelude to an ordinary epidemic, while their occurrence at the termination of such an epidemic may serve as an indication of the danger of recrudescence.

It is probable that *Pestis minor* may also occur without pyrexia, for cases have been observed in which buboes and slight general plague symptoms were present without any evidence of a rise of temperature. Further, the existence of a curious group of symptoms, which probably represents a variety of *Pestis minor*, has been brought before our notice by Colonel Fawcett and Captain Morgan, R.A.M.C. The symptoms were observed by them among Europeans engaged in plague work, who, about half an hour after handling plague patients, suffered from painful tingling and numbness in the hands, which extended to the lymph glands of the axilla, neck or groin, the glands becoming extremely painful, but being neither swollen nor tender. The pains generally disappeared in a few hours. In persons exposed at long intervals to plague infection the attacks were apt to recur, but they failed to do so in persons continuously engaged in plague operations. It may be noted that both of these probable varieties of *Pestis minor* were illustrated in Members of the Commission during their visit to India.

12,897; 20,684–5.

4. *Pestis major*.—In this form of plague the typical symptoms are sometimes produced so quickly that no premonitory symptoms are observed. In most cases, however, a prodromal or preliminary stage may be distinguished of several hours' duration. In this stage, the first indications of illness may be bilious vomiting and bilious diarrhœa. More frequently, the patient suffers from headache, which is usually frontal; sleeplessness, giddiness, and palpitation are often experienced; nausea and vomiting may be present; and there are pains in the limbs, lethargy and weakness. The gait may be unsteady and even staggering. The patient is either unable or indisposed to reply to questions. The mental processes are slow, and the appearance is suggestive of mild drunkenness. After these symptoms have continued for a few hours, rarely for so long as 24 hours, and while several of them are still present, one or more rigors, usually severe and prolonged, occur, and on examination it is found that a febrile temperature has been developed, that the pulse and respirations are rapid, that the skin is hot and dry, and that a bubo or buboes are present, which are painful and tender. The time of the first appearance of the buboes is either early and concurrently with the first symptoms, or from the second to the fourth day and rarely so late as the eighth day. The patient now lies in an apathetic and listless condition, with the face flushed, the eyes half closed, congested, yellow and sunk, and the pupils either dilated or in their normal state, unless contraction has been produced by conjunctivitis or by inflammation of deeper structures in the eyeball. The posture is distinctive of prostration and helplessness, and, usually, the patient lies on the back with the legs extended, and frequently with one of them crossed over the other, unless flexion at the hip has been caused by an exceptionally tense infiltration at the groin. Occasionally, however, the patient lies flaccid on one side, with both legs flexed and drawn up towards the abdomen, and the head bent forwards and downwards; but the posture is often varied by the mental condition of the patient and the accident of the position in which he has been placed by the attendants. Occasionally there is delirium, and the appearance of tiredness and apathy is replaced by that of great anxiety and even of terror. Sleeplessness, varied in some patients by a condition of drowsiness, is present; the speech is husky, slow, and even syllabic; but in a few instances questions were answered in an irritable and angry tone, as if the patient were annoyed at being disturbed. In a considerable number of cases, the power of articulation becomes further weakened and even aphonia is produced. The tongue is dry with red tips and edges, and with the dorsum covered, at the earlier stages, with a grey fur, which later is yellowish brown, and even black. The bowels are generally constipated, there is thirst, and usually the appetite is moderately good. A slight but offensive and distinctive odour may be perceived, independent of suppuration, and especially marked

12,398.

Proceedings, Vol. I., Appendix VIII., p. 352.

0,666–7.

2,897.

79.

2,897.

946; 12,897; Gatacre's Report, p. 58.

in severe cases. In a considerable number of cases the urine throughout the acute stages of the illness contains albumen in small quantity, as well as hyaline and epithelial tube-casts. Should the course of the illness be a favourable one, in from two to five days the temperature gradually falls, the skin becomes soft and moist, tenderness and pain depart from the buboes and they diminish in size, the pulse becomes slower and stronger, and the patient acquires a brighter aspect; but complete restoration to health is usually tardy and may be deferred until many months after the acute symptoms have disappeared.

When, however, as generally happens, the termination is not by recovery, death may be caused by sudden failure of the heart's action, or more rarely, during a protracted illness by a general weakness, often due to long continued suppuration of one or more buboes. 20,925.

This, which represents the usual or standard symptomatology of *Pestis major*, is frequently modified in two great respects: firstly, by the occurrence of symptoms not mentioned in the above description, and secondly, by variations in the severity of the illness.

In the present series of outbreaks in India, the added symptoms and the differences in their intensity have apparently produced all of the varieties of plague reported in the several epidemics which have occurred in other countries from the earliest epochs to the present time. For the symptoms of *Pestis major* have been modified sometimes so conspicuously as to suggest, as in the case of epidemics elsewhere, numerous further subdivisions of plague, by the occurrence of bowel complaints, ranging from simple to dysenteric diarrhœa; by prominent broncho-pneumonic complications; by skin affections, varying from vesicular and pustular eruptions to necrosis of the skin; by cellulitis and gangrene; by eye affections, implicating not only the superficial but also the deeper structures, and occasionally leading to loss or permanent impairment of visual power; by lymphangitis; by hæmorrhages in the skin and from the stomach, bowels, lungs, or kidneys; and by symptoms referable to the nervous system, such as peripheral neuritis and irritative and paralytic lesions of the nerve-centres, leading to convulsions, hemiplegia, paraplegia, coma or insanity. 16,818; Choksy's Report on Bubonic Plague, p. 37.

As to the modifications produced by the variations in the severity of the symptoms, the evidence placed before us has rendered it clear that these symptoms may not only be more restricted and less severe than in the standard type, but also that they may be more restricted and more severe. Cases have occurred which, on the one hand, as it were, together constitute ascending series from mild forms of *Pestis minor* to standard forms of *Pestis major*, and, on the other hand, which much transcend in severity the average or standard type of the disease. In illustration of the former modification, reference may be made to the occurrence, at all stages of outbreaks, of cases which though relatively mild were so in different degrees, and to the occurrence of outbreaks with a large proportion of non-fatal cases.

5. *Pestis Septicæmica.*—Cases transcending in severity the average type of *Pestis major* are exemplified in the numerous instances in which patients have exhibited one or several of the additional symptoms which have been mentioned. They have, however, been more conspicuously apparent in a considerable number of severe cases in which, instead of increase, there has been restriction in the number of symptoms. In these cases, without any premonitory symptoms, rigors, pyrexia, vomiting, cardiac weakness, and great nervous and muscular prostration have quickly appeared, and life has frequently become extinct within a few hours, although in several instances death did not occur until the second, third, fourth, and even fifth day. Such cases are usually termed septicæmic, and those which end in death within a few hours have been further distinguished as fulminant or lightning plague, or *Pestis siderans*. The distinctive characters of Septicæmic plague are not, however, clearly defined. The absence of sufficient definition has been illustrated in tabular statements in which plague cases have been classified as Bubonic, Pneumonic and Septicæmic, and still a fourth class added of "neither Pneumonic nor Bubonic," in which the severity of the disease may nevertheless be so great as to produce a mortality of 90 per cent. Apart from certain features in its morbid anatomy, which by a few observers have been considered sufficient to establish it as a separate variety, the term has most frequently been applied to all cases other than pneumonic where the symptoms were of exceptional severity. In some of the described cases, external buboes, similar to those of *Pestis major*, were present; in others there were no buboes; but in all cases, the lymphatic glands were involved, even although no external buboes were present. The history of the cases of so-called Septicæmic plague suggests a distribution throughout the body Proceedings, Vol. I., Appendix XIV., p. 381. R. N. Jadhav's Report, p. 14. Proceedings, Vol. I., Appendix XIII., p. 368; 26,585.

of virulent plague virus, more rapidly effected than in the cases representing the standard type of *Pestis major*.

6. *Pestis Pneumonica: Primary Pneumonic Plague.*—The pneumonic form of plague is of great interest in reference to the extension of plague and the efficacy of the measures for its control. Although well known in many previous epidemics of plague, it appears to have been first recognised in the present Indian epidemic by Captain Childe, I.M.S., who found that coincidently with the increased death rate from plague in Bombay there remained a large and unsatisfactorily explained increase, which had been assigned to remittent fever and diseases of the respiratory organs. Further investigations showed that many of these cases were actually cases of plague, in which the disease manifested itself chiefly by lobular inflammation of one or both lungs, and in which the true nature of the affection was demonstrated by the discovery of plague bacilli in the sputum.

Proceedings, Vol. I., Appendix XIII., p. 375.

It has now been established that in a considerable number of cases of plague the chief manifestation of the affection is inflammation of the bronchi and of the lobules of the lung, and that in the majority of these cases one of the most significant of the plague symptoms, viz., inflammatory swelling of the external lymph glands, is generally absent. The pneumonic type of plague is characterised by an abrupt development of symptoms, and especially of symptoms indicative of inflammation of the lungs. Early in the disease, the respirations become rapid and laboured; cough, frequently with slight pain in the chest, is present; and the sputum, though occasionally scanty, is generally abundant, frothy and watery, and coloured with blood either in red specks and streaks, or uniformly with a rosy but not rusty appearance. The sputum, therefore, rarely possesses the special characteristics of croupous pneumonia. Physical examination generally shows circumscribed areas of consolidation in detached portions of one or both lungs, but in some cases the consolidation is stated to have involved an entire lobe or even the whole of one lung; and rhonchi and fine and medium crepitations are audible on auscultation. Nearly all of the patients died within a few hours of the commencement of the illness, and but few survived until after the second or third day. In the latter event, the involvement of the lymphatic system, which is so characteristic of plague, has occasionally been made manifest before death by the appearance of gland-swellings in the neck or axilla, or in the sub-clavicular or upper mammary region, or even in the groin. As pneumonic plague has generally been most prevalent during the middle of outbreaks, it may sometimes become an important element in causing the high mortality which occurs at those times.

Gatacre's Report, p. 98.

Proceedings, Vol. I., Appendix XIV., p. 381.

12,915; 13,104; 15,173; 18,204; Proceedings, Vol. I., Appendix VIII., p. 352.

7. *Relationship to each other of the several Types of Plague.*—The intimate relationship which exists between these types of plague is shown by the circumstances that the standard bubonic type, or *Pestis major*, may pass into the Septicæmic, and in the greatest number of fatal cases actually does so; that inflammation of the lungs not only occurs so conspicuously and early in plague as to become the fundamental condition of the pneumonic type, but also constitutes one of the most frequent of the complications of *Pestis major;* and that any one type does not reproduce itself only, but also other types, as when the bubonic produces septicæmic and pneumonic, when the septicæmic produces bubonic and pneumonic, and when the pneumonic produces bubonic and septicæmic cases.

Proceedings, Vol. I., Appendix XX., p. 471. 13,594–7; 15,282–4; 17,988. 1517; 15,280; 17,990–1; 18,214; 18,253.

It may also be stated that the occasional predominance of others of the pathogenic effects of the plague virus has led to further subdivisions of the disease, such as the nervous, the abdominal, the gastro-enteric, and the dysenteric.

8 *Relative Frequency of Occurrence of the several Types.*—Of these several types or varieties of plague, *Pestis major* occurred with much the greatest frequency; but differing statements have been laid before us as to the relative frequency of each type.

In the conjoined cases of Colonel Hutchison and Captain Elphick, I.M.S., amounting to 969 in number, 77·5 per cent. were cases of *Pestis major*, 11·6 per cent. of *Pestis pneumonica*, and 8·7 per cent. of *Pestis septicæmica;* and in the statistics of Lieut.-Colonel Harris, I.M.S., and of Mr. R. N. Jadhav, L.M. and S., pneumonic cases also slightly preponderate over septicæmic. On the other hand, Colonel Benson, I.M.S., out of a total of 1,963 cases, estimates that the cases of *Pestis major* represented 47·1 per cent. of *Pestis pneumonica* only 1·6 per cent., and of *Pestis septicæmica* so many as 34·9 per cent.; in 417 cases tabulated by Mr. Bhatt, L.M. and S., 84·41 per cent. were cases of *Pestis major*, 6·7 per cent. were cases of *Pestis pneumonica*, and 7·43 per cent. were cases of *Pestis septicæmica;* and in 98 cases tabulated by Major Evans, I.M.S., the

Campbell's Report of the Bombay Plague Committee, 1898, p. 138; 9539–40. 7073; Report on Working of the Diliawadi Plague Hospital, 1898, p. 14. 3483. Campbell's Report, p. 151. 5861.

relative frequency of each type was 65·3 per cent., 14·2 per cent., and 20·4 per cent., respectively.

In a few local outbreaks, the recognised pneumonic cases were even in a smaller minority than in any of the above instances. For example, Mr. Maynard, M.R.C.S., stated that in Nasik, between February 1897 and April 1898, there were only five cases, although the deaths from plague numbered 486, representing a case incidence of considerably less than 1 per cent of pneumonic cases; and in Mr. Madhava Rao's classification of 2,053 cases in the Bangalore outbreak of 1898–9, only 1·4 per cent. were cases of pneumonic, whereas 38·5 per cent. were cases of septicæmic plague, the remainder comprising 58·7 per cent. of bubonic and ·2 per cent of gastro-enteric cases. On the other hand, in one or two outbreaks, most and even all of the cases were pneumonic.

14,709, 14,731.

Report on Plague Operations in the Mysore State, 1898–9, p. 8. 7337–44.

In the case of Septicæmic plague, the numerical variations are no doubt chiefly to be explained by the absence of any sufficient symptomological definition of the type, but they may also be explained by its unequal prevalence in different localities and at different times; and these latter circumstances, along with the delay in its recognition at the earlier periods of outbreaks, probably account for the similar variations in the statements regarding the prevalence of Pneumonic plague.

15,182.

III. Brief Account of the Principal Symptoms.

9. *Buboes and Lymphatic System.*—Implication of the external lymphatic glands is so frequent and conspicuous a condition of plague that it becomes one of the leading symptoms, and has suggested its most recent, though insufficiently comprehensive, name, Bubonic plague. Enlargement of these glands is one of the earliest of the symptoms, and frequently coincides with the production of pyrexia, although it has occasionally been found even to precede the pyrexia. In the majority of cases, buboes appear within six hours after the beginning of the illness, but their appearance may be delayed for several, occasionally for three or four, days, and in one recorded instance the interval has been so long as eight days. Before enlargement can be discovered, there is often pain and tenderness at the position where it subsequently appears. The glandular swelling is usually painful and tender, the pain being either dull or sharp and lancinating; but in a few cases these characters were absent and small or large buboes could be freely handled without discomfort to the patient. On its first appearance the bubo is most frequently small, varying in size from a pea to an almond, but growth is rapid, and in a few hours it may even attain the size of an orange. The increase to a large size is, however, most frequently produced by involvement of several neighbouring glands or by extension of inflammation to the contiguous tissues, which become infiltrated with serous or sero-sanguineous effusion, or even with pure blood. The latter was found to occur most frequently in association with cervical and axillary buboes. This infiltration varied much in extent and rapidity of production, and when it occurred in association with axillary or pectoral buboes, it was sometimes observed to increase rapidly, so as to involve the whole of the side of the chest, and to extend to the cervical region and even to the vertebral column. Its extent and position had also an important influence upon the progress of the case, for a large infiltration in the cervical or in the axillary region nearly invariably produced death, whereas a similar or even greater infiltration in the femoral region merely prolonged convalescence, and did not in itself endanger life.

10,202.

Campbell's Report, p. 124; 20,708; 12,897. Dyson and Calvert's Report, p. 3. 20,713–4. R. N. Jadhav's Report on Daliawada Plague Hospital, p. 19; N. H. Choksy's Report on Plague, p. 22. Campbell's Report p. 135; R. N. Jadhav's Report, p.19; Proceedings. Vol. I., Appendix VIII., p. 351. Choksy's Report on Bubonic Plague p. 23. Campbell's Report p. 135; Snow's Report, p. 232 Lloyd Jones' Report on the Plague Hospital, Poona, p. 8. Report on Bubonic Plague, p. 42.

According to Dr. Choksy, the distribution of the buboes in 939 cases of plague admitted into the Arthur Road Municipal Hospital, Bombay, was as follows:—

Femoral	275 cases
Femoro-inguinal	200 „
Axillary	140 „
Inguinal	106 „
Cervical	45 „
Multiple	40 „
Femoral-inguinal and iliac	17 „
Femoral and iliac	12 „
Inguinal and iliac	11 „
Double femoral	3 „
Supratrochlear	3 „
Iliac	2 „
Double inguinal	1 case
Double cervical	1 „
No buboes	83 cases

It appears from this table that the femoral buboes were the most frequently encountered, and after them the femoro-inguinal, and thus groin buboes were shown greatly to preponderate in number. In only 8·5 per cent. of the total cases were there no buboes. If the percentage of each of the first six classes to the total be calculated, representing about 94 per cent. of the buboes in the 939 cases, this preponderance of groin buboes is represented by 67·86 per cent.; the axillary buboes being 16·35 per cent., the cervical 5·25 per cent., and the multiple buboes 4·67 per cent.

Proceedings, Vol. III., Appendix LXI., p. 458. 10,198.

This order in the frequency of buboes in the chief situations in which they occur seems to be generally maintained in the other statements laid before us, but the details of the relative numbers vary considerably. For example, in 3,128 cases which occurred in the Jullundur and Hoshiarpur outbreak of 1897–98, 54·46 per cent. had groin buboes, 10·96 per cent. had axillary buboes, 10·86 per cent. had cervical buboes, and 4·44 per cent. had multiple buboes; while of the remainder, 16·62 per cent. had no buboes. Again, in 2,815 cases having groin, axillary and cervical buboes, collected by Captain Jennings, I.M.S. and Captain Lloyd Jones, I.M.S., 1,947, or 69·1 per cent., were groin buboes, 598, or 21·2 per cent. were axillary buboes, and 270, or 9·59 per cent., were cervical buboes; showing a much greater preponderance than in the Jullundur and Hoshiarpur statistics of axillary over cervical buboes.

Campbell's Report, p. 115; Report on the General Plague Hospital, Poona, p. 18.

Buboes have sometimes been found to occur in other situations than those noted above, as in the popliteal, brachial, and sub-maxillary regions, the forearm, calf of the leg, elbow, knee, chin, and scalp, and in the supra-trochlear, parotid and mammary glands. They have, however, only rarely been found in these situations. In the Jullundur and Hoshiarpur outbreak, buboes in such exceptional positions altogether amounted to only 1·66 per cent. in the 3,128 cases.

Gatacre's Report, p. 49.

10,198.

10. *Distribution of Buboes according to Locality and Sex.*—Buboes appear to occur more frequently on the right than on the left side. In 4,332 cases with buboes, representing plague cases in Bombay, Poona and Porbandar, 51·62 per cent. were on the right and 48·33 per cent. on the left side; but while the majority of the buboes (56·3 per cent.) were on the right side in the Bombay cases, the majority (50·54 per cent.) were on the left side in the Poona cases. In the total 4,332 cases, the greater general frequency of buboes on the right side was maintained in the three chief localities in which they occur, as 1,462, or 51·5 per cent., of the groin buboes, were on the right side, and 1,376, or 48·48 per cent., on the left side; 493, or 50·8 per cent., of the axillary buboes were on the right side, and 477, or 49·17 per cent., on the left side; and 276, or 52·67 per cent., of the cervical buboes were on the right side, and 248, or 47·3 per cent., on the left side.

Proceedings, Vol. II., Appendix XLIX., p. 423 (1); Lloyd Jones' Poona Report, p. 18; Campbell's Report, pp. 115 and 138; Gatacre's Report, p. 49.

In reference to the distribution in the two sexes, in 3,909 of the above 4,332 cases, in which the number of groin, axillary and cervical buboes is separately noted in the two sexes, 1,529, or 58·73 per cent., of the 2,603 groin buboes occurred in men, and 1,074, or 41·26 per cent. in women; 475, or 51·63 per cent., of the 920 axillary buboes occurred in men, and 445, or 48·37 per cent., in women; and 219, or 56·73 per cent., of the 386 cervical buboes occurred in men, and 167, or 43·26 per cent., in women.

Estimating, in men and women separately, the numbers in each group of these 3,909 cases, it is found that of 2,223 buboes in men, 1,529 or 68·79 per cent. were groin buboes, 475 or 21·36 per cent. were axillary buboes, and 219 or 9·85 per cent. were cervical buboes; and that of 1,686 buboes in women, 1,074 or 63·7 per cent. were groin buboes, 445 or 26·4 per cent. were axillary buboes, and 167 or 9·9 per cent. were cervical buboes. It would appear from the last estimation, that, relatively to other buboes, axillary buboes are more frequent in women than in men, that groin buboes are relatively more frequent in men than in women, and that cervical buboes, relatively to other buboes, occur about as frequently in men as in women.

When the side of the body on which each set of the 3,909 buboes occurred in the two sexes is determined, it is of interest to find that while groin and cervical buboes occurred in both sexes more frequently on the right than on the left side, axillary buboes, although also more frequent on the right side in men, were, contrary to the statement of several observers, actually more frequent on the left than on the right side in women, in the ratio of 233 to 212, or 52·36 to 47·64 per cent. In men, the ratio was 242, or 50·94 per cent., on the right, and 233, or 49·05 per cent., on the left side, giving almost an equality between the two sides.

Report on the General Plague Hospital, Poona, p. 18; Campbell's Report, pp. 137-8; Proceedings, Vol. I., Appendix XLIX., p. 485

This preponderance of axillary buboes on the left side in women is also separately illustrated in several of the tabular statements submitted to us, as in that of Surgeon-Captain Lloyd Jones, representing 1,122 cases of plague, of Dr. L. T. R. Hutchinson, representing 854 cases, and of Mr. H. S. Deva, representing 274 cases.

The glandular enlargements, however, are not restricted to the superficial glands, for deeply-seated glands, such as the mediastinal, bronchial, mesenteric, iliac and pelvic, may likewise be affected; and it has not infrequently been found that the iliac and pelvic glands are enlarged in association with superficial glands at the groin, so as to form with them a continuous bubo. Proceedings, Vol. I., Appendix XIII., pp. 368, 371.

Nor does the mere production of these enlargements represent the extent of involvement of the lymphatic system. Lymphangitis has occasionally been observed, which involves both the superficial and deep vessels. It most frequently affects the lower extremities, sometimes causing swelling of the entire limb from the groin to the foot, and when it occurs the prognosis of the case is rendered specially grave. 20,491, 20,497–8, Gatacre's Report, p. 67; 20,662–3.

Buboes may gradually undergo resolution, or they may suppurate. In the former case, they may disappear in from a week to a fortnight; in the latter case, progress is usually very slow, sloughing may occur, and a large open sore may be formed, exposing muscles and blood vessels and weakening the patient by profuse discharge. Death is in this manner occasionally produced long after the immediate dangers of plague have disappeared. 9566. Snow's Report, pp. 233, 234; 20,739–42.

11. *Temperature.*—Elevation of temperature is an early and constant symptom, and sometimes in a few hours after the initial rigor the temperature reaches so high an elevation as 103° or 104° F. More frequently, however, the rise is a less rapid one on the first day, and it gradually continues until the maximum is reached on the second, third, or fourth day. The pyrexia is of a remittent type, a morning decline being succeeded by an evening elevation of one or two degrees. The usual range appears to be between 102° and 105°, and the lowest and highest temperatures recorded in *Pestis major* have been 100° and 110° respectively, and the highest temperature in pneumonic plague, 110·8°. The degree of elevation is not in itself an indication of the severity of the illness, for in several fatal cases it has not exceeded 103°, while in cases which have recovered it has attained so high an elevation as 106·4°. The usual course in favourable cases appears to be that after the maximum temperature has been attained, it continues, with remissions, until the fifth, sixth, or seventh day, and then falls gradually and irregularly, by lysis, to the normal temperature on or about the tenth day. Frequently this gradual fall is of a "staircase" character. An abrupt fall, by crisis, of from four to six degrees, or to any point below the normal, almost invariably indicates collapse and speedy death. Prolonged elevation of temperature was usually associated with a suppurating bubo, and sudden elevation after or before the normal point had been reached with the formation of new buboes or with the advent of some complication, such as broncho-pneumonia, lymphangitis, abscesses, boils, or septic poisoning. In a few cases, recrudescence of the illness during convalescence has, accordingly, been observed, in which fresh buboes have appeared along with pyrexia and other of the usual symptoms of plague. Proceedings, Vol. III., Appendix LXIII. Gatacre's Report, pp. 56–57; 20,687, 1861; 12,897. Gatacre's Report, p. 98; 20,695; 26,585. 20,691. Choksy's Report on Bubonic Plague, p. 21. 12,897; 20,698. *See* Charts, Proceedings, Vol. I., pp. 385–9. Snow's Report, p. 229; 20,699. Choksy's Report on Bubonic Plague, p. 22.

12. *Circulation.*—In a few cases, slight precordial pain and palpitation were complained of, but generally there were no symptoms of disorder of the circulation which attracted the notice of the patient. Nevertheless, almost without exception, the circulation is early and profoundly affected. At the commencement of the illness the pulse rate acquires an exaggerated frequency, so that it is found to be at the rate of from 110 to 120 in the minute, and in the course of the illness it may even attain a rate of 140 or 170; while the normal relation of its frequency to the respiration and temperature is disordered with an irregularity which is observed in few diseases. It is, however, more from the characters of the pulse than from its rapidity that indications are obtained of the great changes in the circulation which occur in plague. When examined early in the illness, it is found to be in some cases of considerable and in others of small volume; but even in the former event, the ease with which the pulse may be obliterated proves it to be very feeble. It soon becomes also intermittent or otherwise irregular, dicrotic and very small, and, ultimately, even in cases which recover, it may no longer be countable, and its rate can be ascertained only by palpation or auscultation of the heart. This weakness of the circulation explains the frequently observed state of coldness of the extremities while yet the thermometer records in the axilla or mouth a temperature of pyrexia. It is also accountable for the numerous cases of sudden death which have taken place when the patient, often apparently in no critical condition, changes his posture from a recumbent to a sitting one, or even when, while still recumbent, he makes some slight physical exertion. A much larger number of the plague deaths for which it is accountable are, however, produced by progressive weakness of the circulation to a point at which life can no longer be maintained. 20,800. Proceedings, Vol. III., p. 529 12,897. 20,407. 20,800. A Report on Plague by Surgeon Captain James, 1897, p. 7; 20,915.

As the heart sounds are stated to be generally normal and the impact fairly strong at the same time as the pulse is very feeble, much of this early induced extreme weakness of the circulation is probably to be explained by vaso-motor paralysis. Several of the *post-mortem* appearances are in harmony with this explanation, but the experimental demonstration of vaso-motor paralysis by plague toxin has not yet been obtained.

Gatacre's Report, p. 59.

13. *Blood.*—Apart from bacteriological examination of the blood, but little has yet been done to ascertain its condition during plague. It would seem that it retains its normal appearance and coagulability, and that the red cells are not reduced in number to any important extent, but that the number of leucocytes is considerably augmented.

20,803 ; Choksy's Report, p. 32.

14. *Respiratory System.*—Even in cases in which there are as yet no lung complications, the respirations are from the beginning increased in frequency. This increase is at first only moderate, but frequently it becomes so great that the rate is 30 or 40 or even 60 in the minute. The latter, however, generally indicates the establishment of pulmonary congestion or œdema, both frequent complications of plague, and this œdema is a more frequent contributory cause of death than the secondary pneumonia or bronchitis occasionally produced. When pulmonary œdema is present the respirations are very frequent and laboured and the dyspnœa is intense. The sputum varies greatly, as, indeed, might be expected from the variations in the lung and bronchial conditions. It is, in some cases, scanty, bloodstained, and even viscous, and in others, abundant, watery and deeply coloured with blood; but actual hæmoptysis has been observed in relatively few cases.

20,823.

15. *Digestive System.*—The condition of the tongue has by several witnesses been regarded as an important diagnostic condition, and therefore its appearance may be described with some detail. At first it is narrow and coated with a whitish, thin and silvery fur, confined to the dorsum, but increasing in depth towards the root, and most marked on each side of the raphe, while the tips and edges are red and usually moist. The fur becomes yellow, then brown and cracked; and it sometimes falls off in flakes, which expose a clean, raw, irritable-looking surface. At a later period in the more severe cases, it is also tremulous, protruded with difficulty, and dry, and in these cases sordes collect on the lips, teeth, and gums.

12,398; 12,897; 20,831 ; Campbell's Report, p. 118.

The patient is generally thirsty, but the appetite is rarely impaired unless the case be an advanced one of severe type. Difficulty of swallowing was observed in a few cases, and in them was accounted for by enlargement of the neck glands and inflammation of the surrounding tissues. Hiccough, nausea, and vomiting are frequent symptoms and the vomited matter is usually bile-stained, but in several instances it has the appearance of coffee grounds, as if mixed with blood. Actual hæmatemisis was a rare event, although at certain periods of the epidemic, and especially when plague had assumed an especially virulent type, several deaths were caused by profuse hæmorrhage from the stomach. The bowels were usually in a state of constipation. Diarrhœa is therefore rarely present, and when, before convalescence, it followed constipation, it nearly invariably indicated an early fatal termination. Blood in the stools was but rarely seen and was always followed by death.

20,842.

20,844–9; 20,854. Gatacre's Report, pp. 29-30; Campbell's Report, p. 130. 20,850. Gatacre's Report, p. 66; 20,852–4; Jadhav's Report, p. 25.

16. *Genito-Urinary System.*—In all cases of *Pestis major*, the urine was diminished in quantity and was high-coloured. Retention was on the whole frequent, and it was stated that even suppression occurred in a few cases. The urea, uric acid, and chlorides were diminished. During the acute stage, albumen in small quantity was present in at least from 33 to 50 per cent. of the cases, but one observer with large experience asserts that it was present in all cases. Whenever albumen was present, hyaline and epithelial tube-casts were also found. Hæmaturia was a rare event. In non-fatal cases, the abnormal conditions of the urine quickly disappeared as convalescence became established.

20,764. Proceedings, Vol. I., Appendix VIII., p. 352. 10,422 ; Gatacre's Report, p. 66. 15,300–4. 20,855; Gatacre's Report, p. 98. 15,307.

In women at the period of menstruation, prolonged and profuse menorrhagia was frequently induced, and when amenorrhœa existed menstruation was re-established. Fatal abortion was nearly always caused in pregnancies which had advanced beyond the third month; and it is a remarkable fact that when the pregnancy was near the full time the infant might be born alive and free from symptoms of plague, while the mother almost invariably died.

20,861 ; Gatacre's Report, p. 66. Proceedings, Vol. I. Appendix VIII., p. 352 ; 20,864; Gatacre's Report, pp. 66, 150.

17. *Nervous System.*—Involvement of the nervous system is indicated very early in the disease. Even in *Pestis minor* it may display itself by a peculiar desire, inexplicable and unaccountable to the patient, to remove from one place to another. In *Pestis*

Proceedings, Vol. I., Appendix XIV., p. 380.

major and in *Pestis pneumonica*, it early manifests itself by severe frontal headache, vertigo, irritability and sleeplessness, and by pains in many situations, such as the back of the head, over the spine, or in the course of the sciatic nerves. As the illness advances, there supervene in many cases tremors and lessened activity of the superficial reflexes. When the disease has fully developed itself, there follow bluntness of the mental power and delirium or coma of varying degrees. The delirium may even appear at the beginning of the illness. It may be acute or of a low muttering type. In its acute form, it may become maniacal, requiring restraint, or it may assume a homicidal character. As occurs in cases of *Pestis minor*, a desire to wander about sometimes manifests itself. In the mountainous districts of Kumaun and Garhwal patients have been known to fall or throw themselves down precipices, and at Banga a woman in a state of delirium threw herself into a well and was drowned. In many cases delusions and hallucinations were present. The patients would show by their movements that they were under the delusion that they were following their customary trades. Hallucinations were manifested by patients imagining that they saw before them relatives or friends when none were present, or that they heard sounds such as of the chopping of wood or the grinding of corn. In children, involvement of the central nervous system was frequently manifested by convulsive movements, general or restricted to certain groups of muscles; and convulsions also occurred in adults, but usually at an advanced period of the illness, and presenting every variety of general or localised spasms. They were often, indeed, so obviously of a tetanic character as to suggest the co-existence of intoxication by the tetanus bacillus. General convulsions in children, and symptoms indicating decided involvement of the central nervous system in adults, were generally followed by death.

Lieut.-Colonel Weir, I.M.S., Snow's Report, pp. 54, 61. 20,756. 26,587. James' Report, p. 32. 20,767–8. Snow's Report, p. 237. Campbell's Repor p. 160; 20,786.

Sensory hyperæsthesia was also frequently observed, during which the slightest touch would produce loud "screams, which not only pervaded the hospital, but even attracted the notice of passers by on the road." Several of the witnesses have described impairment of speech as a frequent symptom, and it is also referred to in many of the reports submitted to us. Impairment and even complete loss of speech have undoubtedly occurred as a result of defective power of articulation, due to general feebleness, or to paralysis of the laryngeal muscles, and probably also to congested swelling of the tongue, glottis, and laryngeal membrane. Besides impairment or loss of speech from aphonia and from greatly enfeebled mental power, it is possible that aphasia sometimes occurs as a symptom of plague, although the evidence on the subject is by no means clear.

20,778. N. H. Choksy, Report on Bubon Plague, p. 29. 12,400–2; 12,901–5. Proceedings, Vol. Appendix XIV., p. 379. 579; 13,044–6; 21,031–2; 21,04 Choksy's Report pp. 30–31. 20,678–82.

18. *Skin and Subcutaneous Tissues.*—The extent and nature of the affections of the skin are of much interest, because of frequent references to skin eruptions in the descriptions of many of the epidemics of plagues that have occurred prior to the present epidemic in India.

While petechiæ have been observed in a few cases, the descriptions of the symptoms of the present Indian epidemic indicate that they were either of relatively infrequent occurrence, or were so much obscured by the dark colour of the skin as to have generally escaped observation. At *post-mortem* examinations, however, they were more frequently observed. Thus, Captain Childe, I.M.S., remarks that they were usually seen on the limb on which the bubo was situated, and "were always in greatest number over the bubo itself; and though there might be some over the body generally, still these were but few." Purpura, also, was but rarely encountered, but in many advanced cases vesicles with hæmorrhagic contents were seen, and extravasations of blood in and around buboes were of frequent occurrence.

1419; 20,883; Gatacre's Report, pp. 98, 147; Snow's Report, p. 245; Proceedings, Vol. II., Appendix XXV. p. 359. 1419; Proceedings, Vol. I., Appendi XIII., p. 367.

Pustular eruptions showed themselves in the form of ordinary boils, and also of so-called "black boils." The latter were observed to originate in vesicles which gradually formed themselves into pustules containing also blood and serum, and which extended downwards into the skin, producing a slough. They varied from a half to one inch in diameter, and were situated on the limbs or trunk near to or in actual contact with the lymphatics of neighbouring buboes; and "inquiries often elicited the fact that the first sign of the plague was this skin manifestation followed by a bubo."

Gatacre's Report p. 67; Snow's Report, p. 127; 4562.

A more definite primary plague vesicle or "phlyctenule" has occasionally been observed. Originating as a small papule, it quickly assumes a vesicular character, so that on the second day of the illness it is about one-fourth of an inch in diameter, pearly-grey, translucent and slightly umbilicated, covered by a thin epidermis and surrounded with a zone of inflammatory exudation. It gradually enlarges, the contents become turbid, and, from the fourth to the seventh day, it resembles a

From description by Dr. Turkhud, Bombay; 4562 7302; 20,597–608; Proceeding Vol. I., Appendi XIII., pp. 374–5

vaccine pustule of seven days' duration. The epidermal covering then disappears and exposes a greyish-yellow slough, which may either dry up and scab, or form a suppurating and slowly-healing ulcer. As this vesicle is always accompanied with a bubo in the lymphatic gland nearest to the skin surface on which it is situated, and as it contains plague bacilli, the view is entertained that when it exists, it indicates the point of entrance of plague bacilli into the body and is produced by the introduced bacilli.

7302. Necrotic sloughs, situated usually on the loins, back, and intra- or supra-scapular regions, were also observed. They likewise originated from pustules which underwent transformations similar to those above mentioned, and ultimately produced scab-like necrotic sloughs covered with small vesicles, varying from 1 inch to 8 inches in diameter, which gradually encroached outwards and downwards until the underlying muscle became involved.

In a small number of advanced cases, gangrene occurred in the peripheral portions of the extremities. It usually started as a blister or a small abscess. The gangrene rapidly involved the neighbouring tissues, and extended also usually from the foot to the knee or from the hand to the elbow. The skin acquired the characteristic coldness and dark green colour, and became covered with large vesicles containing watery pus. In several cases, and even when much destruction of tissue had occurred, it was found that prompt treatment by subcutaneous injection of mercuric chloride was successful in checking the progress of the gangrene.

12,360–74.

Snow's Report, p. 109; 27,166; 6,955; Proceedings, Vol. I., 376; 7073; Gatacre's Report, p. 58; Dyson and Calvert's Report, p. 3; 7302; Snow's Report, p. 244.

It may further be stated that carbuncle, of the ordinary surgical variety, has only occasionally been observed, the condition most closely resembling it being the localised necrosis terminating in slough, above described; and that boils and small abscesses frequently appear during convalescence and constitute troublesome sequelæ.

Snow's Report, p. 248; Gatacre's Report, p. 248; Jadhav's Report, p. 19; 20,577–80; 20,772–6. 16,818; 20,787–91; Proceedings, Vol. I., Appendix VIII., p. 352, and Appendix XIV., p. 380.

19. *Complications and Sequelæ.*—Plague has occasionally been complicated by the concurrence of sub-acute rheumatism, arthritis, malaria, phthisis, cholera, erysipelas, dysentery, and relapsing (or famine) fever, as well as of bronchitis, catarrhal and croupous pneumonia, tetanus, and other affections which have previously been mentioned.

The most important of the sequelæ which have been observed were dysentery, anæmia, phthisis, and diseases of the nervous system, and included in the latter were peripheral neuritis, pareses and paralyses of various kinds, and mental impairment.

7261–9, Vol. III.; App. LXXXVII., p. 657; 18,018; 19,596; 19,355; 23,103–7; 23,710; Snow's Report, 171; 18,025; 19,523; 19,108.

Plague appears to resemble other infectious diseases in so far that one attack occasionally fails to confer immunity against subsequent infections. Several cases have been submitted to us in which two and even three attacks have occurred in the same person, at intervals between each attack varying from eight days to two years and two months.

IV.—Pathology.—Morbid Anatomy.

Post-mortem Appearances.

Gatacre's Report, p. 73; Dr. James' Report on the Outbreak of Plague in the Jallundur and Hoshiarpur Districts of the Punjab, p. 147; R. N. Jadhav's Report, p. 35. Report on Bubonic Plague, 1897, p. 54–7; 20,972; Proceedings, Vol. I., Appendix XIII., p. 367–376, 1419; Report on Plague in Bombay, p. 6; Gatacre's Report, p. 74; Proceedings, Vol. I., Appendix XVII., p. 439; 10,418; 6912; 9552.

20. Caste and other prejudices of the natives, which were consistently respected, have limited the number of *post-mortem* examinations relatively to the large mortality from plague in India. Descriptions of the post-mortem appearances have, however, been laid before us, which together amount to a considerable total, and are sufficient to clearly establish the more important pathological features of plague. Reference may specially be made to the reports of Dr. Choksy, Captain Childe, I.M.S., Dr. Dyson and Captain Calvert, I.M.S., Captain Thomson, I.M.S., and Captain Leumann, I.M.S.; and to the evidence of several of these gentlemen and also of Captain Wilkinson, I.M.S., Major Evans, I.M.S., and Captain Elphick, I.M.S.

Pestis Minor.

21. Death from *Pestis minor* probably never occurs, but, at any rate, no description of the pathology of plague deals with this type.

Pestis Major.

22. *Lymphatic System.*—(a) *Buboes.*—The appearance of the buboes was characteristic. As we have already stated, they varied much in size and to a less extent in number. For the most part they consisted of enlarged lymphatic glands surrounded by extravasated blood and serum, which in many instances extended widely and deeply into the neighbouring areolar tissue, and often penetrated into the substance of muscles. Sometimes only one gland was included in a bubo, but more frequently there were

419.

several, and at times an external bubo was directly connected with one involving deeper seated neighbouring glands, as when a femoral bubo extended inwards so as to be continuous with one involving the iliac glands. The swollen glands and the surrounding tissues were often "matted together . . . so that it was difficult when one cut into it" (the bubo) "to be quite sure which was gland and which was the surrounding tissue." The affected glands were round or oval in shape, and they varied in size from that of a pea to that of a walnut. On section, they were seen to be much engorged with blood, of a light red or deep purple colour, and of firm consistence in some cases, but soft and almost diffluent in others. When the bubo was in the groin, the limb of the same side was usually swollen and œdematous; when in the axilla, the serous effusion and hæmorrhagic exudation was sometimes so great as to form a swelling involving the whole axilla and extending down the inner aspect of the arm to the elbow, and down the side of the chest nearly to the lowest ribs; and when in the neck a large swelling frequently resulted, which pressed upon the larynx, trachea, and cervical nerves and blood vessels, and thus produced the dyspnœa which was so conspicuous a symptom in those cases. As buboes in the groin and axilla frequently pressed upon or surrounded the large blood vessels in these regions, the œdema in the corresponding limb is readily accounted for.

Proceedings, Vol. I., Appendix XIII., pp. 368, 371. 10,418. 1419; Proceedings, Vol. I., Appendix XX., p. 465. Proceedings, Vol. I., Appendix XX., p. 467.

It has been pointed out by Dr. Childe that in axillary buboes and occasionally in chains of enlarged glands along the course of the iliac artery and vein, the extravasated blood may be continuous with blood in the interior of the veins, thus forming "a direct path for the passage of the plague bacillus from the gland to the lumen of the vein, and so into the general circulation."

Proceedings, Vol. I., Appendix XIII., p. 369.

In a few cases of *Pestis major*, no pathological changes were found in other lymphatic glands than those in connexion with which buboes had formed; but in most cases, many other glands, and especially the internal glands, were affected. The changes in them, however, were relatively slight, for they consisted only of enlargement, and of either moderate or great congestion, a section of the gland in the latter instance showing much engorgement with dark blood. In individual cases, many or few of the glands were affected; but indeed no lymphatic gland, whether internal or external, seemed to escape involvement, for these changes were observed, in varied groupings, in the bronchial, mesenteric, peritoneal, iliac, lumbar, trochlear, popliteal, and other glands.

(*b.*) *Lymphatic Vessels.*—In most cases, the lymphatic vessels showed no pathological change, and in only a few were they observed to be congested and swollen, and that only close to the bubo.

Proceedings, Vol. I., Appendix XIII., p. 369.

23. *Alimentary System.*—The pharynx and tonsils were greatly congested, the latter being enlarged and occasionally in a state of follicular inflammation. This congestion usually extended down the œsophagus, and in some cases hæmorrhages were found in its walls. The stomach was almost invariably engorged with blood, and both minute and considerable hæmorrhages were present in the mucous membrane. These hæmorrhages have been classified by Captain James, I.M.S., as follows:—

1420; 10,419. A Report on Plague, 1897, p. 12.

"(*a.*) A mild form in which, on holding the coats of the stomach up to the light, the course of the smaller vessels was found to be picked out by extravasations which also formed small arborescent patches on the mucous surface, or perhaps only reddened the edges of the rugæ.

"(*b.*) A severe form in which there were more or less extensive patches of hæmorrhage into the sub-mucous tissues or even into the stomach cavity.

"(*c.*) Distinct numerous petechiæ with definite circular outlines and varying in size from a pin's head to that of a split pea. In one case the petechiæ were very numerous and covered nearly the whole of the mucous surface of the stomach. The appearance was so striking that it reminded one of the rash seen on the skin in a severe case of purpura."

Similar appearances have been observed in the intestines. Congestion and hæmorrhagic extravasations were most frequently seen in the large intestine and rectum, and less frequently in the small intestines. Occasionally, Peyer's patches and the solitary follicles were observed to be swollen. The liver and spleen were enlarged, the latter being sometimes two or three times the ordinary size. Petechiæ were frequently present on the surface of the liver, and its substance was engorged with dark blood, and was soft and friable, indicating degenerative change. The spleen also was generally in a state of acute degeneration, soft and pulpy, and in some cases almost diffluent. On section, it was brick red or deep chocolate brown and always greatly engorged. The gall bladder usually contained bile, and occasionally there were petechiæ on its

1420. 10,421.

mucous lining. The kidneys were also much enlarged and engorged with blood, and presented the characteristics of acute parenchymatous degeneration. Minute hæmorrhages were sometimes observed on the surface, and petechiæ and coagulated blood in the pelvis and calices. Extensive hæmorrhagic extravasation has also been found outside the kidneys, which then were found "lying in a bed of blood." The ureters and bladder were engorged, and petechiæ were frequently present on their inner surfaces. Besides engorgement, no important change was observed in the supra-renal capsules or thyroid gland, and the genital organs presented a normal appearance. The peritoneum was considerably engorged, but, above all, hæmorrhages, often of large size, were found in many parts, as in the mesentery, in the retro-peritoneal connective tissue, on the under surface of the diaphragm, and over the spleen, liver, kidneys, and intestines.

Proceedings, Vol. I., Appendix XX., p. 465; 10421.

20,972; Proceedings, Vol. I., Appendix XX., p. 465.

24. *Circulatory System.*—The blood was dark and fluid, and gave the impression that it did not coagulate well. The pericardium generally contained more fluid than usual, which was sometimes blood-stained; and minute hæmorrhages were found in both its vesical and parietal portions. The myocardium was generally soft with many muscle fibres undergoing granulo-fatty degeneration; and the right ventricle was always dilated, while in many cases there was also dilatation of the other cavities of the heart. The walls of the larger vessels appeared normal, but those of the veins were engorged, and minute and large hæmorrhages were frequently observed beneath the inner coat. Dr. Childe found that when a large vein was included in a bubo, hæmorrhage into its walls was constantly seen, so that the extravasated blood in the gland itself, in the areolar tissue outside, and in the walls of the vein was continuous.

10,418.

1426; 20,972.

Proceedings, Vol. I., Appendix XIII., p. 369.

25. *Respiratory System.*—The larynx was in all cases injected, and in a few instances there was hæmorrhage beneath its surface. In cases of cervical bubo, œdematous swelling involved the soft structures of the larynx and the aryteno-epiglottidian folds. The bronchial tubes were also congested and swollen, and in many cases contained blood-tinged frothy fluid; but the trachea was merely congested. The lungs were engorged with blood and with serous fluid, occasionally small hæmorrhages were found in the lung substance, and when the lungs were cut into much sero-sanguineous fluid escaped. When pneumonia or broncho-pneumonia had occurred as a complication, the pathological changes produced by these diseases were also present. The pleural cavity contained blood-stained or clear fluid. Sub-pleural hæmorrhages, sometimes of large extent, were found on the parietal, visceral, and diaphragmatic pleura; and occasionally there was evidence of fibrinous inflammation. Hæmorrhages were sometimes present in the anterior and posterior mediastina.

26. *Nervous System.*—The dura and pia mater of the brain, as well as the coverings of the spinal cord, were congested, and in a few instances extravasations of blood were found on the dura mater. The brain substance and spinal cord generally presented a healthy appearance; but occasionally congestion and œdema were present, which involved likewise the choroid plexuses. No pathological change has been observed in the nerve trunks.

20,972.

27. *Bones, Joints.*—It has further been noted that the marrow of the long bones is red and congested as in other infectious fevers, and that evidence of an infective inflammation is found in the fringe-like processes of the synovial membrane of the knee joint.

20,972

Intense or Septicæmic Type of Plague.

28. In those cases where the plague virus or toxin is in the patient widespread from the beginning of the illness, so as early to produce a general poisoning, whether septicæmic or toxæmic, the pathological changes, as might be expected, are much the same as in the more severe cases of *Pestis major*. Some observers, however, believe that pathological differences occur to distinguish this form of plague, and to serve, along with the symptoms, as a justification for the establishment of a so-called septicæmic type of this disease. They consist of the absence of buboes having the characters above described, and of a widespread involvement of glands, with distinctive changes in several of them. Although the lymphatic glands are always affected, in place of the affection consisting of one or, more rarely, of several groups of glands being enlarged and surrounded with sero-sanguineous extravasation, while the other glands are either normal or merely enlarged or congested, in this, the so-called

9541-2.

septicæmic form, the affection of the glands shows itself as a general involvement of all, or nearly all, of the lymphatic glands of the body, although in many instances the affected glands were chiefly those of the mesentery. In no case, however, did the involvement proceed to the formation of characteristic plague buboes, but only to a moderate degree of change, practically restricted to the glands themselves, but still displaying in several of them certain distinctive characters. These were moderate 1421.
enlargement and œdema without much congestion, the glands being pink in colour, firm and rounded, and with a softish interior, often possessing here and there small areas of softening surrounded by firm substance. Several of the affected glands may be thus modified, while others of them are merely enlarged and engorged with blood, thus resembling the less affected glands of ordinary *Pestis major*. Excepting the lymphatic glands, the parts that were affected showed essentially the same pathological changes as in the bubonic variety of *Pestis major*, but usually the number of parts affected was smaller and the degree of change in them was less.

Pestis Pneumonica or Pulmonalis, or Primary Plague Pneumonia.

29. Inflammation of the lungs occurs, as already stated, with considerable frequency during the course of plague, and it then becomes a serious complication. It also occurs so early in the disease as to justify the belief that the plague virus had affected the lungs either primarily or coincidently with the more general affection of the body, thus constituting a form of plague which is distinguished as Primary Pneumonic plague. When contrasted with those occurring in *Pestis major*, the pathological changes elsewhere than in the lungs are less intense, while those in the lungs are more intense. The lymphatic glands are only slightly affected, and external buboes having the specific 1388.
characteristics seen in *Pestis major* are seldom, if ever, encountered. Congestion and 6913;
enlargement of organs and even hæmorrhage in mucous and serous membranes may be Proceedings, Vol. I., Appendix
present, but they do not assume the proportions attained in *Pestis major*. On the other XIII., p. 368.
hand, the lungs are conspicuously affected. The whole substance is engorged, the Gatacre's Report,
large as well as the small blood vessels being distended; and hæmorrhagic zones are p. 140; 17,247.
seen scattered throughout the lungs, filling the alveoli and often breaking down their walls. Within the hæmorrhagic zones are areas in which the alveoli are completely filled with leucocytes, epithelial cells, and granular débris, constituting, with the surrounding zones of hæmorrhage, blood-congested areas of catarrhal pneumonia. In these areas, as well as in the fluid matter contained in the trachea and bronchi, plague bacilli are abundantly present. Greyish necrotic patches have also been found containing large numbers of plague bacilli. The bronchi are engorged with blood, and catarrhal cells are found in their terminations. Over affected areas at the surface of the lungs, the pleura may be acutely inflamed. In most cases, the bronchial glands were congested, and there was a little hæmorrhage into the gland substance; but in some cases, their appearance was normal.

While, however, a catarrhal inflammation of lobular distribution has most 10,431; 20,830.
frequently been regarded as the characteristic type of primary plague pneumonia, several observers have denied its existence, and have asserted that croupous (lobar) pneumonia is the form that most frequently occurs. Major Evans, I.M.S., and Captain 6914; 9549.
Elphick considered that all cases of typical plague pneumonia come under the latter category, and Major Jones expresses the opinion that "lobar pneumonia is common." Major Evans stated that the pneumonia is distributed in small detached patches, constituting lobular areas, only when the inflammation has not advanced far; but that it is lobar to the extent of involving a whole lobe or the greater part of a lobe when the lung inflammation had advanced further. Captain Elphick, I.M.S., described several autopsies in which individual lobes or even an entire lung were consolidated, and he stated that "every case of pneumonic plague examined showed lobar condensation." It may further be stated that in many cases only slight changes were found in the bronchi. It is therefore possible that the pneumonia is lobular in patients who have died at an early stage of the disease, and lobar in those who have survived to a later period; or, otherwise, that lobar pneumonia occurs when the toxin is most virulent and most widely distributed throughout the lung, and lobular pneumonia when it is less virulent and less widely diffused.

A description of the microscopic examination of many of the tissues is contained in Captain Childe's Report. Apart from the distribution of the plague bacillus, which is elsewhere dealt with, microscopic examination has mainly shown general dilatation and engorgement of the veins and smaller blood vessels, and numerous capillary and larger hæmorrhages in almost every structure and organ of the body.

Summary of Pathological Conditions.

30. The distinctive pathological changes produced by the virus of plague would therefore appear to consist of universal dilatation and engorgement of veins and smaller blood vessels, with hæmorrhages, both minute and of large amount, in nearly every part of the body, and of enlargement of the lymphatic glands, with œdema and hæmorrhage in the surrounding tissues, generally mainly implicating the external glands, but occurring likewise throughout the body and involving, in a series of cases, the entire system of lymphatic glands. In the lymphatic glands, the characteristic conditions are largely explainable by vascular changes, and even in the pneumonia of plague, vascular dilatations and hæmorrhagic extravasations give a special character to the lung inflammation. In no other infective disease are these features represented, but it is of some interest to note that the vascular changes, and especially the pervading and characteristic tendency to extravasation of blood in almost every part of the body, are closely reproduced in the toxæmia caused by the organic poison secreted by the venom glands of several species of serpents, such as the black snake (*Pseudechis porphyriacus*) of Australia.

20,972; Proceedings, Vol. I., Appendix XVII., pp. 439–40; 9557–64; 17,247; 26,409; Choksy's Report, p. 54.

V.—Duration of Fatal Cases.

31. The aggregate of several statements of the number of deaths on each day after patients had come under observation, comprising 3,035 fatal cases in all, shows that the largest number of deaths—20·3 and 18·9 per cent. respectively—occurred on the second and third days, and that the number of deaths each day then gradually lessened until the eighth day, after which the daily reduction was large and irregular until the 15th day. In the 3,035 cases, death occurred within 24 hours in about one-sixth, within two days in more than one-third, within three days in more than one half, and within five days in about three-fourths of the cases. By the end of the eighth day 93 per cent. of the patients had died, and nearly all of the small remainder before the end of the 15th day. In several other statements laid before us, in which, however, details are given for only a few days, a greater mortality than the above has been recorded for the earlier days. Dr. Hornabrook found that one quarter died within one day and one half within two days; Dr. Jadhav that 41·5 per cent. died within the first 24 hours and 65 per cent. within 48 hours; Dr. N. H. Choksy that 37·6 per cent. died within the first day, and 65·7 within the second day; and Colonel Benson, I.M.S., that 13 per cent. died within 12 hours and 40 per cent. within 24 hours. These discrepancies are no doubt to be accounted for, in some instances, by differences in the virulence of the disease at different times and places, and, in other instances, by inequality in the promptness with which cases were discovered or removed to hospital. The longest recorded duration of a fatal case seems to have been one month and eight days, but the death was due rather to great weakness produced by prolonged suppuration of many buboes than to the direct effects of plague virus. The shortest authentic durations were 3 hours and 14 hours, respectively; but many cases were admitted into hospitals moribund or even dead before admission, in which the duration of illness was not ascertained. Death occurs in pneumonic cases, and generally, but less invariably, also in septicæmic cases, in from less than one day to three days.

Campbell's Report, p. 131; Snow's Report, p. 251; 10,208; 12,897 Proceedings, Vol. II., Appendix XLIX., p. 485, J.

1859. Report on the working of the Daliawadi Plague Hospital, 1897–98, p. 8. Campbell's Report, p. 131. 3483.

12,897.

Snow's Report, p. 61 7,580.

10,439.

VI.—The Mortality from Plague.

32. *Case-Mortality.*—In regard to variations in mortality during different periods of an epidemic, much interesting information has been placed before us. Dr. Choksy has drawn attention to the circumstance that the Bombay City epidemic has distinctly manifested fluctuations in virulence, illustrated not only by a reduction, at certain stages, in the number of cases, but also in the severity of the disease. The latter was shown in the case-mortality, which followed a regular variation of smallest mortality at the beginning and end of each outbreak, and of greatest mortality in the middle period. This was conspicuously apparent in the outbreak of 1897–98, which gradually reached its highest severity, with the largest number of cases and the greatest percentage mortality, in February 1898, when the death rate rose so high as from 85 to 90 per cent., and then a gradual reduction took place, both in the number of cases and in the percentage of deaths. Similar variations are described by Mr. Madhava Rao and by Mr. R. N. Jadhav, L.R.C.P., and in the account of the medical aspects of plague in General Gatacre's Report on the Bubonic plague in Bombay. Lieut.-

See on this subject, 3520; 26,311.

Campbell's Report, p. 130.

Report on Plague in the Mysore State, p. 5; 5,266; p. 137.

Colonel Henderson has also noticed that mild cases of plague occurred at Hyderabad (Sind), especially at the beginning of the epidemic, and evidence to a similar effect was given by Dr. Simpson with regard to the Calcutta outbreak of 1896. Mr. L. Godinho and K. B. Jadhav, L.R.C.P., Captain James, I.M.S., Dr. Nariman, and Hospital Assistant Cooper stated that towards the end of an epidemic the cases became of a milder type. Further, it appears from the evidence of Captain James, I.M.S., that a limited outbreak may entirely be constituted by mild and non-fatal cases.

26,896; 27,000. 18,043. 15,112. Report on Plague, p. 3, and 10,334. 15,907; 15,922. 14,242. 10,334-7.

These variations, as well as the different prevalence at different times and places of the several types of plague, may lead to erroneous conclusions respecting the case-mortality; for more than 90 per cent., and in some outbreaks, even all the cases of pneumonic plague have proved fatal, while the mortality of the septicæmic is greater than that of the standard bubonic type of *Pestis major*.

6861; 7343–5 9549–56; 12,394; Campbell's Report, p. 138.

In the case of hospitals, the variations have also been great, and in not a few instances the published statistics are confessedly inaccurate. Summing up a large number of their tabular statements, from which, so far as possible, those containing fallacies were excluded, 15,840 hospital cases have been collected, of which 4,200 ended in recovery and 11,640 in death, representing a case-mortality of 73·48 per cent. In individual statements included in this total, the percentage of deaths varied from 60 to 80·6, but in others, not included, it varied from 0 to 100.

Campbell's Report, pp. 107, 118, 132, 134, 149; Snow's Report, p. 238; Gatacre's Report, p. 109; 18,092; 21,685; 22,258; 25,024; 14,443; 15,266; Proceedings, Vol. II. Appendix XLIX., p. 485, K.

It is, therefore, apparent that estimates of the case-mortality may differ greatly in different places and at different times, and that, in order to obtain a true estimate, it is necessary to deal with as many cases as possible, derived from a large area, and including as long a period of epidemic existence as possible.

Gatacre's Report, p. 148.

For this purpose, the Plague Statistics published for each week by the Government of Bombay are available. They show that in the Bombay Presidency, and in the several Native States connected with this Presidency in which plague had occurred, the total number of cases of plague, excluding cases in Europeans, from September 1896 to 1st June 1900, was 392,385, and that 308,140 of these cases terminated in death, giving a case-mortality of 78·53 per cent. Owing to the large concealment of patients in native houses, where the conditions for recovery were almost hopelessly unfavourable, it may be assumed that the case-mortality was even greater than 78·53 per cent. The highest recorded case-mortalities occurred in Bombay City (83·7 per cent.), Cutch (82·5 per cent.), and Bijapur (81·6 per cent); and the lowest case-mortality in the Sachin State (59·3 per cent). While the case-mortality of the Native and other permanent inhabitants of India was thus high, it was only 29·2 per cent. among the 202 Europeans who became infected in the same period of time (see Appendix III. paragraph 85).

Return dated 1st August, 1900.

No equally complete statistics are available for the Bengal Presidency, nor for any other of the largest districts of India; but during the earlier stages of the outbreak in Calcutta (1898), the mortality in 190 cases was 81·5 per cent.*

Proceedings, Vol. I. Appendix XX., p. 474.

33. *Sex Mortality.*—The mortality from plague does not appear to differ in the two sexes to any striking extent. Taking nine collections of statistics, with a total of 5,449 cases of plague in which there are 3,857 male patients and 1,592 female patients, the deaths in the former amount to 2,911 and in the latter to 1,183, giving a percentage of 75·47 for the male patients and of 74·3 for the female patients. In four other groups of collected cases, in which the deaths in the two sexes are, however, noted only in percentages, an excess of female deaths is shown in two of the groups, namely, 68·6 per cent. of male deaths and 71 per cent. of female deaths in the one, and 58·47 per cent. of male deaths and 64·04 per cent. of female deaths in the other; but the number of cases represented in the two groups is only 672, whereas the average percentage of the four collected groups, comprising 5,294 cases of plague, gives 71·15 per cent. of male deaths and 69·37 per cent. of female deaths. Although it has undoubtedly occurred in a few districts and hospitals that the female deaths have preponderated, it accordingly appears that a larger experience demonstrates a higher case-mortality among male than female patients. At the same time, a great fatality has been observed in pregnant women. After the third month of pregnancy, the percentage of deaths was at least 80, and abortions nearly always occurred.

Gatacre's Report, p. 111; Proceedings, Vol. II., Appendix LXIX., p. 485, G.; Appendix LXX., p. 474; 7221; Dr. Choksy's Report, p. 218, Table iii; Gatacre's Report, p. 132; Campbell's Report, p. 139; Proceedings, Vol. II., Appendix XLIX., p. 485, K.; Vol. I., Appendix XX., p. 474. Gatacre's Report, pp. 69, 150, 111; Jadhav's Report, p. 28; James' Report, p. 16. Gatacre's Report, p. 66.

* Since this was written, full details have been received regarding the Calcutta outbreaks of 1898, 1899, and 1900. In 1898, the recorded number of cases was 242 and of deaths 203, showing a case-mortality of 83·88 per cent.; in 1899, there were 2,467 discovered cases and 2,341 deaths, or a percentage of 94·9 deaths; and in 1900, the number of discovered cases was 8,822 and of deaths 8,278, implying a case-mortality of 93·8 per cent. It may be noted that this rapidly increasing growth and very high mortality have occurred notwithstanding the careful and wholesale disinfection with chemical substances of every house known to have contained a plague patient, and, latterly, even of numerous non-affected houses, and that chemical disinfection appears to have been the only plague measure that was consistently applied in this City.

Report of the Epidemics of Plague in Calcutta, by W. R. Bright, I.C.S., 1900; Official Statement showing the number of Plague Attacks and Deaths in the town of Calcutta.

Gatacre's Report p. 111; Jadhav's Report, p. 29; Snow's Report, p. 249; 7221; Proceedimgs, Vol. I., Appendix XIV., p. 391, Tables I. and II.; Campbell's Report, p. 140; Dyson and Calvert's Report, p. 6

34. *Age Mortality.*—Plague appears to be least fatal to very young and very old persons. In 4,215 cases collected from the statistics of seven observers, the percentage mortality among those of one year and less was 12·5. The rate of mortality rises to 75 per cent. for the ages betwen one and five; it then falls to 69·1 for the ages from five to ten; and it very gradually, and with almost perfect regularity, rises in each subsequent decade, until it obtains the high rate of 80·2 per cent. between the ages of 50 and 60. From 60 upwards there is a fall in mortality to 67·5 per cent., and the evidence appears to show that in persons of a very advanced age, such as 70 and upwards, the death rate becomes so low as 50 per cent.

Snow's Report, p. 218; Campbell's Report, pp. 132, 139; 25,015; 7221; 12,897; 14,443; 13,692; Jadhav's Report, pp. 9, 28; James' Report, p. 17; Proceedings, Vol. II., Appendix XLIX., p. 483; Vol. I., Appendix XX., p. 474; Campbell's Report, p. 48; Proceedings, Vol. I., Appendix XIV., p. 391,

35. *Race and Caste Mortality.*—A number of cases showing the death-rate in the chief races and castes have been collected from fourteen sources, representing nearly all the hitherto plague-affected districts of India. These cases amount in all to 14,668, but in some instances the numbers of individual races are only small. The Hindus with 9,302 deaths in 11,582 cases, yield a death rate of 80·3 per cent.; the Jews with 35 deaths in 45 cases, a death rate of 77·7 per cent.; the Musalmans with 1,247 deaths in 1,671 cases, a death rate of 74·63 per cent.; the Native Christians with 798 deaths in 1,115 cases, a death rate of 71·56 per cent.; the Eurasians with 76 deaths in 132 cases, a death rate of 57·6 per cent.; the Parsees with 38 deaths in 66 cases, a death rate of 57·6 per cent.; and the Europeans with 24 deaths in 54 cases, a death rate of 44·4 per cent.* Deductions are perhaps untrustworthy from the small number of Jews, Parsees, and Europeans; but it is believed that the majority of the Jews affected were derived from the lowest class of that community, and it is well known that the Parsees resemble Europeans rather than Natives in their mode of living. The above numbers are, however, sufficient to warrant the statement that the greatest mortality from plague has occurred among the Hindus, Musalmans and Native Christians, and very much in the ratio indicated by the above figures. Only a very few Chinese or Burmese are represented in the 14,668 cases dealt with, and, so far as the figures go, the percentage of deaths among them has been very high.

Campbell's Report, p. 137; Gatacre's Report, p. 110. Snow's Report, pp. 220, 221. Proceedings, Vol. II., Appendix XLIX., p. 484.

22,269.

36. *Mortality according to Occupation.*—Although in Bombay, mill workers, coolies, and domestic servants were among those most largely affected by plague, these classes, at the same time, furnished the smallest case-mortality, while the largest case-mortality occurred among syces, milkmen, tailors, ayahs, and washermen. In domestic servants the rate was 65·4, in shopkeepers 71·4, in coolies 72, and in millworkers 77·2 per cent.; but in syces and milkmen it was 89·47 and 86·66 per cent., respectively. In Porbandar, in a few cases in which washermen, barbers, and goldsmiths were attacked, the mortality was extremely great, and greater than among beggars, domestic servants, labourers, and corndealers and grocers, in whom it varied from 77 to 65 per cent. In Satara, the death rate was so high as 88 per cent. among gardeners, 85 per cent. among barbers, 80 per cent. among tailors, and 71 per cent. among scavengers; and the four washermen who acquired plague all died.

Campbell's Report, p. 138. Campbell's Report, p. 115. Gatacre's Report, p. 49. Report on Poona Outbreak, p. 18.

A short Report on the Plague in Bombay, &c. p. 3. 209,52; Snow's Report, p. 250.

37. *Mortality according to Position of Buboes.*—There is a difference of opinion, supported in several instances by statistical evidence, as to whether, in cases having buboes, the greatest mortality does or does not occur when these buboes are situated in the axilla. Dr. Hutchinson, dealing with 854 cases of plague, and Captain Jennings, with 992 cases, show that in their cases the greatest mortality is associated with groin buboes; in Dr. Hutchinson's cases the percentage of deaths being in groin buboes 91·61, in axillary 85·8, and in cervical 84·8. On the other hand, Captain Thomson, I.M.S., finds that the per-centage mortality in 238 cases was in groin buboes 56·1, in axillary buboes 73·41, and in cervical buboes 65; Captain Lloyd Jones, in 2,244 cases, in groin buboes 67·52, in axillary buboes 74·89, and in cervical buboes 72; and Mr. R. N. Jadhav, L.M. and S., in 476 cases, in groin buboes 64·47, in axillary buboes 78·72, and in cervical buboes 52·94. Dr. Dyson and Captain Calvert and the majority of those who have given evidence on this subject have also stated that the greatest mortality occurs in patients with axillary buboes; and Dr. Choksy's figures lead to the same conclusion with regard to cases having single buboes, but at the same time show that, when, in the same patient buboes occur in the femoral and iliac glands the mortality is slightly greater than in patients having only axillary buboes, the respective percentages being 83·33 and 82·35. It may, therefore, be concluded that cases with axillary buboes are the most fatal; and the frequency with which extensive effusion, sloughing and hæmorrhagic extravasation are associated with

* Where the number of Europeans is considerably larger than in these collected statistics, the death-rate is found to be even lower than 44 per cent. (*see* preceding page).

these buboes probably accounts for the great fatality which accompanies them. Among patients with cervical buboes, those in whom the parotid glands were implicated scarcely ever recovered. Campbell's Report p. 141.

Relatively to the side of the body in which these, the most frequently encountered buboes, occur, there is some evidence to show that in cases with groin and cervical buboes the mortality is almost the same whether they occur on the left or right side, but that in cases with axillary buboes it is greater when they occur on the left than on the right side; for example, in 466 cases with axillary buboes the percentage of deaths was 75·3 in bubces on the right and 82·6 in buboes on the left side. In regard to the mortality in the two sexes relatively to the position of the buboes, the evidence is not large in amount, but, so far as it goes, it appears to show that in cases with groin, axillary, and cervical buboes the percentage mortality on the total number of cases of each in the two sexes is always greater among men than women. Lloyd Jones' Report on the Poona Outbreak, p. 18.

VII.—The Incidence of Plague.

38. *Sex Incidence.*—In India, hospital experience appears to show that a much larger number of men than of women had become affected with plague. Captain Jennings, I.M.S., states the relative numbers as 693 male and 299 female patients; Major Dyson and Dr. Calvert, 133 male and 67 female patients; Mr. Cooper, 910 male and 710 female patients; Dr. Bhatt, 417 male and 93 female patients; Captain Thomson, I.M.S., 368 male and 155 female patients; Dr. Clemow, 331 male and 171 female patients; and Dr. M'Cabe Dallas, 260 male and 114 female patients. The totals of these numbers show 3,388 male and 1,512 female patients, or nearly twice as many male as female patients. In some places, as Poona, the disproportion was even greater than in any of the above instances. According to Lieut.-Colonel M'Conaghy, I.M.S., of 152 cases treated in the Sassoon Hospital, 136 were male and only 16 female patients, giving a ratio of more than eight to one. It would not, however, be justifiable to infer that the plague incidence is to a corresponding, or to any remarkable extent, greater among males than females, for in many cities of India the proportion of males is in excess of that of females,* during panics a larger number of women than men would probably leave affected towns and cities, and as the above figures represented admissions into hospitals, it is not unlikely, owing to caste prejudices regarding women, that a larger proportion of affected men than women were removed to hospitals. A truer indication, however, of sex incidence would be derived from the relative numbers of the two sexes in a general as distinguished from a hospital population. Evidence of this kind was obtained at Baroda and is specially valuable from this point of view. According to Rao Bahadur K. V. Dhurandhar, among 3,685 plague cases which occurred in the urban and rural districts of Baroda State, 1,995 were male patients and 1,690 were female patients, showing a fairly equal number in the two sexes. It may, therefore, be assumed that there is no special sex incidence in plague, both sexes being equally liable when subjected to plague infection, under conditions favourable to the propagation of the disease. Campbell's Report p. 115; Report on Plague in Bombay, p. 5; 15,363; Campbell's Report, p. 151; Gatacre's Report, p. 48; Proceedings, Vol. I., p. 257, 7221; Gatacre's Report, p. 111. 12,897. Proceedings, Vol. I., Appendix VIII., p. 352. 15,219. 22,361.

39. *Age Incidence.*—Many tabular statements have been placed before us showing the numbers of plague patients admitted at different ages into hospitals. Summarising those of Captain Jennings, Dr. Parsons, Colonel Thomson, Major Dyson and Captain Calvert, and Dr. Bhatt, amounting in all to 2,741 cases, the numbers admitted at the following different ages were:—Infancy, one patient; one to five years, 46 patients; five to ten years, 186 patients; 10 to 20 years, 647 patients; 20 to 30 years, 988 patients; 30 to 40 years, 491 patients; 40 to 50 years, 225 patients; and 50 years and upwards, 157 patients. The greatest number, therefore, were from 20 to 30 years of age, and following these in number were those between from 10 to 20 and between from 30 to 40 years of age; the first representing 36 per cent., the second 23·6 per cent., and the third 17·9 per cent. of the total. Otherwise grouped, 77·49 per cent. were between 10 and 40 years of age, 59·67 per cent. between 10 and 30, and 57·26 per cent. between 20 and 40. Campbell's Report, pp. 115, 156; Gatacre's Report, p. 48; Report on Plague in Bombay, p. 6; Campbell's Report, p. 151; Proceedings, Vol. I., Appendix XIV., p. 391.

When, however, the ages of those affected in the general population is examined, several differences from the above are seen. From information supplied by Captain James, I.M.S., and Assistant Surgeon Petigara relating to 4,734 plague patients in the Jullundur District and in Ankleshwar, it appears that 342 were from one to five years old; 525 from five to 10; 1,077 from 10 to 20; 1,021 from 20 to 30; 760 from 30 to 40; 496 from 40 to 50; and 160 from 51 years old and upwards. In this series of cases, the greatest number, 22·77 per cent., occurred between the ages of 10,330; 14,394.

* In Calcutta, where the number of men is, in ordinary conditions, much larger than the number of women, there were, in the outbreak of 1890, from 14th April to 31st July, 152 male and only 38 female cases. (Proceedings, Vol. I. Appendix XX., p. 474.)

from 10 to 20, in place of between 20 and 30 in the other series. As in the former series, the majority of the cases occurred between the ages of from 10 to 40, but this majority is only 60 per cent. of the total, while it is 77·49 per cent. in the former series. A reduction in number is also seen in each of the three decades embraced in this period; for the percentage to the total between from 10 to 20 years of age is 22·77, between from 20 to 30 years of age, 21·54, and between from 30 to 40 years of age, 16. The plague patients in the general population, on the whole, are more evenly distributed over the different ages than in the case of hospital patients: and, more especially, there is a larger number of young patients, 7·2 per cent. being from one to five years old, and 11 per cent. from five to ten years old, as contrasted with 1·6 and 7 per cent., respectively, in hospital patients. The statistics submitted by Assistant Surgeon Petigara have a further interest that they show the relationship of the numbers attacked at different ages to the population at those ages. 5·15 per 1,000 of the population from infancy to five years of age were attacked; 22·3 per 1,000 from five to ten years of age; 42·5 per 1,000 from 10 to 20 years of age; 34·6 per 1,000 from 20 to 30 years of age; 41·61 per 1,000 from 30 to 40 years of age; 36·25 per 1,000 from 40 to 50 years of age; 31·6 per 1,000 from 50 to 60 years of age; and 19·37 per 1,000 of the population 60 years of age and upwards. These figures likewise show that, although only a small number of children are attacked by plague, the number is larger than is indicated by hospital experience.

Gatacre's Report, pp. 48 and 123.

The youngest patients stated to have been affected with plague were seven months and ten months of age, and the oldest patients were 85 and 96 years of age.

40. *Race and Caste Incidence.*—In India, hospital statistics are not altogether trustworthy foundations from which to draw inferences regarding the racial or caste incidence of plague. In any given place, the numbers of each caste were not accurately known during the time of the outbreak, and variations occurred in the readiness with which members of different castes entered hospitals; while in certain places hospitals restricted to special castes were provided, which served to increase the numbers of these castes who were treated in hospitals. These causes of fallacy may to some extent be reduced by grouping together the statistics of hospitals in a considerable number of places. When that is done, it is found, in a collected total of 12,240 cases of plague, that 9,222 were Hindus, and 1,606 were Muhammadans, these two predominating sects thus together accounting for 88 per cent. of all the cases; and that 1,090 were Native Christians, 122 Parsees, 108 Eurasians, 52 Europeans, 29 Jews, and 11 Goanese. The Hindus, therefore, represent 75·34 per cent. of the total; the Muhammadans, 13·12 per cent.; the Native Christians, 8·9 per cent.; and the Parsees, 1 per cent.: the other groups together representing 1·6 per cent., of which only 0·42 per cent. were Europeans. The great preponderance of Hindus, followed, though by a long interval, by Muhammadans, is accentuated when the members of each caste or race are determined in a general as contrasted with a hospital population; for in the Baroda State, out of 3,760 cases of plague that occurred from the 1st of October 1896 to the 30th of June 1898, 3,200, or 85·1 per cent., were Hindus; 429, or 11·4 per cent., were Muhammadans; and 44, or 1·2 per cent., were Parsees. On the other hand, in Calcutta, where plague is believed to have followed in several respects an exceptional course during the first outbreak, the preponderance of incidence among Hindus was not so conspicuously displayed. Dr. Nield Cook has shown that out of 190 cases, 105, or only 55·2 per cent., occurred among Hindus; 56, or so many as 29·5 per cent., among Muhammadans; 24, or 13 per cent., among Eurasians; and two among Chinese, one among Burmese, and one among Europeans.

Gatacre's Report, p. 110; Campbell's Report, pp. 132, 115, 139; Snow's Report, p. 217; 25,015; 7221; 12,897; 14,443; 15,642; 13,692; Proceedings, Vol. I., Appendix XIV., p. 391.

15,219.

Proceedings, Vol. I., Appendix XX., p. 471

A truer conception of the caste incidence might be gained by calculating the number of cases that have occurred in each caste in an affected place. The data for such calculations, however, are limited to only a few places. They have been supplied, with some obvious uncertainties, regarding outbreaks in Porbandar and in the Bangalore District. In the former, where only 18,805 persons and 465 cases of plague are dealt with, the incidence is the same in Hindus and Muhammadans, being 2·5 for each caste; but in the latter, involving 622,628 persons and 4,992 cases of plague, the incidence among 584,083 Hindus is ·65 per cent., and among 34,863 Muhammadans 3·2 per cent. This large and exceptional incidence among Muhammadans is thus commented on by the Deputy Commissioner:—"From the experience so far gained no deductions can be drawn as to the greater or lesser liability of any particular sect or class to succumb to an attack of plague, but it is abundantly clear that the dirtier and worse ventilated the dwelling the greater the liability of the occupants to catch and succumb to plague."

Proceedings, Vol. II., Appendix XLIX., p. 484; Madhava Rao's Report, p. 21.

On the whole, there appears to be among the permanent inhabitants of India a general accordance between the number of cases of plague in each caste or race, and

the number of the caste or race in the affected community; but the variations that occur in many places and between several races suggest that local conditions strongly operate in the distribution of the cases of plague. It has, for example, been pointed out by Assistant Surgeon Petigara that only an insignificant percentage of cases occurred among the Bhils of Ankleshwar as contrasted with the high percentage among other sub-classes of Hindus, due to the Bhils living "on the outskirts of the town, and to the open-air life they led." 14,449.

41. *Occupation Incidence.*—The statistics laid before us in regard to occupation incidence are inconclusive, as the numbers are small, and as great variations occur in different places. In Bombay, coolies, mill workers, labourers, syces, domestic servants, beggars and persons without fixed occupation furnished the largest numbers of plague patients. In Porbandar, the largest numbers were furnished by labourers, boatmen, sailors and fishermen, beggars, corn dealers and grocers, and domestic servants. In Satara, gardeners, tailors, barbers, scavengers, goldsmiths, shoemakers, tanners, oil pressers, and brass founders were the chief victims. Plague occurred in only a few washermen, and prostitutes and wandering beggars nearly altogether escaped. The evidence, generally, is in the direction of showing that, with equal opportunities for personal infection, the incidence of plague is not dependent on the trade or occupation, but rather on the conditions of living, whatever be the occupation. This has been illustrated by many examples, as, for instance, by the occurrence of a much larger number of plague cases among the members of the Bombay Police Force living in ordinary native houses than among those living in the well-ventilated police barracks; and by the freedom from plague of the rice cultivators at Satara, although they lived in huts in the midst of an infected area, which has been explained by their working from morning to night in the open air during the rainy season; and by the immunity of fakirs, whether Hindu or Muhammadan, who live a specially open-air life. Proceedings, Vol. I., Appendix XIV., p. 392; Campbell's Report, p. 137; Gatacre's Report, p. 110; Snow's Report, p. 221. Proceedings, Vol. II., Appendix XLIX., p. 484. 22,266–9. Snow's Report, p. 147, para. 566; 22,127–8.

VIII.—Diagnosis.

42. *Pestis major.*—So preponderating is the number of cases in which plague assumes the type of *Pestis major* that it must be regarded as being generally an easily diagnosed affection. In this type of it, the peculiar expression, the early occurrence of fever and prostration, the existence of a bubo or buboes that are painful and tender, the congested eyeballs, the furred tongue with clean red margins, the impaired memory, the indistinct and hesitating speech, and the rapid, weak, and often irregular pulse, are alone sufficient to render the nature of the case unambiguous. It could only be at the beginning of an outbreak that doubt might be suggested by cases of such diseases as malaria, remittent or relapsing fever, congenital syphilis with enlarged glands, scrofulous glands or parotitis. In some of the less frequently encountered varieties of this type, however, the diagnosis is attended with difficulty. In the variety in which bowel complications occur it may for a time be mistaken for dysentery or enteric fever. In the rare septicæmic variety, in which rapid prostration usually occurs without the production of external buboes, the diagnostic features may be so masked as to suggest doubts which cannot always be removed during life even by bacteriological examination. In many of these cases, however, bronchitic and pulmonary complications yield a sputum in which plague bacilli are present. 27,138. Proceedings, Vol. I., Appendix VIII., p. 352. Campbell's Repor p. 160. 20,618–9.

43. *Pestis minor*, in each of its varieties, must always present difficulties when the diagnosis is based merely on the symptoms of individual patients. In the variety with glandular enlargements, the transient fever may prove of assistance, but the nature of the illness will more often be determined by excluding causes of glandular enlargement other than plague, such as strains, traumatic injuries, scrofula, venereal disease, and ulcers. In the variety without buboes, the diagnosis of an individual case is probably impossible. In both varieties, however, practical certainty may be obtained, when, in addition to the above circumstances, it is found that cases of a similar nature are prevalent, and evidence of infectivity is thus obtained.

44. *Primary Plague Pneumonia* is distinguished from ordinary pneumonia by the great severity of the general symptoms relatively to the manifestations of lung disease, and especially by the high temperature, rapid respirations, and great prostration; by the generally isolated and scattered distribution of the involved portions of lung; by the slightness of the symptoms of lung disease; and by the special characters of the sputum. Microscopic examination of the sputum at all stages of the illness reveals the presence of plague bacilli, and, therefore, in this type of plague, is of specially easy application. 15,666.

Microscopic examination of the sputum is also applicable as a means of diagnosis in the secondary pneumonia and broncho-pneumonia of plague, and of the material obtained 18,906; 18,908. 18,907.

from non-suppurating buboes in ordinary *Pestis major*. Mere microscopic examination of the blood in all types of plague, including the septicæmic, as well as of material from a bubo in *Pestis minor*, would, however, most frequently prove inconclusive; for in all types of plague, bacilli are generally present in the blood in only small numbers, and usually only a few hours before death, while very few are present at any time in the buboes of *Pestis minor*. Definite results could be obtained only by culture experiments and experiments in animals, both of which, obviously, are possible only in exceptional circumstances.

4330-5 18,913; 26,261-2.

26,402.

The agglutination or sedimentation test and Widal's reaction are largely open to the same difficulties of application; but, in any case, trials made of them in India have proved inconclusive.

26,220-6; 2965-80; Proceedings, Vol. I., Appendix XV., p. 398.

45. *Post-mortem Diagnosis.*—The body, if undisturbed, is stated to be generally lying on one side, with the knees flexed, and the head slightly bent on the chest. The skin is dry, and in a few cases petechiæ and purpuric spots may be seen; the muscles are soft, as the rigidity of death is delayed; the features retain a fixed anxious appearance; the eyes are half closed and sunken, with the pupils dilated; and the tongue is swollen, and although it sometimes has the appearance displayed during life and already described, not unusually it is covered with a dry, almost horny, dark or yellowish brown and cracked fur. In pneumonic cases, the body seems specially shrunk and collapsed and has a livid aspect, and blood-stained sputum is often found adhering to the lips.

Gatacre's Report, p. 147.

Our attention was drawn by Captain Elphick, I.M.S., to the existence of œdema, extending over the front and sides of the chest, the abdomen and the upper part of the arms, which had been observed by him and by Captain Chaytor-White, I.M.S., in all plague corpses, and was believed to occur immediately before death. If this condition is generally present, it would be valuable as an aid in determining if death had been caused by plague, and especially in cases where no buboes are present. It has not, however, been observed by the great majority of those who in India have had the opportunity of seeing large numbers of fatal cases both before and after death.

9544.

10,345; 10,379-83. 10,424-9. 12,403-8. 12,925; 15,926. 18,557.

IX.—Prognosis.

46. With a general mortality in fully developed plague of from 60 to 90 per cent., indicating a virulence exceeding that of any other epidemic disease, the prognosis of plague is in India extremely grave.

The chances of recovery are smallest in pneumonic plague, and somewhat better, though still inconsiderable, in the variety of *Pestis major* in which a profound intoxication has been rapidly produced, distinguished as the Septicæmic variety, and also in cases of *Pestis major* with extensive cellulitis proceeding from axillary or cervical buboes. The prognosis is graver in patients with axillary and parotid buboes than with buboes in other situations; in Hindus and Muhammadans and others living in the usual conditions of native existence, than in Europeans or those living in similar conditions to them; and in patients from one to five and from 50 to 60 years of age than at all other periods of life, and especially than the very young and the very old. It is stated to be less favourable in obese and heavy patients than in those of moderate or light weight; but it is in harmony with general observation that the most robust and well fed are attacked and succumb to the disease equally with the feeble and poorly fed. Among the chief circumstances that indicate an unfavourable termination are a very high temperature, "or a sudden fall, sudden extension of infiltration, the simultaneous appearance of buboes in other parts of the body, sharp diarrhœa and intense dyspnœa." An unfavourable termination is also indicated by great weakness of the pulse, hypostatic congestion and œdema of the lungs, secondary pneumonia or broncho-pneumonia, convulsions or early and violent delirium, and hæmorrhage from any surface or organ of the body. As any or several of these symptoms may occur when the condition of the patient was until then altogether favourable, the prognosis of every case of *Pestis major* is uncertain during the earlier period of the illness. It is, however, a distinguishing feature in the prognosis of plague that usually the period of suspense extends over only a few days; for nearly all who survive until the eighth day, rather less than four-fifths of those who survive until the fifth day, and more than one half of those who survive until the third day may be expected to recover.

12,413.

12,413. 20,910; 27,028; Proceedings, Vol. I., Appendix VIII., p. 352; Madhava Rao's Report; Snow's Report, p. 247; N. H. Choksy's Report on Bubonic Plague, p. 39. 12,413; Gatacre's Report, p. 124; 15,677-9; 12,413.

X.—Treatment.

47. The treatment of plague includes—(*a*) measures for preventing the development of the disease in persons who have been exposed to infection, as well as (*b*) measures for curing the already developed disease.

(*a.*) *Prophylactic or Preventive Treatment.*—The subject of the prevention of plague has partly been discussed in the Chapter of our Report dealing with Mr. Haffkine's prophylactic injections, and partly also in the remarks separately made by me on the influence of insanitary conditions on the spread and virulence of the disease (Appendix III.).

The evidence appears to indicate that prophylactic properties are not developed by Mr. Haffkine's fluid until some considerable time after the fluid has been injected. If, therefore, the virus of plague have been introduced into the body previously to or a few days subsequently to the injection, the fluid would fail to prevent the development of plague.

On the other hand, sera, such as those of Lustig, Yersin, Roux and Galeotti—although their operation is more generally associated with the treatment of the already developed disease—are capable also of exerting a prophylactic influence, which, while it lasts for only a few days, has the great advantage of being almost immediately produced. In order to reduce the chances of plague occurring in persons who are suspected or are known to have been exposed to infection or who may soon be so exposed, it is therefore advisable to inject subcutaneously one or other of these sera for the purpose of conferring protection. Should no symptoms of plague manifest themselves within eight days, Haffkine's prophylactic fluid may then be injected with the hope of securing a more lasting although less speedily induced protection. At the same time, if the disease should develop itself within a few days after the injection of the serum, it may confidently be anticipated that the severity of the illness will not be so great as it would otherwise have been; for the curative power of a serum possessing therapeutic properties is more powerfully developed when it is administered during the earlier stages of the disease than during the later stages, and with still greater prospects of success when it is administered shortly before the reception of the virus.

The sera and prophylactic preparations at present available possess, however, only moderate and limited protective power, sufficient, nevertheless, to warrant their use, but insufficient to justify an entire trust in their employment. A generally available preventive, and, in the meantime, the most certain of all preventives, is the maintenance of good hygienic conditions. An open-air life and adequate lighting and ventilation of dwellings, with the removal from them of overcrowding and all other causes of air pollution, have been amply shown in the Indian and in other epidemics to confer effective protection. Whatever other measures are employed, these should receive the first attention and should in no circumstances be overlooked or neglected.

48.(*b.*) *Therapeutic or Curative Treatment.*—Although the mortality from plague in hospitals, especially when those patients are excluded who were admitted moribund or nearly so and who died within 24 hours, is smaller than that of the town where the hospital is situated, it is not easy to appreciate how far this reduction was caused by medicinal treatment. The good hygienic conditions, and especially the pure air and absence of overcrowding and the efficient nursing arrangements, constituting in each respect a strong contrast with the conditions to which patients in the vast majority of cases were subject in their dwellings, could not fail to have a favourable effect on the progress of the illness. At the same time, there are clear indications for medicinal treatment which have been followed and have probably aided in reducing mortality, although their benefit cannot at present be stated definitely.

Snow's Report, pp. 216, 248; Gatacre's Report, p. 150; Lloyd Jones' Report, p. 16; A Report on Plague, by Captain James, I.M.S., 1897, p. 16.

In the general treatment of patients, experience has strongly emphasised the necessity for abundance of fresh air, for complete, passive repose, strictly to be continued for at least four days after the temperature has become normal, and for the maintenance of strength by the frequent administration of food in small quantities and by the liberal use of alcoholic and other stimulants.

The more strictly medicinal treatment has been in the directions of furthering elimination, of endeavouring to reduce the hurtful power of the virus within the body by the administration of substances intended to act directly upon it, of reducing temperature, of strengthening the circulation, and of removing such symptoms of the disease as sleeplessness and delirium.

Purgatives, and especially calomel, have accordingly been largely used at the commencement of treatment, and diuretics or diaphoretics as indicated in the course of the illness. The increasing of elimination is also believed to be a chief advantage of a plan of treatment, which has been recommended to our notice, consisting of subjecting the patient to a vapour bath at the temperature of 112° F., followed by partial immersion in water gradually cooled from about 99° to 80° or 75°, and by friction of the skin; but we had no opportunity of obtaining verbal evidence with regard to it. The effect on mortality is not clearly favourable, as 72·7 per cent. of the patients died. The treatment produces much diaphoresis, and also, it is claimed, profuse diuresis.

Gatacre's Report, pp. 69, 142; Snow's Report, p. 252; Captain James' Report on Plague, p. 16; Dyson and Calvert's Report, p. 16; Lloyd Jones' Report, p. 9; 1860

Proceedings, Vol. II., Appendix A., p. 527.

The latter, however, is not manifest from the statement which has been submitted. Before an opinion can be formed as to the merits of this plan, observations on a more extended scale, with precautions to exclude several fallacies, would be required, especially as the weakness of the circulation, so conspicuous in plague patients, suggests that the plan of treatment is likely to be attended with danger.

Undoubtedly, the rational therapeutics of plague, as of all other bacterial diseases, is to be found in the administration of substances capable of rendering innocuous the specific virus within the body of the patient. The results with the several anti-toxic sera designed to accomplish this, which have been used in India, are certainly encouraging, and justify the hope that with sera of greater bactericidal and anti-toxic power than those hitherto prepared more definite therapeutic benefit will be obtained. To obtain the best results, the serum should be injected at as early a stage in the illness as possible, the quantity should be large, and the injections should be repeated for several days even although the temperature of the patient and the general condition have been much improved by the previous injections. In severe cases, and certainly in the pneumonic and septicæmic forms of plague, the serum should be injected directly into a vein, for the therapeutic power of a serum is much greater when it is injected into a blood vessel than when it is injected into the subcutaneous tissues. At the same time, it is not probable that a serum will ever be prepared which is capable, even by intravenous injection, of preventing death in pneumonic and septicæmic plague after grave symptoms have become developed, and, therefore, when the virus is already diffused in large quantities throughout the body. On the reasonable supposition that in bubonic cases the plague bacilli are for a time concentrated in the primary buboes, it is advisable to inject the serum into the buboes or the tissues immediately surrounding them; and it is probable that the best results will be obtained by this plan of administration, which, however, does not appear as yet to have been employed in practice. The subject of therapeutic sera is more fully discussed in Chapter V. of our Report.

Proceedings, Vol. I., Appendix VIII., p. 353; 20,934–71; 21,050-2; 27,030–64.

22,372; Gatacre's Report, pp. 22, 70, 142; Captain James' Report on Plague, p. 17; 11,905.

11,905.

On the same principle, such bactericidal substances as mercuric chloride and carbolic acid have been administered in large doses. It is stated that much benefit was thereby produced, and with a combination of quinine and carbolic acid Mr. Seymour has claimed that the mortality was reduced so low as 25 per cent. An objection to the employment of medicinal substances for this purpose is found in the absence of restriction of action upon the *causa mali*, and a consequent dissipation of power in unwished-for directions; for the most efficient of them equally affect and combine chemically with many of the ordinary constituents of the body, and thereby fail to be present, in their originally active condition, in sufficient quantity to produce an appreciable destructive action on the virus, unless very large and therefore poisonous doses are administered.

Gatacre's Report, pp. 22, 71; 372.

Snow's Report, pp. 257, 258.

On the supposition that in many cases there is a concentration of the virus in the buboes, preliminary to diffusion throughout the body, several antiseptics, such as mercuric chloride, iodine, carbolic acid, and creoline, have been injected near or into the substance of the buboes. The results were not definitely confirmatory of expectation, although local benefit was believed to be obtained by the process of suppuration having been hastened in the buboes. A more effective application of this principle would probably be found in the substitution of a curative serum for an ordinary antiseptic substance, especially as such a serum, while energetic in its action upon the virus, is free from the objection of extending its action to the tissues and fluids of the body. Even weak sera, such as are at present in use, may be efficient if brought into contact with the virus concentrated in one place, whereas if both were diffused throughout the body the virus will remain unaffected.

Gatacre's Report, pp. 70, 143; Snow's Report, p. 256.

In the endeavour to reduce temperature, to some extent unsatisfactory experience but apparently to a greater extent a supposititious dread of increasing the existing weakness, has restricted the use of anti-pyretic substances such as acetanilide, phenacetin, phenazonum, guaiacol, and even quinine. Reduction of temperature has, however, been obtained, apparently with favourable results, by cold and tepid sponging, and the application of ice to the head.

Gatacre's Report, p. 69; Snow's Report, p. 257.

15,315.

22,376.

As we have already seen, feebleness of the circulation constitutes an early and striking feature of plague. It has accordingly been recognised as a prominent danger demanding special treatment. For this purpose, alcoholic stimulants have been used, and, in many hospitals, a routine treatment for all patients, except Muhammadans and children, was the administration of rum immediately after the admission of the patient. Alcohol and carbonate of ammonia have also been freely given during the course of the illness; but more favourable results have apparently been obtained by the use of

medicinal agents, which act more restrictively and powerfully upon the heart and blood vessels. With this object, digitalis and strophanthus have been used to strengthen the heart's contractions, and digitalis and strychnine to counteract the feebleness of the blood vessels. The greatest success appears to have been obtained by the subcutaneous administration of a combination of strophanthus and strychnine, or by combining the administration of digitalis by the mouth with that of strophanthus by subcutaneous injection.

Gatacre's Report, p. 59; Snow's Report, p. 256; James' Report on Jullundur and Hoshiarpur outbreaks, p. 148; Jadhav's Report, p. 32; Captain Lloyd Jones' Report, p. 9; Captain Wilkinson, I.M.S., Report to Punjab Government, September 1897, p. 29.

Opium, morphine, bromides, hyosciamine, hyoscine, sulphonal and tetranol have separately and in various combinations been employed to produce sleep and control mental excitement and delirium. Apparently the best results have been obtained by combinations of morphine with bromide of potassium or atropine, and by the application of cold to the head.

Snow's Report, p. 255. 4328.

There seems to be a consensus of opinion that many medicinal substances are tolerated to an exceptional degree by patients suffering from plague. This has been particularly observed in the case of mercurial preparations, which are stated to produce no salivation in quantities that usually succeed in doing so, and of morphine and strychnine, which have been given in extraordinarily large doses for long periods of time without causing any toxic manifestation. A similar tolerance is claimed for alcohol, but it does not appear to differ from the experience of its effects in many other acute affections.

Gatacre's Report, pp. 70, 71, 102, 142. Snow's Report, p. 253; Captain James' Report on Plague, p. 17; Dr. Jadhav's Report, p. 33. 4327.

49. *Local Treatment of Buboes.*—While buboes are still in the early stage it has been found that no benefit, but rather injury, has resulted from cauterising or incising them, or from the application of leeches. The practice now followed is to apply extract of belladonna and glycerine, or ice, or strong solution of nitrate of silver. Cauterising or scarifying the surface, or the abstraction of blood by a rude method of cupping, are, however, favoured by unqualified native practitioners. When evidence of the formation of pus is obtained, it is fully recognised that free incision is required, with the subsequent liberal use of antiseptics, of which iodine, mercuric chloride and lysol appear to have been the most generally employed. For reasons that have been stated above, the injection of an efficient therapeutic serum into the bubo is likely to produce beneficial results.

Gatacre's Report, p. 143; Snow's Report, p. 258. Snow's Report, p. 258; 12,908; Jadhav's Report, p 34; Captain James' Report on Jullundur and Hoshiarpur outbreaks, p. 148. Gatacre's Report, p. 143; 12,908.

50. *Treatment during Convalescence.*—On account of the great prostration that accompanies and follows an attack of plague, and especially the great weakness of the circulation, the patient should be kept strictly in a recumbent posture until, at least, four days after the temperature has become normal. In such patients as have recovered from the acute condition, the dietetic and tonic treatment obviously indicated has been followed. Iron, quinine, phosphorus and cod-liver oil have been the chief tonics employed. Convalescence is usually protracted by great weakness, or by the occurrence of prolonged suppuration of buboes, or by slow recovery from skin necrosis, or by one or several of the other complications already referred to. As it is believed, and the belief is supported by bacteriological evidence, that a convalescent may cause infection for from four to six weeks after all acute symptoms have disappeared, isolation for at least four weeks after the pyrexia has subsided is advisable, especially in cases of pneumonic plague and in cases with protracted secondary broncho-pneumonia.

Gatacre's Report, p. 69; James' Report on Plague, p. 16.

20,427; 26,962. Annales de l'Institut Pasteur September 25th, 1900; 25,355; Zeitschrift, f. Hygiene, 1899, p. 405.

T. R. F.

APPENDIX No. III.

MEMORANDUM BY THE PRESIDENT ON THE INFLUENCE OF INSANITARY CONDITIONS UPON THE EXTENSION AND VIRULENCE OF PLAGUE IN INDIA, DISAGREEING WITH CERTAIN OF THE CONCLUSIONS OF THE MAJORITY OF THE COMMISSION.

The Plague in India, 1896, 1897, by R. Nathan, I.C.S., p. 42; Papers relating to Village Sanitation in India, 1888–1895, published by authority of the Government of India; Presidential Address to First Indian Medical Congress, by Surgeon-Colonel R. Harvey, 1895.

1. The opinion is widely entertained that plague forms no exception to other infectious diseases in being fostered and maintained by insanitary conditions. It, therefore, appears of importance to examine the evidence bearing upon this subject, with the view of determining to what extent the progress and virulence of the present Indian epidemic have been influenced by the existing condition of sanitation, and, if they appear to have been injuriously affected, with the view, also, of ascertaining in what directions improvement is mainly suggested and to what extent and by what means it may be effected.

It may at once be stated that the evidence placed before us, as well as personal observations made in India, have rendered it only too evident that the sanitation, both of the urban and of the rural districts of that country, is in an unsatisfactory condition. So generally is this conceded, however, that it is sufficient to illustrate the statement by a few examples relating to places which have been affected with plague.

I.—Examples of Insanitary Condition of Towns, Villages and Houses.

Description of insanitary condition of towns and villages.

Vol. I., Appendix XX., 463.

2. Calcutta is thus described by its Health Officer: "It possesses few advantages except the proximity of the River Hoogly, being flat, low-lying, and alluvial. By making a large number of tanks the level has been somewhat raised in parts, but most of these tanks are so exceeding foul as to come within the category of nuisances. In the native quarters of the town buildings have been run up under little control, so that they are crowded together on the site. Add to this an increasing population, defective system of sewage, and an insufficient establishment of scavengers, and no sanatarian will be surprised that in spite of a considerable expenditure on the part of the town authorities the health of the town has steadily deteriorated." This deterioration is shown by the general death-rate having increased from 28·7 in 1889 to 36·1 in 1897, and the fever death-rate from 8·8 to 14·1 in the same period.

Sanitary Inspection Report on the Calcutta Municipality, October 1898, pp. 3, 4.

In the Report of a Sanitary Survey of Calcutta made in 1896, it is stated that, so far as the Committee who made the Survey had observed, there is very considerable overcrowding in every one of the wards. "Other native cities have been reported on as being bad enough, but the metropolis, which should serve as a model, is obviously at least five times as overcrowded as the most overcrowded municipality in Bengal." Many specific examples are given, as that of a house with three living rooms on the ground floor, each measuring 6 × 8 × 8 feet, which are "pitch dark and quite unventilated," while on the first floor there are six living rooms, "of which

Do., p. 8.

only the two front rooms are decently ventilated and lighted"; and of a house in which only two of the twelve rooms "have any ventilation, except that of the doorways," the house being occupied by 150 people, although the Committee considered that "there is only room for 40 or 50 people at the outside." Though a large portion of Ward 25 is thinly populated, in the areas properly understood as Watgunge and

Do., p. 21 (5).

Kidderpore, there is very great overcrowding both of houses and of people inhabiting them. "This is especially well marked in the part of Kidderpore between Diamond Harbour and Garden Reach Circular Roads, where pucka buildings and bustees exist in such large numbers, and are so crowded and badly arranged as to endanger the public health, if an epidemic, such as plague, should ever break out." The most conspicuous evils to which attention is drawn relate, however, to drainage and

Do., p. 6.

conservancy. "The condition of the house drains and 'down-pipes,' which were intended to convey latrine and house and sullage water, were in a deplorable condition. In a large number of cases, the down-pipe was broken at from 5 to 15 feet from the ground, and the water, urine, and liquid sewage from the houses were simply splashing on the ground, fouling the whole gully or lane, and soaking the walls of the houses which, in many cases, were most thickly covered

Do., p. 8.

with filth." "In a street in Ward 17 the houses are built almost back to back. It is nearly impossible to squeeze between them; sunlight is so far shut out that,

with broad daylight outside the gully, it is absolutely impossible to do more than to grope your way from one part to another within these tenements; rats run about here in the dark as they would at night; a heavy sickening odour pervades the whole place; walls and floors alike are damp with contamination from liquid sewage which is rotting, and for which there is no escape. . . . Jammed in among these overcrowded huts are clusters of filthy, dilapidated privies, which are fouled beyond description, with their drainage discharging into the little space that is left, and polluting everything." Notwithstanding the improvements which have been effected since this Report was published, or since the commencement of the present Indian epidemic of plague, Members of the Commission had opportunities of observing that these insanitary conditions still largely exist in Calcutta.

In Major H. J. Dyson and Captain J. T. Calvert's Report on the Plague in Bombay it is stated that "The population amounted to 821,764 persons, spread over an area of 22 miles, thus giving an average density of 37,352 to the square mile. Unfortunately the population is not equally distributed, but varies within the widest limits amongst the 32 sections into which the city and island are divided. Thus the density varies from 700 per acre in Kumbharwada to 4·6 to the acre in Sion, whilst an average of 12 sections have no less than 458 persons per acre. In these thickly populated wards so narrow are the streets and so close together are the houses that the latter occupy 85 per cent. of the ground area." p. 8.

In one of the most crowded of these areas, Kamathipura, "the level of the subsoil water is the highest in Bombay, and polluted through old reclamations with refuse;" . . . "large areas of Byculla are without drains, and buildings in other portions, where there are no drains, are on such low grounds that they cannot be drained . . . and in Khetwadi, where two large plague hospitals are placed, a large area—more than one-half—has buildings that heavy rains leave standing in sewage and water." On account of its position, the drainage of Bombay is impossible without a large expenditure of money for pumping the sewage from the low-lying grounds; more than a quarter of the city is without drains, and Malabar Hill, where the finest residences are situated, is destitute of sewers, and possesses storm-water drains only. "In the drained districts the solid matter is received in baskets or in pans at the bottom of the privies and removed by halalkhores, and the fluid portion flows into the drainage system." In "the districts where house connexions on the new system have not been carried out liquid matter flows from an open gali into the street sewers. In districts that have no drains the solid matter is removed to depôts and the liquid matter is received in cesspools." It "is the most crowded city in India." The "population is concentrated in houses that are very closely packed and very imperfectly ventilated. There are in some houses in Bombay over 1,000 persons" (according to others as many as 2,000 or 2,500) "distributed in rooms, a family, or even more among the outcast population, to each room." "To 9 out of 11 houses in Mody Street in which cases at that period occurred there were no gullies on one side of the buildings for either ventilation or light." The average mortality in the five years preceding the outbreak of plague was stated by Dr. Weir to range between 50 per 1,000 in the worst localities, including the residences of by far the greatest part of the population, and 10, 15, and 20 per 1,000 in Malabar Hill and the wealthy suburbs; and there is "an enormous mortality" from phthisis, attributed to dampness and to the bad air in the dwellings.

Report on the Outbreak of Bubonic Plague in Bombay, 1896–7, by P. C. H. Snow, Esq., I.C.S., pp. 190, 191.

25,741.

25,750.

257,54.

25,760.

833.

17,289.

Snow's Report, p. 93, para. 295.

883–7.

Cutch Mandvi, a seaport town of 38,000 inhabitants, is, as described by Lieutenant-Colonel Wilkins, I.M.S., "almost square in shape, and, roughly speaking, covers a square mile; there are high walls of about 20 feet in height, with bastions here and there, surrounding the city, and four large gates and some small ones. On proceeding inside the city through one of the gates we come on a dense mass of houses, mostly built of stone and evidently very much overcrowded during ordinary times. The so-called streets are in reality narrow and tortuous lanes, the widest part not being more than 14 feet wide, and most of them very much less than 10 feet wide. The houses are for the most part built of stone, and are usually one storey in height, and nearly all of them enclosed by high walls with gateways leading into the courtyard, which is surrounded by small rooms which are ill-ventilated, gloomy, and dirty, and the lower rooms usually without any plinth, and sometimes below the ground level. The courtyard is generally used as a stable for cows and other animals, and, as it is not the custom to clean these yards, the smell from them is usually very offensive. The habits of the inhabitants are decidedly dirty, the latrines are usually the streets, and it is

Report on the Bubonic Plague in Bombay, by Brigadier-General W. F. Gatacre, C.B., p. 208.

said it is not considered etiquette on the part of a stranger to enter the city before 8 a.m., up to which hour the inhabitants use the streets for purposes of nature. There are a few latrines attached to the walls of the city on its western side, but these are hardly ever cleaned, and the consequence of this free and easy method of sanitation may well be imagined, and it is not to be wondered at that once the plague bacillus found a haven of rest here it flourished to a great degree, for it had all its requirements, *viz.*, filth, darkness, dense overcrowding, and no ventilation. The streets have no drains." A great many cattle are also kept inside the houses.

13,349-55.

Referring to Karachi, Lieutenant Anderson, I.S.C., stated: "In that portion of the Market Quarter in which the disease was most severe, the houses are mostly very old and built of mud plastered over a framework of sticks. Latterly, stone fronts have been built to some of them, but the house behind its imposing exterior remains the same as before. The foundations of these houses are saturated with sewage and filth of every description. There is generally a cesspool below the house which has not been emptied since the house was built, and the neighbourhood of which is honeycombed with rat holes, and which is probably situated close to a well from which water is drawn for washing if not for drinking purposes. This evil was aggravated by the narrowness of the streets and the scarcity of windows. The municipality have followed a fatal policy of allowing householders to encroach upon the streets and selling them strips of land in front of their houses until it has been absolutely impossible to sell any more. Overcrowding, moreover, exists to a terrible extent. . . . It is, therefore, not surprising that the plague was so severe in this quarter."

22,649.

Report on Plague in the Mysore State by Madhava Rao, 1898-99, p. 4.

The Plague Commissioner for the Mysore State has thus described the condition of Bangalore City: "Established three and a half centuries ago, before the laws of hygiene and sanitation were understood, it has been allowed to go unrestricted, according to the exigencies of the moment and confined practically to its original limits. As a result, it has become terribly overcrowded, and since sanitation was unknown until the introduction of the municipal regulations, and as even then there were localities so built over as to defy all attempts of the sanitary authorities at cleansing them, its soil had become saturated with the impurities of generations. It was here, while the work of relieving this congestion and of improving this sanitation was still far from completion, that plague broke out, and it was within this area that it most persistently continued its ravages." When asked what was the sanitary condition of the affected localities in the city, he replied: "Sanitation was very defective in all the localities."

5250.

2308-9

Major Hardy, I.S.C., Chief Superintendent of plague operations in Dharwar City, described the houses as being chiefly small and tile-roofed and without windows; the water supply as being obtained from two public wells, one at each end of the city, and from many private wells in the compounds of houses; the privy accommodation as consisting of five latrines at the south end of the city, sufficient for only 600 of the 35,000 inhabitants, and of numerous pit privies, close to and even inside of dwellings, which are 15 feet deep and from 2 to $2\frac{1}{2}$ feet wide, and as they are never emptied they are often full to the top and very offensive; and the drains as being open gullies in the streets, lined with stones but not cemented, from which the contents escape and foul the soil.

2314-6.

2317-22.

Lieut.-Colonel Bartholomeusz, I.M.S., stated that the sanitation of Ahmedabad is very unsatisfactory, especially "with regard to want of drainage, narrowness of streets, overcrowding of the houses, and removal of night soil. The water supply is very good."

13,957.

Describing the sanitary measures adopted during the first plague outbreak at Surat, a town of 100,000 inhabitants, Mr. Moore, I.C.S., mentioned that several thousand pit privies were closed, which were in the ground floor of the houses either inside or just outside the house wall, very often in close proximity to wells from which water was taken for drinking, and which, in some instances, had not been cleared within the memory of the present owner.

24,459.

In the Baroda State "the dwellings of the ordinary people are of a poor description. They are damp, ill-ventilated, and constructed without regard to sanitary precautions. The population being mainly agricultural, houses and streets are crowded together in small areas. People and cattle dwell under the same roof, and the soil is saturated with human and animal excreta. The pollution of the soil, air, and water is common."

15,193.

3. In the evidence laid before us and in the Reports on plague which we have had an opportunity of considering, the faults of general sanitation, however, occupy a subordinate place to those specially relating to the insanitary state of the dwellings and the nature of the conditions existing within them.

Description of insanitary condition of dwelling-houses.

In their report on the plague in Bombay, Major H. J. Dyson, I.M.S., Sanitary Commissioner, and Captain J. T. Calvert, I.M.S., thus describe the chawls in which a large portion of the inhabitants of Bombay reside: "On the ground floor is a narrow central passage about three feet wide with mud floor running the entire length of the building. At one end of this passage is the opening into the street; at the other is a latrine common to the floor on the one side, and the fresh-water cistern on the other. From this central passage doors open on each side into small cubicles. The floor of the cubicles is of mud, the partitions of wood or of small sticks plastered with mud. There is no window, no chimney, no light or ventilation, save such as penetrate through the doorway from the dimly lighted central passage. In the bright light of an Indian sun at midday, so dark are they that it is impossible to distinguish the presence of a second person in the room, and small oil lamps have to be used when cooking has to be done. The cubicles average about 10 feet square and 8 to 10 feet high. The number of persons who sleep in them is usually limited by the floor space, for on a rainy night, sleeping out of doors being impossible, they lie like sardines in a tin. The expression 'usually limited' is used advisedly, for in some cubicles a small pigeon loft about 6 feet from the ground has been built, access to which is obtained by means of a bamboo ladder, and here in a low garret sleeping accommodation is found for still more. Imagine five more storeys of this description connected by narrow staircases, and you have a picture of an average Bombay chawl." It may be added to this, as a result of personal observations, that the central passages of many of the chawls that were visited were nearly dark and were filthy with moist mud and human excrement; and that, as the above description implies, the chief, and sometimes the sole, means for the admission of air into the small dwellings or cubicles was by these passages. The Medical Officer of Health, Dr. Weir, gave the following further information: "You were going to say something more about the ventilation?—The difficulty is with regard to ventilation at night. There is a front door which closes the central passage. Where there is not over the front door an open grating, the ventilation (except in cases where there happens to be what is called a chauk over the staircase) is from a little space at the back where the privy is." "That opens outside of the house into what?—That opens outside the house into what is called the sweeper's gali, a small gali." "Which is not very sweet?—The galis in many parts of the town are good. The difficulty, as you observed yesterday, is in regard to the pavement permitting percolation, fluid percolating down between the stones when the ground is worn away. The surfaces of the galis are clean." "The old galis are very irregular?—Yes." "What light is there in houses of this kind?—The light is extremely defective. Even in the case of a better house than that I have described the light is very dim indeed." "What does it proceed from?—The light would proceed from a door communicating with the central passage and from a window communicating with a side gali outside, a gali 1 foot 6 inches to 3 feet wide." "One of those narrow galis I saw?—Yes." "The outside wall of this gali being the wall of another high house?—Yes." "Is there much filth found in these places?—Over these galis there is a constant passage of fluid carrying excreta containing organic matter. There is a continual throwing out from the windows of solid matter on to the gali; in fact, the galis are the ashpits of the houses in Bombay. There are no ashpits here." He also stated that 70 per cent. of the buildings in the Kamathipura Ward, having a population of nearly 30,000, and not less than 80 per cent. in the adjoining ward, are in a similarly insanitary condition.

A short report on the plague in Bombay, and the measures taken to suppress it, 1897, pp. 8, 9.

847.

848.

849.

850.

851.

852.

853.

854.

856.

972.

974.

Surgeon-General Cleghorn, late Medical Adviser and Sanitary Commissioner with the Government of India, stated that the houses in Bombay are "worse than any place I had seen, owing to the enormous number of people who lived under the same roof. I have never seen anything like such overcrowding. In a place 6 feet by 12 feet I have seen the floors covered by people lying down, and, in addition, beds hanging from the roofs on which other people slept at night. There was absolutely not one ray of light, or any means of getting air to these rooms. We had to strike a match to see the opposite walls because it was so dark, and that was in the middle of the day." "I have never seen anything like such overcrowding in rooms absolutely without any means of ventilation." Where windows existed, they were

26,644.

26,645.

generally small, and as they opened on to the wall of the next house, from which they
25,646–8. were separated by a narrow alley, "in order to secure privacy, all the windows were covered up with clothing, &c." The separating alley was chiefly occupied by an open sewer which rendered the air foul.

Snow's Report, p. 110, para. 377. In this city, during an inspection made in the first half of 1897 by one medical official, 1,903 tenements were found to be in part or entirely unfit for human habitation, chiefly because of the absence of means for admitting sufficient air and light.

The condition of overcrowding in the houses of the labouring population of Bombay was thus graphically described by Dr. Blaney, who had been a medical practitioner in that city for upwards of 50 years. "If they go into their rooms their feet and heads are together, and the whole atmosphere is vitiated, and, even supposing
17,711. there was no epidemic, they incur every possible risk because of overcrowding in sleeping rooms." The amount of this overcrowding is even more vividly realised in
p. 188. the statement in Snow's Report, that in one of the wards the concentration of population is 469 persons to the acre, whereas the extreme in London, before the recent metropolitan improvements had been inaugurated, was 222. Elsewhere in that
p. 29. Report it is stated: "A lurid light has been thrown upon certain sanitary shortcomings of Bombay by the present epidemic. The principal of these may be found in the density of the population and overcrowding in chawls and houses."

22,810. In Belgaum, the usual house is narrow and deep, with a door at each narrow end. There are three rooms, each opening into the other, and, while the two end rooms have doors, the middle room is quite dark and has no opening to the outside.

13,786–8. The poorer houses in Nasik, and the great majority of the houses in Malegaon, are one-roomed, overcrowded, and without any window, and the floors are of earth and cow-dunged, if the people were well-to-do. "The door lets in all the light and air
16,980–4. there is, and it is closed at night." A one-roomed house, about 10 feet or 12 feet square, would contain a family consisting of the father, mother, and two or three children, or "there would probably be about six or eight people living in it."

15,937–8. In the localities in the town of Surat where plague was most prevalent, the houses have sometimes no ventilating openings except the doorway. The houses of the Golas,
16,107–8. who suffered severely, "were overcrowded, ill-ventilated, insanitary, almost without light and air."

19,690–3. In the villages of the Thana District, which suffered severely from plague, most of the people live in very poor two or three chambered houses, without any windows, the door or doors being the only outside openings.

13,355. Major Hyde-Cates, I.S.C., described the houses in the small town of Salaya, near Cutch Mandvi, as being exceptionally dark. "There are no windows at all except in the outer room, and there are three or four rooms behind that which are, as a rule, pitch dark, with no windows or ventilation of any sort, except from the door of the front room, which opens on to the street."

3251–7. According to the Plague Commissioner for the Mysore State, plague cases in Bangalore occurred chiefly in the prevailing mud houses, which had one or two rooms only, were ventilated by a hole in the roof of the cooking place in each house, were badly lighted, and had a small window or windows on one side generally, but
3258. owing "to being close to other houses there was no free scope of air." "In some cases, rooms only 6 × 6 were occupied by a family consisting of four members."
Report on plague in the Mysore State, 1898–9, by Madhava Rao, Chief Plague Commissioner, p. 58, para. 232. In the villages of the Tumkur District in this State, the virulence of the outbreak is explained by the practice "with all classes of people to tie their cattle in their houses. The excreta of these animals is allowed to soak into the floor, which forms a good breeding ground for germs"; and the number of victims in the same family was considerably larger in the villages than in the towns.

24,275. In Ahmednagar, the mud houses, in which nearly all the plague cases occurred, have "very few openings for ventilation. If there are any, they are very small. They are not suited to bringing in the rays of the sun direct."

11,081–6. In Karachi, the houses are built of sun-burnt bricks, have generally about three storeys, with very small rooms, and are occupied usually by one family. In the opinion of Mr. Giles, Acting Commissioner for Sind, they are overcrowded, very badly lighted—"in fact, when you get inside you can see really nothing at all," and many of the rooms have no openings except the doorway. In the Garden Quarter,
12,456–63. where many cases of plague occurred, the sun's rays cannot enter the houses, as there are inner rooms and downstairs rooms "where the sun could not possibly get."

At Kiamari, the houses "had a dead wall through the middle, and there were rooms on each side. There was no possibility of any free passage of air. When you went inside in the early morning, the smell was very bad as a rule. . . . . Directly we get cases they seem to spread like wildfire." 11,971.

In justification of the unroofing of houses in the Satara District, Lieutenant Steen, I.S.C., stated: "A good many of the houses I have had to deal with probably never had any ventilation for years. Mud-roofed houses, especially, are so constructed that it is almost impossible to ventilate the interior." 22,734.

When examined by the Commission, Captain Meyer, I.M.S., expressed the opinion that the houses in Hubli and the railway chawls, where the first cases of plague occurred, were remarkable for their bad ventilation. "They were so very dark even for an Indian city. The windows were very small and usually sealed up. There were a great many verandahs in the houses which prevented the circulation of air. The internal ventilation of the houses was extremely defective. In other respects the sanitary conditions of the houses in Hubli were excellent for an Indian city." 20,144. 20,207. 20,213.

The Acting Commissioner for the Southern Division of Bombay Presidency (Mr. Cappel) gave evidence regarding the towns and villages of the Dharwar District to this effect: "In this part of the country the houses are of brick; the inner rooms of all the houses are absolutely dark, so much so that you cannot see your way. When the first case occurred here, in Dharwar, I went down to the town, and they said there were some people who were sick in a certain house. I went in, and fell over a dead body, and, getting a light, we discovered another. There are no windows, no light, and no ventilation." Speaking of the villages in this District, the Deputy District Collector stated: "Generally the houses of the agriculturists in villages are very filthy, ill-ventilated, and dark. The agriculturists tether their cattle inside their houses and allow cow-dung and manure to accumulate in the back yards. They never keep the place where cattle are tethered clean. Owing to want of ventilation and light the houses stink so badly that one who is accustomed to live in purer atmosphere can hardly bear the smell." In nearly all the houses in the Dharwar and Hubli Districts the floors are formed of mud and cow-dung, and the walls of mud or mud mixed with lime. 1711. 23,518. 23,250.

Captain James, I.M.S., described the general type of dwellings in the plague-affected villages of Jullundur as constructed "of unbaked bricks, mud houses, usually square or rectangular, with flat roofs. They are very simple houses, many of them not containing windows, and the only means of ventilation and light is the doorway." 10,370.

Among the many interesting facts brought before us regarding the conditions prevailing in the villages of Kumaun and Garhwal in the North West Provinces, where plague has so frequently occurred during the last 60 or 70 years as to have acquired an endemic persistence, none has been more conspicuous than the insanitation of the dwelling-houses. Thus, Surgeon-General Richardson described the houses when inspected by him in 1876 as "generally dark and ill-ventilated. One had almost to enter on hands and knees, and could scarcely stand upright after entering. These rooms, or rather garrets, were crowded with baskets of grain, baskets of wool, and goatskin bags of flour, wooden vessels filled with ghi and other domestic belongings; usually the cattle occupied the ground floor underneath these garrets or lofts." The houses were generally roofed with coarse slabs, and the ventilation found its way through these slabs. "There were bits of holes now and again, in which they stuffed something, as a rule. They were very ill-ventilated and dark places." Surgeon-General Planck describes the local conditions of two plague-affected villages in 1877, as follows:—"The well-built houses were seen to be double-storied, one room below and one above; close unventilated tenements. The lower room used only as a cow-house, the upper room for family occupation. In the lower room, about five feet high, it had been customary to lodge from four to eight head of cattle or goats at night, and, indeed, in some instances, as many as the room had standing place for; the only opening being the small doorway of entrance, tight closed and barred at night. These rooms were seen to be littered for about a foot in depth with a little decaying straw and much manure, moistened with the fluid excretion of cattle, and the entrance way on each side and the stone platform facing the lower storey, were piled with heaps of manure, which had been drawn out of the lower room as necessity required, and there left for eventual removal to the land in the ploughing season. The upper room was noticed to be roughly divided by wooden slabs into a front and back portion, the former used as the family sleeping place, the latter as a granary; the doorway, in some instances, a round hole in the front, and a small round hole for the exit of smoke through the roof of the house, Vol. II., Appendix XXV., p. 360, paras. 251, 6176. 26,611. 26,631. Vol. II., Appendix XXV., p. 338, para. 111

all commonly closed at night, being the only openings. The floor was made of thin wood, with pretty numerous cracks, so that the warmth generated by the cattle below could reach to the sleeping people above." The suggestiveness of this description derives additional force from the circumstance that "as regards the site generally and the precincts of the villages, nothing of insanitary aspect could be seen . . . the surrounding conditions were clear, clean surface, bright flowing water, sweet air everywhere, all of the unwholesomeness of the site was gathered within and immediately around the habitations." Cháni, another plague-infected village, is situated on the sloping brow of a hill which stands out from the great mountain behind it, and its high position provides for perfect drainage. The lower storeys of the houses, however, were found to be "filthy, as a rule, owing to their utilisation as cow-sheds, the upper storeys without ventilation, close smelling and unwholesome." During a recent visit to the district made under our direction, Lieutenants Walton and Douglas, I.M.S., found that the condition of the houses was much the same as described above, the ground floors being still used to stall cattle, and the windows of the houses being still "very small or absent, and the rooms very dark and quite unventilated." The surroundings of the houses were, however, so far improved that manure was not accumulated near the houses.

Vol. II., Appendix XXV., p. 338, para. 112.

Vol. II., Appendix XXV., p. 346, paras. 158, 160.

Vol. II., Appendix XXV.(1), p. 366.

Difficulties in effecting sanitary improvements in India.

4. Before concluding this abstract of some of the evidence relating to conditions of sanitation in India, it is desirable to allude to the great difficulties in the establishment of conditions favourable to public health in that country. To some extent these are stated in the following quotation from General Gatacre's Report on the Bubonic Plague in Bombay: "It must be remembered that in all large Oriental cities a very large proportion of the population are very poor and cannot afford to pay the rent of a really sanitary building. They therefore are forced to live in miserable shanties, dark, low, small, and built on insanitary sites without plinth, added to which, with a view to bringing the cost of this habitation to the lowest point, 16 or 20 persons will sleep, eat, and cook in a place hardly sufficient for the requirements of four."

p. 232.

Much sanitary work has been accomplished.

It is apparent from the Evidence that a considerable amount of effective sanitary work has been overtaken in several of the cities and larger towns of India during the last 20 years, especially in the direction of improving the water supply and introducing sewerage systems. It is to be regretted that, owing no doubt to financial difficulties, the beneficial effects of some of this work has been lessened by its incompleteness, as, for instance, where an abundant supply of water has been introduced, while no proper drainage has been provided. The work remaining to be accomplished is, however, gigantic; and when plague was introduced into India in 1896, the prevailing insanitary conditions were eminently favourable for the diffusion of any infectious disease whose infectivity and virulence are enhanced by insanitary conditions.

7840, 15,932–3; 23,323; II. 379–80; 23,363; Snow's Report, p. 29.

II.—Opinions that the Spread of Plague is due to General Insanitary Conditions.

5. That plague is such a disease has been widely and influentially maintained. The present Director-General of the Indian Medical Service and Sanitary Commissioner with the Government expressed this view in the following words: "Plague is a disease propagated by a specific germ which springs from a pre-existing germ and cannot originate *de novo*. This germ is the seed of the disease, and as seed cannot grow without proper soil, so this plague seed cannot flourish unless it finds an environment suited to it, and that environment is made by all these insanitary conditions. It is a disease of filth, a disease of dirt, and a disease of poverty. The whole history of plague, so far as we know it, shows that to a great extent."

7836.

21,323.

To the same effect was the evidence of Lieut.-Colonel Fawcett, R.A.M.C.: "It is unfortunately an axiom that plague in its epidemic form is only to be found where dirt, darkness, and overcrowding prevail. In a word, in those places where sanitation is unknown, impossible, or neglected. Most fortunately, it cannot be carried to any appreciable extent by water or air, and the bacillus which causes it, though capable of multiplication with extraordinary rapidity in a suitable nidus, has neither power of resistance, locomotion, or spore formation, and dies at once in the light of day."

According to Captain H. Dempster Mason, R.A.M.C., the conditions chiefly affecting the propagation of plague are: "a warm and humid atmosphere, the people living on marshy soil on the sea-coast and creeks; low, badly ventilated, and overcrowded houses; great accumulation of putrefying animal and vegetable matter in the vicinity of the dwellings; the most revolting filthiness in personal habits; and

Vol. I., Appendix VII. p. 351.

the whole cities little better than one large latrine, in such an insanitary state that it requires to be seen to be believed." 2923.

Lieut.-Colonel Dobson, I.M.S., considers "that exposure to conditions such as the following are necessary to develop the disease, *e.g.*, overcrowding, destitution, deficient cubic space, ventilation and sunlight, and a filthy and general insanitary condition of person, clothing, habitation, and its surroundings."

Dr. Clemow considered that the spread of plague is favoured by insanitary conditions, because "the majority of cases in Calcutta were among the poorer class of natives living in dirty and unhygienic places, and the early cases occurred in an insanitary area, and in houses and bustees which abound in sanitary deficiencies. The outbreak in Roop Chand Roy's Street, the most concentrated outbreak in Calcutta, occurred in one of the most insanitary bustees." 7180.

"The predisposing circumstances," according to Mr. D. A. Choksi, Chief Plague Officer in Bangalore, "are overcrowding, ill-ventilating, ill-drainage, and bad lighting." 3386.

Mr. K. B. Jadhav's experience at Baroda led him to conclude that the propagation of plague is chiefly effected "by very bad water, and damp in the house; all filth, and no conservancy arrangements"; and Mr. Patwardhan found that, in Poona, "most of the houses affected were either old buildings with moist floors, or ill-ventilated and with insanitary surroundings." 15,151. 21,548.

6. It is indicated, by the opinions which have now been quoted, that insanitary conditions in general are frequently held to be accountable for the extension of plague. The association of the disease with general uncleanliness has even been emphasised by a repetition of the old statement that it is "a filth disease." Much of the evidence placed before us, however, indicates that a distinction may properly be drawn between the fostering influences exerted upon plague by conditions existing outside as contrasted with those existing inside dwellings, between the effects of sewage and excrement pollution of the open ground in the neighbourhood of houses and the insanitary conditions which are present in the interior of houses. It has not been found, for example, that plague has prevailed largely among people who from occupation or habit came specially into proximity or even contact with the materials of outside pollution. The scavengers of Bombay, "although working with naked feet and hands in the filth from houses where cases had occurred, were the least affected of all classes of the poor." The strenuous efforts made at large expense in Bombay and elsewhere to restrict the advance of plague by cleaning the streets and flushing the drains with solutions of disinfectant substances had no appreciable effect upon its advance; and "anything that was done outside of the buildings in which cases occurred had little influence upon its extension." Although the extensive cleaning operations of the drains, streets, and houses of Poona had, in the plague outbreak of 1897, rendered that city "cleaner than it ever had been within the memory of living man," each subsequent outbreak was more extensive and virulent than the outbreaks which had preceded it. People who slept in the open air, on ground sodden with filth, were rarely affected; "people living in open spaces, even in filthy surroundings, escape infection"; and it was found that few cases occurred among "those who work out in the docks during the day and keep out in the air at night also, and only spend enough time in their rooms to take their food." It has been shown "that people living on the roadside had better chances of recovery, and that a larger proportion of them survived than people living in dwellings." The outbreaks in Bombay, and in other places where seasonal variations are marked, have notably declined in the hot weather and increased in the cold and wet weather, and this has frequently been attributed to the chawls and houses being rendered unhealthy in the latter seasons by overcrowding, the inmates being unable to work outside and to sleep at night in the streets and open ground, usually filthy and sewage-sodden, "as large numbers are accustomed to do during the warmer months." Even when plague had in the hot season been introduced into a large town and had infected the rats there, no further extension occurred until the weather became cold, because during the hot season the people slept out of doors at night, "were probably out in the air all day," and "scarcely used the interior of their houses at all." For this reason, also, it was believed that in Ankleshwar plague became dormant during the hot weather, and resumed its virulence when the cold again caused people to sleep in the much vitiated atmosphere of their houses. His large opportunities for observing the conditions favourable to the extension of plague have led Lieut.-Colonel Weir, I.M.S., to arrive at the definite conclusion that "the view of associating this disease with dirt in the sense of refuse is utterly wrong," and that "the common view of the pythogenic nature of the disease must be modified."

Necessity for separating insanitary conditions outside from those inside of dwellings.

7336, James' Report, p. 22.

Jadhav's Report, p. 15.

Minutes of Evidence, Vol. I., Appendix XIV., p. 378.

Snow's Report, p. 175, para. 626.

Gatacre's Report, p. 185.

Snow's Report, p. 175, para. 627.

Gatacre's Report, p.190, 8707.

19,515.

17,711.

Snow's Report, p. 134, 916.

Vol. I., Appendix XIV p. 378.

9071.

14,384-6.

1003.

Snow's Report, 175.

III.—Opinions that the Spread and Virulence of the Plague are chiefly due to Insanitary Conditions within Dwellings.

7. The great mass of the evidence bearing on this subject that has been laid before us is, indeed, in support of this contention, for it is to the effect that, in so far as insanitary conditions operate, the conditions inside rather than those outside of dwellings are the main causes of the propagation and virulence of the disease.

Surgeon-General Cleghorn, lately Director-General and Sanitary Commissioner with the Government of India, pointed out that plague had chiefly appeared in Bombay 26,638. "in the worst houses, and that the better ones, including the small hotels situated in the infected quarters had practically escaped." The latter, however, were well provided with air and light, having been built according to European ideas, and there 26,672-4. was no overcrowding in them. He also remarked that he "had of course a considerable experience of the conditions under which the poorer classes in other parts of India 26,638. lived," but he "had never seen anything so bad as existed in the chawls of Bombay."

The present Director-General and Sanitary Commissioner with the Government of India, Surgeon-General Harvey, stated that the sanitary improvements specially 8066. required to prevent plague are mostly within the houses. They are "generally damp, they have an absorbent mud floor and very little light, and no ventilation, as a rule, except what they get through the tiles of the roofs."

Report on the Outbreak of Bubonic Plague in Bombay, 1896-7, by P. C. H. Snow, I.C.S., p. 148. One of the Medical Officers in Bombay thus describes the condition of the houses in Kamathipura District, in which plague was specially virulent: "I have gone through all the houses, and almost all the rooms on the ground floor and most of the houses are not fit for human habitation, owing to want of light and ventilation."

Snow's Report, pp. 147-148. Lieut.-Colonel Weir was of opinion that "the incidence of the disease is most influenced by the ventilation of buildings. . . . In localities, the worst ventilated and the most densely crowded with buildings, the incidence of the disease was the greatest. As with localities, so it was with buildings. While in open houses it was difficult to contract the disease, in bad ones it was difficult to escape it."

17,692. Having pointed out that plague has occurred chiefly in insanitary and overcrowded houses, Dr. Blaney, in reply to the question—"By insanitary dwellings, or insanitary conditions, what do you include besides overcrowding?" answered: "Darkness, dimness, and want of ventilation. They are rookeries in lots of cases. In many of the outlying districts a man can hardly stand upright in his house. They are not fit for human habitation at all."

21,505. Professor Gokhale, a volunteer plague officer at Poona, stated: "Generally I found that houses that were damp, dark, and ill-ventilated were most exposed to the 19,515. attack"; while Mr. Kabraji, I.C.S., the Acting Collector at Kaira, observed that "by far the largest majority of plague cases, if not all, occur when the sleeping rooms are badly or insufficiently ventilated or lighted."

The following evidence was given by Captain Bingley, I.S.C., who had charge of 8295-300. one of the district wards in Bombay: "Do the habitations in your district vary much in their characteristics?—Yes, they are very varying." "What sort of variations?—For instance, in Fanas Wadi District, which is inhabited chiefly by Parsees, there are four or five-storeyed houses; the rooms are large, and not, as a rule, overcrowded; the Dhobi Talao District is in parts very poor, and the overcrowding is considerable; the Kumbharwada and Khara Talao Districts are occupied chiefly by the labouring classes, most of the houses being lodging-houses, and the overcrowding is very great. The houses there are of a very inferior type. There are no plinths at all, and the ground floors are below the level of the street. The rooms are very dark, and have all been subdivided by partitions, so as to make one room into four or five. The inner rooms have thus no windows, and no light or air. In fact, in many rooms one would have to use lamps night and day." "Did you find any relationship between the number of plague cases and the nature of the habitations?—Yes." "What was the relationship?—As a general rule, where the houses were very bad, plague was very bad." "Have you seen many cases of plague in the best of the houses?—No." "Some?—Yes, but they were comparatively few."

According to Lieut.-Colonel Weir, the village of Parel "has been drained, but no connexions have been made between the sewers and the houses, and the sewage either flows down on the surface of the narrow lanes or roads into the soil. This village is in an almost hopelessly insanitary condition, and has been a hotbed of fever for many years."

It contains a number of very good and well-ventilated houses, and also "many overcrowded hovels of the most insanitary class, situated in sewage-sodden soil and abutting on narrow lanes, and it was in these houses that the disease chiefly raged." Gatacre's Report, pp. 190, 191. Snow's Report, pp. 108–11.

The fishing village of Worli, at the north-west of Bombay Island, where 124 cases of plague with 104 deaths occurred during December 1897, is situated on the slope of a hill gently descending to the ocean on one side, and to the Mahim shore on the other. "Although in this lovely position," . . . "it enclosed groups of dwellings into which the rays of the sun never entered, nor the pure sea-breeze." These dwellings are, for the most part, huts which are closely packed together; and Lieut.-Colonel Weir has reported that "most of the cases occurred in the centre of the village, in the darkest of the huts." Snow's Report, p. 158.

Major Evans, I.M.S., Resident Medical Officer to the Medical College Hospital, Calcutta, stated that the majority of the plague cases admitted into the hospital came from a very populous district, in which overcrowding during the night is common in the native huts and houses. "It is no uncommon sight to see the floor of a room covered with sleeping men lying in close proximity." 6861.

The Plague Commissioner for the Mysore State considers that "simple momentary or occasional contact with infected people or their clothes does not seem to have had the effect of giving infection. To catch the disease, there should apparently be close and continued contact, especially sleeping in the same house and breathing the vitiated atmosphere of the room in which plague patients are kept." He adds that "the Deputy Commissioner thinks that it is abundantly clear that the dirtier and worse ventilated the dwelling, the greater the liability of the occupants to catch and succumb to plague." Madhava Rao's Report on Plague in the Mysore State, p. 36, para. 121.

Referring to Mysore City, the Health Officer reports that "the incidence of the disease was greatest in localities which were worst ventilated and most densely crowded with buildings; as it was with localities, so it was with buildings. Dark, damp, ill-drained and soil-polluted buildings suffered most. In the newer portions of the town where the houses are of a better type, better ventilated, with open spaces in front and behind them, and where overcrowding exists in only a very slight degree, and where sanitation generally is better, cases of plague were few and far between, and the spread of infection was very limited." . . . "As with the spread, so with the virulence of the disease, for in highly congested, badly conserved, ill-drained portions of the town, the disease was more virulent than in the less overcrowded, better conserved, and fairly well-drained parts." Do., pp. 36–37

In Mr. Giles's opinion, "There was not the faintest doubt that the ill-ventilated houses where the people were crowded got the plague far the worst; it was quite apparent." He considered that the extent and virulence of the disease chiefly depended upon "dirt and overcrowding and want of ventilation," and of these he put want of ventilation first. 11,228. 11,281.

Lieut.-Col. Nariman, I.M.S., was asked, "Are the houses generally furnished with ventilation openings of some kind in the worst localities of Surat?—No, except the entrance. There is sometimes no ventilation in the worst localities." "In which locality was plague most prevalent, the better or worse?—The worst." 15,937–8.

Mr. Maynard, M.R.C.S., who had a large experience of plague at Nasik, was of opinion "that the chief cause of the spread is overcrowding, the effects of which are accentuated by the habit the people have in cold weather of shutting up every aperture in the rooms and crowding into small rooms." 14,716.

Referring to the dormancy, during the months of May and June 1898, of the plague outbreak at Ankleshwar, Assistant Surgeon Petigara stated that it was probably accounted for by the habit of the native people to sleep and lie outside and not indoors when the weather is hot. They were thus, he explained, exposed to a pure and dry air unlike that in their houses "where the atmosphere is much vitiated." He considered that the most favourable conditions for the extension of plague were the want of ventilation, overcrowding, and the uncleanliness of the interior of the houses. 14,386–8.

Captain C. H. James, I.M.S., expressed the opinion that "light and air seem to be inimical to the spread of plague, while dark and badly ventilated places are conducive to it." 10,377.

The Chairman of the Plague Committee of Ahmednagar, who was also Vice-President of the Municipality, was "fully convinced that plague sticks most and does greatest mischief in dark, ill-ventilated, and dirty houses and localities, and can be stamped out of a sanitary house much easier and quicker," and that people living in the open escape being attacked by plague. 24,363.

12,018. Lieut. Law, who had been employed in plague operations in Karachi and Kiamari, summarises his experience in these words: "To sum up from such experience as I had, it seems that the chief enemies of the plague are fresh air, sweetness, and light; that in itself it is essentially a filth disease, and will, *ceteris paribus*, be much more likely to attack those who live in insanitary, unclean, and ill-ventilated abodes than those who have some acquaintance with and pay some reverence to the laws of hygiene."

12,278-86. The fishing villages of Baba, Bolit, and Shamspir, on the western side of the harbour of Karachi, remained free from outbreaks, although cases of plague had been imported into two of them, and although the inhabitants of the three villages constantly went in boats to Karachi and to Kadda, to sell fish, at a time when there was plague in both places. The immunity of these villages was attributed by Captain Jenney, I.M.S., to the people leading "a very open-air kind of life," and living in huts which, although of a very poor and inferior description, were made of matting, freely permeable by air, and "were not particularly overcrowded."

1439. The following opinion was given by Captain L. F. Childe, I.M.S., Principal Physician to the Jamsetjee Hospital, Bombay. "The infectiousness of plague seems to depend mostly upon the surroundings of the patient. I think, in a small room, crowded with many people, and in insanitary conditions the infectious power is high; in a well-ventilated house or a hospital the infectiousness is very slight indeed."

8827. Speaking of Mahamari or Gola-rog, the endemic plague of Kumaun and Garhwal, Lieut.-Colonel Thomson, C.I.E., I.M.S., said that it "generally breaks out in the cold weather, when the people are huddled together in small rooms with practically no ventilation, as doors and windows are religiously closed to keep out the cold. I may mention here that very often for days together the people are snowed up, and sometimes even for weeks at a time, and on such occasions it is almost impossible to conceive anything more filthy than the places where they live, not only the rooms themselves, but also the immediate vicinity of the houses." Surgeon-General Planck, from his local study of the disease, has arrived at the conclusion that "the specific poison of the disease is most potent for mischief in the house in which a case of the disease has occurred," while, "on the other hand, the danger of infection appears to be lessened, if at all existent, in the open air." In his opinion, the most important predisposing cause is "a vitiated atmosphere in and around the habitations," the chief sources of which at Kumaun and Garhwal were, he thought, the gases produced from unripe grain stored in the sleeping apartments, and the exhalations from the manure which rise into those apartments from the chamber on the ground floor in which cattle are housed. He considers that the only reliable method of prevention is an improved sanitary management of the houses and homesteads throughout the country. He also draws attention to the circumstance that improvement in this direction was effected on several occasions with good results, and that when lapses afterwards occurred plague resumed its active existence. It is interesting to note that the improvements referred to were produced by the enforcement of the Mahamari Rules, which not only prohibit accumulations of filth near dwellings and the housing of cattle within them, but also require that every room shall have a window at least 2 feet × 2 feet communicating directly with the open air "for purposes of ventilation," and that the inhabitants of a house which contains more than one room shall "distribute themselves between the different rooms at night, so far as possible to diminish the number sleeping in each room." A repromulgation of these rules in 1894 is introduced by the statement that the observance of them had formerly "entirely stamped out" plague.

Vol. II., Appendix XXV., p. 362, para. 261.

Vol. II., Appendix XXV., p. 360, para. 252.

Vol. II., Appendix XXV., p. 361, para. 256.

Vol. II., Appendix XXVI., p. 377.

22,273. The predisposing effect on plague of insufficient ventilation is likewise illustrated by the history of Sadr Bazar at Satara, during the first two outbreaks of plague in that city. This bazar has a population of 2,325, and there was constant communication between its inhabitants and those of the city, which is a mile distant. During the first outbreak, and while the inhabitants remained yet unaffected, "the houses were unroofed in strips, to let in sun and air, and therefore they were well ventilated. That was not done this year" (in the second outbreak), "and the Sadr Bazar has had an epidemic," which is characterised as a severe one. In Satara, "the districts which were exempt last year were compulsorily ventilated by unroofing the houses by strips at a time, and holes being knocked into the walls before plague came."

22,274.

7331-406. It may be added that the difficulty of extension of plague, even when of the pneumonic type, in fresh air and light, has been further illustrated by the experience in the two closely adjoining villages of Siddhakati and Abhoynil, in the Backergunge

district of Eastern Bengal. The entrance into one of the villages of a plague-infected person from Calcutta was followed within 20 days by 11 cases of pneumonic plague, all of which terminated in death. One of the six infected houses was "a superior masonry house," and it was not evacuated; but the contacts from the other five infected houses were transferred from their thatched and presumably badly ventilated huts to several boats on an adjoining sheet of water, and no further cases occurred among them or in the villages. This result is attributed by Mr. Justice, M.B., to the removal of the contacts from the infected houses to the fresh air. 7343-5.

In the summary of the views of the Medical Officers employed in Bombay by the Plague Committee, contained in General Gatacre's Report on the Bubonic Plague in Bombay, it is stated: "There is now no doubt that it is criminal to allow a plague patient to remain in a close, ill-ventilated, confined room, which is sure to be to his disadvantage as well as a danger to others, and all classes of Indians are well aware of this from their personal experience during the epidemic." p. 142.

8. It is manifest from the above that there exists a strong conviction among those who have had the best opportunities for forming an opinion that the propagation and virulence of plague are chiefly affected by conditions prevailing within dwellings, rather than outside of them, and especially by insufficient ventilation and light, overcrowding, dampness, a polluted atmosphere, and, to a less extent, uncleanliness.

This conviction has been strengthened and supported by many of the incidents which marked the course of the epidemic and occurred during the carrying out of measures taken to suppress it.

IV.—Additional Facts supporting the View that Insanitary Conditions inside Dwellings are the chief Causes of the Spread and Virulence of Plague.

9. Several witnesses have stated that in chawls and in the larger houses occupied by the poorer classes a greater number of cases were found in the lower than the upper storeys. Incidence greater in lower than in higher flats of many-storeyed houses.

Mr. Gokhale, Professor of History and Political Economy in the Fergusson College, Poona, and a plague volunteer worker, stated: "For four or five months I was going about with the search parties. We found that most of the cases which occurred were on the ground floor;" and he added that they were more crowded and worse ventilated than the upper floors. Major Deane, I.M.S., gave evidence to the same effect regarding Bombay. "There was not the same number of cases occurring in the upper storeys of the large chawls there." This statement was repeated by Major Dimmock, I.M.S.; and Mr. Nield Cook has recorded that in the Calcutta outbreak of 1898, the cases were more than three times as numerous in the lower than in the upper storeys of the relatively very few large and well built houses of that city in which plague had occurred. 21,519. 21,521-2. 2784. 20,422. Vol. I., Appendix XX., p. 474.

For the purpose of illustrating the influence of ventilation on the mortality from plague, Lieut.-Col. Weir, I.M.S., placed before the Commission a carefully prepared table dealing with the proportion of deaths to attacks in many-storeyed houses in the wards of Bombay, and in which the percentage of deaths to attacks on each floor was separately stated. It appears from this table that the mortality steadily diminishes from the ground floor upwards to the highest floor; the average percentage of mortality in the houses in seven wards being 84·7 in the ground floors, 75·5 in the first floor, 67·3 in the second floor, 59·7 in the third floor, 48·1 in the fourth floor, and 45·4 in the fifth floor. 916.

The significance of these figures is apparent in view of the obstruction to the scanty means of ventilation in chawls which is produced by the close proximity to them of other equally lofty buildings, and which necessarily affects more prejudicially the lower than the higher storeys.

Frequently, but by no means invariably, the floors of the lower flats of chawls and other many-storeyed houses consist of earth and cowdung, and the floors of the upper flats of concrete or wood. On the probable assumption that the plague bacillus is able more readily to retain its vitality and even to proliferate in cowdung and earth than on concrete or wood, the above differences may to a slight extent be explained. At the same time, there remains the fact that when houses having floors of the same materials are compared with each other, plague is found to prevail much more in the houses in which the air has been rendered impure by defective ventilation and light, than in those in which a pure atmosphere is maintained. Numerous examples in demonstration of this proposition have already been adduced. Snow's Report, pp. 108-110.

Freedom from plague of inmates of well-ventilated and lighted houses.

10. There is also evidence which shows that the disease does not usually affect or spread among the inmates of the better-class houses, if they be well ventilated and lighted and not overcrowded, even when they are situated in the midst of plague-affected districts. Thus, according to Mr. Patwardhan, Municipal Commissioner of Poona, "well-ventilated spacious houses, exposed to heat and sun, which were surrounded by affected houses, could not have escaped the contagion. They were in several cases vacated and locked, or in many cases occupied by a few servants and others. They were not disinfected, and yet no cases occurred in them, and they were re-occupied with safety." According to Lieutenant Anderson, I.S.C., "It is a notable fact that in the really good houses, some of which were situated in the very heart of the most highly infected quarter, practically no cases occurred"; while his large experience at Bombay has led Lieut.-Colonel Weir to conclude that the plague "organism does not seem to be able to develop virulently or survive where there are free currents of air, and hence it passes by or little affects well-ventilated buildings." To the same effect was the evidence of Major Hardy, that in Dharwar plague did not much occur in the large double-storeyed and well-ventilated houses, but chiefly affected the inmates of the small, low houses, either without windows, or with windows which were generally shut up; of Colonel McConaghy, I.M.S., that in houses in which plague cases had occurred it was not uncommon for several of the inmates to be infected if the houses were "small, unhealthy, and badly ventilated, especially if the floors were of earth," whereas "in the better class of houses, which were well ventilated, and with upper storeys, very few of the friends who came in contact with the patient suffered"; and of Captain Childe, I.M.S., Mr. Giles, and Mr. Patwardhan, that no cases of plague occurred in well-built and well-ventilated houses, even although dead rats had been found in them, during outbreaks of plague in Bombay, Karachi and Poona.

21,547. 22,649. Snow's Report, p. 174. 2307–9. 12,880. 1447, 11,345–7, 21,538.

Inmates of the better-class houses attacked if ventilation inadequate.

11. On the other hand, plague has undoubtedly occurred and extended to the better-class houses of the richer Natives. Mr. Stewart, I.C.S., Acting Secretary to the Government of Bombay, stated that the houses in Nasik are of all kinds, and that he could not say that plague chiefly occurred in the bad houses, obviously meaning badly built. "Some were exceedingly well built—the big houses—but they were dark, badly lighted, and badly ventilated"; and it was in the well built houses having these serious defects that plague chiefly occurred. The one-roomed houses, in which plague cases were also abundant, were overcrowded, and had practically no means of ventilation but the doorway: "there is a window, but it is generally blocked up." Referring to the Kaira District, Mr. Kabraji, I.C.S., stated: "It has been observed that by far the largest majority of plague cases, if not all, occur when the sleeping rooms are badly or insufficiently ventilated or lighted. Even if there are windows or ventilators, the people are in the habit of closing them at night. This may sufficiently account for the occurrence of plague among the well-to-do classes." A specific illustration of this was given by Captain Thomson, I.M.S. The jailer's house at Satara "is a well-ventilated house. The jailer's is an ideally perfect house, even for Europeans to live in, but as he only used the centre room, which may be 14 feet × 12 feet, for himself, his wife, his brother-in-law, and six children, who all sleep on the floor, and in the cold weather they have a charcoal brazier in the centre of the floor, and cover their heads with razáis, blankets, and so on, and all the doors and windows are shut, they have made this naturally well-ventilated house perfectly insanitary, and consequently there was plague last year, and there was also plague this year." When his daughter was attacked, in January 1898, Captain Thomson said: "Now, surely, you will give up your bad habits and preserve your health," and he replied, "God will take care of my health." The house was disinfected with 1 in 1,000 solution of perchloride of mercury, "ceilings, walls, and floors, including the verandahs, front and back, on 18th and 19th January once, and again on 31st January." On the 14th of February the jailer and his wife were attacked, and they afterwards died of plague. At the same time the jail, which is next door to this house, "remained free, although there were plague rats in it; and this year a plague case was introduced into the jail, and there were no further cases, because ventilation is enforced there at night among the prisoners." In the two prisons of Bombay in which plague occurred, the Umarkhadi Common Prison and the Byculla House of Correction, it is noteworthy that overcrowding was existent at the time of the occurrence: for in the former there were 402 prisoners, or 222 in excess of the capacity of the prison, inclusive of hospital and cell accommodation; and in the latter there were 346 prisoners, or an excess of 84.

16,977–8. 16,982. 16,983. 19,515. 22,38 . 22,385. 22,330. 59. Administrative Report of the Bombay Jail Department for the year 1897, p. 17; do. for the year 1898, pp. 20, 54.

12. An endeavour has been made to show that the influence of overcrowding upon the extension and virulence of plague is relatively unimportant, because, in several places, the disease has attacked sparsely inhabited districts with as great severity as those which were densely populated. The prejudicial effects of concentration or density of population in inhabited areas are probably much less operative in India than in this country; for in this country nearly all the houses, however unsatisfactory their sanitary conditions may sometimes be, are provided with windows opening from each living room directly to the outside air, whereas in India this is far from being the case. Concentration of population has, therefore, a more definite and readily appreciable effect in inducing disease in this country than it has in India. It may not be superfluous to add that density of population is not synonymous with overcrowding of the interior of houses, a subject which is discussed in paragraphs 58 to 64 of this Memorandum, and that the failure of Mr. Hankin's and Lieut.-Colonel Weir's statistics to prove that density of population is causally related to high plague-incidence, has no direct or necessary bearing upon the question of plague being fostered by insanitary conditions in the interior of occupied houses.

Incidence of plague not necessarily co-relative with density of population. 8707.

13. At the same time, density of population, although by no means co-relative with overcrowding of the interior of dwellings, is no doubt in many cases accompanied with this overcrowding, and is in itself a frequent cause of defective ventilation and light, with consequent vitiation of the atmosphere. It is therefore of importance to examine further into the conditions prevailing in the cities and towns in which plague cases have been numerous in sparsely populated areas.

In Ahmedabad, plague is stated to have occurred more in the suburbs than in the city itself. The congestion in the city proper amounted to 100 persons to the acre—the average in the nine largest towns in Scotland being 38·5 with a maximum of 58·6 in Glasgow—the houses, however, were several-storeyed, with an uncovered courtyard in the middle of each, into which most of the rooms opened by windows, and they had doors at the side opposite to the courtyard. In the suburban plague-affected area, on the other hand, the houses were single-storeyed and were built on three sides of a square, into which the door of each house opened; but they had no openings into the outer air except the doorway, and "perhaps two loopholes, one on each side of the door, and the ventilation was chiefly through the tiled roof." Lieut.-Colonel Weir also told us that, notwithstanding the ravages made by plague in the highly congested districts of Bombay, its virulence was actually greater in some of the relatively sparsely populated suburbs. The houses of the suburbs, however, were "the old-fashioned houses which have a number of inner rooms, not communicating directly with the external air, and very ill-ventilated," dark, and damp; and in these rooms the people generally slept. Further, when plague was most prevalent in the suburbs, the houses there were exceptionally overcrowded, because of an exodus from the previously affected city. When asked if the attacks and mortality from plague in Morland Road, a new street with new houses, were as bad as or worse than in the worst portions of Bombay, he replied, "I can only answer in a very general way. . . . I think they were just as bad, although Morland Road has hundreds of yards of open ground on one side." On the further inquiry, whether there were any special conditions and circumstances in the houses in Morland Road favourable to the prevalence of plague, he, however, said, "Yes; there are a very large number of poor people and outcasts living in very crowded rooms, and rooms which they crowd with screens and purdas"; and he also said that the conditions "are exactly the same inside the rooms only, but not outside," as in the most crowded buildings of the town. Sewree and Mahim are stated to be two of the most sparsely populated districts of Bombay, and yet plague severely attacked them. Apparently, however, the outbreaks occurred in the fishing villages of each district, and the houses in these villages are described as being so constructed as to be effectually deprived of ventilation and light, while drainage is totally absent. Other instances that have been cited, such as Parel and Worli, have elsewhere been dealt with (paragraphs 7 and 85).*

13,938.
13,976–85.
25,675.
25,690–2.
25,691.
25,698.
25,715.
25,784–5.
Gatacre's Report, p. 190–1.

So far, therefore, as the means for admitting air and light into the interior of houses are concerned, a highly congested area may actually be more favoured than one only sparsely populated, and when this occurs plague becomes more virulent in the houses of the less densely populated district.

* The manner in which originally good houses with surrounding open spaces and sufficiently ventilated and lighted rooms are converted into insanitary houses with unventilated and dark rooms has been described by Lieut.-Colonel Weir in Snow's Report, p. 101, paragraph 322, and by Colonel Fawcett in reply to Question 21,292.

Certain classes of people who live in well-ventilated houses or in the open air have been little affected.

Snow's Report, p. 147, para. 566.

Snow's Report, p. 174, paras. 621, 622.

917.

Confirmed also by Drs. Dyson and Calvert at p. 1 of their Report on Plague in Bombay, 1897.

Snow's Report, p. 147, para. 56.

14. Leading to the same general conclusion is the evidence which indicates that certain classes of the community living in well-ventilated houses have been but little affected by plague. In Bombay, where a part of the police force lived in properly constructed quarters and the remainder lived in native houses, it was found that only four cases of plague occurred among the former, while 151 occurred among the latter, showing, according to Lieut.-Colonel Weir, "that in well-ventilated buildings the disease does not readily develop." In the municipal chawls, occupied by scavengers or sweepers, who clean the streets and galis, and by halalkhores, who clean the privies, "very few cases occur, . . . while the people living in municipal chawls suffer little, the same class living in the ordinary buildings of the city, close to the municipal chawls, suffer as the rest of the population. . . . I have referred to the extraordinary freedom of the halalkhores and scavengers living in municipal chawls, but a similar immunity has been observed in the case of disreputable women living in many portions of the city. Dr. Rogers Pasha and Professor Bitter, who examined with me the municipal chawls or barracks in Kamathipura and the houses of the disreputable classes in the same localities, agreed that the conditions of better ventilation and less overcrowding were the conditions in which these buildings differed from the houses around them. The servants of the tramway company, when placed in huts, suffered as little as the most favoured classes. . . . At Falkland Road, where the company had erected, for the segregation of the establishment, huts on open ground, and segregated 150 men in the camp, not one case occurred, and yet the huts were in the centre of a district greatly affected—the district of Kamathipura."

22,128.

13,180.

22,267.

Lieut.-Colonel Hay, I.S.C., had noticed that "the fakirs were immune. A very large number of fakirs came to Bhiwandi. They pass through it on their way to somewhere. I never heard of a single case among them. . . . They live an outdoor life; and I think that is the reason why they do not get attacked." It has been observed by Mr. Hyde-Cates that there were no cases among the Waghris, who are employed as shikaris and also as coolies in Mandvi, but live in grass huts outside the town, nor among the Natras, or gipsies, who also live in "the open," but "were to be met every day in the town, following their occupations of begging and of selling small iron things for household use." For this reason, also, it was believed that no case occurred among the 186 wandering beggars of Satara, although they visited that town every day, and specially collected together there for alms, twice weekly.

Immunity of Europeans. 22,263, 22,426.

Snow's Report, p. 152, paras. 577, 580 917. 7168.

15. The remarkable immunity of Europeans has nearly always been explained on the same grounds. Lieut.-Colonel Weir stated: "The small proportion of attacks amongst Europeans . . . is, I think, entirely due to the ventilation of the buildings, the open condition of the buildings they occupy. There does not seem to me to be any other explanation." Major Charles, I.M.S., has endorsed this view. "For a European in a properly constructed house I think there is not very much danger."

Large incidence among domestic servants. Vol I., Appendix XIV., p. 378; Snow's Report, p. 190, para. 708. Campbell's Report, p. 185; 3,965; 24,137–9. Campbell's Report, p. 185; 0,539–47. 8,189–18,199. Snow's Report, p. 102, para. 328; p. 151, para. 576. 0,474. 147. Snow's Report, p. 106, paras. 349, 50.

16. While, however, Europeans as a class enjoyed a conspicuous freedom from infection, domestic servants, largely employed by Europeans, were numerously affected, with disastrous results. These servants live generally in small shed-like houses or huts, entirely separate from the bungalows, or residences, of their masters, but situated in the same open compound. In accordance with a prevailing native custom, the huts often contain not only the servants and their wives and families, but also, in many cases, numerous other relatives. They are, accordingly, frequently overcrowded, and the generally scanty means provided for admitting air and light into the huts are rendered inoperative, as they are carefully blocked up at night, and especially during cold weather. This was conspicuously illustrated at Malabar Hill, Bombay, where the servants were attacked, even after their quarters had been disinfected, "and the epidemic spread all through the servants' houses, although very few Europeans were attacked"; and it was observed that even in a bungalow in which rats had died of plague, none of the inmates acquired the disease. In the Marine Lines at Bombay the plague incidence in the servants' quarters was also very high, while in the adjoining bungalows, in many instances connected with the servants' quarters by matting sheds and covered ways, the incidence was insignificant. There were, however, "very few houses occupied by servants in the Marine Lines that had a window."

Immunity and prevalence among different castes.

17. Relative immunity was also observed to occur in other classes of the community. In the Brahmapuri Hindu Hospital at Bombay, the mortality from plague was, in two periods, 90 and 83 per cent., while the average in other four hospitals in which

other castes were treated was 38·8 per cent. Commenting on this, Captain Childe, I.M.S., writes: "From the above statistics it will be seen that the Hindus suffer by far the worse; but, considering the ghastly conditions of their existence, the terrible overcrowding in low, badly built, badly ventilated, and worse lighted houses, the total absence of drainage, and the most degrading personal filth, the only marvel is that the whole population has not been annihilated." Gatacre's Report, p. 225.

Lieut.-Colonel Nariman, I.M.S., stated that out of 85 indigenous cases of plague in Surat during 1896–97, 61 occurred among the 72,568 Hindus living in the city, and only four among the 20,377 Muhammadans; while no indigenous cases occurred among the 5,893 Parsees, and no cases, imported or indigenous, among the Europeans and Eurasians. When asked if there was any difference in the habitations of these different sections of the citizens, he replied, "As far as the habitations are concerned, Europeans and Parsees generally live in more healthy localities. There is no overcrowding in their houses, but amongst the Hindus, of course, there is always overcrowding." As to the Muhammadans, he stated, "I do not believe that there is so much overcrowding amongst them as among the Hindus." He added that the habitations of the Eurasians are "like those of the Parsees and Europeans." 15,632-3. 15,637. 15,638. 15,641.

Dr. Blaney, of Bombay, who when examined at first showed an inclination to explain the varying mortality of different castes by susceptibilities due to race, diet and habits, when afterwards asked to what extent the houses of the people affected the immunity, replied, "There the Commission has hit the great point. . . . Overcrowding is *the* great question in Bombay with regard to plague, with reference to the extent, the severity, and preservation of infection in houses. . . . I only wish to emphasize, as much as I possibly can, that the overcrowding has had a great deal to do with the intensity of the epidemic and the greatness of it." 17,681. 17,688.

A similar opinion was expressed by Captain Thomson, I.M.S. Referring to the immunity and to the difference of incidence among races and castes at Surat, he stated: "In the last epidemic, although we had 45 adult Europeans, 20 European children, and 125 European soldiers, not one was attacked, although 20 Europeans and one lady nurse were daily engaged in plague operations. They remained completely immune." "Then we had no Eurasians, native Christians, missionaries, or their families attacked, and there were 527 such in Satara City and cantonment. Thirdly, no Parsees were attacked. They comprised 50 souls. Of the hospital servants, ward boys, sweepers, corpse-bearers, ayahs, of whom there were from 60 to 65 in the first epidemic, only one was attacked, and that is mentioned because it can be clearly showed how he became attacked. . . . The depôt Sepoys, the 3rd Bombay Light Infantry, with their wives and families, remained immune." When asked for an explanation which would cover all these cases of immunity, he replied, "It depends on the habits of the people. Referring to the castes, it depends on the conditions of ventilation of the houses and the habits of the people as regards their knowledge of the necessity for fresh air and cleanliness in and about their houses." 22,263. 22,264. 22,265. 22,274.

This statement has been emphasized and corroborated by the circumstance that, when the conditions of ventilation differ in the houses of members of the same caste or section of the inhabitants, the plague incidence and mortality also differ. Thus, Mr. Madhava Rao has reported, "that the members of the Muhammadan community living in No. 2 Ward, whose number approximates to 3,686, had only 73 plague deaths among them, or something less than 2 per cent., while the Muhammadan population of the rest of the city, numbering about 9,942, had a mortality of 460, or 4·6 per cent." The reason assigned for the comparatively greater immunity enjoyed by the former is "that their houses are better ventilated, and the custom of gosha does not prevail among them." Report on Plague in the Mysore State, p. 35, para. 118.

Immunity in evacuation camps due to good ventilation.

18. We have elsewhere discussed the evidence regarding the effect on the progress of plague which has followed the removal of a part or the whole of the inhabitants of a plague-affected place to temporary habitations in the open country, and we have stated that in our opinion, "considered generally, it is probably a beneficial measure." In the estimation of many of those who have been actively employed in carrying out measures for the suppression of plague, complete as distinguished from partial evacuation of an infected place is the most successful of all measures, provided that certain obvious auxiliary requirements are ensured, such as the provision of proper sites and temporary buildings, the immediate isolation of any cases that may occur, and the prevention of conveyance of infection by persons or things either into or out of the confines of the camp. As was comprehensively expressed by one witness, it "is the one thing, every officer recognises that evacuation is successful." Our Report, para. 634. 10,364; 10,374–77; 10,464–9; 10,634; 11,849; 13,452; 14,452; 15,220; 16,987–89; 18,406–22; 20,238; 20,441–2; 22,288; 22,737; 23,169–98; 24,312–4; 24,363; 26,543–8; 26,672.

Inglis' Report, pp. 10, 107. James' Report, p. 105. 11,248–251. Inglis' Report, pp. 7, 107, 122, 157. James' Report, p. 79.

19. On the other hand, in several places complete evacuation was not adopted as a plague measure, or was postponed until the outbreak had already existed for several weeks, and the course of the outbreak in them serves to illustrate the value of this plague measure. The three villages in the Jullunder and Hoshiarpur Districts of the Punjab which suffered most severely from plague were those in which early evacuation was not enforced. Of these, the disease was most virulent in Kariha, a village with a population of 1,846, which was first affected on the 5th of April 1898, but, because of the determined opposition of the inhabitants, was not evacuated until the end of that month. During this period of about three weeks, there were at least 130 deaths from plague, while only 13 deaths occurred after evacuation had been completely accomplished. In four villages in the Dharwar District, in which evacuation was not carried out because of the rains and the want of suitable hutting materials, the percentage of attacks to population ranged between 30 and 47, and in one of them the plague death-rate was 35 per cent. of the total inhabitants. In Byahatti, one of these villages, with a population of 3,589, plague "spread with intense rapidity," and Mr. Westrop, the Assistant Collector, thought that although this might be due to climatic conditions, he would "be inclined to attribute it to the stay-at-home habits of the people in the rains, and the absence of ventilation in the overcrowded houses." For similar reasons, evacuation was postponed at Igatpuri, a town of 6,000 inhabitants in the Nasik District, but all the houses were disinfected with perchloride of mercury, and then thoroughly cleansed; and 518 cases and 421 deaths occurred during the thirteen weeks that elapsed before evacuation was resorted to.

2169–91.

Evidence, Vol. I., p. 92, Statement A; 2192.

16,884–95; 19,484.

20. The value of this plague measure has also been illustrated in the case of single families, and of several families collected together in one building. One of the most striking examples of the former was mentioned by Lieut.-Colonel Peters, I.M.S. Two of a large family of patawallas, in Belgaum, "came to the camp, and four others perished in the house." Subsequently, the remaining survivors, 13 in all, were removed to the health camp, and "there was not a single case among them." An example of the latter has been reported in Calcutta, where, after it had been discovered that there had probably been 12 deaths from plague in a most insanitary bustee in Roop Chand Roy's Street, an opportunity was given, in accordance with the policy then followed, to the 200 inhabitants of the bustee to remove to a building provided for them by the Municipality. None of them, however, availed themselves of the opportunity, but preferred to adopt the option of finding lodgings for themselves, with the result that at least 12 cases of plague occurred which could be traced to Roop Chand Roy's Street, besides other cases which were not certainly diagnosed, as they had not been seen during life by a qualified medical man, and that seven widely scattered foci of infection were originated.

23,041.

Evidence, Vol. I., Appendix, XX., pp. 477–8.

8; 9; 1355; 2168–2200; 5926; 6036; 9054; 9225; 10,464–5; 14,671–5; 15,775–7; 17,295–6; 18,075; 20,234 21,320–2; 21,538–9; 21,832; 23,938–42; 24,058–63; Inglis' Report, p. 4.

21. The largest amount of evidence in favour of the success of evacuation has, however, been obtained from the numerous instances in which the removal to an evacuation camp of the inhabitants of a town or village in which plague had been extending, often with great rapidity and virulence, was almost immediately followed by a decided diminution or even by an entire cessation of further cases.

It is important to consider what explanation can be found for these valuable results, as thereby a cause of the propagation and virulence of plague may be rendered manifest.

Immunity in camps not due merely to removal from the infected place or site.

22. Several explanations suggest themselves. These results, it might be supposed, are due to removal from the place where the cases of plague have been occurring, on the supposition that that place contains the cause of the infection in the floor or walls of the affected houses, or elsewhere than in the patients or the movable contents of the houses. Admitting that a place or site may become infected, the fact remains that the introduction of persons suffering from plague into dwellings where previously no case had occurred has often been known quickly to infect the healthy inmates. Further, the history of nearly all evacuation camps shows that infection has been introduced into them by the unavoidable admission of persons who were in the incubation stage and afterwards developed the disease, and also by failure in the early isolation of plague cases otherwise introduced into the camps; and yet, although opportunities have thus been given for the infection of the site, it is conspicuously manifest that plague rarely extends in these camps, if the huts be so arranged and constructed as to secure adequate ventilation. On the other hand, there are several instances, as that of Uran, and of other places cited below, in which the removal to an evacuation camp from a place in which plague was actively spreading entirely failed to suppress the disease among the persons who had been removed; the failure being associated with inadequacy in the

12,081–2; 13,808–12 22,041

14,623–4; 14675; 17,295; 18,074.

22,737–43; 22,775; 22,786.

ventilation of the huts, although, it may fully be conceded, in some instances, also, with omission of the precautions, required in all infectious diseases, to prevent extension from infected persons and things.

23. Again, it might be supposed that the cessation of further cases was due to the disinfection with chemical substances, frequently practised of the effects and, less frequently, even of the persons of the inhabitants of the infected place, previously to their admission into the camp. Against this supposition it has to be observed that the disinfection alluded to was not always carried out, as in the successful evacuations in Poona Cantonment described by Lieut.-Colonel Fawcett, and in others referred to in the margin. It was only rarely carried out in villages, and in many of the places where it was included among the precautionary measures preceding evacuation, it was performed in only an imperfect manner, and did not overtake all the probably infected articles taken into the camps ; while, notwithstanding, plague did not materially extend in the camps. In so far as the site is concerned, it was not customary to apply disinfectants to the floors of huts, except in a few camps where the operation, however, was restricted to the huts in which there had actually been cases of plague ; and it may here be remarked that in many camps where evacuation was successful the floors of the huts resembled those of the houses from which the camp inmates had been removed, in so far that they consisted of mud and cowdung, and also that the presence of rats in these evacuation camps has several times been noted. It is, however, undoubted that in Kumaun and Garhwal, the inhabitants of plague-infected houses and villages did not practise disinfection by chemical substances of persons or clothes, or of the temporary huts into which they removed, and yet almost no case of plague occurred in these huts.

Immunity in camps cannot be explained by disinfection with chemical substances.

James, Jullundur, &c., Report, 161. 9294–5 ; 10,471 ; 11,144–48 ; 18,582 ; 18,600–1 ; 21,251–3.

James's Report, p. 160 ; 23,071–2.

24. Assistance in finding an explanation of the beneficial results of the complete evacuation of an infected place may likewise be obtained from a consideration of some of the few instances in which these results were not entirely secured. No doubt, failure was occasionally produced by the isolation of cases occurring in camp not having been immediately affected or by the introduction into the camp of infected articles. Another cause has, however, been made apparent by the evidence placed before us.

Failures of evacuation camps due to overcrowding and insufficient ventilation. 10,603.

In Captain James' Report on the outbreak of plague in the Punjab, it is stated, "The huts made by the villagers may be, and usually are, more comfortable than Government ones, but they certainly do not let in the same amount of light and air, and are therefore not so serviceable for the present purpose."

James's Report, p. 103.

Having described the results of evacuation in Jawalapur, Mr. Winter, I.C.S., adds, "If the people had all been put into Government camps, I think it is possible that there would have been fewer cases in camp. Of course, as they built their own camps, they did it as cheaply as they could. They built the huts adjoining one another or very close together. The huts were thickly populated, and there was not much circulation of air."

9178 (last para.).

When asked, "Is there any exceptional condition with regard to the huts in those camps where you had no success?", Mr. Dodgson, I.C.S., replied, "The absence of success is chiefly due to the rain, not to the nature of the huts. The people build their huts much more solidly during the rains than they do at this time of the year. During the rains they are tighter and closer, and are like little beehives, with a little door which you can just walk in at, and there is, practically, no ventilation whatever."

22,048.

One of the chief causes of incomplete success during the rains is attributed, by Lieutenant Anderson, I.S.C., to the fact that the huts constructed by the villagers are frequently worse ventilated than the houses in the villages, with the result "that a large number in one hut will frequently get plague."

22,607–9.

To the same effect is the observation of Captain C. H. James, I.M.S., that if any further proof of the value of evacuation were required it would be found, even in the voluntary camps of villagers, "in the fact that as the weather became warmer, and the people huddled less together in the camps, the effects of evacuation became more and more marked."

James's Report, p. 122 (for an example, *see* pp. 20–21 and Evidence 10,384, relating to Sirhal Kazian).

Referring to the unauthorised camps at Surat, Mr. Judge found that, where no proper supervision was exercised and the people were allowed to crowd together in the huts and shut out ventilation, "the disease raged quite as freely . . . as it did in a town."

16,016.

Mr. Kabraji, I.C.S., informed the Commission that, with the view of securing "the free admission of daylight and fresh air . . . we insist that when people

19,515.

evacuate a village and go into the fields they shall not create the same unhealthy conditions again by making air-tight and badly lighted huts as they are prone to do."

11,263. Mr. Giles, Acting Commissioner in Sind, stated that there were no cases in well-constructed camps, but "where they were huddled together, as a few of Dr. Seymour's camps were, there were a few cases of plague." He also ascribed the failure of voluntary evacuation in the huts erected in date groves at Rohri principally to the 12,120-1. insanitary condition of the ground and to the very defective ventilation and light, the date trees being "very thick . . . so that you could hardly see the sun at all."*

The following evidence bearing on this point, given by Captain Thomson, I.M.S., is particularly instructive: "You wish to point out that the mere fact of evacuation is not sufficient, but that there must be certain things attended to in that evacuation: what do you think is the most important?—That the Government huts should be 22,289. ready for the whole population of the area to be affected. It is useless removing them from structurally insanitary houses to huts equally air-tight, because that 22,290. would defeat the effect of the evacuation almost entirely." "Is that the result of your experience?—It is from my experience of Satara, and some villages which I know continued to have plague after evacuation." "Can you narrate your experience?—The town of Limb was completely evacuated, and yet plague continued for months afterwards, because the people were allowed to hut themselves in an area of five miles round the village, and they did not go into Government-constructed huts. They continued to have plague for months after evacuation. Plague lasted eight weeks before evacuation, with 45 deaths, and 22 weeks after 22,291-6. with 521 deaths." "How do you account for that?—Because they went into badly ventilated huts." "What were the characteristics of these huts which produced the bad ventilation?—First of all, their structure. They were made of bamboo poles, and they used matting for the walls, while the roof was constructed of grass. We leave air-spaces on the top, but if the people are allowed they build their own huts with karbi, and daub them inside and outside and make them as air-tight as possible, with very small openings, and they will also overcrowd them." "That was the condition of the huts in the instance which you have given?—Yes, of those huts in which plague continued to occur." . . . "Have you any other definite instance in which the evacuated people continued to have plague among them when that continuation was in your opinion due to want of ventilation in the huts they occupied?—The town of Rahmatpur, of 6,670 inhabitants, had plague for 15 weeks after complete evacuation, owing to the insanitary huts to which they went. Before evacuation, plague had lasted seven weeks, and there were 106 deaths before and 162 deaths after evacuation." "Will you describe those huts?—They have the same characteristics as those of Limb which I have described. I have seen them."

25. It would thus appear that the remarkable success of complete evacuation and the equally remarkable immunity from plague in evacuation camps are mainly to be attributed to the adequate provision of unpolluted air and of sunlight in these camps.

Plague does not extend in hospitals. 12,942. Excluding moribund cases, hospital mortality relatively low. Gatacre's Report, pp. 54, 69; 10,401-3; 15,267-8; 21,540; 25,026. Hutcheson's Report, pp. 20, 21.

26. Equally striking in its indications is the evidence derived from hospital experience. Relatively to the conditions which exist in native houses, the hospitals are freely supplied with air and light, and the space appropriated to each patient is ample. For example, in the Parel Hospital, Bombay, "each patient had nearly 2,000 cubic feet of air space and free perflation of air." The experience of Bombay and of other places shows that, if patients who died within the first 24 hours are excluded, the general mortality in hospitals was decidedly under that in private houses. Even in the very few instances in which the surroundings of the hospitals have not been satisfactory, where, for instance, they have been "completely shut in and surrounded by other larger houses," or built on a site with "many sanitary drawbacks," or placed in a crowded locality, the mortality, relatively to that in Native houses, was still favourable, and, above all, plague did not extend among the inmates of the hospital.

27. The latter is a prominent circumstance in the experience of all plague hospitals; and it was displayed in some of them, although the ward servants went about bare-footed, and in nearly all, if not all plague hospitals, even although ward servants and

* It is of interest to find that Professor Koch has observed that in an endemic centre at Uganda and in the Victoria Nyanza Country, in Equatorial Africa, the plague incidence is especially high in banana palm groves. The affected villages lie in dense banana groves, which are almost impenetrable by light and air, and swarm with rats. According to Professor Koch, they are thus typical (förmliche) breeding places of infectious diseases, and there is no cause for wonder if the plague takes root in such a district. (Deutsche Medicinische Wochenschrift, 14. Juli, 1898, 439.)

numerous friends of patients were brought daily and nightly into the closest personal contact with pneumonic as well as with bubonic cases of plague. The following are illustrative examples:—

In the Parel Hospital, Bombay, "in upwards of 240 instances the friends of patients attended their sick, and in 20 instances scarcely even left the bedside, and in no single instance did the disease spread to the friends. Out of more than 140 attendants on the sick belonging to the hospital staff from time to time, only one sweeper was attacked, and he had been constantly helping in the *post-mortem* room, and had a very mild attack with small axillary bubo . . . . In three cases amongst hospital orderlies, there were special circumstances of contagion existing very likely to lead to direct inoculation, and are, therefore, not considered instances of spread of the disease from mere attendance on the sick." In the Arthur Road Hospital, while 4,000 cases of plague were under treatment, only six attendants and one medical man contracted the disease; but the former did not live in the hospital but "outside in infected houses," and the latter occupied a dwelling in an insanitary locality.* In the Grant Road Hospital, "of about 400 people—men, women, and children—who either visited their sick friends or remained constantly by their bedsides, together with the cases under observation, in not a single instance did any of these persons contract the plague. But one of the ward orderlies doing duty in the hospital became affected by direct contagion in consequence of drinking the remnants of stimulants left in feeding cups by patients who did not consume the whole contents, and, most probably, after some cup had been in contact with the mouth of a plague-pneumonic case." Referring to the same hospital, but at a different period of the epidemic, Dr. Hutchinson states: "Relatives of patients were admitted at any time during the day, only two being allowed to visit each patient at a time. At night only relatives of the patients who were dangerously ill were admitted. It was found impossible to prevent them from handling the patients, yet, notwithstanding their constant and close contact, the occurrence of only two cases of plague amongst the relations was brought to notice."

Snow's Report, pp. 165-8, 216-7; Nathan's "History of the Plague in India," Vol. I., p. 289. Campbell's Report, pp. 150, 160. Snow's Report, pp. 209, 212, 269. Gatacre's Report, p. 51. 20,551; 20,973–4. Gatacre's Report, p. 94. Campbell's Report, pp. 134–5.

In the Medical College Hospital at Calcutta, the friends of the patients were encouraged to go into the hospital and stay with the sick, and none of them were known to have become infected, although they remained beside and attended upon pneumonic as well as bubonic cases of plague. There were no cases of plague among the 18 attendants in the Chaudni Hospital, nor among the 56 in the Mayo Hospital.

6963–5. 7020–1.

In the General Plague Hospital at Surat, "out of about 900 attendants who lived in the hospital to look after their relatives, only 12 were attacked with plague. Of these, eight were attacked within 24 hours of admission, two within 48 hours, and one each on the 10th day and 20th day, respectively." In the Hindu Hospital of this town, between October 1897 and the end of January 1898, there were only 19 cases of plague among 1,200 relatives and friends who lived in the hospital with the patients, and several of these cases occurred within the incubation period of 10 days after these attendants had entered the hospital.

15,796. 16,149–52; 21,593.

In the General Plague Hospital at Poona, where, from June 1897 to March 1898, 4,179 patients were treated, no case occurred among the friends who attended the patients and were at least equal in number to them; while of the hospital establishment, consisting at one time of 320 persons, only 26 contracted plague, but it is difficult to know how many even in this relatively small number contracted the disease in the hospital, as all the members of the establishment were frequently given leave to visit their homes in the city. In the Hindu Hospital, Poona, 637 friends of the patients acted as personal attendants, between March 1897 and March 1898, and only four of them became infected with plague.

Lloyd Jones' Report on Poona Hospital, p. 10. 21,539.

In the Plague Hospital of Hubli, Mr. Cappel stated that there had been only two cases of infection among the attendants "who were with the patients day and night," and that there had been only five cases in the Dharwar Hospital "among the people who stayed and slept all night long by the bedside of the patients."

1704.

In the eleven Plague Hospitals at Porbandar, none of the members of the hospital staffs were attacked and only two of the numerous relatives and friends who attended the patients, but they are stated to have been attacked within the period of incubation after the date of entrance into hospital.

13,702, 13,723–6.

* The absence of extension in this hospital is noteworthy, as rats are stated to have been numerous in the hospital compound. 21,008–10.

R. N. Jadhay's Report on the Working of the Daliawadi Plague Hospital, 1897–8, pp. 14, 15, 31.

In the Daliawadi Plague Hospital of Baroda City, where it is estimated that about 700 friends and relatives attended on the patients and often slept in the same ward with them, only 10 became infected; and the members of the hospital establishment altogether escaped, although most of the servants "were in the habit of going all over the hospital barefooted, with numberless cracks and fissures on the soles of their feet."

22,489.

In four hospitals at Satara, employing 106 servants, only two were affected with plague, and neither derived the infection from the hospital.

In the Plague Hospital at Ankleshwar, Mr. Candy, I.C.S., Collector of Broach, arranged that the immediate relatives should be admitted with the patients, "that the husbands, wives, and family should be together." Sometimes there were "four or five children all round a sick bed," and there was scarcely "one instance of anybody being any worse for that." Mr. Candy attributed this immunity "to the

15,500.

excellent atmosphere which is maintained in a hospital," and he added, "If these people had remained with their sick in their own houses, where they shut up the doors and windows, so that the place became a hothouse, a forcing house for the plague bacilli, they would suffer."

Further, even when plague cases were admitted amongst the ordinary patients in a

1439.

general ward, the experience was the same. "In the Jamsetjee Hospital at the beginning of the epidemic a large number of cases of plague were let in by mistake at first, not being properly diagnosed; but no cases of plague spread from them at all, neither among the patients nor the attendants."

So encouraging has been the experience that plague does not spread in a hospital,

Gatacre's Report, p. 51; 2923.

that it has been asserted that the ward of a plague hospital is "one of the safest places during an epidemic."

Immunity in hospitals contrasts remarkably with great and rapid extension in native houses. Gatacre's Report, pp. 50, 222; 21,540; 25,828–9; 1725; 3448–73; 6193; 12,333; 12,482; 14,448; 17,589; 17,984; 19,597–617; 25,831.

28. It is notorious that this statement cannot be made in regard to the great majority of the houses of Natives. Plague is well known to spread in them with appalling rapidity, although the inhabitants of the houses are actually the persons who, when transferred to hospitals, remain unaffected even when surrounded by more numerous sources of human infection than are present in their houses. The introduction of an infected person into one of these houses has been known frequently to cause four or five cases of plague among the limited number of the inmates, and, less frequently, seven, eight, nine, ten, and twenty inmates, and even the whole of the inmates of the house have become infected, although some of the houses had been disinfected on the death of the first patient, and although in many of these instances the infection was communicated by patients suffering from the more common bubonic, and not from the less common and more infectious pneumonic type of plague.

29. In some towns, also, most of the plague patients were treated in their own dwellings. In Bhiwandi, in the Thana District, this system proved a failure, as a very severe outbreak resulted, with a case incidence of 2,000 in an estimated population of considerably under 10,000; and Lieut.-Colonel Hay, I.S.C., calculated that, "supposing one case occurred in a house, it was 100 to 3 against an inmate getting plague if he went at once into segregation, or if he attended his friends in hospital outside, but if he remained there" (in the house) "out of every 100 at least 16 were taken." In Karachi, the result, according to Dr. Niblock, was that, "from the point of view of preventing the spread of the disease, it was very bad indeed;" and he instanced a house containing from 35 to 40 inhabitants in which no less than 21 people "caught the disease from one another." In Belgaum, where during 1898–99 the patients were left in their houses along with the other inmates, the outbreak continued, with a high mortality, for at least seven months, and cases kept occurring one after the other in disinfected houses. In Byahatti, in the Dharwar District, after all the non-affected had been removed to evacuation camps, the sick, numbering about 150, were left in the village and treated in their houses, each with one attendant, and many cases occurred among the attendants, although all the infected houses had been disinfected.

Snow's Report, pp. 54–70. 22,114–8; 23,359. 12,332–4; 12,479. 22,803–25. 2192.

Causes of immunity in hospitals. 1440–3; 10,374–7; Gatacre's Report, p. 69.

30. Among the chief reasons which have been advanced for the remarkable freedom in hospitals from the infection of attendants and other healthy persons, although they were in the midst of plague patients, and often in circumstances of close and prolonged contact with them, in all stages of the disease and in all varieties of it, are the absence of overcrowding, and the provision of adequate ventilation and light in the interior of the hospital wards. Those who have had opportunities of observing the relationships that are maintained between the patients and the attendant relatives

or friends can entertain no doubt regarding the intimacy of this relationship, for not only are they most assiduous in supplying the patient with food and drink, and in assisting to change an irksome position, but saliva escaping from the mouth is often removed by the hands of the attendant, and the hands are even used as a receptacle to receive sputum when the patient expectorates. 17,985. Vol I., Appendix XIV., p. 379.

31. It is no doubt the case that in many hospitals the risks of infection were attempted to be lessened by the use of disinfectants applied to the floor and walls of the wards, and even to the excreta of patients, and, occasionally, also to their bedclothes and persons. On the other hand, the habits of the patients render it impossible effectually to disinfect all the infective material proceeding from them; and disinfecting operations have been carried out in several of the few hospitals regarding which detailed information is available, in only a perfunctory manner, while the freedom from infection in these hospitals has been as decided as in the hospitals where greater precautions appear to have been taken. The Officiating Principal of the Medical College at Calcutta stated in reference to the Medical College Hospital that the floors of the wards "are washed twice a week and disinfectants are placed about, and disinfectants are put in the pans in which the patients expectorate. There is also a solution of the disinfectant for the nurses, &c., but I do not know that they are invariably used. . . . We placed the antiseptic solution in the spittoons, but the patients expectorate on the walls and the ground and all over the place, so that one really cannot disinfect a ward as well as one would like. These patients are often quite delirious, and, therefore, did not expectorate where one wanted them to." He also stated, "I have seen no cases of infection either among nurses or any other person from any living cases of plague." In reply to the inquiry, "You said you had seen no cases of infection from plague patients occurring in the hospital: do you use any antiseptics in the wards, do the nurses wash their hands with antiseptics?" Major Evans, I.M.S., said, "They certainly do not wash their hands each time they touch or feed a plague patient. They have done their work very bravely, and, as a matter of fact, I think they wash their hands very seldom while on duty." He further stated that the nurses "certainly have not taken any very great precautions."

Absence of extension in hospitals not due to disinfection by chemical substances. 7022. 20,628. 7083. 7085. 7081. 6961. 6962

The practice followed in the Daliawadi Plague Hospital at Baroda, where the mortality, deducting deaths which occurred within the first 24 hours, was 59·7 per cent., and therefore by no means high, is thus described by Dr. R. N. Jadhav: "After the removal of dead bodies from the rooms of the hospital, the floor was burnt with a layer of straw, and the walls scrubbed with coir and washed with lime mixed with perchloride of mercury. This practice was invariably followed in the case of every death that occurred in the hospital, except when, on account of a large number of deaths and a sudden rush of patients to the hospital at one and the same time, it was impracticable to carry out all the details of disinfection." It need scarcely be pointed out that the chemical disinfection employed in this hospital was probably useless; as the disinfecting properties of the perchloride of mercury were destroyed by mixing it with lime, while lime itself has very little bactericidal action on plague bacilli, even in the improbable event of its having been applied before it had absorbed carbonic acid. As already stated (para. 27), there was, however, no extension of plague among the members of the hospital establishment, and almost none among the numerous attendant relatives and friends of the patients.

Report on the Working of the Daliawadi Plague Hospital from 6th Oct. 1897 to 30th April 1898, p. 7.

32. In corroboration of the independence of immunity in hospitals on disinfection by chemical substances, the following evidence given by Captain Childe, I.M.S., regarding the Jamsetjee General Hospital, Bombay, may be quoted: "I understand that you said infection did not spread in your hospital when you had plague patients in it: were the urine and the fæces disinfected, or was the fact of non-infection the result of light and air?—In these early cases no precautions were taken at all, because they were not diagnosed; but still no spread occurred. At the beginning of the epidemic, cases were admitted before buboes appeared, and they might be there two or three days in the hospital before they were diagnosed at all. They were mostly admitted into the best wards of the hospital."

1516.

Plague patients, who were not known to be suffering from plague, were admitted into other general hospitals, especially at the early stage of outbreaks, and, nevertheless, although no special precautions were taken, the disease was not communicated to other patients or to the ward servants or members of the hospital staff. A prominent example may be cited of a patient who was treated in the Medical Cottage Hospital, Calcutta, as a case of simple pneumonia, whereas it was shown at a *post-mortem* examination that the case was actually one of plague pneumonia.

6777; 6787; 7102–10; 7123; 7129–32; 7136–7. 6764.

No extension of plague when patients treated in their own residences if in well-ventilated rooms. 22,510–14.

33. Further, in several private houses, patients have been treated in well-ventilated rooms without the use of any chemical disinfectants, and still plague has not extended to the inmates of the house. This was done, for instance, by Captain Thomson, I.M.S.: "The room in which the patient remained was plague-proof, as I call it, and no case followed. . . . There were five other members of the family, and they were allowed to remain in the house. The house was well ventilated and not overcrowded. There were no chemical disinfectants, but the house was disinfected with sunlight and air." None of the other inmates of the house were attacked. This procedure was also adopted at Bombay, in the municipal chawls occupied by the scavengers and halaikhores. The few persons who became affected with plague in these chawls were allowed to remain in the rooms they usually occupied, or, in one or two instances, were transferred to rooms at one end of the building they lived in, "and their friends came to see them and went away as they liked." The result was perfectly successful, for very few cases occurred in these chawls, as they were sufficiently ventilated, although there were on an average seven persons in each room, and one of the chawls contained 800 people and was situated in Kamathipura, "in the midst of the most affected area."

Snow's Report, p. 174.

Plague does not much extend in ships. 745.

34. It is also a noteworthy circumstance that only a very small number of persons have become infected on board ship, even in vessels starting from plague-infected places. The number of these vessels and of their crews and passengers has been very large, 87,571 vessels having been inspected for plague at Bombay alone, between 1st January 1897 and 30th November 1898, with a total of 1,519,135 of crews and passengers. Yet, as we have elsewhere stated (our Report, para. 693), we were able to obtain information of only 30 cases of plague having occurred on board ship, which were distributed over 13 vessels of large size, six being mail-carrying steamers, one a troopship, one a ship having 1,021 pilgrims on board, and one a steamer conveying a large number of emigrants to Uganda, and bearing a crew of 100 officers and men. In thirteen vessels, therefore, cases of plague occurred while the ships were at sea; and, notwithstanding the fact that there was a large number of persons on board the ships to whom plague might have been communicated, only a most insignificant number of cases occurred. Further, in two of these vessels there were fatal cases, one of which, registered by the ship's surgeon as a case of plague, also suffered from acute pneumonia, and in at least two of the vessels dead rats were found. It may be assumed that this striking immunity can partly be explained by the favourable conditions existing in ships for the early discovery of cases of important illness; but, on the other hand, the conveniences for the separate treatment and complete isolation of infectious patients and the possibilities of disinfection by chemical substances must, necessarily, have been restricted, as we have pointed out in our Report. One condition of existence on board ship, however, cannot be ignored, namely, the provision of a good supply of pure air, which is secured not only by the regulations for preventing gross overcrowding and ensuring sufficient ventilation in the sleeping quarters, but also by the open-air life of everyone on board.

753.

12,244, 25,496.

759, 763.

Chapter III., para. 256.

This absence of any marked tendency on the part of plague to spread in ships contrasts favourably with the rapid spread in badly ventilated houses on shore, and also with the instances in which on board ship the extension has been great and rapid of other infectious diseases, such as small-pox, yellow fever, measles, scarlet fever, and influenza.

Occupation during the night of infected houses which are inadequately ventilated is a potent cause of extension of plague. 1439; 21,538.

35. Infective material, apart from an infective person, may be present in a house, as in the floor, walls, or ceiling, or in articles contained in the house. It has been shown by the evidence that while the infectivity of this material is augmented by insufficiency of light and air, it may remain only slight when this insufficiency is not specially in operation.

36. Thus, in many places, evacuation was accompanied with the granting of permission to return from the camps to the evacuated towns or villages for the purpose of carrying on business or trade, and this policy, although obviously attended with some risk, appears to have been carried out with nearly complete impunity so long as the visits were restricted to the day-time, and did not involve the removal of articles of clothing or foodstuffs. Mr. Giles and Colonel McCloghry told us that at Karachi the people in the evacuation or health camps were permitted to go as they liked to the city during the day, and that the results in the camps proved that no harm resulted; and in General Gatacre's Report it is stated that when 400 of the inhabitants

Inglis' Report, p. 15. 6007–9.

11,324–5; 11,433.

of Worli Koliwadi were in camp, many of them went daily to the incompletely evacuated village to get grain, but no cases occurred in the camp after "the ten days' incubation period," although the type of the disease in the village was one of "extraordinary virulence, over 90 per cent. of the cases dying, often after a few hours illness only." pp. 187–9.

37. This was not the case, however, when the visits were made surreptitiously, as they occasionally were, and the visitors remained and slept in the houses. For instance, at Malegaon, after all the houses had been disinfected with either perchloride of mercury or sulphuric acid, a number of persons from the evacuation camp were allowed to return to the town during the day-time for the purpose of continuing their work as weavers, in open and selected shops. "There were a few cases of those people stealing into their houses at night, and it was among them we had further cases of plague." 18,075 In Nasik, cases of plague were constantly occurring in the camps, and this could only be explained, according to Mr. Silcock, I.C.S., by people returning secretly to their houses and sleeping there; while it was found that no infection appeared to be caused by allowing people to return to their houses during the day to "open their shops, do their ordinary work and carry on their trades," so long as they did not remain at night in shut-up houses. 13,807–8. 13,809–12; 14,733–5. Further, Mr. Maynard, M.R.C.S., observed that several cases occurred after evacuation, because people could not be altogether kept out of their houses at night when the nights became cold. After the town of Ankleshwar had been evacuated in August 1898, many of the people in the camps were permitted to return to the town to transact business during the day, and beneficial results were not thereby prevented from following the evacuation. 14,222. 14,226. 14,408–9. Although precautions were taken to prevent the people from remaining and sleeping in the houses at night, several of them secretly did so and became infected with plague. 14,229. 14,230. In a health or evacuation camp at Bombay, under the charge of Mr. F. M. Gibson, B.Sc. in Public Health, in which the inhabitants, averaging 500, were allowed free ingress and egress during the daytime, the only plague cases that occurred, excepting among those who had been less than ten days in camp, were in people who were distinctly known to have slept outside of the camp. 17,295.

38. Individual instances have also been narrated. Dr. Petigara stated that several had come within his knowledge at Ankleshwar, even in houses that had been carefully disinfected with perchloride of mercury. He described the case of a policeman who, after the police lines had been removed to new quarters on account of an outbreak of plague, and while no further cases were occurring in the new quarters, visited and slept in a house in the vacated lines, contrary to orders and to the advice of his friends, "with the result that he soon acquired plague and died." 14,412–4. Colonel Hutcheson, I.M.S., and the officiating Commissioner for the Nagpur Division of the Central Provinces have reported that after the town of Wardha, at the end of December 1898, had been completely evacuated and the houses disinfected, two cases of plague occurred early in January 1899, in persons who had evaded the guards and slept in evacuated houses in the town. Report on Plague and Plague Operations in the Central Provinces, pp. 12, 13. Captain Walton, I.S.C., considered from his experience in the Cantonment of Ahmednagar, that one of the requirments for success in evacuation was always to make the people sleep in camp, although they may be allowed to go to the city during the day for business. 24,197–8. He instanced the case of a man who became infected with plague because he slept in the city, although he had been strongly enjoined not to do so. 24,155. Mr. Sladen stated that the system adopted in the Karachi Evacuation Camps was to allow the people to return to city in the day-time for their daily work, but to prevent them from living in the city, and that evacuation with this privilege proved successful in so far as the disease did not extend in the camps. 12,752–3. 12,755. He specifies an instance, in which the regulations were evaded by a man who went back to his house at night and slept there, with the result that he became infected and died of plague. 12,795–8.

39. On a larger scale, the evil effects of prematurely occupying and remaining all night in evacuated houses have been exemplified in several instances. Some of them, as that of Bantwa and Chak Kalal, and of villages in the Sirohi State, have already been referred to by us; and to them may be added the three following, which occurred in villages in the Cutch State and in Kumaun, where it is notorious that the houses are badly ventilated and overcrowded. Chapter VI., para. 632.

The villagers of Salaya, who had been removed to huts on the sea-shore after ten cases of plague had been discovered in the village, and had had only one case while in camp for seven days, returned to their houses in the village because of a gale of wind, and during the two months they remained in the village, 102 cases occurred among them. 13,431. Bada, also a village in the Mandvi District, was evacuated, and after the people had remained free from plague for 17 days in the evacuation camp, they were allowed,

13,456. on the occurrence of rain, to re-occupy their houses for four days; and immediately on returning to the evacuation camp several of their number became affected with plague. After forty-four cases of plague had occurred in the two adjoining hamlets of Bintola and Balt, in Kumaun, and the disease had been suppressed by the people removing to huts of grass and tree-branches on the hill-side, a snowstorm caused them to re-occupy their houses, and within a few days, four deaths from plague took place; but the outbreak was again suppressed when the villagers a second time sought refuge in huts.

Evidence, Vol. II. Appendix XXV., pp. 337–8.

That conditions favourable to the propagation of plague are produced within native dwellings by circumstances connected with their habitation during the night, has also been exemplified in a case described by Major Hyde-Cates. Although the fishermen and sailors, who constitute the greatest part of the population of Salaya, were all day occupied in fishing or otherwise exercising their calling out-of-doors, they suffered severely from plague before the village had been completely evacuated. They, however, "sleep in their houses at night, and it may be noted that the houses are very dark and ill-ventilated, though clean."

13,476.

13,477.

To the same effect was the statement of Mr. Dhurandhar that, in Baroda, the plague incidence increased when the wind had an easterly direction, and diminished when it changed to the west, and that during the former people crowd into their houses; and that of Mr. C. C. James, that in Bombay northerly and easterly winds had, often immediately, an unfavourable influence on the mortality of plague, because, being cold, they drive people into their houses. Instead, therefore, "of a large number of people sleeping out-of-doors, as is generally the case during the hot weather, they all sleep in-doors, and, moreover, they shut their doors and windows. It is calculated that, in Bombay, 350,000 people sleep in the streets here during the hot season."

15,244–6.

26,104.

The prevailing opinion among those who have had the largest experience of plague has been expressed by Mr. Winter, I.C.S., who, in reference to the relative unimportance of the outbreak of plague at Kankhal during the hot and dry weather of May, stated that it was unlikely that plague should then spread rapidly, as "the people are sleeping out-of-doors all night, and they are probably out in the air all day," scarcely using "the interior of their houses at all;" and by Dr. Watson, who from his observations at Garhwal and Kumaun, where, as we have seen, the condition of the interior of the houses is eminently unsatisfactory, concluded that "it is almost certain death to sleep in the same room as a person who is suffering from plague."

9071.

Vol. II., Appendix XXV., p. 363.

40. This great liability to become infected by remaining during the night in insufficiently ventilated chambers containing infective material—which in some of the instances cited, appears to have been reintroduced into disinfected chambers after their evacuation, by the entrance into them of plague-infected rats—contrasts in a remarkable manner with the immunity enjoyed by members of the same classes of people when they remain, as attendants on plague patients, even during many successive nights, in the well-ventilated, or it may be only fairly well-ventilated wards of a Plague Hospital, notwithstanding that the wards contain large numbers of infectious persons (paragraph 27, *ante*).

Comparison as to the incidence of plague between sanitary and insanitary towns, and between sanitary and insanitary houses in the same town.

41. This evidence would be strengthened if, on a comparison between a town in which the sanitary conditions within the houses were satisfactory with one in which they were not satisfactory, while the other chief circumstances that are known to modify the course of an outbreak were much the same in both towns, it were found that, on plague being introduced into each of them, it failed to produce a serious outbreak in the former, while it succeeded in doing so in the latter.* The possibilities of instituting such a comparison are extremely restricted on account of the difficulty of finding any native town with satisfactory sanitary conditions in which plague has occurred.

Contrast between outbreaks of plague in Hardwar and Banga. 9004–15.

42. Hardwar, however, is one of the few towns in which this requirement is fairly well obtained. Much attention is continuously given to its sanitation on account of the risks of outbreaks of infectious diseases which are incurred by the large number of pilgrims who periodically visit the place. The houses are well built and fairly well ventilated, and overcrowding is prevented by regulations and by frequent

* It would obviously be futile to institute a comparison between any two places in which the more important of these circumstances were widely dissimilar, as, for instance, between Calcutta and Bombay, or between Bangalore and Mysore City, or between Hong Kong and Canton.

inspections. While the town contained a population of from 4,000 to 5,000, a plague case was discovered on the 8th of April 1897, but there was "very conclusive evidence" that several cases had occurred prior to this date. After the 8th of April, isolation of patients, disinfection, and several partial evacuations were carried out, and the outbreak terminated, without complete evacuation having been required, on the 8th of June, or about two months after its commencement, during which time only 18 cases occurred, representing ·45 per cent. of a population of 4,000, or ·36 per cent. of a population of 5,000, and, therefore, scarcely deserving to be termed an outbreak. 9,024. 9003. 8908; 8912; 9025; 9028. 9029–38. 9041. 9058.

To institute a comparison with another town it would be desirable that that town should be of about the same size, that the outbreak in it should occur at the same period of the year, viz., during the warm weather, and that the chief suppressive measures should be similar in kind to those adopted at Hardwar. These conditions were complied with in the town of Banga, which is also situated in the Punjab, and has a population of 4,727. The references to it in Captain James' Report and Evidence show that it also supplies the remaining requirement for a comparison with Hardwar, in so far that the sanitary condition is in much the same unsatisfactory state as that of nearly all the towns of India, and that the houses are insufficiently ventilated. The first case of plague occurred in this town on the 11th of March 1898, and the outbreak continued until the 20th of June; and during these three months there were 103 cases and 65 deaths, making a percentage of 2·2 attacked, with a case-mortality of 63·1 per cent. During this time, also, the patients and contacts were isolated, the houses were disinfected, and nine separate partial evacuations were carried out as soon as plague appeared in non-evacuated areas; and although extreme precautions were taken to isolate these areas, the extension of the disease was checked only when complete evacuation had been effected. Captain James' Report, p. 29. Captain James' Report, p. 30; 10,235; Vol. II., p. 100; 10,373. Captain James' Report, p. 34. Captain James' Report, p. 32.

In two towns, therefore, of nearly the same size and situated in the same part of India, in both of which outbreaks occurred in the warm weather, and were treated with much the same measures, excepting that they were carried out with exceptional precautions and to a greater extent in one of them, the outbreak was much less severe and shorter in duration in the town in which the sanitary conditions of the interior of the houses were exceptionally satisfactory than in the town in which they were of the unsatisfactory character prevalent in India, although in the former the suppressive measures were initiated later in the outbreak and were less completely carried out than in the latter.

The comparison fails in one point only in each town. In Banga prophylactic inoculations were commenced at the beginning of the outbreak, and 865 of the inhabitants of the town were inoculated; but Hardwar did not have this advantage, as all of its inhabitants remained uninoculated throughout the whole of the outbreak. In Hardwar, on the other hand, no plague-infected rats were seen, whereas in Banga they were present in small numbers during the outbreak. This circumstance, however, does not materially affect the comparison, as no diseased rats were found in Banga until several indigenous cases had already occurred, and until nearly three weeks after the discovery of the first plague patient. It is highly probable that in Hardwar the conditions unfavourable to the serious extension of plague among human beings also prevented its extension among rats. Captain James' Report, p. 133. 9063; 9210. 8913; 9448. Captain James' Report, pp. 30–34. 10,235. 1447.

43. An instance may, however, be given in which, notwithstanding careful searchings, so few dead rats were found that the possibility of the outbreak having been increased by them cannot reasonably be entertained. Rohri, a town in Sind, north of Hyderabad, received several imported cases in February, March, and April 1897; and, although some of them were not at once discovered and isolated, it is believed that no local case occurred until the 17th of April. The outbreak then extended, and it continued until the 30th of June. During this time, representing the hot season, the plague measures adopted were isolation of the sick and the contacts, and numerous partial evacuations of blocks of houses in affected areas, with their disinfection by exposure to sunlight and air and with perchloride of mercury. 151 cases in all occurred, or 129, if the imported cases are excluded, the latter being equivalent to 3 per cent. or 2·58 per cent., respectively, of the estimated population of from 4,000 to 5,000. Although the godowns and houses were carefully searched and everything done to discover infected rats, only six were found; and Mr. Mountford, I.C.S., Assistant Collector, was able to assure himself that there was "no mortality among the rats." The sanitary condition of the town was very unsatisfactory. There were many high houses in narrow streets, the conservancy was greatly neglected, and the houses were badly lighted and ventilated. Contrast between outbreaks of plague in Hardwar and Rohri. 12,026. 12,032; 12,041. 12,043–4. 12,057–8. 12,049.

The conditions as to the season when the outbreak occurred and as to the measures adopted and the absence of any influence produced on the extension of the disease by rats are, therefore, comparable with the conditions in Hardwar. The sanitary state of the buildings was, however, markedly different in the two towns; and in the town where the dwellings were badly ventilated and lighted, the incidence of plague was about six times greater than in the town where the state of the dwellings was relatively satisfactory.

44. While in the prevailing insanitation of the plague-infected towns and villages it is extremely difficult to obtain examples of towns or villages where the sanitation is in a sufficiently satisfactory state to institute comparisons such as have been made, the object of the comparisons may equally be attained by contrasting in the same town or village the incidence of plague in well-lighted or well-ventilated groups of houses with the incidence in houses which are inadequately ventilated and lighted. This has already been done, but for other purposes, in many of the preceding paragraphs, and especially in those in which attention has been drawn to the freedom from infection of the occupants of houses in the European quarters of plague-infected towns, and of the occupants of sufficiently ventilated and lighted chawls and other Native houses situated in the midst of plague-affected insanitary houses (paragraphs 10 and 15).

Results of geological and meteorological investigations support the view that conditions inside of houses are the dominating factors in the propagation of plague.

45. That the predominating factors in the extension and virulence of plague are to be found in the interior of the dwelling-houses is also indicated by the results of investigation into the relationship between the incidence of plague on the one hand, and the geological character of the soil and the conditions of climate on the other hand.

8311, 8315, 8327.

46. The former subject may be briefly considered. Professor Griesbach, Director of the Geological Survey of India, informed us that "so far there is no evidence that plague could be specially generated by any of the formations in India," for, although the soil of the affected districts is chiefly derived from trap and crystalline rocks and, to a less extent, consists of alluvium of unknown depth, the disease has spread from Bombay over such a fan-shaped area as it would naturally cover were human communication the main agency in its extension; while, looking outside of India, the broad statements may also be made that the plague "occurs practically in all soils," and "that the disease is primarily a soil disease is certainly borne out neither by experiment nor observation."

Lowson's Report, p. 9.

Relationship between climate and plague.

47. In regard to any relationship that may exist between variations of climate and variations of plague incidence, we have had before us the views of three authorities on the subject.

20,301.

Dr. Moos, the Director of the Government Observatory at Bombay, concluded from his investigations that the maximum mortality from plague in that city coincided with the periods when the ground evaporation was greatest.

Evidence, Vol. II., Appendix LVIII., pp. 437–9.

Dr. Buchan, F.R.S., dealing chiefly with Professor Moos's figures, points out that in Bombay there are two annual maxima in the plague mortality: one—and much the larger—which occurs during the coldest and driest weather, and especially when the cold is greatest at night, from January to March; and another which occurs during the wettest and still very warm weather, from August to October. "The larger maximum," he remarks, "is practically coincident with the coldest weather of the year, and such weather, particularly during the night, and, considered in connection with the habits, housing, and clothing of the population, may perhaps account for a part of the great mortality." He considers that the mortality curve does not coincide with the temperature of the air, humidity, temperature of the soil, rainfall, or any single meteorological element taken by itself, but that a curve of agreement may be obtained by calculating the difference between the temperature of the soil 5 feet below the surface and the temperature of the air over the soil, plague being most prevalent when the temperature of the ground-air is the highest above that of the super-natant air. Both Professor Moos and Dr. Buchan appear to consider that plague increases in direct ratio with the increase in the conditions favouring the exit of air and moisture from the ground, and therefore of the exit of any noxious substance contained in that air.

20,308.

Professor Hunter Stewart has dealt with the subject from the broader standpoint of the climatic conditions of all parts of India affected with plague. He considers that when the atmosphere has its minimal drying power the plague incidence is least, and

that as the drying power of the atmosphere increases the plague incidence also increases, until the soil has finally become so dry that bacterial growth is checked, and then the plague incidence greatly diminishes or altogether ceases. One of the most important statements in his Report, harmonizing with the views of Dr. Moos and Dr. Buchan, is that: "When the temperature of the soil at (say) 5 feet is higher than the temperature of the atmosphere, the gases in the soil, pressed on by the colder and heavier gases above, escape at places where there is least resistance." Such places, he points out, are found in the earth-floored houses of the natives, especially during the night and in the cold seasons, for then the outside air is coldest, and therefore heaviest, and it exerts a greater pressure on the air of the soil, while on the other hand the air in the houses is at its warmest and therefore lightest, and favours the rise of ground-air into them. As, however, the maximum velocity with which the ground-air can diffuse into the atmosphere is only slight, the detachment of micro-organisms from the pores of the soil is, in his opinion, impossible, and therefore any noxious substances which are thus carried into the atmosphere must be volatile, and would act only as predisposing causes of plague.

"Rainfall *per se* seems to have little, if any, effect on the epidemic *appearance* of plague." On the other hand, the severity of an outbreak is increased by the occurrence of heavy rains, as was illustrated in the Karad villages in 1897, in Poona in 1897 and 1899, and in Belgaum in 1898 and 1899. He thinks, however, that "this effect of rainfall is probably to be explained as resulting from the impossibility of securing efficient evacuation of houses during heavy rains. There is either no evacuation at all, or all openings for ventilation in the huts at the evacuation camps are closed up on account of the rains. The bad hygienic conditions which, from the evidence, seem to be a most important factor in the epidemic occurrence of plague, are thus continued in the camps."

The following conclusions have been arrived at by Professor Hunter Stewart:—

"(1.) That in the areas affected by plague the temperature conditions are suitable for the epidemic appearance of plague at all seasons.

(2.) That the amount of moisture in the atmosphere has little if any influence.

(3.) That the rainfall seems to exert its effects chiefly or entirely by interfering with remedial measures."

48. It would appear, therefore, that geological formation has no obvious influence upon plague extension, and that the variations in climatic conditions encountered in India have only a slight effect; but, in so far as that effect is operative, its influence is prejudicial in the interior and not the exterior of dwellings.

Cyclical character of successive local outbreaks not explainable by temperature or rainfall.

49. During the four years that the present epidemic has continued in India, one of the most striking facts which has distinguished its history is the cyclical character of its annual increase and decrease. As these cyclical variations are unexplainable by lowness of temperature or severity of rainfall, corroboration is given to the view that they are usually dependent upon the increase of the insanitary state of the interior of dwellings resulting from their being occupied by larger numbers of inhabitants, and from the greater prevention of ventilation by the closing of external openings, during the cold and wet seasons than at other times of the year.

As already indicated in paragraph 47 of this Memorandum, and in paragraphs 327, 332, 334, 335 and 375 of our Report, in Bombay and elsewhere in India plague has displayed a growth and decline which have recurred with singular regularity during each year of the epidemic; the maximum mortality having been attained in the cold weather, and a second but much smaller maximum in the rainy season. If these maxima, representing cyclical recrudescences, are caused by the conditions produced within dwellings by their being occupied by larger numbers of inhabitants during cold and wet weather than at other times, it is almost superfluous to point out that each recrudescence of plague mortality would be gradually and not abruptly produced, and would not, indeed, be conspicuously manifest until several weeks had elapsed since the advent of the cold and of the wet seasons, respectively; and this feature has been clearly displayed in the outbreaks in Bombay and in other places. (*See* Charts, Evidence, Vol. III., p. 439, and our Report, paragraph 322).

It is, however, well known that there have been many exceptions to this annual cyclical growth and decline and to the occurrence of the chief outbreaks in the cold and the wet seasons. Plague has, for instance, been most prevalent in several villages in the Satara District during heavy rains; in the towns and villages of Sind and Cutch it has been most severe during hot and dry weather, and in the Cutch Territory it has also displayed a further irregularity by devastating the town of Mundra in the cold

22,737.
18,143-4; 18,441-3; 22,423, 27,146.
13,485; 27,028; Vol. II., Appendix XLVII., pp. 476-7.
6166.

weather; in successive outbreaks at Poona and at Karachi, it has attained its maximum development in one year in hot and dry, in another in hot and moist, and in a third in cold and dry weather; while in the mountainous elevations of Kumaun and Garhwal, it has spread in the villages and hamlets chiefly during the ice and snow of winter.

50. It is obvious, therefore, that the occurrence and extension of plague are independent of climatic conditions. Nevertheless, the plague bacillus thrives best at a temperature of about 37° C. (98·6° F.), and it requires moisture for its existence and growth. It is killed in from five to ten minutes by exposure to a heat of 62° or 63° C. (143°—145° F.), and in from one to two hours by exposure to 50° C. (122° F.); and even the highest of these temperatures is considerably below that of the sun's rays in many parts of India. Its growth is greatly retarded when, even in a moist state, it is exposed to sunlight; and, as it perishes in less than four hours when thus exposed, the duration of its life must be much briefer in the open air, under the aided influence of desiccation and of free oxidation in intense light. It is also devitalised, so that both its pathogenic power and its vegetative activity become much reduced, by exposure to cold, and it would be brought under the influence of very low temperatures during the rigorous winters of Kumaun and Garhwal.

26,342.
27,377–9; Centralblatt für Bakteriologie, 1898.
26,342.
Die Einwirkung der Winterkälte auf die Pest- und Diphterie-Bacillen; Centralblatt für Bakteriologie, 1899, p. 182.

51. In those parts of India, therefore, in which plague has been prevalent, the plague bacillus, except in narrow lanes and overshadowed courtyards such as are numerous in Calcutta, would be unable to maintain its existence for more than a brief period on the surface or at a short distance below the surface of the ground in the open air, and could do so only where it is protected from the destructive influences of climate. This protection can be obtained in the interior of dwellings, and there also the warmth requisite for the active vitality of the bacillus is secured when the temperature is either too high or too low outside of the dwellings, and the requisite moisture is provided when in the outside air the bacillus would be killed by desiccation. Excess of moisture, such as that of the ground in the rainy season, is likewise destructive to the bacillus; nevertheless outbreaks which had become dormant in the preceding hot weather acquire renewed activity during rains, and, as we find from the evidence, the people who then remain shut up in the houses during the day and night "with the windows and doors closed" suffer from plague, while the renewed outbreak does not involve those who remain much outside of the houses in order to follow in the open air such occupations as rice cultivation, hawking, and begging.

22,127.
22,184.
22,129.

Further, as has elsewhere been stated in this Memorandum, the influence of climatic changes has occasionally interfered with the almost unvarying success of complete evacuation, because of the indirect effect of climatic changes in causing people to leave the huts in the camps in order to shelter themselves in the evacuated houses; as in the declining outbreak at Nasik during the cold months of January and February, and in the town of Salaya, when the same interference was caused by a gale of wind. The influence of climatic changes has likewise increased the number of plague cases in a non-evacuated population, as when the temporary occurrence of cold northerly and easterly winds during the hot weather had prevented many people in Bombay from sleeping out-of-doors as they usually do at that time.

14,733–5.
13,431.
26,103–4.

Growth and decline of local outbreaks prove that spread and virulence of plague chiefly depend on insanitary conditions within dwellings.

52. There appears to be no satisfactory explanation of these regular and exceptional incidents in the history of the plague epidemic other than that the predominating factor in the extension of the disease is to be found in the conditions which exist within and not outside of the houses of those who have been the chief sufferers. Plague proves itself to be independent of climate, and independent because the temperature and moisture requisite for its active existence are provided within the houses of each place where it has established itself, and where also, as will afterwards be explained, the polluted atmosphere of the dwellings probably supplies further elements for its extension and increase in virulence. All the definitely ascertained facts appear to be in accordance with this explanation, and it is highly probable that were fuller information obtained regarding the exceptional cases of seasonal incidence they would be found entirely to accord with it, and to render unnecessary any theory of epidemicity (such as has been suggested in paragraph 384 of the Report), which is independent of temporary aggravations of air-pollution or of the accident of the time at which infection happens to have been introduced into, or to have been set free in, a place.

Influence of filth. Para. 6, *ante*.

53. The value of the assertion that plague is increased by general insanitary conditions has already incidentally been considered, and the conclusions have been arrived at

that the influence of these conditions is a secondary one, and that plague cannot justly be regarded as a filth disease in the ordinary acceptation of the term. Filth even in this acceptance cannot, however, be altogether eliminated as a causative condition. In some form of it, it has been present wherever outbreaks of plague have occurred. The opinion of the few witnesses who have expressed themselves with some hesitation on this subject must be received with reservation, as, for instance, when we are informed regarding the generally wealthy Jains of Bombay, among whom the plague incidence and mortality were high, that the sleeping and cooking places in their houses are not very dirty and "look comparatively clean," while it is added that the Jains "will not sweep their staircases or sweep their sleeping rooms out, or their cooking room very often." Although it is sometimes found that the interior of native houses, even when constructed of the poorest materials, is in appearance clean and obviously suggestive of attention and care in preventing accumulations of filth, true cleanliness can never be obtained so long as the floor and frequently also the walls are saturated with moist and decomposing cow-dung, and so long as cattle are stalled in the dwelling-houses, and, as was observed during visits to several towns and villages, in central parts of the houses into which sleeping chambers had their only openings for ventilation, and in which the inmates of the houses even lived and fed and slept. Mere superficial cleanliness is, however, absent in the great majority of the houses, both in towns and villages; for, as it has been shown, their inside condition as well as their immediate outside surroundings are usually unsatisfactory, in an extreme and often an appalling degree.

5903. 17,681–2. Snow's Report, p. 83; 7841; 15,182; 24,194.

54. While there is no direct evidence that plague bacilli continue to exist or to proliferate for long periods of time in filth, there is much indirect evidence in support of their being able to do so, and it is to be borne in mind that experimental investigation, in so far as it has advanced, has not proved that this does not occur. The experiments with earth and cow-dung made by Hankin, Gibson, Marsh, and others have no doubt failed to discover a prolonged existence of the plague bacillus in these substances when non-sterilised; but the failure appears to be mainly due to the absence of any known process for their identification by culture or microscopic examination, caused chiefly by difficulties in the separation of plague bacilli from the numerous contaminating saprophytic micro-organisms always present in earth and cow-dung. If these organisms exert an injurious action upon the vitality of the plague bacillus, they would do so by the poisonous action of the toxins they produce, or by strangulation of the plague bacilli, or by exhaustion of the food on which they subsist. Captain Spencer, R.A.M.C., however, has shown that the fluids in which a large number of the organisms found in earth and cow-dung had been separately grown, and which, therefore, may be assumed to contain the soluble toxins produced by them, failed to cause any obvious deleterious influence upon the vitality of the plague bacillus. Further, in the large mass of matter constituting the earth or the cow-dung or other organic filth in which plague bacilli would be found in nature, it is improbable that their existence could soon be terminated by strangulation from an excessive growth of co-existing micro-organisms, or by starvation resulting from exhaustion of nutritive material by other more actively vegetating micro-organisms.

8509; 8511; 18,867; 18,880. Campbell's Report, pp. 211, 619–13. 18,867–8. 211,619–633. 8509–14; 8522, Vol. II., p. 10; 18,867–8; 26,398. 26,150.

55. While, however, the vitality of the plague bacillus is certainly retained for considerable periods of time in earth and cow-dung and other filth, it is equally apparent that its vitality is lessened and even destroyed by free exposure to air and light. The evidence brought before us indicates that in many places those persons who by their occupation are brought into the closest association with night-soil and other filth have not been much affected by plague; but it is not to be overlooked that this association with excremental matter occurs for the most part in the open air, and, also, that the occupation of these people requires them to live much out-of-doors. Further, excrement, even when derived from plague patients, cannot frequently contain plague bacilli, and never in more than minute quantities; as plague bacilli are not present in the excremental discharges during the whole period of illness, but only a few hours before death, and even then in but small numbers.

Vol. I., Appendix XIV., p. 378; 22,266–9; R. N. Jadhav's Report, p. 15. 24,054–7.

A portion of Lieut.-Colonel Weir's evidence on this subject is highly significant; for he stated that while the scavengers and sweepers who lived in well-ventilated and lighted municipal chawls suffered to only an insignificant extent from plague, even although the chawls were situated in the midst of plague-affected districts, many cases, on the other hand, occurred among the scavengers and sweepers who lived in the ordinary native houses.

Snow's Report, p. 174, para. 621.

Irrespective, however, of the protection afforded by air and light in open places, it is improbable, for reasons that have been discussed, that the plague bacillus could retain its vitality for longer than brief periods of time on the open ground outside of houses, whether in an ordinary or in a sewage or excrement polluted soil.

23,191–6.

56. On the other hand, filth inside of or in close proximity to dwellings has a detrimental influence by increasing the vitiation of the atmosphere of the dwellings, and, possibly also, when it is present within houses, by supplying nutriment for the growth of the plague bacillus. It thus adds to the conditions within the dwellings that are favourable to the vitality and virulence of the bacillus and are unfavourable to the health or powers of resistance against infection of the inmates of the dwellings.

57. While, however, filth, either within or in the neighbourhood of dwellings, increases the spread of plague, the conditions that otherwise exist in the great majority of native houses are so powerfully favourable to the propagation of the disease, that it may well happen, as several witnesses have pointed out, that plague has occurred and on rare occasions, even spread to a slight extent in houses which are clean, in so far as obvious and superficial uncleanliness is concerned. It is apparent, also, that the absence of filth and of the other conditions referred to cannot absolutely prevent the occurrence of an infectious disease, however much their absence may diminish the occurrence and, above all, lessen the extension and virulence.

Overcrowding chiefly operative by increasing the vitiation of the atmosphere within houses.

58. As we have seen, overcrowding is stated to be one of the most frequent of the causes of vitiation of air in the dwellings of natives of India. A common, but not a necessary, incident of overcrowding, if it occur in proximity to infective persons or materials, is that it increases the number of persons subjected to the risks of infection. It may, therefore, be supposed that the undoubtedly predominating influence exerted on the propagation of plague by conditions so frequently existing within dwellings is capable of sufficient explanation from the circumstance that the plague virus is given the opportunity of infecting a larger number of persons in an overcrowded place than in one which is not overcrowded.

59. This resolves itself into the propositions that the extension of plague in a place which contains infective material is mainly caused by the opportunities for infection afforded by there being a large as contrasted with a small number of persons in the place, and that the extension is caused only to a slight extent, if at all, by the vitiation of the atmosphere and other conditions which result from overcrowding. When these propositions are examined, they are found to have no foundation in fact.

Almost no infection when exposure is in open air.

It has over and over again happened that people brought into the closest proximity to plague infection in the open air have so seldom been affected, that the circumstance has attracted general interest. In Bombay and other plague-infected cities and towns, a large proportion of the inhabitants have, in accordance with a prevailing custom, slept in the streets and courtyards, often in such numbers that thoroughfares are actually obstructed by rows of human beings lying close to each other, and the presence of plague-infective matter has been demonstrated by the occasional discovery of a dead body in the early morning; but so few cases have occurred among them, that the decadence and cessation of outbreaks in the warm weather has very commonly been explained as a result of this custom.

Jadhav's Report, p. 15.

Snow's Report, p. 107.

Many European and other official and volunteer plague workers have, day after day, exposed themselves to infection in superintending and taking an actual part in the removal of plague patients and dead bodies, and in attending plague patients in hospitals and private houses; and the incidence of plague among them has been so small as to be almost incalculable.

Almost no infection when exposure is in well-ventilated places.

Even in enclosed spaces, people have freely associated with plague patients, and have been in proximity with infective material, without succumbing to the disease, provided the enclosed spaces were not inadequately ventilated. A large number of illustrations of this have been given in the preceding pages. Reference may be made to the experience of physicians, nurses and attendants in hospitals; to the free association with impunity between plague patients and their relatives and friends in well-ventilated municipal chawls in Bombay; to the remarkable freedom from plague on board ship, even when a large number of persons have been confined in a limited space, containing the most infectious form of plague; and to the instances that have been recorded of policemen, sweepers, and tramway men in Bombay, who, while following their several occupations during plague outbreaks, were necessarily brought into daily

Snow's Report, p. 146, para. 565.

contact with plague infection, but suffered little from the disease so long as they lived, during the night, in houses where the atmosphere was not polluted by the accumulated exhalations from human beings and from decaying organic matter.

Infection promptly produced in badly ventilated places.

60. If, however, plague-infective material be allowed to operate in confined spaces containing an atmosphere rendered impure by defective ventilation, experience has shown that the infective matter then actively asserts its disease-producing power. This has abundantly been illustrated in the prevalence of plague in the usual unventilated houses of natives; in the failure of health camps to suppress the disease when the huts were "hermetically closed"; and in the disappearance of freedom from infection in dwellings that are insufficiently ventilated, among the members of the same bodies of policemen, sweepers and tramway men, as escape infection when occupying houses that are adequately ventilated. 11,971.

Mere number of people of much less importance than effects of bad ventilation.

61. It is definitely indicated by these and many other incidents brought under our attention, which have already been described, that the number of people contained in an enclosed space does not so much govern the infectivity of plague as the vitiation and other changes of the atmosphere, which may indeed be grossly produced by only two or three persons, although in any given enclosed space they are, no doubt, produced in a greater degree by a larger than by a smaller number of persons. It has not even been found that increased opportunities of infection are necessarily followed by an increased number of infections. An European living in Malabar Hill is no doubt exposed to fewer risks of infection than a Bhatia living in Mandvi, but so also are the occupants of a chawl in Kamathipura which contains 2,000 inhabitants than the dwellers in a suburban hut, accommodating only six or eight persons, and yet the incidence of plague has been greater in the latter than in the former. As we have already seen, in each of these cases, the larger incidence has been accompanied with the greater degree of pollution of the interior of the dwellings, resulting from inadequate ventilation. 17,691.

Overcrowding not co-relative with number of persons.

62. Overcrowding, however, is a relative term, and is not co-relative with any specific number, but with the number present in a given space and the efficiency of the ventilation of that space. In enclosed spaces of equal capacity, it may be produced by five persons, and may fail to be produced by twenty persons. A single individual in a small enclosed space, without sufficient means for allowing entrance and exit of air, would overcrowd that space, although there is no crowding together of people in it, and this single individual would, by vitiating the air, produce conditions specially favourable for the proliferation and virulence of any plague-infective matter that is present. This has frequently been illustrated in the history of evacuation, which has shown that the daily visits of people to recently evacuated and well-ventilated and infected places, such as to the open booths of bazars and even the opened-up living rooms of dwelling-houses, have not been followed by infection; whereas the surreptitious sleeping of a single individual in one of these booths or rooms, implying, in the existence of penalties for doing so, the careful closing of all external openings, has almost without exception been followed by infection.

Conclusion respecting overcrowding.

63. It is therefore apparent that the number of persons in an enclosed space, such as a dwelling-house or room, is mainly operative by producing conditions favourable to the proliferation and enhanced virulence of plague, and, according to the number of persons, of materially or immaterially adding to the detrimental conditions produced by defective ventilation; while the actual number of the occupants of the enclosed space has only the secondary importance of bringing a larger or a smaller number of persons within the range of infection.

Criticisms of statements relied upon to disprove above conclusion.

64. As this constitutes one of the fundamental conclusions relating to the influence of sanitary conditions with which, I regret, my Colleagues are not in agreement with me, the following observations may be made upon the statements relied upon by them. While admitting as a possibility that pollution of the atmosphere may have a fostering influence upon the spread of plague, they appear to consider that the conclusion at which I have arrived is inconsistent with the circumstance "that plague may occur in houses whose atmosphere is quite untainted" (Chapter III., paragraph 350). This circumstance, of course, is highly probable, for plague is an infectious disease. It is not, however, obvious that it possesses any value when used as an argument against the definite proposition that a polluted atmosphere within occupied dwellings has been in India the main cause of the spread and virulence of plague.

The only places cited by my Colleagues in which plague is believed by them to have occurred "in houses whose atmosphere is quite untainted" and "in well-ventilated and airy houses" are Hardwar, Karachi and Ahmednagar, and also "the lofty and, from every sanitary point of view, unimpeachable buildings of the Byculla Jail in Bombay." Descriptions of the condition of sanitation in the houses and buildings in each of these places have already been given in this Memorandum (paragraphs 2, 3, 7, 11, 42).

Hardwar is one of the few places whose sanitary condition is fairly satisfactory. It has already been dealt with (paragraphs 42, 43, *ante*) in order to show that plague does not extend in a town where sanitation has received careful and successful attention. The outbreak was an altogether unimportant one, and it contrasts most favourably with outbreaks in towns of similar size but with highly unsatisfactory sanitary conditions within the dwellings, such as Banga and Rohri, although the measures for suppressing the outbreaks in them were of the same kind as the measures employed at Hardwar.

On consulting the reference given as the authority for the above statements respecting Karachi (our Proceedings, 11,095), the only fact revealed is that the probable first case, that of a Brahman cook, occurred in a house "in quite a good street—a broad street"; but, unfortunately, these qualities are not inconsistent with the existence of the gravest air-pollution within Native dwellings. Mr. Giles, 11,096. who gives the information, immediately adds, "it was a curious thing if the first local case occurred here; local cases generally occur in the worst places." There is, however, nothing in the evidence to show that the house was "well-ventilated and airy," or that its atmosphere was "quite untainted." Both Mr. Giles and the Health Officer, Dr. Kaka, were of opinion that the disease had been imported from Bombay. Before this patient, who was suspected to have introduced the disease, had been discovered, he had been surreptitiously removed from Rampart Road, where he first became ill, into the City (11,846); and while, apparently, no cases occurred in Rampart Road, the broad and good street referred to, the disease gradually "laid a 11,848. firm hold of the City, raging in the form of an epidemic." Dr. Kaka also stated that in Karachi, plague chiefly occurred "in low, damp, dirty, and ill-ventilated places."

My Colleagues refer to No. 24,228 of our Proceedings as the basis of their statement regarding Ahmednagar; but it is not there affirmed that any plague case or cases occurred in "well-ventilated and airy houses," or "in houses whose atmosphere is quite untainted." On the other hand, it is rendered apparent from other parts of the 24,275. Evidence that, during the first outbreak, to which the reference relates, plague cases chiefly occurred in mud houses or huts where "there are very few openings for ventilation . . . if there are any they are very small." There was, however, a second small outbreak of plague in Ahmednagar, during which two of the earliest 24,276–7. cases occurred in "the Sisters of the S.P.G. Mission," after a good many rats had died in it; but there is nowhere in the Evidence any description of the condition of ventilation or any description of the relationship between the size of the living quarters of the Mission and the number of persons occupying them. The Chairman of the Municipality, whose authority has been appealed to in the above reference, 24,228. definitely mentions "want of ventilation" as one of the conditions most conducive to the spread of the infection. The houses are "so constructed that they have very 24,311. little ventilation, and even that is closed during the night. There are certain houses which even require a light in the daytime."

In regard to the occurrence of a considerable number of cases in the Byculla House of Correction, it has already been pointed out that overcrowding existed at the time of the outbreak (this Memorandum, paragraph 11). Nothwithstanding the great and predominating influence of a polluted atmosphere in fostering the extension and virulence of plague, it undoubtedly does not follow that when infection happens to be introduced into a crowded building, several or many of the inmates will not be affected if sufficient opportunity for personal infection has been given, even although no gross insanitary conditions are present.

The above examples, which have been cited in support of the statement that "plague may occur in houses whose atmosphere is quite untainted," therefore, not only fail in the purpose for which they have been cited, but actually constitute illustrations of the fact that, after plague has been introduced into a place, its extension and virulence are chiefly fostered by the pollution of the atmosphere and other conditions which result from the inadequacy of ventilation and sunlight, and from the uncleanness within and near dwellings, which characterise the great majority of Native houses.

V.—The Conditions within Dwellings which increase the Extension and Virulence of Plague, and the Manner in which they do so.

65. If, in addition to the indications which have thus been supplied by experience, it should be rendered probable by experimental investigation, that the insufficient ventilation of a confined space containing infective matter and persons liable to infection is in itself productive of conditions conducive to the proliferation and enhanced virulence of the infective material, and to an increase in the susceptibility of individuals within the range of its action, further support would be given to the conclusion that the conditions produced by insufficient ventilation exercise a predominating influence. The experiments bearing on the subject may conveniently be next considered; but whatever value they may possess, the various facts and incidents which have already been brought forward point very clearly to plague extension, if not also plague virulence, being greatly dependent on conditions existing within dwellings, and, indeed, they appear sufficient in themselves to afford a complete demonstration of this dependence. In view of these considerations and of the circumstance that, unless in a badly ventilated chamber, infective matter, even when originating from the pneumonic type of plague, has not been found to be powerful in propagating the disease, the fostering conditions inside of houses assume the position of the highest importance. What they are and how they operate may now further be considered.

Experience has proved that insanitary conditions within houses are the chief causes of the spread and virulence of plague

How far corroborated by experimental investigation.

26,715–725.

66. As is apparent from the foregoing, a singular unanimity has pervaded the evidence in indicating that the extension and probably the virulence of plague are chiefly increased by deficiency of light and of ventilation and the consequent pollution of air and other changes produced by them. Operating less generally, are also the conditions which originate from the special nature of the floors in a large number of the houses in which plague has occurred.

Conditions within houses that increase the infectivity and virulence of plague.

The effects produced by each of these conditions may conveniently be considered in regard, in the first place, to their effects on the plague micro-organism, and, in the second place, to their effects on the individuals subjected to their influence.

67. The action of direct sunlight on the plague micro-organism has frequently been examined, and, as we have elsewhere stated, (our Report, para. 658), it has been found that the organism is speedily devitalised by exposure to it. No serious attention has, however, been given to the action upon these organisms of obscured or indirect sunlight, for little more has been determined than that such light does not destroy this vitality under many days' exposure; and no accurate or detailed examination has been made of the effects of darkness upon them.

Effects on the plague micro-organism of deficiency of light.

These subjects have, however, been experimentally examined in regard to several other micro-organisms. It has been shown, for instance, by Dr. Downes and Mr. Blunt, that the retention of the vitality of bacteria is in accordance with the degree of obscurity of the light to which they are subjected, and this conclusion has been confirmed by M. Duclaux and by Professor Tyndall. The experiments of M. Duclaux have likewise shown that when they are kept in the dark, certain micro-organisms may retain their vitality for several years. As, also, it is in accordance with general observation that the vitality of micro-organisms is retained more certainly and for longer periods in darkness than in light, it cannot be doubted that darkness and, to a less degree, deficiency of light are conducive to the prolonged existence of the plague bacillus.

Proceedings of the Royal Society, Vol. XXVI., 1877, and Vol. XL., 1886.

Annales de Chemie et de Physique, 6me Ser., t.v., Mai, 1885.

68. The most readily appreciated elements constituting air pollution in dwellings that are insufficiently ventilated are diminution in the amount of oxygen, with increase in the amount of the carbonic acid, water and nitrogenous compounds derived from the lungs, and of water and nitrogenous compounds derived from the skin exhalations of the inhabitants of the house. Notwithstanding the many directions in which the bacteriological knowledge of plague is still imperfect, it is fortunate that sufficient work has already been done to indicate, and, indeed, to establish on the part of some of them, the manner in which these elements exercise a deleterious influence.

Effects on the plague micro-organism of deficiency of ventilation.

69. In common with all other disease-producing microbes, that of plague largely depends for the continuance of its existence upon moisture and warmth. In an occupied and insufficiently ventilated room, the former is abundantly supplied by the exhalations from the lungs and skin of the inmates, and the latter by the heat radiated from their bodies. Thus it happens that plague flourishes in India in all

The required moisture and warmth provided in unventilated rooms.

6166. conditions of climate, in the snowy altitudes of Kumaun and Garwhal as well as in the tropic heat of the plains, in the moist atmosphere of Bombay as well as in the dry and arid sands of Karachi. It is independent of outside and general climatic conditions, because it finds those elements of climate which are conducive to, or even necessary for its vitality and virulence, within the houses of the people in all places and seasons.

Deficiency in amount of oxygen is favourable to the vitality of the plague bacillus. 8552 (Evidence, Vol. II., pp. 8–10).

70. The conditions produced within the houses do not, however, merely maintain the existence and virulence of the bacillus. The defective supply of fresh air leads otherwise to important modifications in the composition of the air confined within the rooms. In the first place, the normal amount of oxygen is reduced, and the experiments of Mr. Hankin have a suggestive bearing upon this reduction. He found that oxidising agents rapidly killed the bacillus, and also that when the amount of oxygen in the atmosphere is reduced by an inert gas such as hydrogen, the bacilli resist the destructive action of desiccation for a much longer period of time than when desiccation is accomplished in an ordinary atmosphere. Further, in Mr. Marsh's experiments, plague bacilli were grown in air diluted with the inert gas, nitrogen, and therefore containing less than the normal amount of oxygen, with the result that the growth and reproduction of the bacilli were increased. Apparently, therefore, the vitality of the plague bacillus is enhanced when the amount of oxygen in contact with it is reduced within certain limits.

18,893.

Excess of carbon dioxide increases the proliferation and virulence of the bacilli.

71. The effect of a mere reduction in the amount of oxygen in the air, however, would appear to be surpassed when the reduction is caused by the presence of an abnormally large quantity of carbonic acid. During respiration, this gas is abundantly evolved into the expired air. About 17 cubic feet are given off by each adult in 24 hours, and therefore, about 5·4 cubic feet in the eight hours during which a Native sleeping chamber may be assumed to be closed to ventilation. While sufficient ventilation is maintained, an accumulation representing more than ·06 or ·08 per cent. is prevented; but the percentage of carbonic acid in the air of a sufficiently ventilated chamber must necessarily be much increased whenever both the exit and entrance of the air are prevented or are greatly reduced. Provided that by some special arrangement pure air were continuously supplied to the lungs, one person would in eight hours evolve sufficient carbonic acid to raise the amount of it to ·6 per cent., and four persons to 2·4 per cent., in a chamber $10 \times 10 \times 8$ft. and, therefore, of the capacity of 800 cubic feet; and these percentages would be much increased were the chamber to contain a lighted oil lamp or candle. Further, the defective ventilation would lead to an accumulation of moisture and an elevation of temperature favourable to the evolution of carbonic acid from the organic matter abundantly present in the rooms of the great majority of native houses; and, as has been pointed out by the meteorologists, any carbonic acid thus generated in the earth of the floor would, by the pressure of the external atmosphere, be diffused into the occupied room along with the ordinary carbonic acid of the ground air, especially during the night and in cold weather.

18,893 In a well devised and carefully conducted series of experiments, Mr. Marsh, M.B., found that an atmosphere containing 3 per cent. of carbonic acid and 97 per. cent. of ordinary air at 80° to 88° F. (27° to 31·5° C.) enchances the growth and the multiplication of the plague bacillus. He further found that the bacilli thus treated retain their vitality for exceptionally long periods of time, and, above all, that their virulence actually becomes increased.

These effects were also produced, within temperatures ranging between at least 80° and 92° F., (27° to 33·5° C.), by all mixtures of air containing from 3 to 15 per cent. of carbonic acid. The effects of mixtures containing less than 3 per cent. were apparently not tested, but it may confidently be assumed that smaller percentages which are within the maxima generated in unventilated and overcrowded native houses would produce effects of the same kind. The experiments on this subject made by Captain Douglas, I.M.S., and also by Captain Spencer, R.A.M.C., but with unknown quantities of carbonic acid, have, so far as they have gone, confirmed Mr. Marsh's results.

26,233; 26,145.

The vitiation of the atmosphere which is produced when the ventilation of a chamber occupied by human beings is inadequate has apparently, therefore, the direct effect not only of stimulating the reproduction of plague bacilli, but also of enhancing the virulence of the bacilli whose number has thus been increased.

Earth and cow-dung floors increase the vitality and virulence of plague bacilli.

72. On account of the peculiar characters of the floors of the houses in which plague has most frequently occurred in India, several investigations have been made for the purpose of determining if plague micro-organisms are able to maintain a saprophytic

existence and, at the same time, retain their virulence in the substances constituting these floors.

The abundance of organic matter and moisture which the floors contain and the frequent alkalinity of their reaction, are conducive to the vitality and proliferation of all micro-organisms, and it was therefore to be expected that investigation would prove, as it has done, that plague bacilli are able to live for considerable periods of time in earth and in cow-dung and in mixtures of earth and cow-dung, constituting the materials of the floors. The further facts, however, have been determined, that when cultures of plague bacilli in agar were buried nine to twelve inches deep in the floors of unoccupied native huts or in the soil of gardens, the growth of the bacilli was notably exaggerated, and after 23 and 28 days' burial, in place of presenting the usual ground-glass appearance, the cultures had the appearance of "a thick opaque growth," and also that the virulence of the bacilli, even after burial for 28 days, was decidedly augmented when compared with that of bacilli grown in ordinary conditions in a laboratory. It is not improbable that these results are mainly caused, as Mr. Marsh has suggested, by the carbonic acid, which is so largely present in the ground air.

20,217–8.

18,874.

18,893.

Additional evidence has thus been obtained that the conditions existing in the interior of the greatest number of native dwellings are eminently conducive to the extension and increase in virulence of plague.

73. We may next consider the effect, in so far as the incidence of plague is concerned, of the insufficiency of light and ventilation upon the persons subjected to their influences. On these subjects there is no direct evidence. It would, indeed, be extremely difficult to obtain, as the conditions produced by deficient light and ventilation upon the plague bacillus are themselves so powerfully operative in enhancing the infectivity of plague, that their influence upon the susceptibility of the individual scarcely admits of being separately determined.

Insufficiently lighted and ventilated dwellings so affect individuals as to increase their susceptibility to plague.

74. This is especially the case with regard to the influence of insufficient sunlight. The necessity of sunlight for proper growth and nutrition is a physiological axiom. Its absence or deficiency is universally recognised as being detrimental to health, and conspicuously so by disordering the nutritive functions and thereby deteriorating the blood and impairing the strength. Further, the absence or deficiency would increase the air-pollution of insufficiently ventilated chambers containing organic matter. It appears, however, to be only on exceptional occasions, as in very cold or in wet weather, that native houses are occupied for long periods during the day, and as the doors and all other external openings are closed on these occasions, the detrimental influence of insufficient ventilation is introduced, and almost hopelessly complicates the problem of the determination of the effects upon individuals of the inadequacy of sunlight. Considering, further, that, in ordinary conditions and circumstances, the majority of the people of India live much out-of-doors during the day, when only any effect favourable to the extension and virulence of plague could be produced on them by the insufficient admission of sunlight into their dwellings, the influence of this insufficiency is probably exerted more upon the bacillus of plague than upon human beings. As has been shown, this influence, however, is of much importance.

Effects of insufficient sunlight on persons exposed to infection.

75. On the other hand, the deleterious effect upon individuals of the conditions produced by deficient ventilation can more readily be appreciated and are of a more definite character.

Effects of deficient ventilation on persons exposed to infection.

Experience teaches that prolonged subjection to an impure atmosphere is one of "the most potent and widespread of all the predisposing causes of disease." It operates, not only because the impure atmosphere is defective in oxygen and rich in carbonic acid, but also because it contains volatile poisonous substances. It thus reduces the health and vitality of individuals, and it may therefore be inferred that it reduces also their resistance to hurtful influences, such as the disease-producing influence of the plague bacillus.

Wilson's "Handbook of Hygenic and Sanitary Science." 8th Edition, p. 91.

If, in accordance with general opinion, there be any susceptibility engendered by a deteriorated physical condition, it is contrary to this opinion that plague so largely infects persons in the prime of life—the highest incidence being between 15 and 30 years of age—unless it be admitted that strength and vitality are impaired by the debilitating effects of the impure atmosphere, which is especially produced and in operation in Native houses during the night. In connexion with this it is of interest to note that, in his valuable report on the Gola-rog, or Mahamari plague of Kumaun and Garhwal, Surgeon-General Planck states that "the extraordinary closeness" of the apartments in which cattle are penned at night in the same dwelling-houses as the

Evidence, Vol. II. Appendix XXV., p. 36.

Natives, "has an undoubtedly hurtful effect on the cattle . . . apparent enough to any casual observer who may see the cattle as they issue from these pens in the morning." It has already been stated (para. 3 of this Memorandum), that the basement compartment of the dwellings in which the cattle are stalled, throughout the day and night in cold weather and during the night in warm weather, is in direct atmospheric communication with the upper chamber in which the people live and sleep.

See also Chapter III., para. 350 of the Report.

Further, the facts that have been mentioned above regarding the action of carbonic acid upon the vitality, and not less upon the virulence of the plague bacillus, suggest that when in a confined and impure atmosphere, bacilli, which in a pure atmosphere would be insufficient in quantity or in virulence to produce the disease, are deposited in a skin tissue or on a mucous surface, such as that of the respiratory tract, the exalting action of the excess of carbonic acid present in the insufficiently ventilated space, aided by the elevated temperature of the insufficiently ventilated space, would so stimulate the proliferation and increase the virulence of the bacilli as to render them capable of producing a serious or fatal attack of plague.

It may also be inferred that the undue accumulation of carbonic acid in the blood and tissues, caused by the breathing of an impure atmosphere which contains it in excess, would augment the proliferation and enhance the noxious properties of any plague bacilli which may already have been introduced into the body, and would thus render the inmates of inadequately ventilated houses and rooms more susceptible than they would otherwise be to the development of plague.

Annales de l'Institut Pasteur, 25 Mai, 1899, pp. 391, 392.

These suppositions are strongly supported by the experiments of Dr. Batzaroff. Experimenting with plague bacilli, which had become so definitely attenuated by agencies hurtful to them ("milieu où il séjourne, l'oxygène de l'air, la lumière, &c."), that large quantities inoculated into the skin caused only slight and unimportant symptoms in mice and rats, and altogether failed to cause any symptoms in guinea-pigs, he found that these bacilli, when placed on the nasal mucous membrane of the latter animals, regained their virulence and produced fatal attacks of plague. When a series of guinea-pigs was thus treated, the bacilli rapidly became exalted in virulence and soon acquired an activity sufficient to kill all animals inoculated with only minute quantities. No satisfactory explanation has hitherto been advanced of these remarkable results. The bacilli, no doubt, were subjected to warmth and moisture, but these influences in other situations, such as in the skin tissues and the peritoneal cavity, have not been found to enhance the virulence of plague bacilli. An explanation may, however, be found when it is recollected that bacilli placed in the nares are during each expiration subjected to the influence of air containing so large a proportion as 5 per cent. of carbonic acid.

76. It would accordingly appear that insufficiency of sunlight and of ventilation, aided by the peculiarities of construction which distinguish the great majority of the houses in India, are conducive to the spread of plague by stimulating both the vegetative and the pathogenic activities of the plague bacillus, and, in a less degree, by increasing the susceptibility to, or reducing the powers of resistance against, infection on the part of individuals.

VI.—Summary of preceding Evidence in support of Conclusion that Insanitary Conditions within Houses are the Chief Causes of the Extension and Virulence of Plague.

Summary of evidence that insanitary conditions within houses are the chief causes of extension and virulence of plague.

77. It has thus been shown by illustrative examples, which represent only a few of the instances contained in the Evidence and in the Reports before the Commission, that the sanitation of India is in a most backward condition, even as regards the elementary requirements of healthy existence, and that the evidence brought before us very emphatically indicates that both the extent and severity of the present epidemic of plague in that country have been largely influenced by the prevailing insanitary conditions. When this evidence is examined, it becomes apparent that a distinction is required to be made between the insanitary conditions which exist inside of dwellings and those which exist outside of them, for the great majority of the witnesses who base their opinion upon large personal experience have arrived at the conclusion that the former rather than the latter have constituted the predominating factors in the extension and virulence of the disease.

78. This conclusion has been founded upon, and supported by, many incidents in the history of the epidemic, and of the measures which have been adopted for its suppression and restriction.

It has been observed that the inmates of well lighted and ventilated houses, even when the houses are situated in the midst of plague-affected districts, have enjoyed a relative immunity from plague; while the inmates of the better class and well-built houses have suffered if the houses were insufficiently ventilated. Concentration of population has not been found to be co-relative with the incidence of plague, for the incidence may be at least as high in a sparsely populated area as in a densely populated one, provided that conditions conducive to the air vitiation of the interior of the dwellings are more powerfully in operation in the former than in the latter; while, conversely, in a densely populated area, the inmates of houses in which the air is free from gross pollution have remained unaffected, while the disease has prevailed in the surrounding air-polluted houses. The same lesson is taught by the immunity from plague which has been enjoyed by certain classes and religious communities while they live in well ventilated houses or in the open air, and even among classes—such as policemen, tramcar conductors, prostitutes, tinkers and beggars—who come much into contact with the general community in an infected place; and it has been emphasised by the disappearance of this immunity in the same classes and communities whenever, from racial habits or other causes, the protection afforded by purity of air inside the habitations did not exist. Quite as emphatic are the indications derived from the generally successful results of evacuation of infected places, and not less from the causes of failure: and from the absence of extension of plague in ships when infective material has been present; from its absence in general and plague hospitals, especially among the numerous attendant relatives and friends who, on the other hand, are rapidly and virulently affected in their own houses; and from the absence of extension in dwellings provided with the means of securing purity of the atmosphere, in which plague patients have been treated without infecting the usual inmates of the dwellings, although no special precautions against infection were taken.

It has also been observed that houses containing infective matter may be occupied without much risk during the day, when outside openings are not closed, or are closed for an insufficient period of time to produce gross pollution of the air; but that plague is propagated in them with almost unerring certainty if they are occupied during the night, when air-pollution is produced by the closing of the doors and other ventilating openings. Further, when plague-infected towns, differing mainly in the sanitary state of the interior of the houses, are compared with each other in reference to the incidence of plague, it has been seen that that incidence has been much greater where these conditions were unsatisfactory than where they were satisfactory.

79. An examination of the relationship of geological formation and of variations of climate to the incidence of plague has also supported the conclusion that the conditions existing in the interior of the great majority of native houses exercise a predominating influence upon the extension of plague; for while the former have apparently no influence, the latter have only a slight influence, and it is exerted in the interior and not the exterior of dwellings. Filth in the open air, also, appears to contribute to the spread of plague only to the extent in which it increases the air-pollution of the interior of houses, but the effect of filth within the houses operates not only in the same way as filth outside of houses, but likewise by supplying, in some forms of it, a suitable material for the vegetative life of the plague micro-organism.

Each and all of the most important circumstances relating to the history of the epidemic have, therefore, led to the conclusion that the extension of the disease, and even its great fatality, are largely dependent upon the vitiation of the air in dwellings.

80. The causes of this vitiation have been shown to be not only uncleanliness, but also the absence or the insufficiency of the means of ventilation and the overcrowding, which are both so general in native houses. While the last of these is operative by increasing the number of persons subjected to infection, it is operative to a much more important extent by adding to the air-pollution of insufficiently ventilated dwellings. Of the several elements which together constitute this air-pollution, scientific investigation has established that the increase of moisture and the elevation of temperature, so definitely produced in occupied chambers that are inadequately ventilated, are conducive to the vitality of the plague micro-organism, and has rendered it probable that the deficiency of light, and, above all, that the increase in the quantity of carbonic acid and the reduction in that of oxygen in the atmosphere of insufficiently lighted and ventilated rooms containing human beings, stimulate the

vegetative activity and enhance the virulence of any plague bacilli which may be present, and, at the same time render the occupants more susceptible to plague than they otherwise would be.

General conclusion.

81. The conclusion, therefore, appears to be completely established that the insanitary conditions existing in the majority of native houses have exerted a direct and important influence, which is indeed a preponderating one, upon the extent and fatality of the present epidemic of plague in India.

As to other causes, they are for the most part those which contribute to the extension of all infectious diseases, with the gravely important addition of the propagation by several of the lower animals, and especially by rats; and they have elsewhere been considered. However great has been the share which has been taken by any of them, many events recorded in the Evidence clearly indicate that their influence would be much lessened, and thus the suppression and restriction of outbreaks of plague would become much less difficult than it now is, were effective measures adopted to remedy the sanitary defects so generally existing within the houses of the Natives of India.

VII.—Great Importance of improving the Sanitary Condition of Dwellings.

82. The accomplishment of this object is, therefore, of paramount importance. Irrespective of the reforms that have been originated by the epidemic of plague, sanitation has, no doubt, made substantial progress in India during the last quarter of a century, but, perhaps inevitably, to a much greater extent in the cities and large towns than in the rural districts, where, however, the greater part of the population live. In these districts, efforts have apparently been confined to "attempts in the way of cleaning tanks and improving wells, and getting away redundant vegetation"; and, occasionally, to "induce the people to resort to latrines, instead of the old plan of making every courtyard a latrine." The keeping of cattle in dwelling-houses and of heaps of manure close to houses are so universal that they do not appear to be considered defects by the sanitary officials.

7838–40; 7810.

7841.

7922

83. In so far as dealing with plague is concerned, the highest authorities on the medical affairs of the country, the present Sanitary Adviser with the Government of India and his immediate predecessor, both of whom have had large experience in the present epidemic, have expressed decided views respecting the fundamental importance of sanitary reform.

After stating that inoculation, disinfection and segregation were limited in their applicability, Surgeon-General Harvey affirmed that improvement in sanitary conditions is "the essential thing" and "the one real way of dealing with plague." Surgeon-General Cleghorn, who added to the above limitations the evacuation of large cities, which, from the experience gained, appeared to be hopeless of attainment, expressed the opinions that "plague cannot appear and spread if the locality is in a proper sanitary condition," and that "the extinction of plague and the prevention of recurrence depend upon sanitation." These propositions are in accordance with the experience gained during all previous and contemporary epidemics of plague,* and, as it has already been shown, they are in absolute conformity with the evidence that has been submitted regarding the present epidemic in India. No propositions, indeed, that have been considered by us appear to me to have been supported by so strong and clear testimony, which also has very definitely indicated that the chief sanitary improvement required in order to prevent the extension of plague and to suppress the epidemic, is a merely elementary reform in the conditions and immediate surroundings of dwelling-houses. Until it has been made, India, it is to be feared, must continue to suffer in so far as the results of any plague measures are concerned, for all other measures will now probably remain unsuccessful, unless, not only affected villages and small towns, but also affected large towns and cities have been entirely evacuated, while the crude and destructive, but in the circumstances necessary, emergency methods adopted for introducing fresh air and light into dwellings are at the same time applied.

7988–98.
7999; 7837.
26,663; 26,706.

26,711; 26,712.

* The recent epidemics in Hong Kong, Alexandria, Sydney, Oporto, Madagascar and Mauritius may be cited as examples; and in each place the highest incidence and the highest mortality were associated with the greatest degree of air-pollution within dwellings. To discuss further the import of the experience acquired from these epidemics would be neither convenient nor desirable, as the consideration of the immediate subject would thereby be greatly extended, and, more especially, as the facts relating to most of them have not been fully collected and are, therefore, only imperfectly known.

84. This consideration becomes all the more important as it is unfortunately apparent that the effective evacuation of cities and large towns cannot be accomplished, and that they will, therefore, continue to be centres of infection from which plague will be imported into previously unaffected places, as well as into places which, at great expense and after many hardships, have freed themselves from outbreaks by carrying out evacuation with disinfection by air and sunlight. 26,664; 26,683–5.

Further, if in the absence of this sanitary requirement, measures, other than those proved by experience to be the only effective ones, are employed during the first outbreak of plague in a place, a false and dangerous trust may readily be engendered in altogether ineffective action, by the fact being overlooked that the first outbreak is not infrequently a mild one, irrespective of the action that is taken, as has been exemplified, for instance, in Calcutta, Mysore City, Baroda and Poona. (*See* Report, paras. 36, 37, 38; and Appendix II. *ante*, footnote, p. 437.)

85. On the other hand, if the elementary requirements of pure air and light in dwelling-houses were provided, it is probable that the existing epidemic could be suppressed, and the possibility of recurrences of similarly wide-spread and virulent epidemics would be greatly lessened, if not altogether removed. Sanitary improvement of dwellings would render the suppression of the epidemic possible by practicable and relatively simple measures.

In addition to the many incidents that have already been cited in support of these statements, the conspicuous relative immunity of the Europeans in India may again be referred to. In the Bombay Presidency, from September 1896 to 1st June 1900, the total number, exclusive of Europeans, of the discovered cases of plague was 392,385, and of plague deaths 308,140, giving a case-mortality of 78·5 per cent. In the same period, the number of cases among Europeans was 202, and of deaths 59, giving a case-mortality of 29·2 per cent. Further, the Europeans may be divided into those who lived in Bombay and those who lived in other parts of the Presidency (Mofussil), including the small towns, cantonments and rural districts. In the former division, there were many Europeans who resided in houses in the streets of Bombay, in the midst or in the immediate neighbourhood of the insanitary conditions existing in the native quarters; while in the latter division, all but a small number lived in the Cantonments or the Civil Lines, or outside of the native quarters, and for the most part in isolated villa-like bungalows. In the former group, the case-mortality from plague was 33·9 per cent., while in the latter group it was only 22·6 per cent. Where, therefore, the dwellings in which plague has occurred are grossly defective in their supply of pure air and light, the case-mortality is the greatest; where these important factors in limiting the virulence of the disease are not grossly defective, the mortality is the least; and, further, when among the latter, a number of the dwellings are less perfectly supplied than others with these and other sanitary requisites, the plague mortality is greater than in the dwellings where they are more perfectly supplied. Weekly Statement of Plague cases and deaths, issued by the Government of Bombay.

To the same effect was the experience gained in Bangalore of the mortality from plague in three groups of patients, whose relationship to fresh air and sunlight likewise varied definitely. One of these groups consisted of plague contacts, who, on becoming affected in the well ventilated segregation camp, were removed to the plague hospitals; the second group consisted of plague patients who became affected in the city, and were thereupon removed to the hospitals; and the third group of plague patients who also became affected in the city, but were treated in their own houses, and not in the plague hospitals. In the first group, the case-mortality was 53·69 per cent.; in the second group, it was 68·77 per cent; and in the third group, 97·06 per cent. Madhava Rao's Report, p. 9.

The relationship between air-pollution and the virulence of plague is also illustrated by the following incident. While plague of an extremely virulent type was present in Worli, so that of the persons attacked 96 per cent died, 250 of the inhabitants went by road to the Karli Caves at Lanauli, some 60 miles distant from Worli, and while residing there several cases occurred, but they were of so mild a character that none of them proved fatal. The great deficiency of ventilation and of light in the affected houses of this village has already been described (paragraph 7). The virulent infective matter carried from these houses was subjected to the influence of fresh air and of light, both during the journey by road and in the open caves at Karli, and apparently it was thereby so mitigated that, although it still retained sufficient activity to produce the disease, it was no longer capable of causing death. Gatacre's Report, pp. 188–9.

Sir Andrew Wingate, whose knowledge of the circumstances attending the extension of the epidemic in India is probably unequalled, has observed, in a statement more fully quoted in Chapter III., paragraph 378, of our Report, that "the outflow from an infected area is probably more dangerous close to the centre of infection than at any 26,537.

distance from it; that as the outflow gets further away from the centre, so the danger seems to lessen," notwithstanding the circumstance that communication with the more distant place may be greater in amount than with the less distant,—a character of the spread of plague which clearly harmonises with the supposition that exposure to air and sunlight reduces the infectivity and virulence of the disease.

Satisfactory sanitary conditions greatly lessen the difficulties of prevention and suppression.

11,379–80; 22,703; 22,720. 23,923–8; 23,943; 23,916.

86. As, however, plague is an infectious disease, produced by a specific micro-organism, it is not to be expected that its occurrence could be altogether prevented by the removal of air-pollution and other fostering agencies from dwelling-houses. At the same time, were these fundamental and predominating causes of the active extension and enhanced virulence of plague removed, the other measures necessary for its suppression would become so reduced in number, and would be rendered so much easier of application, that the suppression of a wide-spread epidemic, involving cities of large size, would no longer remain an almost hopeless task. The complete evacuation of cities and villages, for example, would not then be required, the disinfection of all houses in affected places would be unnecessary, and the crude and temporary method of effecting ventilation by unroofing houses and breaking holes in their walls, would become superfluous. In so far as repressive measures are concerned, the conditions would be much the same as in a European bungalow, or in a plague hospital, or in the properly constructed huts of an evacuation camp; in none of which are harassing and expensive measures considered necessary, and still plague so rarely extends in them that it becomes an easily controlled infectious disease, and loses the formidable characteristics which distinguish it in a native town or village. In the sufficiently lighted and ventilated Native house, the suppression of plague would be as easily accomplished as in the bungalow of the European, and would require little more than the isolation of the patient and the adoption of such relatively simple measures for the prevention of extension by persons and things as, in Great Britain and elsewhere, are successfully employed in scarlet fever, or small-pox or typhus fever.*

Satisfactory sanitary conditions greatly reduce the danger of spread by rats.

22,649.

Snow's Report, pp. 70, 82–95; James' Report, pp. 97, 98; 23,196; 23,317; 20,158; 20,184–90; 20,197–202; 22,062; 22,137; 24,450–5;

87. Even the very special and, in Indian towns and villages, at present insuperable difficulty of preventing the spread of infection by rats would be greatly lessened, for these animals shun well-lighted and airy quarters, in which, further, the infective power and virulence of the plague bacillus is much reduced, and may, indeed, be so greatly reduced that this medium of communicating the disease would entirely be deprived of its formidable potency. As the providing of reasonably pure air also requires that polluting accummulations should not be present within dwellings or in their neighbourhood, the means for securing this provision would further lessen the propagation of plague by rats, by the requirement of properly laid and trapped sewers in municipal towns, and, both in the larger towns and in villages, by the removal of pit privies and "filth of man and beast" from the interior or vicinity of houses, and by the prevention of the keeping of cattle, with accumulations of manure and fodder, in the interior of dwellings, or in close proximity to them. When by these several sanitary reforms, obviously required irrespective of plague, and by the judicious regulation of the storage of grain, the vast numbers of rats which now infest the houses of many native towns and villages have become reduced, a reasonable opportunity will be given for success in the employment of disinfection by chemical substances, the signal failure of which as a plague measure, even when carried out with every care by trained operators using trustworthy disinfectants under skilled supervision, has hitherto proved one of the most disappointing incidents in the history of the present epidemic. These considerations acquire an additional importance from

* The relative simplicity of the measures that are adequate, as well as the relative mildness of plague where gross air-pollution of dwellings is not present, have been fully illustrated in the recent outbreak in Glasgow (1900), a city comparable with Bombay in the number of its inhabitants, and having the highest ratio in Scotland of population per acre. Indigenous plague cases had occurred in two widely separated parts of that city at least a fortnight before the existence of the disease had been discovered by the Sanitary Authority. Every known case was thereafter removed to hospital, the contacts were segregated but allowed to follow their occupations during the day in any part of the city, and the infected houses and their contents were disinfected with chemical substances and with free exposure to the air.

The outbreak was suppressed in about twenty-four days; and in that time the total number of definitely diagnosed and of probable or suspicious cases was 36, but only 16 of them proved fatal, representing a case-mortality of 44 per cent. Eight of these fatal cases, however, were merely suspected cases, which were not diagnosed during life, and, therefore, they cannot properly be taken into account in estimating the case-mortality; for if it be assumed that they were cases of plague, an unknown number of non-fatal and undiscovered cases must also be assumed to have occurred. If only the undoubted cases are considered, the case-mortality was so low as 28·5 per cent., and, therefore, was much the same as that of the Europeans who became infected with plague in the Bombay Presidency.

the circumstance that even if there were a reasonable prospect of success in materially reducing the number of rats by poison, or capture, or the employment of microbean agents of destruction, grave difficulties in applying these measures would be caused in India by religious beliefs and caste prejudices. 19,455–64; 9175; our Report, App. V., paragraphs 5, 6, 10.

88. The position may briefly be summarised as follows:— Summary

Plague is an infectious disease fostered chiefly (*a*) by insanitary conditions, the most important of which is air-pollution within dwellings, and (*b*) by rats.

If both *a* and *b* be present, the suppression of a wide-spread epidemic is practically impossible, unless complete evacuation of every infected place be carried out.

If there be no considerable number of rats, the suppression is less difficult than it otherwise would be.

If there be no gross air-pollution within dwellings, the suppression is not attended with any special difficulty, for, in that case, the chief agency in enhancing infectivity and virulence would be absent, and rat-infection would cease to be an important factor in the extension of the disease.

VIII.—Minimal Sanitary Requirements for Dwellings.

89. It would thus appear that, in order to render it possible to suppress in India an existing wide-spread epidemic of plague,* involving cities and towns of so large size that their complete evacuation cannot be effected, and also in order to prevent in the future the occurrence in that country of extensive epidemics of plague, it is necessary that—

1. Each room in a dwelling-house should be provided with a window for the admission of air and daylight; and that—
2. Each house, and its immediate surroundings, should be kept reasonably free from all causes of air-pollution.

The measures required for securing the latter condition are self-evident, but on account of the special conditions encountered in India, it may not be unnecessary to state that cattle should not be stalled in dwelling-houses, and that it is advisable that stone, cement, tiles, or wood should be used in place of earth in the construction of floors, and that cow-dung should not be applied to the floors or interior walls of houses.

90. The prevention of the overcrowding which is so common in Native houses, is naturally suggested as a requirement that should be included among the above. As, however, the main object of this prevention would be the securing of an atmosphere of sufficient purity for healthy existence, it is obvious that this could not, at present, be attained by any limitation in the number of the occupants of a chamber based on its cubic capacity; for in the great majority of the towns and villages, the houses of Natives are either unprovided with sufficient means of ventilation or are provided with external openings which could only admit air already much polluted by external filth. It is almost superfluous to add that as soon as provision has been made for the admission of pure or even fairly pure air into dwellings, the prevention of overcrowding would become an effective and necessary measure for maintaining an unpolluted atmosphere. Until means for sufficient ventilation and lighting provided, reduction of "overcrowding" of little use.

91. When contrasted with several other infectious diseases, plague, therefore, requires for its extinction and prevention relatively simple, inexpensive and always obtainable conditions. Cholera, for example, has been greatly lessened wherever a pure water supply has been provided. This, however, has only been accomplished by sinking new wells and constructing carefully protected tanks to take the place of polluted wells and pools, and by carrying out large schemes of water collection, storage and distribution, which constitute some of the most gratifying results of enlightened enterprise in the government of India; but even when all this has been done, drought and famine may render nugatory the provision that has been made. There are, besides, vast districts and many towns and villages in India where a pure supply of water is either altogether unattainable or attainable at so large an expense as to be beyond the resources of the country. On the other hand, pure air, the chief requirement for the extinction and prevention of plague, is everywhere obtainable, and may everywhere be introduced into dwellings by relatively simple and inexpensive measures. Minimal requirements not great and always obtainable. 8061.

* The measures which appear to me to be necessary for extinguishing a local outbreak in its early stage, even when it has occurred in a large town, have been described in Appendix V., paragraph 11, of our Report.

Where any considerable expenditure of money is required, it is not for the purpose of admitting the always available pure air, but for that of preventing its pollution by such gross neglect of sanitary requirements as not only favours the propagation of plague, but continuously, year after year, acts deleteriously upon the public health, and largely increases the death-rate of the country.

Difficulties in effecting the minimal sanitary improvements.

92. Although the sanitary requirements, which have been shown to be essential before the epidemic of plague can be suppressed, appear to be of an elementary character, the history of sanitation in India renders it probable that much difficulty may be encountered, and that a strong effort may be necessary before they are carried into effect. The gain, however, would be so great, for it implies no less than the removal of an otherwise hopeless position in so far as human agency is concerned, that it is important to consider what constitute the chief difficulties, and to what extent and by what means they may be surmounted.

Much sanitary reform effected since the epidemic began. 7868.

93. To assist in the consideration of these subjects, it would be useful to recall what has been done in the direction of sanitary improvement during the existence of the epidemic. As the progress of the epidemic throughout the country, as well as the development of local outbreaks, has been slow, while, especially in cities and large towns, outbreaks have so frequently recurred, that it may be anticipated that any place which has once been affected will again suffer from the disease, ample opportunities have been afforded for activity in sanitary work. In many places, these opportunities have been taken advantage of both with the object of reducing the chances of threatened outbreaks, and with that of aiding in the suppression of already existing outbreaks. In Bombay City, 5,000 coolies, in addition to the permanent establishment, were at one time employed, day and night, in flushing the drains with expensive disinfectants, in removing the tiles of houses, and in limewashing the exterior and interior of buildings with an expenditure of hundreds of tons of quicklime; the outside of houses, and the gullies, lanes and streets were flooded with disinfectant solutions by means of fire-engines and other steam and hand pumps; in September and November 1896, 1,337 men were employed in removing silt from drains, which were then thoroughly flushed with millions of gallons of sea-water and carbolic acid, and provided with 450 manholes, and, at a later time, 200 men were employed in opening up drains and removing deposited matter from them; the "down-pipes" of numerous houses were removed in order that they might be cleaned, by washing with boiling water, scraping the interior, and steeping in phenyle baths; and between September 1896 and March 1897, houses to the number of 10,748, in at least half of which plague cases had occurred, were thoroughly flushed with disinfectants, cleansed, and limewashed. Ahmednagar was twice completely cleaned and disinfected, and in the Cantonment, all the private latrines were disinfected and destroyed; in Surat, several thousand pit privies were filled in and open privies constructed in their place, and a large establishment was engaged to clean the town; many uninhabitable houses were demolished in Mandvi, the latrines were removed outside of the boundaries of the town, and, with the aid of a large staff of additional sweepers, the town was at length made fairly clean; in anticipation of plague, great activity was manifested in the larger cities of the North West Provinces, in improving latrines and closing dirty wells, and in 96 villages of the Balachour Division the "people were persuaded to cleanse their houses;" Quetta and all the important district out-stations were thoroughly cleaned, and houses found unfit for human habitation were pulled down, but the ordinary villages were not dealt with; two or three of the towns in Baroda State were put in good sanitary condition by widening streets, improving the water supply, and organising a system of house conservancy; and the ventilation of several of the houses in Bombay, Karachi, Baroda, and a few of the other larger towns, was improved by permanent structural changes.

968, Gatacre's Report, p. 185.

Snow's Report, pp. 4, 82, 87, 92, 100.

24,191.

24,459; 15,929–30.

13,505–6.

8843.

Inglis' Report, p. 143.

8096–7.

15,156.

25,700–2; 22,654.

In many other places, sanitary operations, restricted for the most part to the removal of filth, have been carried out in a large, and even a gigantic scale, but the above examples are sufficient to illustrate the sanitary activity which has been stimulated by the epidemic, and to demonstrate that, when in India it is earnestly desired to effect sanitary improvements, the difficulties that exist are by no means insuperable.

The mere removal of filth shown to be inadequate as a plague measure.

94. It would be premature to expect any definite results from these emergency measures upon the public health, and, probably, also unreasonable to do so, in view of the enormous residue of insanitation which has been left untouched, even in

the places where the measures were carried out. Their object, however, was to curtail the extension of plague; and while it is impossible to assert that no impression was produced, the evidence clearly shows that they have failed to appreciably affect either the extension or the virulence of the disease. This conclusion, which unfortunately, has been fully established by the growth of the epidemic, and the records of the mortality during four years of sanitary activity, as well as by the recurrence of successive outbreaks, frequently of increasing severity, in the large towns and cities, has been expressed in the following words in General Gatacre's Report: "Where the surroundings are hopelessly insanitary, and where buildings are overcrowded, . . . and the rooms contain no means of ventilation except the door, sanitary operations" (by which are implied such cleansing and filth-removal operations as have been described above) "have little or no effect in stopping the disease." p. 186.

95. For reasons which have already been stated, it is not to be overlooked, however, that as the predominating factors in the diffusion and the increase in virulence of plague are the polluted atmosphere and other conditions existent in the interior of inadequately ventilated and lighted dwellings, the measures for the removal of this pollution must include such sanitary improvements outside of dwellings as have almost alone received attention during plague operations; although they themselves, however complete and thorough, cannot be successful so long as pure air and light are not admitted into the interior of the dwellings. That efforts have not been more generally made to secure these requisites cannot be explained by the absence of appreciation of their great influence upon the diffusion and virulence of plague, for, as we have seen, this has very largely been recognised by both Europeans and Natives, and, indeed, it could scarcely fail to be made manifest by the everyday incidents of the epidemic, such as the immunity in hospitals and evacuation camps. It is possible that their predominating importance has, to a large extent, been overlooked in the presence of the more obtrusive sanitary evils prevailing outside of houses, but the explanation of the absence of any general effort to include the permanent ventilation of dwelling-houses among the sanitary improvements which have so largely been carried out, may likewise be found in the expectations that the financial difficulties are too great to be overcome, and that the people would themselves so uncompromisingly oppose this reform as to render its introduction inadvisable or impracticable.

Financial difficulties.

96. With regard to the financial difficulty, no doubt expenditure would be required before each room of a dwelling-house could be provided with adequate external openings for the admission of air and light. In so far as this difficulty affects places which are suffering or have recently suffered from outbreaks of plague, it is probable that little additional expense would be incurred in rendering permanent the temporary openings for ventilation, which, when combined with the evacuation of the houses, constitute the most frequently and successfully adopted of plague measures, instead of closing the openings when the houses are re-occupied, and, in many places, repeating this process of breaking holes in walls and repairing the destruction thus caused on the recurrence of successive outbreaks.

In places threatened with plague, the expense of providing permanent means for admitting sufficient air and light into houses would add but little to the pecuniary expenditure incurred in the removal of accumulations of filth, the clearing of drains, and the cleansing of streets and houses, with a lavish distribution of disinfectants, which are the sanitary measures that have most frequently been trusted to in the defence against plague. If, however, the houses were already provided with permanent openings for ventilation and were sufficiently supplied with pure air and light, plague when imported into the place would fail to produce so extensive and virulent an outbreak as would otherwise occur, while the breaking of walls and roofs and other plague measures would be unnecessary; and thus not only would the outbreak become controllable, even in a large town, but the expenses incurred in suppressing it would be much reduced.

Further, as the adequate provision of unpolluted air and of light in dwellings is necessary before the epidemic can be stamped out, and is an essential condition in the defence against outbreaks of plague, the consideration naturally arises that expenditure for all other measures—and both in individual places and collectively it has already been great—must be futile and unavailing so long as that which is necessary and essential is not also provided.

Nathan's "Plague in India," Vol. I., pp. 193-7; Madhava Rao's Report, p. xlix; 26,664.

Both in plague-affected and in plague-threatened places, the existence of imminent danger would greatly lessen the pecuniary and other difficulties which might otherwise

present themselves. That it is by no means hopeless to secure the acquiescence, if not co-operation of the people, has been shown by the experience gained at Baroda, where windows have been introduced into houses which were previously unventilated, without much expense and with almost no opposition, by the exercise of "a little persuasion and a little firmness."

15,157, 15,160–1

Opposition of the people.

Address to Members of the First Indian Medical Congress by Surg.-Colonel Harvey, 1895, p. 44.

97. As to the anticipated opposition from "the ignorance, prejudices, callousness and superstitions of the people," it may, in the first place, be noted that in all countries sanitary reform has had to contend with these causes of obstruction, but that wherever a policy of reform has been seriously and earnestly adopted, the obstruction has been unable to prevent its progress. The stimulus of the plague epidemic has already done much to advance sanitary education in India and to break down the barriers raised by the apathy and prejudices of the people. While the first introduction of plague measures caused much disfavour and excitement, and in a few places even active hostility, as the objects of these measures and the evidence of the benefits produced by them became manifest, the opposition has largely, and in some districts, entirely disappeared. In nearly every plague-affected place, evacuation, on its first adoption, encountered much opposition, but the inhabitants soon learned to appreciate its advantages. In many places, indeed, they have shown an inconvenient preference for the open and sunny huts of the evacuation camp over their ill-ventilated and lighted houses, and the prediction of Surgeon-General Planck has over and over again been verified that "when plague becomes well-known on the Plains of India, the people will flee out of their houses of their own accord." To mention only another and an extreme example, no plague measure has excited more determined resistance, arising from family relationships, caste prejudices, and grotesquely childish misconceptions, than the isolation and treatment of patients in hospitals; and, nevertheless, by tact and by the natural diffusion of knowledge gained by experience, this opposition has already subsided in several places, and patients are with their own consent brought voluntarily by friends and relatives for isolation and treatment in plague hospitals.

14,758–63; 15,115–6; 15,168; 15,865–7; 23,996–24,001; 24,058–9. 26,656; 26,668. Burnett's Report, pp. 3, 5; 9; 11,375; 14,635. 26,575.

Campbell's Report, p. 112; Gatacre's Report, p. 133; 1863–7; 7863, 11,246; 26,550.

Structural sanitary improvements effected without opposition.

98. During the activity in sanitary reform, originated by the epidemic, few attempts were made, excepting in some of the larger cities, to effect permanent structural improvements by which air and light could be introduced into dwelling-houses, and, therefore, little experience is available for judging of the attitude of the people towards this reform. In the few places where it was carried out, however, the experience that was gained is not unfavourable for the future success of this pre-eminently valuable plague measure. While the systematic opening up of houses was being effected in the city of Ahmednagar, many of them were found to be so destitute of means for ventilation that "air and light had never got into them since they were constructed." The owners were required by the Municipality to provide windows and other ventilating openings, and although many of the poorer and more ignorant of the inhabitants obstructed the windows with curtains and purdahs, the better class of the inhabitants made proper use of them, and it was believed by the Chairman of the Plague Committee, that even the poorer class would "gradually come to appreciate the value of light and ventilation." This opinion was endorsed by the Cantonment Magistrate, Captain Walton, I.S.C., who had, apparently on a larger scale, effected similar reforms in the houses of the Cantonment, and had required that condemned houses should be rebuilt "on a particular pattern," with "two windows in each room and a ventilator in the roof," so as to secure "lots of ventilation, big doors, and a good height to the roofs." In Baroda State, houses without means of ventilation were provided with windows in each room, so as to allow "plenty of light and air to get in," and this, as well as the other sanitary improvements which were made, was stated by the district magistrate to be much liked by all the people except the Machis, who feared that their property would not be secure if the walls of the houses had openings, their custom being to "turn their houses into a regular box." In drawing attention to the freedom from plague, during 1898–9, in certain wards in Satara, which had been infected during the previous year, Captain Thompson, I.M.S., stated that the inhabitants of these wards, induced by example and by the persuasion of their leaders, were "following those supreme safeguards, and running the risk of dying of diseases which they thought were caused by cold air rather than die of plague," and that native doctors had told him "that many in the city are following these safeguards with regard to ventilation and cleanliness of their houses." The experience gained in the Mysore State is, however, even more definite and encouraging. Among the plague measures in Bangalore City, was the sanitary improvement of a large number of houses, which had been temporarily vacated because of an outbreak of plague, by

24,347.

24,362.

24,203–9.

24,191; 24,203.

15,157.

15,163–5.

22,432–4.

providing them with permanent openings for admitting air and light. The Plague Commissioner of the State informed us that the people were already beginning to appreciate the advantages of ventilation and of the improved lighting of their houses, and he anticipated that they would themselves voluntarily make the structural alterations required for obtaining these advantages. Subsequently to his examination, he has supplied the information that a large number of houses in other parts of the State have been provided with windows, that no cases have come to his knowledge in which windows made by the Authorities have been closed, and that all the chief plague and medical officials whom he had consulted had also met with no instances in which that had been done. In the villages as well as in the towns, the advantages of the free ventilation obtained by the openings made to admit air and light into dwellings were being appreciated, and there had been many instances of people themselves making such openings. In further confirmation of the opinion that sanitary improvements, which include ample provision for securing pure air and light in dwelling-houses, are not rendered impracticable by the opposition of the people, it may be added that improvements on a large scale have been initiated in Bangalore City, which involve the demolition of 2,600 houses in order that over-populated districts might be opened out, and involve also the formation of two large extension areas of nearly a thousand acres, for the accommodation of 50,000 people, in houses liberally supplied with means for admitting light and air, and provided on every side with well-drained open spaces.

3268; 3271.

3261; 3481.

99. It is frequently stated that the great majority of the Native houses are unprovided with sufficient openings for the admission of air and sunlight because ventilation and light are obnoxious to the habits and prejudices of the people and are otherwise distasteful to them. This, however, is not the explanation which is advanced by several of the most intelligent of the natives themselves. Mr. Madhava Rao, C.I.E., for example, considers that the cause is not to be found in family or caste requirements or the anticipation of discomfort, but in the survival, from a bygone period of misrule and insecurity, of now unnecessary precautions against robbery and violence. This explanation has also been advanced by European Medical Officials who have had long experience of native customs and prejudices.

Causes of general existence of unventilated houses.

100. It has also been stated that even if the houses of Natives were provided with windows, a deeply rooted disinclination to admit air and daylight would lead the inhabitants to close the windows. It does not appear justifiable to assume that this would generally be persisted in, in view of the circumstance that the great majority of the Natives of India live in houses either altogether or very insufficiently provided with special means for admitting air and sunlight, and as yet, therefore, have had no opportunity of experiencing and enjoying the benefits of pure air and daylight in their dwellings.

101. It is not improbable that the chief cause of the prevailing absence of windows or other special permanent openings for the admission of air and light into houses is that suggested by a witness as applicable to the general neglect of sanitation in native towns and villages, previously to the occurrence of the present epidemic of plague, "the people did not understand its importance before, and no one took any interest in it."

15,155.

IX.—How the necessary Sanitary Reforms may be carried out.

102. The importance of this reform can, however, no longer be overlooked, for there appears to be little hope of suppressing the epidemic until it has been effected. The manner of accomplishing it may, therefore, now be considered, and for this purpose it would be convenient to draw a distinction between (*a*) places which are or have been infected or are in such proximity or relationship to them as to be in immediate danger of becoming infected, and (*b*) places which are so remote from infected localities that the danger of outbreaks in them has not been sufficiently realised to originate precautionary measures.

103. In the former group, municipal and emergency organisations are already in operation which would be able to include in their sanitary work the providing of permanent means of ventilating all houses which could with propriety continue to be occupied, while of the number which do not fall within this category, these organisations would be sufficient to ensure that the new houses which are constructed in

In places already affected or threatened with plague.

substitution should have a window in each room. The reform would be much facilitated by the extension and improvement schemes in operation or under consideration in Bombay, Calcutta and other cities and towns, where many of the existing houses have been so constructed and are so situated that windows opening into the external air either cannot be provided, or, when provided, would fail to supply the interior of the house with a sufficiency of air and sunlight, even although pollution of the air near the dwellings were prevented by efficient drainage and conservancy.

26,803.

Snow's Report, p. 185, para. 684, p. 187, para. 692.

In places remote from centres of infection.

104. The second group includes places which are either unprovided with a sufficient organisation, or are provided with one that has not yet taken action under the panic of impending danger, nor under the influence of a reasonable desire to adopt measures for averting or lessening a possible calamity.

The importance of effecting at least the essential sanitary reforms in these places is apparent when it is realised that no place, however remote from centres of infection, can be regarded as protected by its position. It is no doubt the case that both in India and other countries, former epidemics of plague have occasionally been limited to restricted areas, and, from causes which are not in all cases apparent,* have finally disappeared, after an existence of only a few years. Any expectation that the first event may be realised in India has, however, already been dispelled. Any hope that the epidemic will have only a short duration cannot be entertained irrespective of the considerations that this has been rendered improbable by the recent great increase in the facilities for inter-communication, and the large extent to which they are being used, and by the great difficulty, if not the impossibility, of totally evacuating every plague-infected place and freely opening up the vacated houses; without which, in the prevailing insanitary conditions and especially the insanitary conditions within dwelling-houses, plague cannot be suppressed in the great centres of infection. Further, if there be any agency, in addition to effective plague measures, which can cause an epidemic to cease, that agency is entirely unknown and therefore it is neither available for intentional application nor can its interposition confidently or even reasonably be trusted to.†

It is possible that in the places included in this group, inaction may be stimulated by persuasion, or by the diffusion of information, or by obtaining the co-operation of the leaders of the people; for the Native of India appears, for the most part, to be easily led by precept and example. If these methods fail, official pressure would no doubt prove successful, and the amount of it will necessarily rest with the governing authorities, who can best appreciate the issues that are involved. In doing so, consideration should primarily be given to the circumstance that there is little prospect of the epidemic being suppressed until the large cities and towns have been freed from plague.

26,802.

X.—A New Sanitary Organisation required for India.

105. All these methods, however, must be regarded as temporary expedients by which the present emergency may be met. While they would provide the means for obtaining a satisfactory condition of sanitation within dwelling-houses, they are inadequate to ensure that this condition would be maintained, and they are also insufficient to provide for the continuance and expansion of the sanitary reforms that may be effected and are urgently required.

106. From the information that has been placed before us it is impossible to arrive at any other conclusion than that the existing sanitary organisation would be unable to realise these objects. It is, indeed, generally admitted to be an organisation which, notwithstanding the zeal and accomplishments of its members, is inadequate for the needs of India even when the aims are restricted within very moderate limits, with such results, however, as have been illustrated in the earlier portions of this Memorandum. We have therefore recommended, in broad outline, a scheme for a new organisation, leaving numerous details for adjustment and future development. (*See* Chapter VII., II., of our Report.)

26,793.

* Several causes which may probably explain the cessation of former plague epidemics, when no measures likely to produce this result were applied, are not here referred to, such as the accidental destruction of the whole or large portions of affected cities, or the lessening of the insanitary conditions within dwelling-houses by a large reduction of the population, produced by panic or by heavy mortality from the disease.

† In this category may be placed the slowly produced extinction of an epidemic possibly caused by a gradual attenuation of the plague bacillus, or by a considerable proportion of the population having acquired immunity on recovering from slight attacks of the disease, or by a combination of both circumstances.

107. The fundamental proposals in that scheme are an increase in the number of officials specially qualified in Public Health, and the formation of a separate Sanitary Service controlled by one medical director,* attached to the Supreme Government, who shall be required to devote his whole time to sanitation, and shall be provided with an adequate staff, in which, I would venture to suggest, an expert in sanitary engineering should be included. Scheme proposed by Commission. 26,638 (last para.

108. Further details would be matters of administrative convenience and efficiency, such as (*a*) whether there should also be in each Presidency and Province, a chief officer acting under the medical director, in which event a Presidency or Provincial staff would also be required; but, in any case, Local Governments as well as Urban and Rural Authorities should be entitled to initiate and carry out any sanitary improvements, subject to the supervision and approval of Government; (*b*) whether the distribution of the chief sanitary officials should be made in consultation with provincial governments and municipal and district Authorities; and (*c*) whether such Municipalities or other Sanitary Authorities as contribute to the payment of Medical Officers of Health should be entitled to appoint, although not to dismiss, their officers, subject to approval by the sanitary director of their qualifications and satisfactory performance of duty.

109. Our recommendations also include proposals which, if adopted, will, it is to be hoped, afterwards give place to more satisfactory permanent arangements. In view of the engrossing and multifarious nature of their other duties, it does not appear possible for Civil Surgeons even to inform themselves of the sanitary requirements of their extensive districts, much less to supervise or direct sanitary affairs in them; and, therefore, the recommendation we have made regarding these officials should, I think, be regarded as only a temporary expedient, to be afterwards superseded by the appointment of Health Officers whose time would be entirely devoted to sanitary work. 7730, 7813-4; 7912; 10,508-12

110. Further, several obvious requirements have not been included in our recommendations, one of the most important of them being the appointment of intelligent and trustworthy subordinate inspectors, trained in the elements of sanitation, qualified to disinfect houses and things, and able, in their daily intercourse with Natives, to give advice and encouragement in rudimentary sanitary precautions.† 3920-3; 3956.

111. The local officials, both urban and rural, would thus constitute an organisation for removing the prevailing ignorance regarding the beneficial aims and results of sanitation, and for obtaining the co-operation of the people in the adoption of sanitary reforms, which are not only at present urgently required for the suppression of plague, but, on account of the rapid increase of population and of inter-communication between all parts of India, are daily becoming even more necessary than they formerly were for the prevention of the excessive mortality from preventible diseases.

112. While hasty and inconsiderate action is to be deprecated and much patience and tact exercised in the education of public opinion, it does not appear that the conditions even now existing, are everywhere unfavourable to effective sanitary work. Many instances have already been adduced of successful operations in towns and villages, and the Sanitary Commissioner of the North West Provinces has described a systematic series of operations—including the visitation of nearly every house in the cities and towns of those Provinces by specially appointed Health Officers and by Civil Surgeons, and the removal from them and their surroundings of the observed insanitary conditions—which were carried out in anticipation of plague, apparently without causing irritation, and which are actually some of the most important of the duties that would devolve upon the officials of a properly constructed Sanitary Service. It was stated, also, that the required sanitary improvements were made by the owners of the houses, except in the cases in which the owners were poor, when assistance was given by the Municipalities. If these and similar operations are carried out with tact and discretion, there is some difficulty in understanding how, as one witness informed us, popular discontent, sufficient to raise a political issue, would be produced by the endeavour to prevent the keeping of cattle in dwelling-houses, or the accumulating of manure close to houses, or the washing of clothes or voiding of urine in the same part of a stream from which drinking water is drawn. 8843; 8973-8. 7922-5.

* The word "director" is used merely for convenience, and not with any intention of suggesting a designation for the Head of the Service.

† The progress of sanitation would also be much benefited by the introduction of a system of registration of all medical men holding qualifications from institutions recognised for the purpose by Government, for thereby the foundation would be laid of a much required accuracy in Death Returns. 26,734; 26,760-9 8054; 8855-9.

8069. 113. The explanation can only be found in the absence of information regarding the dangers that are incurred, and the injury to health that results from these and similar practices; for many incidents during the epidemic have shown that the Native of India, although capable of bearing hardships with much patience and fortitude, does not generally exhibit any inhuman disregard for personal safety in the presence of realised and avoidable danger. The diffusion of information regarding even the mere elements of sanitation thus acquires much importance, and to further this object, it is 3982. desirable that special attention should be given to the training in Hygiene of the native practitioners, so that intelligent interest may be taken in sanitation, and the knowledge of its requirements extended through their instrumentality and that of the members of the Sanitary Service to the inhabitants of villages as well as of towns.

114. If by the several means that have been indicated the activity in sanitary reform, which has been created by the present epidemic of plague, should be maintained and developed, India will ultimately be more than recompensed for the great sacrifice of life and the hardships and pecuniary losses which are being caused by the pestilence.

T. R. F.

APPENDIX IV.

Measures for the Discovery of Plague Deaths.

Memorandum of Dissent by Dr. Wright and Dr. Ruffer.

1. In placing on record our dissent from the views expressed by our colleagues on the subject of the measures for the discovery of plague after death, we desire at the very outset to make it plain that the differences between our views and those which are held by our colleagues are differences, not on matters of detail, but on matters involving very grave practical issues.

Dissent not on matters of detail, but on matters involving grave practical issues.

The gravity of the issues which are involved in a decision to rely upon this or that measure for the discovery of plague will be manifest when it is considered that the effectiveness of a particular measure, whether it be one of disinfection or of isolation, will depend primarily upon the adequacy of the information which is placed at the disposal of the person entrusted with the carrying out of that particular measure.

2. We need not here pass again in review the measures which have been employed with a view to securing the discovery of plague cases during life, for the evidence which has been laid before us has convinced us, as it has convinced our colleagues, that it is impracticable, unless in small places and under exceptional circumstances, to adopt any measures which will effectively secure the discovery during life of all, or anything like all, the plague cases.

Measures for discovery of plague cases during life ineffectual, excep in small places an exceptional circumstances.

3. Failing the means of coming by such information, we are persuaded that no measures for the isolation of the sick can ever be effective unless where plague is confined to very small limits. We desire, however, to make it plain that in participating in the recommendation that the attempt compulsorily to remove all plague cases to hospital should be abandoned, unless under circumstances favourable to the effectual carrying out of this measure, we have looked beyond the fact that difficulties are experienced in carrying out this measure, and also beyond the fact that the hardship which is inflicted by it on individuals is disproportionate to the benefit achieved by it. We have looked in particular to the probability that, in cases where measures for isolation are not insisted on, there will be less difficulty in securing that all plague deaths should come to the notice of the sanitary authority under conditions allowing of the subsequent disinfection of every house in which a plague death has occurred.

Abandonment of compulsory isola tion of sick agre to in expectation that this will render less difficu the obtaining of full information all plague deaths Para. 610 of this Report.

4. It is in particular with regard to the necessity of taking steps to ensure that complete information shall be obtained with regard to the occurrence of plague deaths that we find ourselves in disagreement with our colleagues.

Measures to obta information re-garding all plagu deaths of greatest importance; neglect of such measures justified only in special circumstances.

We, for our part, are convinced that a plague policy which makes no effective provision for the discovery of plague deaths is a policy which is foredoomed to failure; foredoomed to failure in places where strenuous efforts are made to isolate the sick, and even more so in places where the efforts which are now made to isolate the sick shall have been abandoned. The alternative policy which, in our opinion, should be applied to the case of Bombay City, and, unless where special circumstances forbid, in the case of all other large towns, is one which should ensure the discovery of all suspicious deaths and the effectual disinfection of the houses in which suspicious deaths have occurred.

Let us place clearly before us that to abandon compulsory isolation of the sick, and to acquiesce in a plague policy which makes no effectual provision for the discovery of plague, is to acquiesce in a system under which isolation will never be resorted to, and under which disinfection will be applied to the houses only of those who volunteer the information that they have been infected with plague. Justification for such a policy would, in our opinion, exist only if there was any serious doubt as to the possibility of checking plague in cases where complete information of plague deaths and adequate disinfection arrangements are available, or if, on the other hand, there were strong reasons to believe that the application of the measures required for the discovery of plague deaths would in a particular place, or in a particular set

of circumstances, entail disadvantages on the community which would be more serious than the disadvantages associated with the prevalence of the plague.

We propose briefly to consider these objections, which are the only objections which could be brought against the policy proposed by us.

The discovery of all plague deaths, and the disinfection of houses in which they have occurred, would be successful in combating plague.

5. We may begin by considering whether there is any reasonable doubt that, even after abandonment of isolation, success would attend such a policy.

We think there is no reasonable doubt that this policy, even without the aid of any measures of isolation of the sick, or of contacts, would be successful—we do not say in extirpating plague, but, at any rate, in holding its ravages within check. We cannot appeal to direct experience to show that this result would follow. It is true that we have instances, such as Hardwar and its neighbouring towns and villages, and Poona Cantonment in the second epidemic, where a complete, or all but complete, success attended the adoption of a policy which had as its cardinal features the discovery of all plague deaths and the disinfection of all houses in which plague deaths had occurred. Still, these cases are not directly applicable to the resolution of the problem before us, inasmuch as measures were also taken for the isolation of the sick and contacts.

In default of actual experience to guide us in this matter, the following considerations will enable us to gauge, to some extent, the degree to which the complete plague policy, which was proved so effectual in the instances referred to, would be rendered less effectual by the abandonment of all efforts at isolation.

(1.) It has not, in the case of any single town, been possible to find all the plague cases during life, but, of the cases which were found, there were very many who were discovered and isolated only when they were either moribund or in the very last stages of the disease.

It follows that what is abandoned, when all measures of isolation are abandoned, is not the isolation of all plague cases, but the isolation only of those cases which can be found.

Para. 213 of this Report.

(2.) In the case of those who are suffering from bubonic plague uncomplicated with lung symptoms—and these constitute a large number of the total of plague cases—plague bacilli have only in the very rarest cases been found in the excretions of a patient before that patient becomes practically moribund. We may, therefore, assume with probability that such patients are not infective in the earlier stages of the disease. If this is really so, it would follow that to postpone the disinfection till after the patient has succumbed is to postpone it for only a few hours after the time at which that disinfection first becomes necessary.

Para. 659 of this Report.

(3.) Inasmuch as the disease generally runs a very short course to death, and inasmuch as the bacilli—at least in cases of bubonic plague uncomplicated with lung symptoms—escape from the body of the patient only very shortly before death, and inasmuch as, further, the bacillus is capable of surviving for a very long period outside the organism, it will be manifest that the chances of plague infection being contracted from a bubonic patient during the life of that patient are relatively small compared with the chances of the infection being acquired from the room after the death of the patient. Thus the risk to the co-inhabitants of the dwelling will not, in cases of bubonic plague, be very much greater in cases where disinfection is instituted immediately after, than in cases where it is instituted before, the death of the patient.

Inefficiency of existing system of obtaining information of causes and place of death at burning and burial grounds. Paras. 585 and 596 of this Report.

6. The system which is adopted in Bombay, and in the other large towns of India, for the purpose of obtaining information with regard to the place and the cause of death has already been described. As explained by our colleagues, subordinate Native officials are posted at the burning ghauts and burial ground with a view to their eliciting the required information from the funeral parties.

We need not dwell on the fact that the information as to the cause of death, which is collected in such a manner, and through such an agency as this, must be, and indeed is, grotesquely inaccurate. This is everywhere conceded. Neither need it again be pointed out that where the relatives have any motive for concealing either the locality or the cause of death—and such motives will continue to be operative as long as any plague measures are adopted—there will never be any difficulty in throwing the authorities off the scent. We may, however, insist on the fact that all the attempts which have been made to render this radically vicious system of death registration effective have failed to achieve this object. For even when the police were given gratuities for reporting the place of origin of the burial party, and even when men were employed at the graveyards and burning ghauts to follow back the relatives to their houses, and even when the authorities availed themselves, in addition to the information

which was obtained from the volunteer agency which is described in Section II.(1)(*d*) of Chapter VI., the place of death remained in many cases unknown.

System of special inquiries by medical men into causes of death not successful. 25,617.

7. Again, even when the elaborate system of special inquiries by medical men, which has been referred to by our colleagues, was resorted to, the cause of death in many cases remained unknown. We were told by Mr. Du Boulay, I.C.S., who was in charge of the plague operations in Bombay City, that he estimated at 259 the number of plague deaths which escaped discovery in the week ending the 4th March 1899. The estimated number of plague deaths for that week was 1,661. Taking these computations as very close approximations to the real numbers, and the week chosen as a typical week, the number of plague foci left undetected in Bombay corresponds in each week to one-sixth of the total plague deaths. Even if we were to omit from consideration all other possible means by which the plague is kept alive, we shall find in these undiscovered foci alone sufficient cause to account for the continued prevalence of plague in Bombay. If we now take it that each of these foci of plague may set up, when the external conditions are favourable to the survival of the plague bacillus, five or six cases of plague on an average, before the infective material is destroyed by the disinfecting agencies of nature, we should arrive at a total of plague cases comparable to that which is recorded week by week for Bombay.

Under present system, in spite of better disinfection, plague mortality in each successive epidemic in Bombay City increases.

8. It will be seen, on a comparison of the curves set forth in the chart given in Appendix No. LXXXIV. to our Proceedings for three successive years, that in Bombay City the total plague mortality was greater in the second epidemic than in the first, and greater again in the third than in the second. It seems probable from the records of the epidemic of 1899–1900, so far as these have reached us, that the total plague mortality in the fourth epidemic may exceed even that of the third. This, we may note, is occurring in spite of a steady improvement in the methods of disinfection which are applied to discovered cases.

Seasonal variations in Bombay not influenced by plague measures.

9. Again, if we turn and examine the features of the curves of individual years, we shall see that there is in each year an extraordinary seasonal increase in the plague mortality, which is followed by an equally remarkable seasonal decrease. In each year the mortality begins to rise in the later months of the year, and then rises steadily till February or March, when it begins again to decline.

Now inasmuch as disinfection operations are continued throughout the year, and inasmuch as they are not carried on less vigorously during the period of the year when plague mortality is on the increase than during the period of declining plague mortality, it will be manifest that we must impute the increase and decline of plague, not to the lesser or greater efficacy of the disinfection operations and other measures which are applied by the plague authorities to the discovered foci of plague, but to the lesser or greater efficiency of the natural disinfection agencies which came into operation without the intermediary of the plague authorities upon the undiscovered foci of plague. The more these facts are considered the more, in our opinion, does the necessity for taking steps, wherever plague is rife, to secure that all plague deaths shall be discovered, enforce itself upon the mind.

Death registration accompanied by disinfection in all cases where cause of death not ascertained a possible means of dealing with plague, but involves unnecessary disinfections. 10,989–94. 14,631.

10. When we turn to the question of seeking a remedy for the concealment of plague deaths which now exists, consideration shows that the only possible way by which the place of death could be ascertained would be to render it penal to proceed to the removal of any corpse until the death had been notified and a permit had been issued by the sanitary authority. Only in this way would it be possible to locate every death. Major Ross, who has had considerable experience both in Poona and in Bombay, has suggested that it would be well to provide that all deaths should be registered within a period of, say, six hours. This first step having been taken, it would be necessary to follow this up by taking steps to ascertain the cause of death in each case, or, failing this, to disinfect every house in which a death from any cause occurred. This was done at one time in Baroda. But to do this in the City of Bombay would, taking into consideration the ordinary rate of mortality in Bombay, involve the institution of some 500 disinfections per week, which would be unnecessary so far as plague was concerned. If only for the sake of avoiding the unnecessary expense and the hardship which would be inflicted on the people by these 500 disinfections, it would plainly be well to have recourse to some method of discriminating the deaths due to plague from the deaths which are not due to plague.

Sources of accurate information of cause of death.

11. It will be manifest that hearsay evidence based on inquiries from relatives or others, and conjecture based on the occurrence of two or more cases of deaths in the same house within a short period, will supply only inaccurate, and in many cases also

belated, information. There will thus come into consideration, for our purposes, only medical information based on examination of patients during life, or medical information based on examination after death. The former will be available in the form of death certificates signed by unofficial medical practitioners, and the latter can be based on an examination of the body after death conducted by official medical men.

Death certificates of little practical use in the past. Paras. 582–4 of this Report.

12. With regard to death certificates, it has already been noticed by our colleagues that very little information has been derived from this source. This, as has been seen, is due to several causes. In the first place, only a very small section of the population is attended by medical men. In the second place, the obligation to report deaths from plague has not generally been imposed upon practitioners even in plague-infected Municipalities. In the third place, even when the obligation has been imposed, this has not been generally enforced, while a powerful motive in the form of loss of practice has rendered practitioners unwilling to report deaths as due to plague. Again, it imports us to note that, as many persons of very inferior medical education are in India popularly included within the ranks of medical practitioners, many of the certificates must be regarded as practically worthless. Lastly, let it not be forgotten that, owing to the desire of the patients and their relatives to escape the imposition of onerous plague measures, false medical certificates have often been put forward for the purpose of concealing the fact that death has been due to plague.

Owing to these causes, medical certificates which ought, in conformity with the greater facility of arriving at a diagnosis during life, to supply the more valuable information, have, as a matter of fact, supplied very little useful information with regard to plague deaths.

Means of improving system of death certificates.

13. While we recognise that this source of information cannot, under any circumstances, be rendered fully available in India, we think that every endeavour should be made to make it more fully available and more trustworthy than it has been up to the present. We think that this result would be brought about spontaneously with regard, at any rate, to that section of the population which is attended by medical men, if a policy were adopted under which the notification of plague ceased to involve either compulsory removal to hospital or segregation of the contacts, and under which corpse inspection, or, failing it, disinfection, was insisted upon in every case where the presumption that death was due to plague was not rebutted by a medical certificate from a reputable practitioner. Further, we would point out that, given such a policy, it would at all times be possible largely to increase the amount of information available from this source by providing medical men who should visit cases of sickness and issue certificates to them which could, if these cases issued fatally, be used as death certificates. Arrangements on these lines for supplementing the information derived from ordinary death certificates, and for reducing the number of necessary corpse inspections, were, as a matter of fact, made in various places, among others, in Poona and Cutch Mandvi.

1,984–999. 3,552.

Corpse inspection; outline of policy recommended.

14. Failing information derived from medical death certificates, there is no source, except corpse inspection, from which the required information could be obtained.

Before proceeding to discuss the question of the efficacy of this measure, and the further question of the attitude of the people with regard to it, and finally, the question of the expediency of introducing it, we think it well very briefly to state our attitude with regard to this measure. While we think that every European Medical Officer of Health ought to be empowered, failing the production of a medical certificate, to proceed to corpse inspection, we are far from thinking that this practice should be resorted to to the prejudice of more important interests, and, indeed, we do not contemplate that corpse inspection should be adopted as a general measure, except in places where the Public Health is threatened by epidemic disease. Possibly, under certain circumstances, it might occasionally be wise for the Sanitary Officer to stop short of the actual inspection of the corpse. He might then content himself with classing the death as suspicious, and proceed to the disinfection of the house.

Means by which information can be gathered by corpse inspection at the place of death.

15. So much having been said in the way of preface, we may pass to consider the question of the efficacy of corpse inspection as a measure for the detection of plague deaths. What is necessary to say on the question may be said in a very few words.

It may be pointed out, in the first place, that just as in the case of medical examinations of the living, so in the case of corpse inspection conducted at the place of death, information will naturally be gleaned, firstly, from the consideration of the

history as detailed by the relatives; secondly, from the inspection of the surroundings of the patient (in illustration of our meaning it will suffice to refer to the fact that a valuable inference could be drawn from the fact that there were other sick persons in the house); and thirdly, from the objective signs noticed on the corpse. It may be noted in passing, that the fact that information can be gleaned from all these sources only if the inspection is done at the place of death would indicate the necessity of conducting the corpse inspection there and not, as has sometimes been proposed, at the burning ghauts and burial places.

16. The next point of general consideration which must be kept in view in connexion with corpse inspection is that, just as we must expect a percentage of error in the diagnoses arrived at by medical examination during life, so there must similarly be expected to be a percentage of error in the diagnoses of plague after death. In view of this, we do not think that diagnosis by corpse inspection ought to be discarded, as our colleagues would seem to suggest, any more than we think that diagnosis by examination of patients in life ought to be discarded, because it is, in many cases, impossible to arrive at a diagnosis as to the existence of plague by such an examination. And, in particular, in view of the fact that plague pneumonia is one of the most difficult diseases to diagnose in life, we cannot see that there is any reason for rejecting corpse inspection, on the ground that, in many cases, pneumonic plague cannot be diagnosed after death. Lastly, we need, perhaps, do no more than glance at the argument that has been advanced to the effect that diagnosis by corpse inspection will give, at best very uncertain information when it is committed to unskilful and uneducated classes of practitioners. This consideration will obviously apply with not less force to medical examinations conducted during life.

The fact that there will be a percentage of error not a reason for rejecting corpse inspection more than medical examination during life. Paras. 594–6 of this Report.

17. To come to the essential point, it cannot be denied that diagnosis by corpse inspection, like medical examination during life, if committed to medical men having a sufficient knowledge of their profession, will, on the one hand, lead to the detection of the large majority of cases of plague, and, on the other hand, will enable us frequently to differentiate suspicious cases from unsuspicious cases among the residuum of deaths.

Services which corpse inspection is capable of rendering.

18. It may, perhaps, be well here to detail the objective signs which seem to us to be of greatest importance in establishing the diagnosis of plague by inspection of a corpse.

Objective signs of plague in corpses.

In the large majority of cases of bubonic plague a diagnosis can readily be established, even after death, by noting (1) the presence of buboes, (2) the presence of œdema in the region of these buboes, (3) the occasional presence of a purpuric eruption or of patches of subcutaneous blood effusion over the buboes and under the conjunctiva, and (4) in certain cases the existence of plague carbuncles or plague blisters. Of less diagnostic importance, but still of considerable value, will be (5) the furred condition of the tongue, which, while comparatively clean round the edges, will generally be covered in the centre with brown or blackish fur, (6) the high temperature of the body when seen soon after death*, and (7) the comparatively well-nourished condition of the body.

In the case of septicæmic plague we have as objective signs, in most cases, only the appearance of the tongue, the high post-mortem temperature, the comparatively well-nourished condition of the body, and, possibly, evidence of the occurrence of hæmorrhages.

In the case of pneumonic plague we should probably have, in addition to the characters of septicæmic plague, also the presence of frothy blood-tinged sputum about the person, or in the neighbourhood of the corpse. Further, as the observations of Captain Elphick and Captain James show, œdema may in some cases be found on the front of the chest.

9544–9. 10,344–6.

19. We may now pass to consider the question of the attitude of the people towards corpse inspection. The evidence on this question has been reviewed by our colleagues in Section II. (2)(*b*) of Chapter VI., and it has already been shown that there has not in any single instance been serious opposition offered to the practice. We take it from the fact that our colleagues have omitted to refer to the case of the Bombay riots of March 1897, that, in their opinion, these riots were connected rather with other plague measures than with the proposal, which had at the time been discarded, that corpse inspection should be introduced. So far as the evidence on this matter has been placed before us, we are in agreement with what we take to be our colleagues' opinion.

Attitude of the people towards corpse inspection; review of the conclusions drawn by our colleagues. Paras. 587–92 of this Report.

* We are indebted to the courtesy of Captain Beveridge, R.A.M.C., for information with regard to a case of plague, in which he observed a post-mortem rise of temperature from 108° to 112° F.

In certain cases, however, we have, from the evidence placed before us, formed a different opinion from our colleagues. Thus, for instance, in the cases of Karachi, Hyderabad (Sind) and Kotri, we think it important to note, not only the fact that corpse inspection 'was not objected to,' but also the further fact that, after the panic which followed immediately upon the first outbreak of plague had subsided, the people
18,133. "were," said Colonel Henderson, I.M.S., "entirely with us." Similarly, in the case of Cutch Mandvi, we think it important to note that, although Major Hyde Cates did 'mention incidentally that the relatives very much object to their dead being examined,' this statement must be taken in connexion with the fact that the same witness also stated, in response to one inquiry, that he had "had no bother at all
13,550-1. with the natives," and, in response to another inquiry, that "the Muhammadans—the class who would be most likely to object—did not object at all." It has also to be taken in connexion with the fact that Captain Mason, R.A.M.C., who was engaged on
27,174-6. plague duty in Cutch Mandvi, stated that no opposition of any kind was offered by the natives to corpse inspection. Lastly, in connexion with Mr. Gidumal Lekhraj's statement that 'the people objected to corpse inspection, even although it is well-
15,521. conducted, "because it is against the Muhammadan and Hindu religions to touch a corpse after death,"' we think that it is well worthy of consideration whether the opinion of this native witness may not have been unconsciously influenced by the form of the question which was addressed to him.

Our own conclusions regarding the attitude of the people.

20. Just as we differ from our colleagues with regard to the conclusions which are to be drawn in these particular cases from the statements of the witnesses, so we differ from them also with regard to the deductions which are to be drawn from the whole body of evidence which was put before us with regard to the attitude of the people to corpse inspection. For our part we draw the following conclusions from that body of evidence with regard to the attitude of the people on the subject of corpse inspection:—

We are of opinion that there is nothing in the evidence which has been placed before us to show that there is anything in the practice of corpse inspection which is generally repugnant to native opinion. It appears to us that, in certain cases, at any rate, such a feeling is perfectly absent from the native mind. In support of this opinion we would refer to the experience of Lieutenant Steen, who was engaged on plague operations in the Satara district. That experience, as stated in answer to a question by one of our colleagues as to whether, "though the people may not have made any" actual opposition, there may not have been something in the practice "which was offensive to their feelings," is as follows: "No, I do *not* think corpse
22,748-9. inspection is offensive to their feelings. They show their corpses quite freely, both male and female. Before death, also, I have often examined both male and female patients, and have never noticed that my doing so was offensive to their relatives. The people are often quite callous as to what is done with their sick and dead." Again, Mr. Cadell, who had experience of corpse inspection in Karachi, when interrogated by one of our colleagues with a view to the elicitation of any facts which might point to the existence of a feeling of repugnance to the medical examination of the dead, and in particular to the examination of female corpses,
3805-7. replied as follows: "I do not think they regard the dead with anything like the same degree of respect that they do the living."

We are aware, of course, that it is not possible to generalise from experience in these cases, and we think that it is impossible to doubt that the people in many places may have disliked corpse inspection. But we take it that, where such dislike exists, it has not differed in anything from the dislike which was felt to all other plague measures which interfered with the people in any way. And if we may judge of the antipathy to a measure by the amount of active or passive resistance which has been evoked by it, we think that the feeling against corpse inspection has not been nearly so intense as the feeling against the compulsory removal of the sick to hospital, or against the segregation of contacts, or, lastly, as the feeling against evacuation in places where this involved either the interruption of trade or the taking of pardah women into camp. In support of this opinion, we may note that Lieut.-Col. Nariman, I.M.S., stated that in Surat, where corpse inspection was resorted to whenever the Vigilance Committee thought it necessary, the measures to which the
15,865. people objected most were "evacuation and segregation." The same opinion was expressed also by Assistant Surgeon Dadachanji who had experience in Bulsar, where
16,169-72. all corpses were examined. He was asked, "What do you think is the measure that
16,232. people object to most?" and replied, "From the Indian's point of view, separation of the sick is objectionable." Neither of these witnesses mentioned corpse inspection as

a measure to which the people objected. In addition to noting the above opinions we may note also that in the Punjab the people "did not object in the least anywhere," though corpse inspection was resorted to in a number of villages, and was "carried regularly on for some time" at Banga, a small town of 5,000 inhabitants, a place of which the people "were noted for being exceptionally troublesome," and where considerable opposition was offerred to the authorities in connexion with evacuation. While corpse inspection was thus carried on without opposition, and while we learn from Captain James' Report that the plague officers "frequently obtained permission to open glands after death for the purpose of bacteriological examination," the opposition to evacuation was in many cases extremely serious; in the case of Garkshankar, this opposition culminated in a riot, in which 9 persons were killed and 35 wounded. In comparison with this, all the objections which have been offered to corpse inspection would appear to be insignificant.

10,253.

10,235.

Capt. James' Report.

21. The only specific instances which have come to our knowledge of objections being raised to corpse inspection are the few instances enumerated by our colleagues in connexion with the inspection of the corpses of pardah women, one further such case at Sholapur, and one instance at Poona, where the body of a fakir was in question. The objections in the first class of cases were easily got over, in some cases by the employment of a nurse or a lady doctor, in other cases without a resort to such measures. The objection in the case of the holy man was readily got over by the employment of a Brahman doctor.

Specific instances of objection to corpse inspection and cases showing absence of antagonism to it. Para. 588 of this Report. 23,884, 21,373.

In addition to pointing out that the instances referred to are the only specific instances which have come to our knowledge of the raising of serious objections, we would further note that we have evidence that in certain cases, where the people have become familiarised with the practice, and especially, we may take it, where they have appreciated that the practice may save them from the institution of unnecessary disinfections, the native feeling has been rather favourable than antagonistic to corpse inspection. We take it from what we personally saw of corpse inspection in Poona, together with what we heard from the witnesses there, that this was the attitude of the people in Poona.

We have already stated that the people were, we were told, generally favourable to corpse inspection in Karachi, Hyderabad (Sind), and Kotri. Nor is evidence wanting that corpse inspection is not viewed with aversion by, at any rate, certain sections of the population in Calcutta and Bombay. We would point, in this connexion, to the fact that corpse inspection was often, as Dr. Nield Cook tells us, permitted when the Medical Officers went to make inquiries about cases of suspicious death. In the case of Bombay, we would point to the fact that Mr. Macdonald, who has already been quoted by our colleagues, found that the natives did not object to their corpses being examined by him. He said, "My experience is that they are quite willing that a European should do it, but they do not like dark persons to examine them if they are of another faith." With regard to this last statement, however, we would point out that corpse inspection was carried out without objection by Hospital Assistants, not only in the several places enumerated by our colleagues, but also in the towns of Hubli, Bhiwanda, and Bhagda. We take it that these Hospital Assistants must have examined the corpses of many persons "of another faith," and in some cases they are specifically stated to have examined female corpses.

6514.

17,968.

Para. 595 of this Report. 3077, 22,185–8, 16,199.

We may note that it is difficult to suppose that the objection to corpse inspection can be greater in India than it is in a Muhammadan country like Egypt, where corpse inspection has been carried out most efficiently for the last half-year without hitch or difficulty. In Alexandria, for instance (and the population of Alexandria is notoriously turbulent), no one is now buried without having been seen either during life or after death by a qualified medical man.

22. Having now, so far as we can, summarised what seems to us to be the real teaching of the experience of the past with regard to the attitude of the people towards corpse inspection, so far as it has been disclosed to us in the evidence laid before us, we may proceed to consider the question of the general expediency of the measure. We do not propose here to consider the question of the expediency of resorting to corpse inspection in the cases where plague is present, or where it is suspected to be present, in villages and in smaller towns. For we take it that, given that the necessary staff is available, the expediency of resorting to corpse inspection in such places will not be in question. What we do propose to consider here is merely the question of the expediency of resorting to the practice of corpse inspection in the larger towns. In connexion with this we have to refer again to the expediency of taking the necessary measures to secure that the body shall not be carried out from the place of death until the death has been reported and a permit to remove it has been issued.

The expediency of corpse inspection in large towns.

Para. 596 of this Report.

Our colleagues have, as has been seen, declared themselves to be of opinion that corpse inspection is, in the case of large towns, and in particular in the case of Calcutta and Bombay, an impracticable measure. They recommend that no change should be made in the present confessedly very defective system of death registration.

The opinion of Surgeon-General Cleghorn, late Director-General of the Indian Medical Service, is practically identical with that of our colleagues, unless perhaps in the fact that the advantages of corpse inspection appeal more strongly to him. His opinion will be gathered from the following extract from our proceedings:—

26,732. *Q.* "Do you think that infective disease will never be successfully combated until you get a system of death registration?"

A. "That is so."

26,733. *Q.* "And until you ascertain the causes of death?"

A. "Yes, but that is practically impossible."

26,737. *Q.* "Do you think it would be possible to pass laws and to make it penal to move or dispose of corpses before they had been examined?"

A. "I think it would be very difficult to do that."

26,738. *Q.* "Do you think it is only playing with plague to attempt to deal with it without introducing corpse inspection?"

A. "That is the cardinal measure before sanitary and preventive measures, if you can get it. Of course, the difficulty was how we could do it."

Similar is the opinion of Mr. Lund, a merchant of Bombay, who also has been quoted by our colleagues. Mr. Lund, while he thought that more cases of plague might be detected by corpse inspection, was of opinion that it would be inexpedient to resort to this measure. The mental attitude of this witness may be gathered from the fact that he was of opinion that it was like "ploughing the sand" to attempt to deal with plague in Bombay, "one can, after all, only do one's very best, and trust to Providence for the result. . . . The quieter you keep the people the better."

17,886.

17,887.

Our colleagues have further quoted Dr. W. Venis as being of opinion that corpse inspection could not be adopted in Bombay. A reference to the evidence will show that Dr. Venis qualified his opinion, and that he stated that he had had no experience of plague in Bombay.

19,924; 19,928.

Major Ross is further cited by our colleagues as being of opinion that corpse inspection could not be adopted in Bombay. A reference to the evidence will show that although this is Major Ross' opinion, the witness who had a wide experience of plague operations both in Bombay and in Poona was so impressed with the fact that "plague is spread only by the undetected cases," that he has insisted on the necessity of every death being reported before the corpse is carried out, and further on the expediency of widely extending the system of death certification by providing a sufficient medical staff to visit all the sick who are not attended by private medical men. Under the scheme which is proposed by Major Ross, every house, in which there occurred a death which had not been certificated, would, as a matter of course, be promptly disinfected.

10,986–94.

In connexion with the question of the adoption of a system of corpse inspection in Bombay, our colleagues finally quote the opinion of Mr. Du Boulay, I.C.S., who was in charge of the plague operations in Bombay, under the Municipal Commissioner. Mr. Du Boulay is quoted by our colleagues as thinking 'that more complete information *might* (the italics are ours) be obtained from corpse inspection, but that it would cause much opposition.' In view of the ambiguity of the auxiliary verb employed in the first clause of this quotation from our colleague's Report, it may be well to quote from our proceedings the exact questions and answers which our colleagues have in view. These are as follows:—

25,618. *Q.* "Could you not receive more complete information than you do at present by a system of corpse inspection?"

A. "Undoubtedly."

25,619. *Q.* "Do you think corpse inspection would arouse much opposition?"

A. "Undoubtedly it would."

25,620. *Q.* "Do you think it would arouse less opposition if conducted at the graveyards, or burning ghauts, than if conducted at the houses?"

A. "I think it might be conceivable by very very slow processes to arrive at a certain amount of compulsion that they should be examined in the houses, but it would be a very slow process, and probably take a couple of years to work up to."

25,621. *Q.* "Do you feel inclined to think that a system of corpse inspection should be gradually introduced; do you think it would be wise and that it would be an advantage to introduce it?"

A. "Yes."

The opinion of Dr. Nield Cook, showing within what limits he thought it would be expedient to introduce it in Calcutta, has already been quoted by our colleagues. So has the opinion of Mr. Winter. This, again, is, in our judgment, distinctly favourable to a resort to corpse inspection, in the case of plague-infected cities in Northern India, provided always that this were done with tact. 6605-7. 9247i

23. We for our part are, as we have already said, convinced that it is essential to introduce corpse inspection into the large towns if it is seriously intended to deal effectually with plague there. We have also, after weighing as carefully as we can the evidence which has been laid before us, arrived at the opinion that it is expedient to resort to a general measure of corpse inspection in the case of large cities when seriously infected with plague, and, in particular, that it would be expedient gradually to work up to the adoption of such a measure in Bombay. We have gathered from the evidence which has been placed before us—and we have especially sought information on this point from the Bombay Government—that the resistance which has been actually experienced on introduction of the measure has always been much less than appears to have been anticipated. We have gathered further that wherever objections have been raised to the practice, these have always been dispelled by providing a lady doctor or a nurse where the objection happened to take the form of an objection to the examination of female corpses, or, when it took a more general form, by providing facilities for the sick being seen in life with a view to death certificates being provided by doctors officially appointed for those who are not attended by private practitioners, or, lastly, when the difficulties could not be met in this way, by treating the case as a suspicious one and by disinfecting the house. Recommendations.

When we pronounce ourselves satisfied of the expediency of introducing corpse inspection and of carrying it out as a general measure in a town where the loss of life is as great as it has been, and still is, in Bombay, we do not propose that corpse inspection should be carried out at once as a general measure in towns like Calcutta, where plague has up to the present shown itself to be less dangerous to the Public Health than many other of the ordinary infective diseases. What we would, however, insist upon as being in our opinion absolutely essential is that, in every town which is or shall in the future be provided with a European Medical Officer of Health, a system of death registration should be introduced which should be effectual in locating every death. Only when this shall have been done will it be possible to take effective measures in the form of corpse inspection on the outbreak of a serious epidemic of plague or other zymotic disease.

24. We hold that to appoint Medical Officers of Health, and leave them unprovided with the means of obtaining the information which is to be a basis of all measures taken against infective disease, is to adopt a policy of appointing Medical Officers of Health who will necessarily be powerless to arrest the spread of epidemic disease. We hold, in particular, that where the power of resorting to corpse inspection is withheld from the Medical Officer of Health, that Medical Officer ought, *ipso facto*, to be entirely relieved from all responsibility for the success of the measures taken, unless perhaps in cases where he is authorised to disinfect every house in which a death occurs. While we recognise that there may quite well be special circumstances where, owing to the fact that other interests may be jeopardised which are more important even than those of the Public Health, it will perhaps be necessary occasionally to withhold the power of corpse inspection, we would point out that, until corpse inspection, in default of medical certificate, is introduced, there is only too much reason to believe that most of the money spent on plague measures will be wasted. Further, we think that, given the case that plague were to cease to be epidemic in Bombay, Calcutta or any other sea-port town, it would be necessary, failing the reform of the present system of death registration in these towns, to continue to enforce the present regulations for the prevention of the spread of plague oversea, and that for a very prolonged period. We do not see how, on a system under which over three-fourths of the dead are disposed of without being seen by a medical man, it would in the future be possible to declare a particular port to be free from plague. Concluding remarks.

A. E. WRIGHT.
M. A. RUFFER.

APPENDIX V.

NOTE BY THE PRESIDENT REGARDING THE EFFICIENCY OF MEASURES OF DISINFECTION WHICH HAVE BEEN ADOPTED, WITH A STATEMENT OF THE MEASURES RECOMMENDED BY HIM FOR DEALING WITH PLAGUE, DISAGREEING WITH CERTAIN OF THE CONCLUSIONS ARRIVED AT BY THE MAJORITY OF THE COMMISSION.

A.—Instances advanced in proof of the preponderating value of disinfection by chemical substances are insufficient to do so.

The evidence adduced is insufficient to show that chemical disinfection has had a preponderating effect in destroying plague infection. It was impossible effectively to control chemical disinfection in the Punjab, and chemical disinfection there was not alone trusted to, but was accompanied by other measures. Capt. James' Report, pp. 111, 16, para. 36 and 37; 116; 112.

1. The President is unable to agree with the opinion expressed in paragraph 671 of the Report, that where skilled and efficient control was exercised in the operations of disinfection, as under the supervision of Captain James, I.M.S., of Col. Fawcett, R.A.M.C. and Major Ross, D.L.I., and of Mr. Winter, I.C.S., there is little doubt "that chemical disinfection exerted a preponderating influence."

2. In Captain James' Report on the outbreak of plague in the Jullundur and Hoshiarpur districts of the Punjab, it is stated that the strength of the phenyle emulsion used in the disinfection operations was determined, not by accurate measurements, but by adding phenyle to water until "a dense white fluid looking like milk was obtained." The disinfectors were "for the most part village menials"; it was only to a very small extent practicable to employ permanent disinfectors; "the immediate supervising agency consisted ordinarily of Hospital Assistants"; and Captain James has observed that "when the Inspecting Officer came on his rounds, a few hours after the disinfectant had been sprayed on the buildings, not a sign of it could be seen, so that there was practically no means of verifying the fact that the process had actually been performed." Chemical disinfection, further, was not alone trusted to, but was accompanied with total evacuation, which was "carried out on a very large scale in the Jullundur and Hoshiarpur districts," and with the making of large ventilation holes in the roofs of all non-infected houses and the removal of the entire roof of infected houses.

Chemical disinfection in Poona and Kirki accompanied by opening up houses and other measures. 21,289; 21,290. 21,251–253.

3. In Colonel Fawcett's description of the method of disinfection followed in the Poona and Kirki Cantonments, it is stated that "every house in which a case occurred had the roof or a portion of it removed," and all rags and rubbish were burned before the house was chemically disinfected, while a considerable number of the houses were demolished. Further, when plague broke out in several Native Regiments, they were removed into camps on new ground, without any disinfection, and the outbreak soon ceased. The instance in which special care was taken in the disinfecting operations appears from the evidence to have been that in which several cases of plague re-appeared in the Transport Lines among the followers and their families, after they had been removed to camp because of an outbreak of plague. The camp was thereupon transferred to a new site a few yards distant, "each tent was sprayed with perchloride of mercury solution, then carried across the road and put up; the followers and their families, including even their cats and dogs, their cocks and hens, were carefully disinfected before being allowed to cross the road and re-occupy their tents." Although the experiment is stated to have been eminently successful, two cases of plague subsequently occurred. It may be added that Colonel Fawcett disclaims responsibility for a part of the extreme application of chemical disinfection adopted in this instance.

21,254. 21,257. 21,254–5.

Major Ross, Durham Light Infantry, who is also referred to, stated, that in Poona, disinfection with non-acidified perchloride of mercury was at one time inefficiently, but latterly efficiently, carried out, and, in his opinion, it became absolutely effective from that time. This chemical disinfection, however, was conjoined not only with the removal of the patients and all contacts from the infected houses, and with the evacuation of considerable adjoining areas, but also with disinfection by sunlight and free access of air, for "the roof was taken off to let the sun in."

10,930. 10,914–16. 10,921. 10,933.

Soon after Major Ross assumed duty in the Poona Cantonment, it was found by Colonel Fawcett that the solution of perchloride of mercury which was being used in the disinfecting of houses and clothes had been made from an impure preparation containing much insoluble matter. The error was corrected, and the mixing of the solution was afterwards "personally superintended" by Colonel Fawcett; and this, apparently, constituted the conjoined action of Colonel Fawcett and Major Ross, referred to in paragraph 671. It is to be noted that the corrected and approved solution of 1 in 1,000 of perchloride of mercury was made without the addition of any

21,305–10. 10,930. 10,931.

acid, although it has been indicated, on experimental data, that such a solution is not only "absolutely useless" when applied to earth and cowdung floors, but may lead to an "apparent increase" in the number of microbes by adding to the floor the water of the solution, while the perchloride of mercury is itself neutralised by many of the constituents of the floor. 8555–4; 8580; 18,828 Para. 652 of our Report.

Major Ross, however, was unable to say if any cases of plague occurred on the re-occupation of the chemically disinfected houses from which patients and contacts had been removed. On the other hand, in Igatpuri, where a similar error was committed, and is instanced in paragraph 670 as an example of the failure of chemical disinfection resulting from the employment of too weak a solution of perchloride of mercury, there is clear evidence that the substitution of a solution of the approved and intended strength (1 in 1,000) did not produce any beneficial effect on the progress of the outbreak. The solution of insufficient strength (1 in 10,000) was used during the greater part of August 1897, and the plague attacks and deaths were 123 and 85 respectively. The solution of approved strength was used during September, and in that month the plague attacks were 292, and the deaths 251. Apart from the mistake in the first preparation of the solution of perchloride of mercury, the disinfecting operations appear to have been conducted with much care: "A disinfecting party, under an Assistant Surgeon, who had been working at disinfecting work in Bombay, was obtained at once, and thorough disinfection with corrosive sublimate solution was carried on." This case is specially instructive in so far that no other plague measure but chemical disinfection was adopted, on "the theory of disinfecting the house, its inhabitants, and their belongings, and allowing them to re-occupy the house at once." The result was disastrous; and plague continued to extend until complete evacuation of the village was carried out, before which several chawls "had been disinfected four or five times, and still cases occurred" in them. 10,937. 23,317. 23,196.

4. Mr. Winter, I.C.S., deals in his evidence with the Hardwar district, in which the towns of Hardwar, Kankhal and Jawalapur, and several villages, were affected with plague. In the towns of Hardwar and Kankhal the houses are well built and well ventilated, and overcrowding is prevented by special regulations, enforced by frequent inspections by the authorities. They are, therefore, exceptional among Indian towns, in so far that the opening up of the houses to sunlight and air was not required as a plague measure. Evacuation and prolonged vacation of houses the principal measures adopted in the North-Western Provinces. 9005–7. 9013–5.

The Hardwar outbreak was a small one, only 18 cases having occurred. On the occurrence of the first cases, isolation of the sick and the contacts with disinfection of the houses was adopted; but as these measures proved unsuccessful, compulsory and voluntary evacuation was superadded, and carried to such an extent that only 500 people, out of a normal population of from 4,000 to 5,000, remained in the town. It was only while the town was thus nearly emptied of its inhabitants, and actually after the outbreak had ceased, in the hot weather of June, that disinfection of all the unaffected houses was carried out. Hardwar. 9064. 9025–62. 9045–50.

In Kankhal, after the occurrence of three cases of plague, and of many deaths among rats, an affected area was evacuated, and all the houses in it were disinfected with perchloride of mercury. These measures, however, proved ineffectual, and ultimately, after continued failures following nine or ten subsequent partial evacuations with disinfection of the unoccupied houses, the whole town was evacuated, and soon afterwards the outbreak ceased. Further, the inhabitants were not allowed to return to their houses until $3\frac{1}{2}$ months after the total evacuation had been accomplished. Kankhal. 9090–9113. 9102, 9117.

In Jawalapur, also, the earlier measures were evacuation of successive areas and chemical disinfection, but as cases continued to occur an attempt was made "to stop the disease by the thorough disinfection of the whole town." This also proved unsuccessful—it is incidentally stated that one case at least occurred in a disinfected house—and complete evacuation was then adopted. "On the whole it was satisfactory," although the evacuation was a voluntary one, and the people built their own camps and overcrowded the huts. Jawalapur. 9175. 9173–4. 9176. 9178.

In the affected villages of the Hardwar district, the organisation which was employed succeeded, in most of the villages, in discovering the first, or "at any rate one of the very earliest cases in each village." In each of these villages a large area was evacuated and the infected houses were destroyed by burning them, and outbreaks were thereby prevented from occurring. In two villages in which the organisation was not so successful, and where several cases of plague were simultaneously discovered, complete evacuation was at once adopted: and in one of the two villages a few cases occurred in the camps within a few days after the evacuation, and the outbreak then ceased; and Villages in the Hardwar district. 9134. 9143. 9167.

9168. in the other village, cases continued to occur for a longer period before the outbreak ceased, owing to a failure in preventing intercourse with plague patients, and notwithstanding scrupulous care in disinfecting the clothes and other possessions of the patients with solution of perchloride of mercury.

Banga freed from infection by evacuation and prolonged vacation of infected houses rather than by chemical disinfection.

Capt. James' Report, pp. 30–34.

5. The experience of Banga, in the Punjab, is cited (paragraph 671) in support of the opinion that disinfection with chemical substances might succeed in destroying the infection of plague, irrespective of the influences exerted by the opening up and prolonged evacuation of houses, in cases where all these measures were used in combination. The method employed in preparing the solution of phenyle used in Banga has already been described (paragraph 2, *ante*). The town of Banga was partially evacuated on the discovery of the first case of plague, and the infected houses were disinfected; but as the disease continued to spread, even when these measures were repeated on three successive occasions during the months of March and April, the town was finally completely evacuated. As the re-occupation of the evacuated houses was not permitted until two months after the date of complete evacuation, the disinfection to which the houses had been subjected cannot be credited with the freedom of the inhabitants from further cases of plague. Although Captain James reports that "the usual ventilation holes" could not be made in some of the houses, and the authorities "had to trust to a plentiful application of disinfectants and limewash," and although ventilation holes were not made in the larger houses, which were already well lighted and were free from overcrowding, the long period during which they and all the other houses in Banga were left unoccupied prevents any conclusion from being arrived at regarding the value of the chemical disinfection to which they had been subjected. It is to be recollected that the words "in some of the larger double-storied infected houses in Banga, where we could not get direct sunlight through many of the rooms, and where the walls were made of rough brick, which did not take the lime-washing, we had to trust entirely to phenyle lotion made as here described. The result was quite satisfactory," relate to the limitation of the disinfection to the employment of chemical substances, as contrasted with the method, nearly always practised in Banga, of combining the disinfecting influences of chemical substances with those of sunlight and air, and that in the few cases in which, apparently, the disinfection was only by chemical substances no attempt is made by Captain James to eliminate the effects on the spread of the disease caused by prolonged evacuation of the houses. Besides, inoculation was adopted at the commencement of the outbreak, and a large number of the inhabitants had been inoculated before the complete evacuation of the town.

Capt. James' Report, p. 30.

Capt. James' Report, pp. 111–112.

10,172.

Criticism of case of Poona, and Major Ross' opinion regarding disinfection.

6. The statement regarding Poona (paragraph 671) is not in itself complete, as it does not assert that people returned with impunity to their houses after the houses had been vacated for only ten days. On the other hand, there are many cases which show that occupation within so limited a period after disinfection with chemical substances has been attended with the most unfortunate results, and several of them have been cited in paragraph 632 of our Report. The experience gained in the Cantonment and Suburban Municipality of Poona—the parts of the City in which Colonel Fawcett and Major Ross directed the plague operations—is, however, unambiguously opposed to the practice of disinfecting houses and allowing the inmates to return to them ten days after the houses had been vacated. In a report submitted to the Government of India by the Committee under the chairmanship of Major-General Burnett, his experience is thus stated: "As the disease increased in virulence, the disinfection and evacuation of a house for ten days only was found to be quite useless. The houses still retained the infection and as sure as the occupants returned they were attacked by plague within a few days." Although more than 2,000 houses were disinfected, and although the strength of the solution of perchloride of mercury was increased from 1 in 1,000 to 1 in 500, the results were "very disappointing"; and the Committee report that "it seems proved that, after the most careful disinfection, it is quite unsafe to allow a house to be re-occupied after a period of ten days."

Report on Plague Operations in Poona Cantonment and Suburban Municipality.

In paragraph 671, also, the reply of Major Ross to Question 10,972 is adduced in corroboration of the opinion "that the freeing of the houses from infection might have been achieved independently of these measures," namely, "the opening up or the prolonged vacation of the houses." The question and reply, however, deal primarily with the relative values of lime and of perchloride of mercury in preventing the spread of plague when all the houses were systematically treated "through the Cantonment, irrespective of cases, taking street by street." The great majority of the houses, therefore, did not contain plague infective material, and the great majority, also, had

10,972.

not been opened up to light and air, for this was done only in houses in which plague cases had occurred. As no distinction is made between the effects observed in the several groups of houses, this evidence seems inadequate to support the opinion "that chemical disinfection exerted a preponderating influence in destroying the infection." Report, para. 67

7. The opinions expressed in paragraph 671, to which objection has thus been taken appear, therefore, to convey a very exaggerated estimate of the value in India of chemical disinfection as a plague measure, and an estimate which receives no support from the evidence that has been adduced in its favour. Chemical disinfection has never, in itself, proved sufficient to arrest the progress of the disease, for it has failed wherever it has alone been employed, as in Bombay, Parel village, Malabar Hill, Lanauli, Satara, Nasik, Igatpuri, Poona, Ankleshwar, and Jawalapur; whereas the opening up of houses and evacuation, without any disinfection by chemical substances, has in numerous instances proved successful, as in Sukkur, and in many villages in the districts of Dharwar, Surat, Sholapur, and elsewhere. The exclusive application of chemical disinfection is, however, doomed to failure, as it does not provide for the special difficulty, in dealing with plague, caused by the extension of the disease which is effected by rats. At the same time, disinfection by chemical substances is occasionally the only form of disinfection that can be adopted in several houses, but even in these instances it cannot alone be trusted to, and should be combined with prolonged evacuation. It is probably useful as an auxiliary to disinfection of houses by other agencies, and especially by sunlight and air, for it is not unreasonable to suppose that a solution of an efficient disinfectant would prove more serviceable than water alone in reducing the dangers that are incurred during the opening up of houses; and there is sufficient evidence to show that disinfection by chemical substances is the only convenient method of freeing from plague-infection many of the personal effects of patients and other articles in infected houses.

Chemical disinfection, though occasionally useful as an auxiliary measure has never by itself proved sufficient to arrest plague. 20,439. Gatacre's Report, p. 190. 18,189–99. 20,158; 20,184–190; 20,197–202 22,062; 22,137 16,887–891. 18,427; 19,455–64. 21,614. 14,412. 9175. 9; 2145–6. 23,163–8. 23,187–8. 19,953–6. 24,036–40. 8.

B.—The opening up of houses inadequately provided with means for ventilation is a valuable plague measure.

8. The President considers that the fact that openings in the roof were purposely made so as freely to admit sunlight into rooms has not adequately been recognised in paragraph 672 of the Report, and especially in the sentence, "Thus the question of the utility of knocking holes in the roofs and walls resolves itself into a question as to the utility of these openings for bringing the agency of desiccation effectually into operation." It appears to be assumed in the argument against the making of openings in the roof, by untiling or by other means, that the openings were in most cases insufficient for the purpose of admitting sunlight, whereas the contrary is stated in many of the descriptions of the manner in which the openings were made. In confirmation of this statement it is necessary to refer only to the following. Capt. James, I.M.S., describes the holes made in the roofs as being "large enough to admit plenty of sunlight and air into the room below." "An opening not less than 6 feet by 4 was not allowed, and often much larger ones were made in every house in the village." They were so made as to "allow the direct rays to act on the largest portion of the room." Further, referring to houses which were not entirely unroofed, he states that, "owing to the great variation in size of the rooms it is impossible to give any definite dimensions applicable to all cases, but the strip should certainly never be less than 4 feet wide, and in large rooms it may be necessary to remove two or more strips. The only way of seeing whether enough of the roof has been removed is to visit the house at different times of the day, and to notice whether, at one time or another, the sunlight reaches every part of the room." It was the practice in the Mysore State to make such openings in the roof, supplemented generally by openings in the walls, as would ensure the access of direct sunlight to all parts of the floor and to a large part of the interior walls. At Ahmednagar "openings were made so that the rays of the sun could penetrate into the rooms"; at Satara a portion of the roof, about 3 feet broad, was taken off; in the towns and villages in the Cutch State, the tiles were removed about a yard in width, sometimes in two parts of the roof, "and a strong light let into the rooms"; in the villages of the Dharwar district the roofs were "opened by removing the earth and lifting up the bamboos, the whole length of each room for a width of 2 feet;" and in the Sirohi State pretty nearly the whole roof was untiled. Further, the intensity of the sunlight in India and the large daily amount of it during the greater part of the year must be taken into account.

The opening up of houses often done efficiently, so as thoroughly to bring the agencies of sunlight and air into effect. Capt. James Report, p. 112, Major Inglis' Report, pp. 33, 155, 162, 171 Capt. James' Report, p. 169. 24,274. 22,157. 13,474. 23,163. 9,911.

The opening up of houses not an unnecessary measure.

9. It is also stated in paragraph 672 of the Report that the openings made in the houses were unnecessary for bringing the agency of desiccation effectually into operation. While it is undoubtedly the case that openings in the roofs and walls have been so made as to allow of the disinfecting agency of sunlight to come into operation, it does not appear justifiable to assume that they were not also of great value in furthering desiccation. In the great majority of the houses in which plague has occurred, windows were practically absent, and, where present, they were usually represented by small openings, sometimes described in the evidence as mere "slits" in the outside walls. Many of the houses were one-roomed, and could be ventilated only by admitting air through the open doorway. Others contained three rooms, one of which usually depended for its air supply upon the doorway opening into it from a dark and practically unventilated chamber. The floors of the rooms most frequently consist of earth and cowdung placed directly upon the soil, and therefore of materials retentive of moisture, which is supplied to them not only by the water placed on the surface during, for instance, the customary weekly or bi-weekly application of cowdung and water, but also continuously by imbibition from the underlying ground, which in many parts of India, owing to the natural condition of the soil and also to the absence of drainage, contains a large quantity of water. Further, plague bacilli, in the conditions in which they would naturally occur outside of the body, are not easily dried so completely as to be devitalised.

Appendix III., sections 1, 11, and 111, *ante*.

Evidence, II., pp. 8, 9, and 13. 8552; 18,886; 26,344-5.

It is therefore extremely unlikely that, unless the roofs, and often also the walls, were opened up in native houses, unprovided with outside openings sufficiently large to allow of a free circulation of air in the rooms, dryness of even the upper surface of the floors could be produced otherwise than so slowly as to render highly inconvenient and dangerous, if not impracticable as a plague measure, a process of disinfection which partly depends for its success upon the desiccation of the floors.

Evidence, II., pp. 13, 8-10, 8552. Annales de L'Institut Pasteur, 1887. Proceedings of the Royal Society, LVI., 1894, p.318.

At the same time, it is not to be overlooked that the disinfection, which is so undoubtedly effected by the opening up of houses, is most probably caused not only by desiccation, but also by oxidation, which depends for its accomplishment upon free access of air as well as of light. In connection with this, it may be pointed out that Professors Percy Frankland and Marshall Ward, confirming the previous observations of Roux, have demonstrated that microbes exposed to sunlight are destroyed more quickly when the access of air to them is free than when it is obstructed, and that the exposure of the spores of *Bacillus anthracis* to sunlight *in vacuo*, and therefore without air, "results in no perceptible retardation or destruction of the bacillus, whereas if exposed in contact with air nearly all the spores are killed in the time given."

C.—Results of opening up of houses of greater efficacy than their disinfection by chemical substances.

The opening up of houses recommended as a plague measure. 7855-6; 15,179-81; 20,161; 20,164; 21,153; 21,156; 24,316; 24,342. 1680; 7862; 11,273; 21,097; 23,187-9; 24,448-50. Our Report, para. 650.

10. The President is unable to agree with the opinion implied in section (II.) of paragraph 680 of the Report, that the opening up of houses is not necessary as a measure of disinfection—an opinion which is repeated in section (VI.) of the paragraph. It was frequently stated in evidence that the construction of a large proportion of the native houses was such as to render effective disinfection by chemical substances an operation of so great difficulty as to be practically impossible, and that the providing of sufficiently skilled and constant supervision was unattainable. It is not to be overlooked, also, that the probabilities of success by the use of solutions of even the most powerful disinfectants are dependent upon the supposition that the large majority of the plague bacilli are found only upon the surface of the floor, whereas the relatively porous nature of the materials of earth and cowdung floors, and the conditions to which their surfaces are subjected, are against the supposition. Further, the chemical composition of these, and likewise of concrete floors, is such that the effective ingredients in even strong and acidified solutions of perchloride of mercury would nearly instantaneously be neutralised by contact with them. The assumption that plague bacilli are almost only upon the surface of floors does not, however, greatly strengthen the probabilities of their being devitalised by disinfecting solutions, for it is not the case that the bacilli are generally free or naked, but rather, for the most part, incorporated with sputum and excreta, the constituents of which would protect them from contact with the disinfectant, whose action upon these constituents would, indeed, result in the formation of a surrounding covering, nearly impermeable by the disinfectant. Further, if any bacilli were below the immediate surface or were actually upon the surface but covered "by leaves, bits of rags, &c." they "could hardly be hurt at all," by eight gallons, applied to a 10 by 10 feet surface, of even so strong a disinfectant

18,862.

as an acidified solution of 1 in 725 of perchloride of mercury. Even supposing that these serious difficulties could be overcome, there remains the important fact, introducing an exceptional complication in disinfection for plague, that the room or house is liable to, and very frequently does, contain infected rats, which burrow in the walls and roofs and obviously are not affected by chemical disinfection. However effectually, therefore, the process of chemical disinfection may have been carried out, the house remains as dangerous for occupation after the process as it was before it, for the infected rats would enter the rooms and re-infect them on the first opportunity. It is probably because of this that chemical disinfection alone has so generally failed, while the providing of openings sufficiently large to admit air and light into each room, conjoined with the removal of the inmates from the houses for a period of several weeks, has proved so successful. In so far as adequate unroofing, whether complete or partial, of a house unprovided with a sufficiency of outside openings is a necessary part of the process of disinfection by the agencies of light and air, it does not appear to be in accordance with the evidence to characterise it as "a needlessly drastic measure." The difficulties connected with the efficient application of chemical disinfectants, and the uncertainties regarding the strength of the solution or mixture of any of them required to insure the death of plague bacilli in earth and cow-dung floors, give further support to the value and importance of utilising the disinfecting actions of sunlight and free access of air, which experience has amply shown to be effective for the purpose.

11,460-2; 22,125; Capt. James' Report, pp. 95-100.
11,202-5; 22,693; 22,716; 22,864; 25,794.
1220; 18,189-90; 22,893-7; Campbell's Report, p. 206; General Burnett's Poona Report, p. 2; James' Report, p. 162.
8558-8563; 8578; 18,838-42; 18,862; 26,350-2, 26,369
Our Report Chapter VI., para. 680 (iv.).

D.—Statement of the measures recommended by the President for adoption in towns and villages.

11. In connexion with the above expression of his opinion, and with reference to the policy which has been recommended in paragraphs 726, 727 and 728 of the Report, the President considers that a brief and definite statement of the measures applicable at the beginning and at a later stage of an outbreak of plague, in places of large and small size, respectively, would with advantage be added to the above. In his opinion, the measures may thus be stated:— Summary of plague measures.

A.—In Large Towns, including Cities and Towns of more than 10,000 Inhabitants.

(a.) *At the beginning of the outbreak.*

1. Removal of all plague patients to hospital, except in the very rare cases in which a patient with one or two attendants can be effectively isolated in a sufficiently capacious and well ventilated and lighted room;
2. Isolation of contacts;
3. Evacuation, for at least one month, of a considerable area in proximity to or surrounding the house or houses in which the first cases had occurred;
4. Disinfection by chemical substances of the houses and effects of patients;
5. Disinfection by exposure to sunlight and air of the interior of infected and evacuated houses, wherever required and practicable, and of such articles as cannot without injury be subjected to disinfection by chemical substances;
6. Wholesale destruction of the rats in the city or town, and if mice are numerous, of them also;
7. Inoculation with prophylactic fluid of as large a number of the unffaected as possible, with the inducement of shorter residence in the evacuation camps of those who become inoculated.

Provided that the first cases are discovered soon after their occurrence, it is highly probable that the outbreak would be suppressed by these measures.

(b.) *At a later stage of the outbreak.*

If, unfortunately, the discovery of the first few cases has not been effected, and the failure of the first five of the above measures would itself suggest this, it may be anticipated that the disease has extended by human communication, and, even more disastrously, because less under control, by communication through infected rats. The prospect of suppressing the outbreak must then be regarded as doubtful, unless the resources at the disposal of the Government and the co-operation of the people render it possible to evacuate the whole city or town. As, however, this is improbable in places which contain considerably more than 10,000 inhabitants, efforts should be made to limit the extension of the disease in the city or town by all or as many as

possible of the measures stated under 1, 2, 3, 4, 5, 6, and 7 above, and to limit or prevent the extension into other places by every practicable means. Although the extent of the outbreak may thus be restricted, its extinction cannot confidently be anticipated, and, therefore, it is of supreme importance that the measures recommended for the earliest cases should be strictly and completely carried out at the commencement of the outbreak, while there is yet a reasonable prospect of complete success.

B.—In small Towns and in Villages.

The suppression of an outbreak of plague in a small town or a village is not attended with the great difficulties which are encountered in cities and large towns, nor, in so far as the mere suppression of the disease is concerned, is it necessary to draw a broad distinction between the earlier and the later stages of the outbreak. While, for obvious reasons, it is desirable that early information of the first case or cases should be obtained, so that the measures may be applied as soon as possible, these measures in all stages of the outbreak should be as follows:—

1. Isolation of the patients and of the contacts;
2. Complete evacuation of the town or village for at least one month;
3. Disinfection by opening up to the influence of sunlight and air of all the evacuated houses;
4. Disinfection by chemical substances of the personal effects of the inhabitants before their removal to the evacuation camp;
5. Inoculation of as many of the inhabitants as possible;
6. Destruction of rats, and if mice are numerous, of them also.

If the first four of the above measures are efficiently carried out, it may confidently be anticipated that the outbreak will be suppressed. At the same time, all measures that can be applied for preventing the extension of the disease into other places should also be adopted, and the President thinks that experience has shown that the most effective of them are the regulated supervision of the evacuated and the securing for them of properly constructed huts in a well arranged and situated camp.

T. R. F.

SUMMARY

OF THE

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RETURN showing Information regarding PLAGUE under the following Headings for the Week ending 22nd April 1899.

Division.	Population by Census of 1891.	Average Weekly Mortality.	Actual Mortality for the Week.	Number of Plague Cases Discovered.			Number of Patients removed to Hospitals.	Suspicious Cases not Definitely Diagnosed as Plague.	Number of Persons under Observation in Private Quarters.	Houses Inspected and Deaths Inquired into,		Premises Disinfected.	Premises Vacated.	Remarks.
				Attacks.	Deaths.	Total.		Number of Cases not Definitely Diagnosed as Plague.		Houses.	Deaths.			
1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	12.	13.	14.	15.
Address uncertain	—	—	—	3	—	—	2	—	—	—	—	—	—	
District I., North—														
Ward No. 1	36,885	32	25	3	3	—	1	12	—	58	22	25	—	
Do. 2	26,614	20	19	6	4	—	—	3	—	28	17	15	—	
Do. 3	36,431	38	35	11	9	—	1	12	—	62	33	31	—	There were 83 deaths this week from plague against 118 last week. The general mortality was only 34 in excess of the 5-yearly average. The excess has not been so small since the beginning of March, and since March 10th there have not been so few cases in one week. Wards 5 and 6 still show a disproportion between the amount of plague and the general mortality. Wards 8, 9, 10, and 25 show high death-rates, but little plague. The decrease in plague noticed last week has continued.
District I., South—														
Ward No. 4	34,828	28	35	5	6	—	—	—	—	32	32	18	—	
Do. 5	39,180	33	31	8	8	—	1	—	—	39	39	17	3	
Do. 6	41,657	40	30	16	16	—	2	2	—	36	36	28	1	
District II.—														
Ward No. 7	20,646	14	16	6	5	—	2	3	110	23	23	19	11	
Do. 8	50,781	36	45	3	1	—	3	—	—	31	31	19	11	
Do. 9	49,472	47	56	3	3	—	2	4	50	49	49	31	10	C. R. M. GREEN, Major I.M.S., Special Health Officer.
Do. 10	22,668	16	25	4	2	—	3	3	40	23	23	13	11	
Do. 11	20,761	19	15	—	—	—	—	—	—	8	8	3	—	
District III.—														
Ward No. 12	6,083	3	2	1	1	—	1	—	—	—	—	—	—	
Do. 13	28,366	18	22	6	5	—	2	3	6	10	10	14	—	
Do. 14	29,207	19	21	3	2	—	1	6	91	21	21	10	—	The reported cases of plague and the general mortality were both lower than in the preceding week.
Do. 15	13,218	9	14	—	—	—	—	2	12	10	10	6	—	
Do. 16	4,620	1	5	—	—	—	—	1	—	1	1	—	—	2. If plague and suspected plague deaths be deducted from the total mortality, the remainder is 103 below the average of the last five years. But cholera, small-pox, fevers, and bowel-complaints together were 116 below the five years' average, which accounts for the low mortality from causes other than plague, and should have been allowed for last week in estimating the diminution in the population from the mortality.
Do. 17	4,598	4	2	—	—	—	—	1	4	2	2	2	—	
Do. 19	4,820	28	32	—	—	—	—	3	7	12	12	11	—	
District IV., West—														
Ward No. 18	33,147	5	2	—	—	—	—	—	—	—	—	—	—	
No. 23	23,020	8	7	1	—	—	—	—	3	3	3	—	—	
No. 24	22,831	11	11	—	—	—	—	—	17	5	5	—	—	J. N. COOK, Health Officer.
No. 25	42,591	15	27	—	1	—	—	—	36	15	15	—	—	
District IV., East—														
Ward No. 20	14,804	16	21	1	—	—	—	3	9	13	13	5	—	
Do. 21	15,340	22	9	—	—	—	—	—	9	9	9	2	—	
Do. 22	26,833	32	41	18	17	—	—	3	53	36	36	27	—	
Total	649,401	514	548	98	83	—	20	61	—	526	450	296	47	

RETURN showing Information regarding PLAGUE under the following Headings for the Week ending 29th April 1899.

Division.	Population by Census of 1891.	Mortality. Average Weekly	Actual Mortality for the Week.	Number of Plague Cases Discovered.			Number of Patients removed to Hospital.	Number of Suspicious Cases not Definitely Diagnosed as Plague.	Number of Persons under Observation in Private Quarters.	Houses Inspected and Deaths Inquired into.		Premises Disinfected.	Premises Vacated.	Remarks.
				Attacks.	Deaths.	Total.				Houses.	Deaths.			
1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	12.	13.	14.	15.
Address uncertain -	—	—	—	2	1	—	2	—	—	—	—	—	—	Plague, 107 deaths, against 83 last week. General mortality 108 in excess. Last week the excess was 34. Ward 3 showed 23 deaths from plague and suspect, leaving only 9 deaths from other causes, against a five-yearly average of 33.
District I., North—														
Ward No. 1 -	36,885	30	34	5	5	—	—	9	—	52	19	25	2	
Do. 2 -	26,614	19	15	3	3	—	—	4	—	25	11	12	2	Wards 8, 9, 10, 11, 13, and 25 still show a high mortality.
Do. 3 -	36,431	33	32	10	9	—	1	14	—	67	33	33	8	
District I., South—														There is apparently more plague than is reported in Wards 11, 24, and 25.
Ward No. 4 -	34,828	30	34	9	9	—	—	—	—	15	27	20	5	
Do. 5 -	39,180	33	32	7	7	—	—	—	—	15	19	18	1	There has been a slight increase in the amount of plague in the past week.
Do. 6 -	41,657	34	42	11	9	—	3	—	—	20	24	29	2	C. R. M. GREEN, Major, Special Health Officer.
District II.—														
Ward No. 7 -	20,646	14	22	5	8	—	1	1	363	14	14	14	7	
Do. 8 -	50,781	41	56	8	9	—	2	3	423	44	44	40	14	The reported plague cases and the general mortality show an increase on the preceding week.
Do. 9 -	49,472	50	66	11	10	—	2	—	75	50	50	34	21	
Do. 10 -	22,668	16	27	3	3	—	—	4	122	24	24	20	14	J. N. COOK, Health Officer.
Do. 11 -	20,761	17	28	2	2	—	—	—	46	10	10	8	5	The 4th May 1899.
District III.—														
Ward No. 12 -	6,083	2	2	—	—	—	—	—	—	—	—	4	—	
Do. 13 -	28,366	19	33	8	7	—	1	10	51	26	26	23	—	
Do. 14 -	29,207	18	27	4	3	—	1	4	37	22	22	11	—	
Do. 15 -	13,218	11	16	5	4	—	—	2	20	12	12	11	—	
Do. 16 -	4,620	1	2	—	—	—	—	—	4	3	3	—	—	
Do. 17 -	4,598	3	5	2	2	—	—	—	7	2	2	4	—	
Do. 19	4,820	30	22	1	—	—	1	5	12	20	20	7	—	
District IV., West—														
Ward No. 18 -	33,147	6	3	—	—	—	—	—	—	1	1	—	—	
Do. 23 -	23,620	7	8	—	—	—	—	—	—	—	—	—	—	
Do. 24 -	22,831	12	18	1	1	—	1	1	38	12	12	8	—	
Do. 25 -	42,591	12	26	1	1	—	—	1	50	32	32	14	—	
District IV., East—														
Ward No. 20 -	14,804	17	20	—	—	—	—	1	24	11	11	9	—	
Do. 21 -	15,340	18	10	—	—	—	—	—	6	8	8	3	—	
Do. 22 -	26,833	33	34	19	14	—	—	2	32	25	25	25	—	
Total -	649,401	506	614	117	107	—	15	61	1,310	510	449	372	81	

C. R. M. GREEN, Major, I.M.S., Special Health Officer.

J. N. COOK, Health Officer.

W. R. BRIGHT, Chairman.